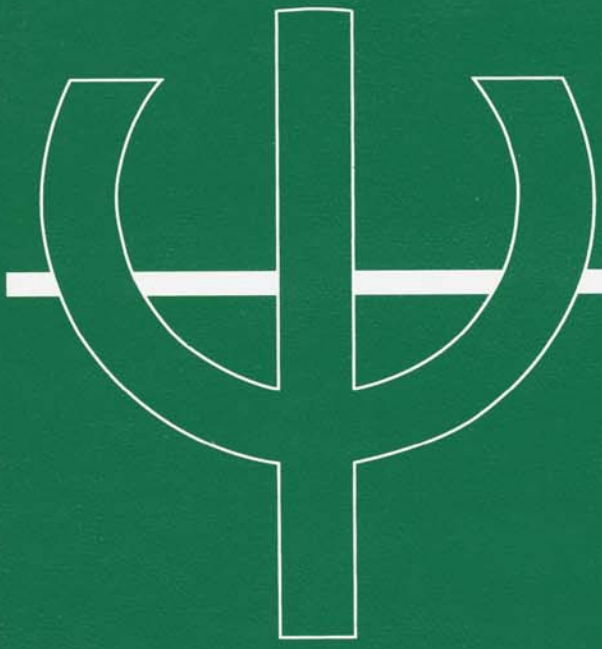

ADVANCES IN PSYCHOLOGY

75

Freud Evaluated

The Completed Arc

Malcolm Macmillan

North-Holland

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NORTH-HOLLAND
AMSTERDAM • NEW YORK • OXFORD • TOKYO

FREUD EVALUATED

The Completed Arc

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1991

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PREFACE

Freud Evaluated had its origins in a series of lectures to undergraduate students in the Department of Psychology at Monash University. In the beginning the lectures had dealt separately with the twin themes of psychoanalytic personality theory and the application of scientific method in psychology but, as the series developed, it became apparent that aspects of the evolution of the theory could be used to demonstrate some of the principles of scientific enquiry. Both an earlier version of *Freud Evaluated* and the present one reflect that aim.

Because the lectures relied so much on original sources it was hard to provide reading and suitable reference material. I attempted to meet that need by expanding my lecture notes into the manuscript of the first version of *Freud Evaluated.* The work was begun during some spare time I found during a sabbatical leave in 1972-73, in the Department of Physical Education - Women at the University of Wisconsin, Madison, and the final draft was completed in Melbourne during the summer vacation of 1973-74. Soon after its completion I found myself dissatisfied with what I had done and was tempted to undertake a revision. It was one occasion on which I should have resisted Oscar Wilde's advice because, having yielded to that temptation, I found myself gradually succumbing to another: that of producing the most comprehensive critique of psycho-analytic personality theory I could. *Freud Evaluated* is therefore very different from a simple lecture supplement and is virtually a new work.

In the lectures I had avoided secondary sources and interpretative accounts as much as I could and wherever possible drew on the publications with which Freud was familiar, especially the works of Charcot, Bernheim, Janet, Meynert, Jackson, and Darwin. Much of the light which those works provided for illuminating Freud's thinking in the lectures and in *Freud Evaluated* was provided by the skilled translations of Lillias O'Dea, North Melbourne, Drs. P. J. Weir and Andrew Wood, Melbourne, and Dr. James W. Coleman, Department of German, University of San Francisco. Lillias' translations were especially important: her multilingual skills enabled me to compare, among other things, the original of Charcot and Bernheim with Freud's German translations and with the various English translations. Most of the re-translations are also hers.

To display a serious critical interest in Freudian theory in Australia is, as elsewhere, to declare oneself a member of a special kind of minority: one whose numbers guarantee intellectual isolation. Having written *Freud Evaluated* in a virtual intellectual vacuum, I am therefore more than usually appreciative of the encouragement which several of my colleagues and friends gave me and of the critical reading which a number of them made of draft chapters. I am particularly indebted to Emeritus Professor William O'Neil, Professor Frank Cioffi, Professor Ross Day, and Dr. Dianne

Bradley who each read all or almost all of them. The late Professor Oliver Zangwill, who read the whole manuscript and discussed much of the detail with me, was, of course, kindly and encouraging as well as penetrating and direct in his criticism. Oliver and Professors E. R. ('Jack') Hilgard, Stanford University, Max Coltheart, then of Birkbeck College, and John Kihlstrom, University of Arizona were good enough to allow me to visit their departments, mainly to use nearby libraries, and they also arranged for me to give seminars. For helping make some of the drafts more literate, and especially for improving my treatment of theoretically obscure points, I am much in the debt of Liz Gallois and Lindsay Image of Melbourne. I am also more than pleased to acknowledge much valuable information very generously provided by Peter Swales.

I have been helped a great deal by the clerical, professional, and technical staff of my own Department. *Freud Evaluated* could not have completed had it not been for the typing, computer programming, electronic, and photographic skills shared by Lesley Anderson, Nan Appleby, John Dick, Mike Durham, Jan Gipps, the late Desi Green, Vladimir Kohout, Lola Pasieczny, Lynne Steele, and Pam Ward. Help from Graeme Askew and Graeme Ivey, of the Monash University Educational Technology Service, and James Meehan, of my own Department, made the camera ready copy possible and Dr. Garry Thorp provided most of the assistance for the index. Cathy Cook and Cheryl Roberts, Technical Officers in my Department, worked very hard at finding references and Cathy, who had also carefully checked so many of the drafts, worked just as cheerfully in the final stages at solving what threatened to become an endless series of problems.

I have also to thank various Libraries, Librarians, and Library Staff for the rapidity with which they were able to meet my requests. My special thanks go to the Librarians and staff of the Biomedical Library of Monash University, the Brownless Library of the University of Melbourne, the Australian Medical Association Library, Melbourne, the Middleton and Memorial Libraries of the University of Wisconsin, Madison, the Stanford and Lane Medical Libraries of Stanford University, Cambridge University Library, Birkbeck College Library of the University of London and the British Library.

Finally, to two of my friends I am especially grateful. Professor Ross Day, the Chairman of my own Department provided encouragement and formidable critical advice as well as funds which defrayed the costs of research assistance and translation preparation not met by Grants from the Monash Special Research Fund. My very special thanks go to Dr. Leonie Ryder not only for an enormous amount of critical help but also for encouraging me to begin and, once having started, to persevere.

Box Hill and Clayton, Victoria, May 1990.

CONTENTS

Preface v

Introduction 1

PART I BEGINNING ASSUMPTIONS

Ch.1 Anna O. and the origins of Freud's personality theory 10
Ch.2 Charcot, hypnosis and determinism 32
Ch.3 Freud, determinism and hysteria 55
Ch.4 Freud's adaptation of Breuer's treatment method 74
Ch.5 Causes and the actual neuroses 122

PART II FIRST THEORIES AND APPLICATIONS

Ch.6 Mechanisms of symptom formation 146
Ch.7 A theory of the neuroses 167
Ch.8 Expectations, actual neuroses and childhood seduction 200
Ch.9 Dreams and symptoms 232
Ch.10 A theory of sexuality 281

PART III THE FINAL SYNTHESIS

Ch.11 The ego and the ego-instinctual drive 328
Ch.12 The instinct theory finalized 368
Ch.13 The structures of the mind 430

PART IV EVALUATION

Ch.14 Psycho-analysis as theory and therapy 508
Ch.15 Psycho-analysis as a method 549
Ch.16 Psycho-analysis as science 577

REFERENCES 613

INDEX 661

ACKNOWLEDGEMENTS 668

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INTRODUCTION

> Alles Gescheite ist schon gedacht worden;
> man muss nur versuchen, es noch einmal zu denken.
> Goethe: *Sprüche in Prosa. Maximen und Reflexionen, I.*

Freud Evaluated: The Completed Arc is a critical evaluation of Freud's personality theory which, because it is historically based, provides an evaluation very different from most. What I do is to describe the observations which Freud made and set out the theoretical ideas he put forward for explaining them. I then try to judge the adequacy of Freud's explanations against the logical and scientific standards of Freud's own time. It is largely this historical basis which leads me to believe that *Freud Evaluated* is a justified addition to what seems to be veritable torrent of books on Freud. My hope is that the historical perspective will give the reader a sound basis on which to make a judgement about psycho-analysis as a method of investigation and a theory of personality as well as a sense of what Freud was about from Freud's own standpoint.

Freud in context

I site Freud's endeavour, particularly the first twenty years of it, in the psychological and psychiatric context of the time. The period has not been given the critical attention it warrants, despite the important work of Andersson (1962), Stewart (1969) and Sulloway (1979). All of Freud's important assumptions and characteristic modes of thought are to be found in this formative period. Many of the tests of his theoretical propositions were also simpler then than they later became and the sources of many of the current difficulties in psycho-analytic theory more readily identifiable.

By examining the early period, one sees more clearly the continuity of Freud's thought with that of his predecessors, especially with Charcot's, and that it was not until about 1900-1905 that he developed a theory radically different from any that had gone before. Bringing out these kinds of continuities is not meant to detract from Freud's originality. What I hope it does is to allow his contributions to be seen as the development of already existing trends. To me, the specific characteristics of Freud's approach then seem more distinct than when they are related only to a general intellectual context, as in Ernest Jones' (1953-1957) account, or to a general social context as in Ellenberger's (1970) history.

Placing Freud in the psychological and psychiatric context of his day also brings out more clearly the basis of a number of the unresolved problems of contemporary psycho-analytic theory. For example, Breuer's original notes on Anna O. show how he and Freud misinterpreted the

significance of her treatment. Rather than being a specific if limited therapy for hysteria it was a quite typical patient-initiated and directed treatment. Breuer's notes also confirm that the affective quality of early psycho-analytic treatments were actually retrospective re-interpretations. In the fullness of time, affect became libido and Breuer's specific cathartic method became a more complicated but just as specific reliving transference therapy. Some of the basic problems about the role of libido and the essential features of therapy - problems which currently plague psycho-analytic theorists and clinicians - are the result, I argue, of Breuer's and Freud's misunderstanding their original case observations.

The core criticism

Although what I have said might suggest that *Freud Evaluated* is primarily an historical work, that is not the case. The core of the evaluation centres on Freud's basic method for gathering data - free association. The method is not much written about and hardly ever criticised. I believe what I have to say about it is new and that my criticisms are much more substantial than the few which have been made. A very large amount of contemporary psycho-analytic literature is also examined in order to trace how various present-day theoretical difficulties have their roots in Freud's originally inadequate observations and methods, in the faulty framework he adopted for identifying causes, and in his poorly formulated theoretical concepts. So infrequently is methodological criticism of this kind made in works on psycho-analysis that mine almost amount to a novelty.

There are some genuine novelties, too. Of them, the most important relate to the deterministic assumptions on which the psycho-analytic method of observation is based. Delboeuf's experimental investigations of the hypnotic phenomena demonstrated by Charcot, which he observed practically at the same time as Freud, are used to establish Freud's naïveté about the determinants of psychological phenomena, a naïveté which was carried over to the method of free association. I also believe *Freud Evaluated* to be the first major work on the development of Freud's ideas in which Masson's edition of the complete Freud-Fliess correspondence is used. From the letters, I have been able to show that Freud began his search for the causes of hysteria with the explicit intention of extending his already formulated but incorrect hypotheses that sexual factors caused the 'actual' neuroses.

Although *Freud Evaluated* is a very critical work, I do not believe there is any other appraisal which allows Freud and his colleagues and followers to speak so directly for themselves. My method of working is largely responsible. For the most part my analyses of Freud's observations and theoretical concepts were made before I turned to the psycho-analytic literature. I have to say that I was more than a little surprised to find that so much of what I had independently arrived at was stated explicitly in it, although mostly in a fragmentary and unorganised way. However, the

degree of dissatisfaction psycho-analysts have with the theoretical aspects of their discipline is not widely known outside it. *Freud Evaluated* will let the intelligent reader in on what is almost a professional secret and, I believe, does so comprehensively. Psycho-analytic critical writings have three characteristics. First, as I have already noted, the criticisms tend to be isolated and not related to one another. One might find an absolutely devastating argument for doing away with Freud's concept of instinctual drive, for example, in which the effects of so abandoning it on hypothesised processes like repression are not considered. Second, although many critiques begin with a summary of the earlier literature, particularly of Freud's own writings, they display little real historical sense. That concepts change is usually made very evident but the reasons for the changes are not brought out so clearly. Third, the criticisms never probe the evidence very deeply. Occasionally issues involving the interpretation of data are raised, as for example in the charge of a masculine 'bias' to Freud's developmental theses, but the status of the data itself and the method by which it was gathered is hardly ever questioned. Free association, having been immaculately conceived by Freud, is maintained in its virginally pure state by even the most thorough of psycho-analytic critics.

The place of historical evaluation

Historical analyses, or even historically based analyses, are not common. In fact, combining historical and logical methods runs counter to one of the most influential modern arguments which says that historical considerations are irrelevant for judging the validity of scientific theories. The argument derives from Popper (1959), who emphasised that theories should be tested through the logical consequences which can be derived from them. In his view, the test of a scientific theory is identical with testing the consequences of a deductive argument. An hypothesis can be regarded as the premise of an argument from which factual consequences may be derived. Just as the premise of a logical argument is rejected if its conclusions or logical consequences are false, so an hypothesis is rejected if the predicted factual consequences are at variance with observation. Popper believed science progressed through successive falsifications of its hypotheses. Sometimes the rejection of a single, crucial hypothesis was supposed to lead to the rejection of the theory from which it derived. More often though, a set of complex judgements about the relevance of a number of such disconfirmations were required to overthrow a theory (Popper, op. cit., p.50). Whatever the complexities, theories were to be discarded because observation falsified their logical consequences.

For Popper, non-logical considerations are irrelevant. It does not matter what the propounder of a theory intended, what motives led to the theory being formulated, or how it evolved. Only its present logical consequences allow it to be confirmed or rejected. All that matters are the consequences of the tests to which it can be put. While the history of a

theory may be interesting socially or in its own right, analyses from those points of view are quite different from scientific evaluations. Feigl sums up this position:

> It is one thing to retrace the historical origins, the psychological genesis and development, the social-political-economic conditions for the acceptance or rejection of scientific theories; and it is quite another thing to provide a logical reconstruction of the conceptual structures and the testing of scientific theories. (Feigl, 1970, p.4)

At least since Kuhn (1962) reservations have been expressed about this conclusion. History shows that in reality scientific theories are rarely accepted or rejected simply because of the tests of their consequences (Lakatos, 1970; Feyerabend, 1970). Nor do scientists work by systematically trying to falsify their theories and by probing the possibilities of giving them up.

In fact, the two rather different disciplines of history and logic may complement each other. It is even possible that historical evaluation can help to solve some of the problems that have so far defeated the logicians. An historically based evaluation sets the record straight and enables us to see what the relations between the empirical facts and the theoretical constructs really were. That clarification in itself assists in the process of evaluation. For example, suppose the attempt to confirm a theory fails. By itself, the failure provides no guide as to where the fault lies. Perhaps the original facts were inaccurately described or the original theoretical terms inadequately formulated. Would it not be sensible to see how those terms or statements were arrived at? Was there a worthwhile theory to begin with? Until the relation between fact and construct is clarified, we cannot tell whether the theoretical ideas were required by observation alone, by theory alone, or by some combination of theory and observation. In brief, historically based evaluations help us establish what has to be explained and whether any explanatory effort is justified. We are also placed on more certain ground in deciding which kinds of evidence should count as confirmatory and which as disconfirmatory.

A third reason for undertaking an historical analysis is that it sometimes helps to solve two related problems in the conventional interpretation of Popper's falsification principle. Both have so far resisted logical solution. The first is that of determining whether a given negative observation or experimental result is really crucial. That is, does a particular failure to confirm an observation require the theory to be rejected? The related problem is that of determining when the point has been reached at which successive disconfirmations require the theory to be abandoned. We certainly need help in making these decisions. No well-defined logical threshold separates good science from bad; by itself, Popper's principle of falsification does not even demarcate science from pseudo-science, as he thought.

An example may illustrate my points. At one stage, Freud's theory of neuroses required him to assume that certain childhood sexual experiences recalled by his patients had actually occurred. Later, after concluding that they were reporting false recollections, he formulated an alternative explanation based largely on the assumption that a sexual drive having quite specific characteristics existed in childhood. Since then, a number of observational and clinically based studies has failed to confirm the existence of a drive having those qualities. Now, by themselves, these later disconfirmations did not (and could not) point to Freud's misrepresentations of the little observational data he did have, or to the theory-driven nature of the original conceptualisation and its internal contradictions, or to the restricted range of alternatives which had led him to his particular formulation. Nor did the failures illuminate just what it was that required explanation.

I would not want to over-value the virtues of historically based enquiries, even were I to succeed in demonstrating them. Not only is the history of each science unique, but empirical and theoretical factors may interact differently at different stages in the development of the one discipline. Consequently, it is not possible to make a statement about the value of historical evaluations which holds for scientific theories in general. One has only to think of the fate of Kuhn's thesis that scientific theories developed through the effects of revolutionary paradigm shifts on the scientists' mundane puddling about in his or her normal work-a-day world. Without claiming to great knowledge of the history of science, I have the distinct impression that there are now few historians of science who feel Kuhn did much more than describe very broadly *one* of the ways changes sometimes come about. Analogously, whatever it is that can be established about the value of an historical evaluation of psycho-analysis may not apply elsewhere.

Some psycho-analysts have made appreciations of the value of historical analyses not entirely inconsistent with mine. Thus, after discussing the effects of the separation of the historical sciences from the natural sciences on the methods of enquiry used by psycho-analysts, Klauber (1968) concluded that placing psycho-analytic theories in an historical context followed a mode of explanation which had been used "impressively" with other sciences. He also thought the historical method might be of value for assessing the significance of theoretical controversies in psycho-analysis. Even though they agree that historical analyses say nothing about the scientific status of a theory, Ellman and Moskowitz (1980) point out two "extra-theoretical ... instructive" features of them. First, understanding the origins of a theory may help in determining whether an adherence to the analogies which theories tend to generate has been detrimental to the theory's own development. Second, historical analyses may aid in showing if a theory and its analogical model have become confused.

A plan

The journey through *Freud Evaluated* is a long one. To make it as easy as possible the book is divided into four parts. In Part I, I try to establish what the initial assumptions were which Freud brought to his study of psychological phenomena and the neuroses. Because most of them are related directly or indirectly to the treatment of the patient known as Anna O., I begin with a detailed consideration of her case. The assumptions examined include those about the role of ideas and the importance of affect as determinants of symptoms, Freud's naïve deterministic views about the causes of hypnotic and hysterical phenomena, and the faulty framework he developed for gathering and evaluating data about the causes of neuroses. Although Freud did not develop the method of free association until much later, the assumptions underlying it also underlie the methods he did use and I make the problems with it explicit.

Part II contains a description and an evaluation of Freud's first theoretical ideas and his applications of them. His claims for the mechanism of repression are compared with those of its rivals. No basis is found to choose it over them. The sources of the theory of the neuroses Freud formulated with Breuer are traced and the theory itself formally set out and assessed. I find it to be based on concepts of nervous system functioning which were out of date even in Freud's day, not to have a genuine logical structure, and not to generate acceptable explanations. I argue that Freud's expectations about the causes of neuroses were incorrect and they led him astray in investigating what he called the psychoneuroses. The same expectations were also responsible for the childhood seduction hypothesis and its collapse. From the evaluation of the new theories in Part II, I conclude they did not explain dreams or symptoms or childhood sexuality and they do not provide an adequate general theory of the mind or of sexuality.

The final synthesis which Freud arrived at is set out in Part III. In it I examine how Freud introduced the concepts of ego and ego-instinctual drives to his theory of the mind and conclude that their introduction was a consequence of the earlier sexual theory. For a fragile childhood ego to control an instinctual childhood sexual drive, it had to be of similar strength and only something like an ego-instinctual drive would be able to provide it with that kind of energy. My analysis brings out the general unsuitability of the notion of instinctual drive which Freud adopted and the fact that conceptually it was not able to power the ego's functions in the way he required. The introduction of the death instinct and the tripartite apparatus of id, ego, and super-ego are seen as somewhat more remote consequences. Difficulties with the ego-instinct laid the foundations for the death instinct. Because the death instinct could not be found a home in the earlier theory of the mind, Freud had to construct an id, an ego, and a super-ego to house it. However, the death instinct itself is inadequate conceptually and Freud was forced into making inconsistent assumptions

about it. Nor is Freud able to describe adequately how the mental structures form or how they carry out their functions.

I bring the preceding criticisms together in Part IV. First there is a general evaluation of psycho-analysis as a personality theory. In it I pay particular attention to the constituents of the personality and their supposed origins, citing what empirical data there is bearing on their validity. I try to clarify the relevance which the marginal effectiveness of psycho-analysis as a type of therapy has for judging its validity as a theory of personality. Evidence for the validity of the method of free association is then examined in detail and the status of psycho-analysis as a science is discussed. My conclusion is that Freud's method is neither capable of yielding objective data about mental processes nor of potential value for those seeking to turn psycho-analysis into an acceptable historical or humanistic discipline. Because some may find it puzzling that such negative conclusions can be made about a putative science which is so widely believed in, I bring *Freud Evaluated* to an end with some speculations about the continuing appeal of Freud's ideas.

I believe the historical evaluation I have attempted shows where the major weaknesses of psycho-analytic personality theory lie and that it helps to clarify just what it is that requires explanation. I also think it complements the more usual methods of evaluation. Whether my judgement has more or less value than this can be assessed only by those who persevere to the end of Chapter 16.

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PART I

BEGINNING ASSUMPTIONS

Chapters 1 to 5

1 Anna O. and Origins

2 Charcot, Hypnosis and Determinism

3 Freud, Determinism and Hysteria

4 Freud's Adaptation of Breuer's Therapy

5 Causes and the Actual Neuroses

1 ANNA O. AND THE ORIGINS OF FREUD'S PERSONALITY THEORY

Sebastian: This is a strange repose, to be asleep
With eyes wide open...

Shakespeare: *The Tempest*, Act II, i.

During the night of 17th-18th July, 1880 a twenty one year old girl anxiously awaited the arrival of the surgeon who was to operate on her seriously ill father. Sitting alone at the patient's bedside, her right arm over the back of her chair, she went into a state of mental *absence* and:

> During this state she hallucinated black snakes that crawled out of the walls, and one, that crawled up towards her father, to kill him.
>
> Her right arm had become anaesthetic through its position and her fingers turned into little snakes with death's heads (the nails).
>
> It seems probable that she tried to use her paralysed right arm to drive off the snake. When the hallucination had vanished, she tried to pray in her terror, but language failed her, she could not speak any, until at last she thought of a saying in English and found herself able to think and pray in this language only. The whistle of the train, which was bringing the Prof., broke the spell.
>
> The next day her listening for the arrival of the expected Professor, reduced her to such a state of abstraction, that he was finally in the room, without her having heard anything at all ....
>
> as often as the hallucination of the "black snake" occurred evoked by an intensified state of anxiety [angst] or by some snake-like object, her right arm became extended and completely rigid. As often as she listened in an anxious and tense way, she became completely deaf again. (Hirschmüller, 1978, p.350. Cf. Breuer and Freud, 1895, pp.38-39)

Described here, in a fairly literal translation I have had made of the original case notes, is what purports to be the basis of one of the most famous and influential illnesses in history. It is the description penned by the Viennese physician Joseph Breuer, the older friend of the then much younger Sigmund Freud, at the end of his treatment of the pseudonymous Anna O. It sets out what he believed was the root cause of her hysteria (Hirschmüller, 1978). Freud's interest in what came to be called the psychoneuroses was first aroused by this case and it was his collaboration with Breuer on similar ones that decisively influenced the discipline he was to found. It is a good place with which to start a critical evaluation of Freud's personality theory.

In Part I of this book, which consists of Chapters 1 to 5, I try to bring out the assumptions on which Freud based his study of this and other cases of neuroses. The problem Freud set himself was to explain how symptoms like Anna O.'s hallucinations and paralyses were isolated from the patient's normal consciousness, that is, how these symptoms were outside the control of the patient. In discussing his explanations, I make explicit his views of what constituted the legitimate determinants of psychological phenomena and I evaluate the methods on which he based his treatment and how he went about establishing causes. I argue that Freud's views of determinism were wrong and that his causal analyses led him to an incorrect identification of the causes of neuroses. Free-association, Freud's basic method of treatment and for gathering data, is shown to be based on these faulty deterministic assumptions.

In this Chapter I argue that the case of Anna O. provided the shakiest of foundations on which to build either a theory or a therapy of hysteria. Not only do the case notes tell us quite definitely that she was not cured, they reveal with especial clarity that neither Breuer nor Freud understood the extent to which she shaped the treatment or the significance of her doing so.

My main reason for using the original case notes rather than the later published account (Breuer and Freud, 1895) is that they throw doubt on the affective interpretation of Breuer's therapy. Breuer's early description of the treatment is significantly different from the later account in placing little or no emphasis upon Anna O. expressing previously unexpressed emotions. There is, in other words, little indication that what Anna O. called her "talking-cure" required her to "abreact" while she recalled and relived the emotionally charged circumstances under which she had acquired her symptoms. My argument is that this affective interpretation of Breuer's "cathartic method" is a re-interpretation of the talking cure that originated with Freud some ten to twelve years later. By that time, as I shall argue in Chapters 2 and 3, Freud had come to such a peculiar view of the determinants of psychological phenomena that he was unable to appreciate just how much he was contributing to what he was investigating and treating.

BREUER'S OBSERVATIONS

Breuer first attended Anna O. in November, 1880, for a nervous cough she had developed during a general deterioration in physical health attributed to an over-zealous nursing of her very sick father. Although the snake hallucination is supposed to have occurred before Breuer's first visit, only minimal signs of disturbance were evident to her family. But, according to Breuer's original case notes, her "peculiar behaviour" led him to diagnose her immediately as "mentally ill" (Hirschmüller, 1978, p.352). This peculiar behaviour included strange sleep-like, auto-hypnotic states in the

afternoons that were sometimes replaced by a heightened excitement, as well as momentary *absences* during which Anna O.:

> would stop in the middle of a sentence, repeat the last words only to continue talking again after a short pause. (Hirschmüller, 1978, p.353. Cf. Breuer and Freud, 1895, p.24)

She seemed only partly aware of the fact that an *absence* had occurred and she had no knowledge of what happened during it. Much later, Breuer reconstructed the events of this *first phase*, which began in about mid-July, 1880 and lasted until 10th December of that year. According to it, Anna O. again experienced the hallucination during the *absences* and a second state of consciousness began to develop around it. Toward the end of the phase she rested more and more in the afternoons, 'waking' in the evening to the sleep-like state that now usually preceded the excited condition. Breuer described this phase as one of latent incubation remarking that, unlike most such phases, the events occurring in it were to become completely accessible to him. In the original case notes, muscular twitches or jerks and a not unimportant facial neuralgia were also reported but these symptoms are not described in any detail in the later account (Hirschmüller, 1978, pp.349-350. Cf.Breuer and Freud, 1895, pp.21-41).

By 11th December Anna O. was so ill she became a bed patient herself. The *second phase* of the illness then commenced immediately with what Breuer called "apparently quite new" symptoms (Hirschmüller, 1978, p.352. Cf. Breuer and Freud, 1895, p.23). These included headaches, complex disturbances of vision, a convergent squint, paralysis of the neck muscles, and contractures and anaesthesias of the arms and legs. Anna O. now complained of having:

> two selves, a real one and an evil one which forced her to behave badly and so on. More and more clearly it became evident that ... she had two entirely distinct states of consciousness which became all the more clearly distinctive, the longer her illness lasted. In the one state she recognized her surroundings, was melancholic and moody, but relatively normal, in the other state she hallucinated, was "naughty"; if after the lapse of this phase something had been changed in the room, or someone had entered or gone out, she would complain: of having lost some time and would remark upon the gap in her train of conscious thoughts ....
>
> These interruptions gradually increased till they reached the dimensions that have just been described, and during the climax of the illness ... she was almost never normal ... during the day. (Hirschmüller, 1978, p.353. Cf. Breuer and Freud, 1895, p.24)

The second state of consciousness stabilised as the illness progressed and was essentially a prolongation of the *absences*.

A complex speech disturbance also manifested itself during this time. Anna O. gradually lost her speech. Initially she was unable to speak in

grammatically correct German and later she was unable to speak at all for two weeks. Breuer hypothesised that the speech loss had come about after she had been "offended by her father and had decided not to ask after him any more". Breuer then "forced her to talk about him" and she regained her power of speech completely (Hirschmüller, 1978, p.354. Cf. Breuer and Freud, 1895, p.25).

Breuer made a related observation about her mood changes:

> during her *absences* in the day-time she moved around in a definite sphere of thought ...
>
> In the afternoon she lay as if somnolent and in the evening she complained: "torment, torment". At first accidentally, and later, when we learned to pay attention to it, on purpose, someone would repeat ... a word which was connected with these thoughts, and soon she ... began ... to tell a story in the style of Andersen's Picture-Book or a fairy-tale .... A few moments after the end she then awoke, was obviously calmed down, or as she herself called it "comfy" ["gehäglich"].... The stories were all tragic, some of them very charming, but mostly dealt with the situation of a girl anxiously sitting by the bed-side of a sick man. (Hirschmüller, 1978, p.354. Cf. Breuer and Freud, 1895, pp.28-29)

A degree of what can be called deliberateness now became apparent:

> We noticed, e.g. that she would occasionally, during the day, speak such words as desert, when I then gave her the cue-word "desert" in the evening, she would start a story about someone lost in the desert etc. (Hirschmüller, 1978, p.354. Cf. p.357)

Or, as Breuer later said, during these daytime *absences*:

> she was obviously creating some situation or episode to which she gave a clue with a few muttered words. It happened then - to begin with accidentally but later intentionally - that someone near her repeated one of these phrases of hers while she was complaining about the 'tormenting'. She at once joined in and began to paint some situation or tell some story. (Breuer and Freud, 1895, pp.28-29)

Breuer encouraged her to tell the stories, for he found the comfortable or normal state that ensued lasted until the following day. Concurrently her physical symptoms were somewhat relieved.

After her father's death in April, 1881, the *third phase* of the illness commenced. Not having been allowed to see her father during the previous two months, she was totally unprepared for his death. Two days of stupor followed and more symptoms developed: all human beings looked wax-like, and she could not differentiate one from another; she developed a pronounced "negative instinct" toward her relatives; and she now lost even the ability to understand German. She did not seem to see the famous psychiatrist Krafft-Ebing, who had been called in as a consultant (Hirschmüller, 1978, p.357), and his forcible breaking down of this

negative hallucination by blowing smoke into her face caused her to fall to the ground unconscious. A severe anxiety attack then ensued. Breuer calmed her down with difficulty but on his next visit several days later he found:

> she had been abstaining from food the whole time, was full of feelings of anxiety, her *absences* full of horror images, with death's-heads and skeletons. (Hirschmüller, 1978, p.357. Cf. Breuer and Freud, 1895, p.27)

Breuer now observed that if during her evening auto-hypnoses she could be persuaded to talk about her hallucinations she would come out of that state:

> calm and cheerful, would sit down to work, and draw or write far into the night, completely rationally, she would go to bed around 4 o'clock and in the morning the same thing began again. It was a truly remarkable contrast, between the irresponsible patient in the day-time pursued by hallucinations and the completely clear-headed person at night. (Hirschmüller, 1978, p.357. Cf. Breuer and Freud, 1895, pp.27-28)

Over the next six or seven weeks, as the two states of consciousness became more distinct, Breuer continued encouraging her to talk about her hallucinations. A more tolerable state ensued for at least a part of each day but there was little other effect on the mental symptoms and none at all on the physical. In fact, her condition so deteriorated, with suicidal gestures becoming so very frequent, she was forcibly transferred to the Inzersdorf Sanatorium outside of Vienna on 7th June, 1881. She returned to Vienna itself only at the beginning of November.

With the exception of five weeks during July and August, Breuer saw her daily during this part of the third phase. After her return to Vienna she extended her talking to include some bizarre behaviours, not reported at all in the published account of the case, that Breuer referred to as *caprices*. The first *caprice* she talked about appears to have been 'stocking-wearing':

> In the evening when she ... was put to bed, the patient had never allowed her stockings to be removed ... she woke up at 2 or 3 o'clock ... complaining about the slovenliness of letting her sleep with her stockings on. One evening she told a true story which had happened a long time ago, of how she used to sneak into her father's room, to listen (she was no longer allowed to do night duty at that time), which was why she slept in her stockings, and of how her brother had caught her once and so on. (Hirschmüller, 1978, p.360)

Since the stockings had been left on during a different state of consciousness from that into which she woke, she naturally enough had no recollection of how she came to be wearing them. The *caprice* of stocking-wearing ceased after she told Breuer about its origin.

Talking about the origins of her temporary inability to drink had a similar effect:

> For 6 weeks during the hottest time of the year, she did not drink a single drop and quenched her thirst with fruit and melons. At the same time of course she complained about her tormenting thirst, but when water was brought to her lips, she could not be persuaded to take a single drop, without giving any explanation. At last one evening she told how she had seen her lady-companion's small dog, which disgusted her, drink from a glass of water, and how she had said nothing, in order not to appear rude (many weeks ago). 5 minutes later she complained of thirst, drank 1/2 a bottle of water and from then on her inhibition about drinking disappeared. (Hirschmüller, 1978, pp.360-361. Cf. Breuer and Freud, 1895, pp.34-35)

The disappearance of this minor symptom seems to have marked the end of the third phase of the illness and the beginning of the *fourth phase*.

Breuer was "astonished" at the removal of the stocking-*caprice* and the inability to drink. He seems then to have systematically exploited Anna O.'s willingness to talk during her evening auto-hypnoses, concentrating on the origins of her contractures, paralyses, anaesthesias, disturbances of hearing and vision, and her other symptoms. It is Anna O.'s talking about the origins of these major symptoms that properly constitutes what she named, in English, as the "talking cure" and it's use defines the fourth phase.

During the fourth phase a quite remarkable feature of the disorder appeared. In endeavouring to clear up the remaining symptoms, Breuer found Anna O. spent a good deal of each day in living through the events of the same day exactly one year earlier. According to him, entries in her mother's diary confirmed the accuracy of the re-enactments. Over a period of six months, from January to June 1882, Breuer had to contend with the 1881 memories as well as those of the corresponding day in 1882. Two independent sets of recollections were now required in order to reconstruct the complex chronology of each symptom. For, unless Anna O. talked about *each* of the many appearances of a symptom in a particular order, it was not removed. And that order had to be exactly the *reverse* of the order of its many manifestations. Because of this complexity, Breuer decided to hypnotize Anna O. and make up a list of each of the times on which a given symptom appeared:

> I used to visit her in the morning and hypnotize her ... I would next ask her to concentrate her thoughts on the symptom we were treating at the moment and to tell me the occasions on which it had appeared. The patient would proceed to describe in rapid succession and under brief headings the external events concerned and these I would jot down. During her subsequent evening [auto] hypnosis she would then, with the help of my notes, give me a fairly detailed account of these circumstances. (Breuer and Freud, 1895, p.36)

This induced hypnosis, which is not described in the case notes at all, provided an *aide mémoire*: Anna O.'s "talking cure" continued to take place only in the evening auto-hypnoses.

Even with the list the evening work of recollection was laborious. For example, there had been some three hundred times when she had failed to hear something said to her and these were classified into seven groups: 108 instances when she had not heard someone come into the room, 27 instances of not understanding when several people were talking, and so on. Each instance had to be classed correctly in its group, then placed correctly in the sequence, and then talked about in reverse order. Central to these recollections was the hallucinatory image of the snake about to bite her father. Its re-enactment ended the fourth phase of the illness. According to Breuer's 1895 account "the whole illness was brought to a close" although "it was a considerable time before she regained her mental balance entirely" (op. cit., pp.40-41).

Psycho-analytic mythology tells a different story about the termination of treatment. Anna O. is supposed to have developed a pseudo-pregnancy in which she fantasied Breuer as the father of her child. However, as Ellenberger (1972) remarked long ago, neither of the original sets of case notes mentions the pseudo-pregnancy. Breuer, the mythology continues, is supposed to have calmed Anna O. down and to have left Vienna immediately with his wife for a "second honeymoon", during which Breuer's second child was supposed to have been conceived. By the simple expedient of consulting the birth registry, Ellenberger (1970, p.483) showed the daughter to have been born three months *before* the supposed incident.

Breuer himself was wrong in implying that she was cured. Although neither described in his published account nor referred to publicly by him or Freud, within five weeks of the close of treatment, Anna O. had the first of four relapses. On 12th July, 1882 she was admitted to the Sanatorium Bellevue, Kreuzlingen, Switzerland, where she remained until 29th October, 1882 (Ellenberger, 1972; Hirschmüller, 1978, pp.152-156, 362-364). Many symptoms remained: the hysterical features, speech disorders, alterations of consciousness, and the facial neuralgia were all still present. Not surprisingly, she was described as criticising "in an unfavourable manner the inadequacy of science in the face of her suffering" (Hirschmüller, 1978, p.364). Her symptoms seem not to have changed by the time she was discharged from Bellevue.

During the next five years Anna O. was re-treated in the Inzersdorf Sanatorium on three occasions for a total of ten months (30.7.1883 to 17.1.1884, 4.3.1885 to 2.7.1885, and 30.3.1887 to 18.7.1887). In the admissions book, from which Hirschmüller derived these data, diagnoses were recorded only in general terms. For Anna O. it was "hysteria" each time. She was pronounced "better" on the second occasion and "cured" after the first and third. Binswanger wrote to Breuer on 13.1.1884 that she was "quite healthy, without pain, or anything else". But in January and May, 1887, Freud's fiancee wrote to her mother that although Anna O. was

apparently quite normal during the day she still suffered from hallucinations as evening approached (E. Jones, 1953-1957, II, p.225).

No one knows how she was finally relieved of her symptoms. Some time before the end of the 1880's she was symptom free, however. She then commenced a quite notable career, now being regarded as the founder of the profession of social work in Germany.

THE SIGNIFICANCE OF "THE TALKING CURE"

Breuer, as we have said, believed Anna O.'s symptoms to be hysterical. Before going on to discuss what he made of her case, let us consider some alternative diagnoses. Her symptoms were, of course, quite severe and the immediate outcome of the treatment quite limited (E. Jones, 1953-1957, I, p.225; L. Freeman, 1972) the degree of which became apparent only with Ellenberger's (1972) paper and Hirschmüller's (1978) book. Diagnoses other than hysteria have therefore been suggested. First, several writers, mainly American psycho-analysts, have proposed or implied the lack of success might have been because Anna O. was a schizophrenic or borderline psychotic (Goshen, 1952; Reichard, 1956; Karpe 1961; Bram, 1965; Schur, 1972, p.38; Martorano, 1984; Masterson, 1984; Noshpitz, 1984; Spotnitz, 1984). The overuse of the diagnostic category 'schizophrenia' in the United States, as well as the continuing confusion between it and the category 'multiple personality' (Bliss, 1980; Greaves, 1980; Marmer, 1980; M. Rosenbaum, 1980; Boor, 1982), is probably responsible for the retrospective diagnoses of schizophrenia. While a schizophrenia might not be inconsistent with the initial outcome it is not consistent with her contemporaneous correspondence, which lacks any sign of schizophrenic thinking (e.g. Hirschmüller's 1978, pp.369-370, 379-380).

Nor does a schizophrenia or borderline condition fit well with the long term features: the absence of cognitive or social impairment as well as the intellectually complex, demanding, and socially useful life she led in the struggle for women's rights, for relief for refugees and orphans, and against prostitution (Jensen, 1970; L. Freeman, 1972). For much the same reasons, "substituting the fashionable term 'borderline state' is not the answer", as M. Rosenbaum (1980) particularly warns. Another 'mental illness' explanation is Pollock's (1972, 1973) proposal that Anna O. suffered from pathological mourning or melancholia. He assumes it took several years to clear up and implies that the diagnosis explains the partial remissions. However, Pollock produces no facts and what he presents as evidence consists of psycho-dynamic interpretations of doubtful validity.

More recently, Eysenck (1985/1986), Thornton (1986), and Orr-Andrawes (1987) have argued that Anna O. suffered from some organic malady, either by itself or that her hysteria was based on such an illness. The difficulties in establishing any retrospective diagnosis with certainty

are enormous and it is worth noting first of all that none of these writers agree with one another. At best we have an organic pathology of an unspecified and indeterminate kind. Second, one finds that these opinions are based on very partial readings of Breuer's case notes. Thus Thornton claims that Anna O. had "fits". Now, the only convulsions Breuer observed occurred "in the last few months" of treatment, prior to the end of June, 1882. Others were recorded among the withdrawal symptoms as she was treated for morphine addiction in the Bellevue Sanatorium but only *after* Breuer's own therapy had ended (Hirschmüller, 1978, pp. 367-369, 362-363). This late appearance, some eighteen months after the frank onset of the disorder, is not at all consistent with tubercular meningitis. Third, in arguing for Anna O. having that disorder, neither Thornton nor Eysenck seem to put much weight on Breuer's exclusion of that very possibility (Hirschmüller, 1978, p.354). Given Breuer's medical skill, one may presume the exclusion to have been reasonably soundly based. Thornton practically dismisses Breuer's consideration of it altogether. Thus, she claims - quite incorrectly - that Breuer "recounts the entire case history from an entirely psychogenic basis" (Thornton, 1986, p.132).

In general, Thornton does not conduct her search for an organic pathology with much balance. Thus, she goes so far as to describe one of Bernheim's "hysterical young woman" subjects as "i.e. a victim of epilepsy or other neurological disease" and therefore to classify the negative hallucination induced in her as "genuine". Thornton took its probable basis to be an epileptic discharge. In turn, these considerations led her to a similar interpretation of Anna O.'s negative hallucinations, particularly the one of Krafft-Ebing (Thornton, 1986, pp.138-139). Her equating the hypnotic trance with a psychomotor seizure is based on a similarly unbalanced view of the evidence (op. cit., pp.90-96).

Thornton also claims it was the cough and "the introduction of the famous cathartic method" that led Breuer to rule out meningitis (op. cit., p.139). However, the latter forms no part of Breuer's basis for his conclusion: he implicates the temporary disappearance of the aphasia after she woke from her afternoon sleep and all the other data on the variability of her symptoms (Hirschmüller, 1978, p.355). Anna O.'s story telling and letter writing rules an organic aphasia out completely. What is relevant here is neither the content of what she said nor its effect but that she spoke and wrote at all. As is the case with some hysterical symptoms, her aphasia was quite variable. Fourth, Anna O.'s later life, free as it was of symptoms of any kind, also rules out most of these diagnoses as firmly as it disposes of the psychotic conditions. Finally, there is the independent confirmation of Breuer's diagnosis of hysteria in the records of her subsequent admissions to the Inzersdorf Sanatorium.

In opposition to these 'illness' interpretations, Swales (1986a, January) has suggested that the disorder was simulated. While his argument has some merit, he seems not to appreciate the diagnostic significance of

Breuer's observations that the paralyses and contractures were not diminished in intensity during sleep, which they would have been had they been simulations.

Were it the case that Anna O.'s symptoms had an organic basis in whole or in part or that she was a simulator the foundation she would then provide for psycho-analysis would be even shakier than I argue. My main point is not that there is no serious challenge to Breuer's diagnosis. Rather it is that, although Breuer pictured Anna O. as the inventor of the "talking-cure", he failed to appreciate what lay behind her invention, domination, and direction of it. Freud, as well as most of the recent commentators, also missed its significance. Ellenberger (1970, p.484) seems to have been the first to point out that the direction Anna O. gave the treatment exactly parallels a number of classic eighteenth century cases of what were undoubtedly hysterical illnesses in which, during either induced or auto-hypnosis, patients instructed those treating them about the causes of their symptoms and the methods of cure. Examples are the cases reported by Deleuze (1813) and Bertrand (1823), both cited by Dingwall (1967, I, pp.16, 49). The apparently absolute accuracy of Anna O.'s memory for the events of a year before and the intensity of the treatment also have their parallels in these and other cases mentioned by Ellenberger. More recently, van der Hart and van der Velden (1987) have drawn attention to the case of Rika van B. treated in 1851 by the Dutch physician Andries Hoek. Not only were the features of guidance of the treatment prominent - Rika van B. predicted various aspects of the course of her illness, including her recovery, and she instructed Hoek how to treat her - but in her regularly induced hypnotic state she recalled the circumstances under which her symptoms were acquired, reliving them with emotional expression in the later *waking* state.

Features like these were regularly reported in the mesmeric press of the nineteenth century and were not confined to the early period (Dingwall, 1967, I). In about 1900, Edgar Cayce, the American 'medical clairvoyant', diagnosed the cause of his aphonia by "inspecting" his own vocal cords while hypnotized by another. Cayce then instructed his hypnotist to give him a suggestion to increase the circulation to the affected parts. When administered by the hypnotist Cayce's self-prescription restored his voice. Interestingly, his hypnotist knew of similar self-diagnoses from the work of de Puységur, the early mesmerist (Cerminara, 1950).

As an aside, it is worth making the point that there is a good deal of confusion in the literature about Breuer's use of hypnosis. Most of it centres around his induction of hypnosis, it being asserted or implied that Breuer hypnotized Anna O. in order that she might recall the symptom-producing events (e.g. Ferenczi, 1908/1950; Freud, 1893a, pp.30-33; 1910a, pp.12-13; 1925a, p.20; Nichols and Zax, 1977, p.29). Breuer induced hypnosis only during the final stages of treatment (mainly in 1882), and then only in the mornings, and solely to obtain the list of topics to be dealt

with during the subsequent evening auto-hypnosis. Even the context in which Breuer referred to the simple methods of hypnotic induction he used suggests they had not been in use before that time (Breuer and Freud, 1895, p.36). As if to emphasise this point, it is stated in the *Preliminary Communication* that the therapeutic observation of Anna O. "was made possible by spontaneous auto-hypnosis on the part of the patient" (Breuer and Freud, 1893, p.7) and, in summing up the case, Breuer speculated whether it was only possible:

> to discover the state of affairs in other patients by means of some such procedure as was provided in the case of Anna O. by her auto-hypnosis. (Breuer and Freud, 1895, p.44)

Breuer's original case notes also only describe the talking cure as taking place in the evening somnolent, hypnotic, or "cloud"-hypnosis states.

The parallels between Anna O.'s case and the earlier ones of self-directed cures make it most unlikely the "talking cure" could provide a firmer foundation for understanding hysteria than they had. Her contribution to her treatment therefore bears examination. Initially, *she* had spoken of the hurt done to her by her father and, from Breuer's later description, it can be seen it was actually *she* who played the crucial role in overcoming the speech inhibition even though it was Breuer who "forced" her into speaking:

> As *I knew*, she had felt very much offended over something and had determined not to speak about it. When *I guessed* this and *obliged* her to talk about it, the inhibition, which had made any other kind of utterance impossible as well, disappeared. (Breuer and Freud, 1895, p.25. My emphasis, MBM)

Breuer's treatment strategy was based upon the expectation that overcoming the specific suppression would result in a more general effect. But it was Anna O. who had somehow communicated to Breuer both her feeling of offence and the deliberateness of her decision not to speak. It was his response to her communication that set the train of therapeutic events into motion.

Breuer's response is too consistent with what Ellenberger (1966) has called the concept of the "pathogenic secret" for it to have been merely idiosyncratic. From the earliest times and in many kinds of societies illnesses of the most varied types have been seen as both the result of secrets and as being curable through their disclosure. By the time Breuer came to treat Anna O., the concept was well established in religion, literature, and criminology, as well as in the specialised literature on hypnosis and hysteria. Ellenberger specifically mentions a recognition of the concept by the mesmerist de Puységur and its explicit use by the Viennese physician Moritz Benedikt as the fundamental principle of his method of treating hysteria, a view of Benedikt's treatment already taken

by Andersson (1962, pp.114-116). Breuer's first step in the treatment of Anna O. was therefore consistent with a body of belief that the general speech function would be restored once the specific 'secret' had been revealed. This would seem to be the significance of Breuer's later remark that it was through his observations of the speech inhibition that "for the first time the psychical mechanism of the disorder became clear" (Breuer and Freud, 1895, p.25). Anna O. may well have shared this belief about the role of secrets. It was not restricted to medical circles. In any case, she created a situation that demanded Breuer force her to reveal hers.

The second step in the treatment was Anna O.'s telling of the fairy-tale-like stories. Each story was based upon a situation she had "obviously" created earlier during the day and each elaboration required the presence of another person to repeat a word or some of the words from that earlier situation if her mood was to be made more "comfortable" afterwards. A major influence on this step seems to have been a very widely accepted "medical" interpretation of Aristotle's doctrine of the essential role of catharsis in tragedy, one much discussed among Viennese intellectuals in the very year that Anna O.'s illness began. This medical interpretation came from Bernays (1857/1970) and had generated enormous interest at the time of its original publication in 1857 and again on its re-publication in 1880. According to Lain Entralgo (1958/1970, p.186) some one hundred and fifty works appeared soon after its first appearance and Ellenberger (1970, p.484) cites Dalma as having shown a similar enormous concern with the relation between catharsis and drama in Vienna following its re-publication. Coupled with the pronounced interests of Breuer and Anna O. in the theatre, that interest could have created a set of beliefs in both of them about the likely effects of story-telling. And those effects would only have been to calm her general mood and not to alter her symptoms. [1]

Bernays argued that the catharsis Aristotle had mentioned in his *Poetics* (VI) was the process by which the audience watching a tragedy were purged of the emotions of fear and pity (Bernays, 1857/1970). What Aristotle actually meant is totally opaque. There are difficulties in knowing which parts of the ten word clause in which the word "catharsis" is used relate to which other parts (Butcher, 1902, p.254, n.1; Lain Entralgo, 1958/1970, p.195, n.27) and this basic ambiguity also interferes with attempts to relate the clause to other passages in the *Poetics*. As Else notes:

> The isolation and difficulty of the catharsis-clause are indeed notorious .... But critics and philologists are not the men to be daunted by lack of evidence: the mass of writing about [the clause] is almost in inverse

1. Quite independently of me, Hollender (1980) has arrived at a similar conclusion: "it is likely that Anna O. was introduced to catharsis as a method and then put it to use as a means of capturing and holding the attention of her scientifically minded physician". My argument goes further, of course.

> proportion to the extent of visible material. (Else, 1963, p.225. Cf. Lain Entralgo, 1958/1970, pp.185-186)

The connection of the thought in the *Poetics* with the *Politics* is unclear and there has always been uncertainty about where Aristotle's doctrine is to be placed in the corpus of other Greek writing on catharsis (Susemihl and Hicks, 1894; Butcher, 1902; Bywater, 1909; Lain Entralgo, 1958/1970; Else, 1963; Hardison, 1968). For what my opinion is worth, I am convinced the catharsis clause ought to be interpreted from within the *Poetics* itself (e.g. Else, 1963, p.228). There is even something to be said for interpreting it to mean that catharsis purifies the emotions of pity and fear in order that the audience can experience the same pleasure from the tragedy as from other dramatic forms and learn from the events portrayed in it in the same way (Hardison, 1968, pp.113-117).

Bernays' thesis was in a very long line of medical and quasi-medical interpretations construing catharsis as removing fear and pity from the soul, much as a suitable medicine might purge the body of a disease (Bywater, 1909, pp.152-153, 361-365). What was novel about Bernays' argument was how he envisaged the mechanism of purgation. Most other commentators had been vague. Milton, one of the few to be explicit, had thought of catharsis as a kind of homeopathic reduction. The effect of arousing pity and fear was:

> to temper and reduce them to just measure with a kind of delight, stirr'd up by reading or seeing those passions well imitated. Nor is Nature wanting in her own effects to make good [Aristotle's] assertion: for so in Physic things of melancholic hue and quality are us'd against melancholy, sowr against sowr, salt to remove salt humours. (Milton, Preface to *Samson Agonistes*, 1671)

For Bernays, however, purging came about not because of a *reduction* of the emotions but because of their *discharge*:

> The tragedy causes by (stimulation of) pity and fear the alleviating discharge of such (pitiful and fearful) emotions.
>
> [die Tragödie bewirkt durch (Erregung von) Mitleid und Furcht die erleichternde Entladung solcher (mitleidigen und furchtsamen) Gemüthsaffectionen]. (Bernays, 1857/1970, p.16)

According to Bernays, discharging pity and fear in this way expelled them or removed them from the soul, brought about a short-term pleasure and over a longer period, although not permanently, quieted the disturbing feelings.

Bernays' interpretation is open to grave objection. It is not really clear that Aristotle meant emotions were *discharged* (Susemihl and Hicks, 1894, pp.641-650; Else, 1963, pp.225 and n.14, 227 and n.18, 439-442). Nor do we really know what Bernays meant by an *alleviating* discharge. But,

however construed, it does not seem to me to require the audience to *express* the emotions portrayed in the tragedy so much as to *experience* them. In Chapter XIV of the *Poetics* Aristotle even has pity and fear being calmed by a mere listening to an account of a properly constructed tragedy. I believe the same would be true for Bernays; the passive spectator merely watching a tragedy would experience catharsis.

Whether Bernays' interpretation of Aristotle is correct or not is, of course, irrelevant. What matters is whether Breuer and Anna O. agreed with it for it is, as Else (1963, p.440) aptly describes it, "inherently and indefeasibly *therapeutic*". Bernays' was "the prevailing 'medical' interpretation" and continues to dominate "most thinking on catharsis" (op. cit., p.225, n.14). Anna O.'s days were filled with hallucinatory fears and her evening stories with pity for a poor nursing girl in a fearful situation - precisely the emotions Aristotle had incorporated into his definition of the tragedy. To someone interested in the theatre it might not have seemed strange that telling stories involving precisely those emotions would benefit the teller's general emotional state. And Breuer was just such a person. Quite apart from Bernays' book or his own general cultural background, Breuer had a special and highly developed interest in the Greek drama (Meyer, 1928). He could hardly have been ignorant of Aristotle's theory. Anna O.'s expectations would at least have matched Breuer's. She was a person of considerable culture (Jensen, 1970), who conceivably knew of Bernays' book directly, and who had, on the evidence of her later writing, a pronounced theatrical talent. Long after her treatment with Breuer she wrote stories and plays in which pity for the central character was the dominant motif (Karpe, 1961).

There is an additional more direct connection of Anna O.'s hysteria with the theatre. In response to being restricted to an extremely monotonous life she "wallowed" in:

> her highly developed poetic-imaginative talent. While everyone believed her to be mentally attentive, she lived through her fantasies, but when addressed was always mentally present immediately, so that no-one knew about this. This became, under the name of "private theatre" an established part of her mental life. (Hirschmüller, 1978, p.349. Cf. Breuer and Freud, 1895, p.22)

Anna O.'s repetition of the word "tormenting" was a hint that she be allowed to enact the situation or episode created during the daytime *absence*. Once the prompt of the cue-word or words muttered during the *absence* was provided, Anna O. responded with the lines from her rehearsal. Breuer's expectations then allowed for the elaboration of a calming story that was little more than another performance in her private theatre. This time, however, Breuer joined her in the audience to watch the melodrama of the pitiful girl fearfully nursing her sick father (Bram, 1965). Ellenberger (1970, p.256) also implicates the "theatrical and affected way of life" in Vienna during the 1880's as a cause of high prevalence of hysteria

at that time, a thesis very plausibly supported by Bolkosky's (1982) analysis of the effects of problems of identity and communication upon Viennese society and its citizens. True or not, it is at least the soil on which a "cathartic method" could grow.

The medical and other literature of the time devoted to altered states of consciousness might also have led Breuer to think of Anna O.'s two states of consciousness in theatrical terms and therefore as modifiable by catharsis. For although the number of cases of what would now be regarded as multiple personality reported by then was not more than five or six the amount of discussion generated by them was considerable (Taylor and Martin, 1944; Sutcliffe and Jones, 1962). Nor was the discussion limited to the medical literature; in the nineteenth century such cases were thought to be especially relevant to questions about the nature of the self and the structure of the personality. Taine, an enormously influential French philosopher, cultural historian, literary critic, and one of the founders of French psychology, devoted a substantial part of his *De L'Intelligence* to double consciousness and multiple personality. He used a striking theatrical metaphor to sum up the implications of these cases:

> The human brain is a theatre where, on several planes, several different plays are staged simultaneously, but only one of which is illumined. (Taine, 1873, p.16)

Of a case of mediumistic possession he concluded in similar vein:

> Certainly one finds here a *doubling* of the self [*dédoublement* du moi], the simultaneous presence of two series of parallel and independent ideas, of two centres of action, two psychological persons juxtaposed in the same brain, each with a different mission, one at centre stage and the other in the wings. (op. cit., p.17)

This metaphor was much quoted, and the paragraphs in which it appeared much referred to, for example by Ribot (1884/1910, p.122n.), Janet, P. (1886), and Binet (1889, 1892,1892/1896). One may presume its central theatrical point was in even wider circulation, possibly being known to Breuer directly, for he was familiar with many other works by Taine (Meyer, 1928). He must almost certainly have known of a similar metaphor used by his friend and colleague, Ewald Hering, describing consciousness as a stage and ideas as actors (Hering, 1870/1913, p.8).

If talking about the hallucinations simply extended what had already happened in telling the fairy-tales, no more than a general calming of mood would be expected. And that was what happened. As we have seen, Anna O.'s symptoms were not only unchanged, her condition so deteriorated she was hospitalised. The "talking cure" proper began only when Anna O. began to talk about the *origins* of the *caprice* of stocking-wearing and the inability to drink. The expectation would then have been that that talking would remove both the *caprice* and the inhibition of drinking - both being

based on secrets (the secret listening, the suppressed remark to the lady-companion). There would also be an "alleviating discharge" of emotion resulting in a general, more comfortable mood. Again, that is exactly what happened. Ellenberger has proposed Bernays' reading of Aristotle as a candidate for the therapy as a whole. On my reading of the case it can only be a candidate for the initial calming of Anna O.'s mood. Only one fact is not accounted for by this explanation: why did Anna O. take so long to bring about her final 'cure'?

Anna O.'s direction of the treatment continued until the very end. She decided how and when it would terminate:

> The patient herself had formed a strong determination that the whole treatment should be finished by the anniversary of the day on which she was moved into the country ... At the beginning of June, accordingly, she entered into the 'talking cure' with the greatest energy. On the last day - by the help of re-arranging the room so as to resemble her father's sickroom - she reproduced the terrifying hallucination ... which constituted the root of her whole illness. (Breuer and Freud, 1895, p.40)

In proposing this re-staging (which is not mentioned explicitly in the original notes although the hallucination itself is) Anna O. nominated the hallucination of the snake as the cause of her illness. Over the course of her illness Anna O. had combined features of two cases described some fifty years earlier by Colquhoun:

> One of them announced repeatedly, several months previously, the day, the hour, and the minute of the access and of the return of epileptic fits. The other announced the period of his cure. Their previsions were realized with remarkable exactness. (Colquhoun, 1833/1970, p.172)

We have already seen that the phenonema were known many years before these two cases (Dingwall, 1967, I). What was novel about Anna O.'s 'prevision' was how it directed Breuer to reconstruct the evolution of her illness.

If Breuer thought he had come upon a new and powerful method for treating hysteria, which Freud certainly thought he had, he was profoundly mistaken. Not only was Anna O. not cured but she had misled him about the cause of the temporary remission of her symptoms. How had she been able to do this? At the time treatment began he was a most distinguished general practitioner who had made two major and recognised contributions to scientific research. As a newly graduated physician he had undertaken original, basic physiological research into some of the regulatory mechanisms of respiration and his conclusions had been communicated by Ewald Hering to the Academy of Sciences (Ullman, 1970). Later he was proposed by Mach, Hering, and Exner for Corresponding Membership of the Academy, a distinction not often granted those lacking University titles or appointments (Meyer, 1928). In between, and quite independently of Mach and Crum-Brown, he had established the role of the semi-circular

canals in maintaining balance and discovered the function of the otoliths (Breuer, 1923; Camis, 1928/1930; Ullman, 1970). Undertaken in a laboratory established in his own home, and maintained entirely at his own expense (Meyer, 1928), his was the delicate surgical work contributing most to the discovery (Camis, 1928/1930; Schlessinger, Gedo, Miller, Pollock, Sabshin, and Sadow, 1967). The most eminent of Vienna's medical men were among the patients in his private practice (Breuer, 1923).

Although Breuer had had only the ordinary medical training of his day in psychiatry (Hirschmüller, 1978, pp.120-131) and lacked specialist experience in the treatment of neurotic disorders, his general scientific training and his extensive medical experience prepared him, almost as well as anyone in that era could have been prepared, for the treatment of Anna O. Yet the 'talking cure' was not, and could not be, a sovereign remedy for hysteria. Knowing little about the history of either hypnosis or hysteria, Breuer failed to see that Anna O.'s 'talking cure' was simply one of many therapies that patients experiencing spontaneously occurring hypnotic states devise to instruct those caring for them to bring about their ephemeral 'cures'.

BREUER'S INFERENCES

Having missed the main significance of the case what then were the inferences Breuer drew from it? Unfortunately, they are not known in any detail from any contemporaneous source. The 1882 notes contain neither theoretical concepts nor empirical generalisations. In a letter to Forel some twenty-five years later Breuer wrote:

> This much, however, I believe I can say: What follows immediately from the case of Anna O. is mine - that is to say, the aetiological significance of affective ideas, deprived of their normal reaction, which operate permanently like psychical foreign bodies; 'retention hysteria'; the realization of the importance of hypnoid states in the development of hysteria; analytic therapy. (Cranefield, 1958)

Breuer's recollection of the last three points may be accepted as they are but analysis is needed of the role he said he gave affective ideas.

"Analytic therapy", the process by which the memories of the various manifestations of the symptoms were classified, ordered, and connected to some root cause, had not been attempted previously. "Retention hysteria", the generation of hysterical symptoms by the withholding of a response, was new. And, if one excludes Charcot, the role given "hypnoid states" was peculiarly Breuer's.

Breuer used the term hypnoid to refer to those mental states resembling ordinary hypnosis. For him hypnoid states ranged from reveries filled with emotion, such as day-dreaming, through the fatigue following protracted

emotion, such as after prolonged nursing of a loved relative, to the true auto-hypnoses. All of these varieties of hypnoid state were present in Anna O., her illness showing a progression from the milder to the more marked forms. Just as in deliberately induced hypnosis, where it had been known for many years that there might be spontaneous amnesia in the waking state for the events of the trance (Chertok and Saussure, 1973/1979, pp.157-165), so Anna O.'s memories for the events taking place in her hypnoid states were lost. The absence of memory provided the starting point for Breuer's explanation. Anna O.'s symptoms were based upon sensations and perceptions first experienced in an hypnoid state. Her paralysis, for example, was an elaboration of her inability to move the arm that had gone to sleep; her hallucinations simply repeated what were originally false perceptions. However, she could not become aware of the causes of those symptoms simply because she could know nothing about what happened during the hypnoid condition. None of the memories arising in it could be assimilated into her normal consciousness. The creation of this split off section of the mind, this second state of consciousness, was, in Breuer's view, the fundamental precondition for hysterical symptoms to form.

It is obvious that ideas played a major part in the generation and removal of Anna O.'s symptoms but it is much less easy to accept the common interpretation of Breuer's claim that he recognised the etiological role of affective ideas "immediately". On the orthodox interpretation, Breuer required Anna O. to recall the memories of *emotionally charged events* and to give *full vent* to the previously unexpressed affect when she did so. Two things are wrong with this claim: it is quite inconsistent with what Breuer *describes* as actually happening in the treatment and, as will be seen later, it is also inconsistent with too many aspects of Freud's adoption of what he came to call 'Breuer's method'.

Neither in the original case notes (Hirschmüller, 1978, pp.348-364) nor in the *descriptive* parts of the published account (Breuer and Freud, 1895, pp.21-41) does Breuer stress *emotional* expression. Rather, what he emphasises is *verbal* utterance. Some examples make this clear. While all of the Andersen-like stories had sad or tragic themes, *emotional* arousal during the telling of them is nowhere mentioned. Indeed, some were described as "very charming" and as like freely-created poetical compositions rather than as responses to the pressure of strong emotion (Hirschmüller, 1978, p.354. Cf. Breuer and Freud, 1895, p.29). Breuer's later portrayal of Anna O. had her "shaking with fear and horror" as she reproduced and gave verbal utterance to the "frightful images" of the hallucinations that pursued her after her father's death (Breuer and Freud, 1895, pp.29-30) but, in the original case notes, where the images are described as "horror images", there is no mention of her talking about them at all. Nor are emotional reactions mentioned in the account of the removal of the stocking-wearing *caprice*. Perhaps of greater significance is their absence from the original description of Anna O.'s telling of the dog drinking out of the glass. In the later-published account we find that that

emotional quality has actually *been added.* Paragraph 1 of Figure 1.1 is from Breuer's original case-notes and Paragraph 3 is from the published account. In Paragraph 2 I have added the affective phrases from the published account to the original. From this it is clear Breuer uses the words "with every sign of disgust" to characterise Anna O.'s description of the scene before interpolating the phrase "after giving further energetic expression to the anger she had held back" between the end of the description and the request for water (Breuer and Freud, 1895, pp.34-35. Cf. Hirschmüller, 1978, pp.360-361).

1. ... one evening she told how she had seen her lady-companion's small dog, which disgusted her, drink from a glass of water and how she had said nothing, in order not to appear rude (many weeks ago). 5 minutes later she complained of thirst, drank 1/2 a bottle of water ... (Hirschmüller, 1978, p.361)

2. ... one evening she told, *with every sign of disgust*, how she had seen her lady-companion's small dog, which disgusted her, drink from a glass of water and how she had said nothing, in order not to appear rude (many weeks ago). 5 minutes later, *after giving further energetic expression to the anger she had held back*, she complained of thirst, drank 1/2 a bottle of water ...

3. ... one day during hypnosis she grumbled about her English lady-companion whom she did not care for, and went on to describe, with every sign of disgust, how she had once gone into that lady's room and how her little dog - horrid creature - had drunk out of a glass there. The patient had said nothing, as she had wanted to be polite. After giving further energetic expression to the anger she had held back, she asked for something to drink ... (Breuer and Freud, 1895, pp.34-35)

Figure 1.1. Breuer's interpolations

Breuer's original case notes make clear and direct reference to only a verbal mechanism by which Anna O. was relieved of the pathological stimuli with which she was burdened:

> When ... everything *that had been added* during the five weeks of my absence had been *worked off* ... we achieved this good state. From all this it was clear that every product of her abnormal mental activity, whether it was a spontaneous product of her imagination or an event which had been interpreted by the pathological part of her psyche (sentire), affected her as a psychical stimulus and persisted until *it had been narrated*, after which its effectiveness completely ceased to operate. (Hirschmüller, 1978, p.360. My emphasis, MBM. Cf. p.361)

Breuer's later re-working of the paragraph makes the point more clearly:

> every one of the spontaneous products of her imagination and every event

> which had been assimilated by the pathological part of her mind persisted as a psychical stimulus until it had been narrated in her hypnosis, after which it completely ceased to operate. (Breuer and Freud, 1895, p.32)

The overall mechanism Breuer described in the later account was also clearly a verbal one: "stimuli" accumulated in the mind during the secondary state of consciousness and had to be "removed", "disposed of" or "worked off" (op. cit., pp.29, 31, 32, 34, 35) by being given "verbal expression" or "narrated" (op. cit., pp.31, 32). Talking about these accumulated stimuli "relieved" or brought about an "unburdening" of them (op. cit., pp.30, 45).

Consistent with this, almost each time Breuer made specific mention of his systematic exploitation of the talking cure in the later account he spoke of symptoms being removed by being "talked away" rather than through the emotion associated with them being discharged (Breuer and Freud, 1895, pp.35-37). Again and again Breuer uses phrases appropriate to only a *verbal* process: "narrate the hallucinations", "talked herself out", "verbal expression during hypnosis", "giving regular verbal expression", "calmed by verbal utterance", "verbal utterance of her hallucinations", "giving verbal utterance to her phantasies", "brought to verbal utterance", and so on (op. cit., pp.27, 30, 31, 32, 34).

Nowhere in the original case notes nor in the descriptive parts of the later account, then, is emotion nominated as that which is to be removed, disposed of, or worked off. There is, in other words, little indication in the description of what Anna O. called her "talking-cure" of her being required to "abreact" the emotionally charged memories of the circumstances under which she had acquired her symptoms.

Even had emotional expression been involved, modern opinion is very sceptical about its therapeutic value:

> clinical experience tells us that catharsis and abreaction by themselves are effective in grief reactions and perhaps traumatic neuroses, and then merely to a limited extent. They are not usually effective *in treating hysterical symptoms*. (Hollender, 1980. My emphasis, MBM.)

Nichols and Zax (1977) express similar reservations about the value of catharsis. They go on to emphasise the cognitive role of the "verbal utterance", placing it even more centrally than emotional expression. Hollender (1980) also stresses the role of talking *per se*: utterance enabled Anna O. to translate the non-verbal message of her symptom "into verbal language".

These modern opinions confirm the conclusions of C. S. Myers (1920-21) and McDougall (1920-1921) in their ancient debate with Brown (1920-1921a, 1920-1921b) on catharsis. All three had used some type of cathartic method to treat war neuroses victims from the 1914-1918 War, Myers for longer than the others. Only Brown thought emotional expression to be

central. Myers had actually cured patients who, following his explicit instruction, revived the memory of the trauma *without* emotion. Although unable to compare this method with Brown's, he did not think its results to be inferior, an opinion with which Brown concurred. Naturally Myers attributed more significance to the effect of reviving the affective and cognitive *experience* than to the effect of expressing the emotions. Brown allowed that some part of his results may have been due to "the faith which I had in psychocatharsis". Of interest here is that Hoek's report of the treatment of Rika van B. (van der Hart and van der Velden, 1987) also emphasised her calmness during the hypnotic reliving. Only in the later waking state was emotion expressed. In this respect, her treatment falls somewhere between the methods of Brown and McDougall.

On the basis of his own psycho-analytic clinical experience, Marmor questioned the assumption that abreaction produced lasting therapeutic effects:

> The dramatic evocation of anger or tears or a repressed memory may, it is true, leave [the patient] feeling transitorily calmer or more relaxed, but I have never seen it, in and of itself, produce the lasting personality changes which are the therapeutic objectives of the psychoanalytic process. (Marmor, 1962)

This is, of course, basically what we find in Breuer's description of the effects of the talking cure in Anna O.

Foreshadowing the detailed analysis in Chapter 4 of Freud's adoption of Breuer's treatment method, it can be confidently stated that it was not until well after the end of 1889, possibly as late as the early part of 1893, that emotional expression during the reliving of traumatic episodes became the basis of his therapy. It will also be seen later, in Chapter 7, that it could not have been until some time after the middle of 1891 that Freud arrived at the theoretical ideas that enabled him to give affect a role in the generation and removal of symptoms.

My view that the expectations of Breuer and Anna O. derived from a "verbal utterance" interpretation of Bernays' thesis is not, of course, inconsistent with the view that Freud's stressing of emotional expression dates from a later period. Although Bernays proposed that the alleviating discharge of emotion took place in the spectator it would have been odd had he believed that under the influence of the performance the spectators in the audience would actually *express* the emotions of fear and pity. Attendance at almost any performance of a tragedy confirms this. Few in the audience express the emotions being portrayed - what they do is *experience* them. If an "alleviating discharge" takes place it does so through the watching itself. Consequently, far from there being a discrepancy between a psychotherapy based on *verbal* utterance or *verbal* expression and Bernays' thesis of catharsis as an *alleviating* discharge, the two express the same idea. Talking about an event involving fear and pity

discharges those emotions in exactly the same way as the watching of a play in which they are represented. Overt emotional expression is quite unnecessary.

What can be accepted is the importance Breuer placed on the accumulation of ideas in hypnoid states as the basis for the generation of hysterical symptoms and on the patient's talking about those ideas if the symptoms were to be modified. Breuer's caution in theorising is also apparent. His major theoretical construct, the hypnoid state, went only a little way beyond the observable and was based on the fairly obvious resemblance it had to the true hypnotic states. Once it had been supposed there might also be amnesia for the experiences occurring in hypnoid states, the isolation of the symptoms followed. The hypnoid state made the pathological ideas unavailable to normal consciousness. Symptoms were due either to the reappearance of the hypnoid state in its entirety or to the intrusion of mental content from that state into normal consciousness. To the extent that verbal utterance rendered pathological ideas powerless it was because it allowed them to be integrated with the primary consciousness.

CONCLUSION

Freud accepted what Breuer took to be the significance of his conclusions altogether too readily. He never seems to have regarded the isolation of the symptom as other than the central problem in the neuroses or talking about them as other than the central fact of therapy. Although his primary purpose in visiting Charcot in 1885 had been to study neurological disorders he soon became much more interested in Charcot's work on hypnosis and hysteria. Very probably this shift came about because Charcot, like Breuer, placed the transformation of ideas occurring in hypnosis-like states as central to the development and isolation of hysterical symptoms. While Freud soon moved beyond Charcot's ideas about the causes of hysteria and the determinants of its symptoms, we shall now see that he never abandoned Charcot's quite incorrect view of the determinants of hypnotic and hysterical phenomena.

2 CHARCOT, HYPNOSIS AND DETERMINISM

> Mangan: Don't you hypnotize me, though. Ive seen men made fools of by hypnotism.
> Ellie: Be quiet. Ive seen men made fools of without hypnotism.
>
> Shaw: *Heartbreak House*, Act II.

Soon after his interest in neurological disorders had been aroused, Freud was introduced to the dispute between Jean-Martin Charcot and Hippolyte Bernheim on the nature of hypnosis. Freud was to become thoroughly familiar with the viewpoints of Charcot and Bernheim. While his attitudes to the issues separating them reveal something of his thinking about psychological phenomena generally, their greatest importance is in revealing some of the more peculiar features of his assumptions about the lawful determination of mental processes. Those features were to persist and have far-reaching consequences for one of the most fundamental of Freud's concepts, that of psychic determinism. It is those assumptions which I consider in Chapters 2 and 3.

During the last quarter of the nineteenth century it was the diametrically opposed views of Charcot and Bernheim that overshadowed most scientific inquiry into the subject. Charcot believed hypnosis to be governed by deterministic laws that produced, with strict regularity, well defined, physiologically based phenomena. Bernheim, on the other hand, proposed the different features of hypnosis simply reflected differing degrees of suggestibility, that is, the essence of hypnosis was to be found in a change in psychological functioning. Charcot thus held to what has since been characterised by Sutcliffe (1960) as a *credulous* position: he believed that the changes in behaviour seemingly produced by the induction procedure reflected real alterations in the subject's physiology. Bernheim's view was what Sutcliffe termed *sceptical*: he had demonstrated that many of the phenomena attributed to the hypnotic trance were to be observed in the waking condition. It was partly on these grounds that he had concluded that the varied features of hypnosis reflected only differing degrees of suggestibility.

Most of the particular issues over which Charcot and Bernheim argued derive from Mesmer's theory of animal magnetism or from the important developments stemming from it. Consequently, a discussion of Mesmer's work and what it led to provides an appropriate backdrop against which to consider Freud's views on the lawfulness of psychological phenomena.

MESMER AND ANIMAL MAGNETISM

The beginnings of modern thinking about hypnosis derive from the work of Franz Anton Mesmer. Because of the similarities between Mesmer's ideas and those of others, his originality has always been doubted. However, his sources have usually been incorrectly identified. Even the knowledgeable Pierre Janet, for example, mistakenly believed Mesmer to have drawn primarily from the Scots physician William Maxwell's compilation of the so-called "wisdom of the ancients". According to Janet, Maxwell:

> regarded all diseases as an outcome of the withdrawal of a vital fluid from our organs, and he believed that a proper balance could be reestablished by simply restoring the requisite amount of magnetic force. (Janet, P., 1919/1925, Vol.1, p.30)

There is now no doubt whatever that Mesmer's ideas are almost all plagiarised from the London physician Richard Mead (Pattie, 1956). Mesmer's medical thesis of 1766 set out Mead's argument that tides in the atmosphere produced by movements of the sun and moon caused disease by disturbing the nervous fluid in the body. One of Mesmer's few original points seems to be a development of an idea of Newton's in proposing the disturbance might be mediated by the force of gravity rather than, as Mead had suggested, through the air itself. By 1775 Mesmer referred to this force as either animal gravitation or animal magnetism (op. cit., p.285). His was clearly not a conventional astrological theory, as is sometimes said. In its final form, Mesmer's theory held the whole of space to be filled with an invisible gas or fluid that, like a magnet, could both attract and repel. The distribution of this medium was said to be disturbed by the movement of the planets (in which he included the sun and the moon). The human body was itself a magnet, divided into poles, one on each side, and illness was due to the planetary movements producing imbalances in the distribution of the magnetic fluid within the body. Because the fluid was subject to the influence of magnets, a proper balance of polarities could be re-established by applying magnets to the body (Mesmer, 1779/1970, pp.54-56).

Mesmer's training had been in medicine and his early medical practice was a quite conventional one (Goldsmith, 1934; Walmsley, 1967). In circumstances that are not entirely clear, the possibility of using magnets therapeutically occurred to him; a possibility at least reinforced, or even suggested, by the results of magnetic treatment achieved by an erstwhile friend, the astronomer to the Court of Vienna, the Jesuit Father Hell [1] (Binet and Féré, 1887a/1887; Sarton, 1944; Walmsley, 1967). Increasing experience with magnets led Mesmer to devise his own highly individual method of treatment and to 'discover' animal magnetism by

1. The name is sometimes given as Hehl. In the interests of English language humour at least it should be noted that the name, which derives from Holl, actually appears as Hell on the title pages of his astronomical works (Sarton, 1944).

simply dropping the references to gravity from his theses. In 1779, shortly after his removal to Paris, he announced that in animal magnetism he had discovered a principle enabling all diseases to be cured.

The following descriptions give some idea of Mesmer's therapy and the response of the afflicted:

> All the world wished to be magnetized, and the crowd was so great that Mesmer employed a *valet toucher* to magnetize in his place. This did not suffice; he invented the famous *baquet* or trough, round which more than 30 persons could be magnetized simultaneously. A circular, oaken case, about a foot high, was placed in the middle of a large hall, hung with thick curtains, through which only a soft and subdued light was allowed to penetrate; this was the *baquet*. At the bottom of the case, on a layer of powdered glass and iron filings, there lay full bottles, symmetrically arranged, so that the necks of all converged towards the centre; other bottles were arranged in the opposite direction, with their necks toward the circumference. All these objects were immersed in water, but this condition was not absolutely necessary, and the *baquet* might be dry. The lid was pierced with a certain number of holes, whence there issued jointed and movable iron branches, which were to be held by the patients. Absolute silence was maintained. The patients were ranged in several rows round the *baquet* connected with each other by cords passed round their bodies, and by a second chain, formed by joining hands. As they waited a melodious air was heard, proceeding from a pianoforte, or harmonicon, placed in the adjoining room, and to this the human voice was sometimes added. Then, influenced by the magnetic effluvia issuing from the *baquet* curious phenomena were produced. (Binet and Féré, 1887a/1887, pp.8-9)

One eye witness described these curiosities in the following way:

> Some patients remain calm, and experience nothing; others cough, spit, feel slight pain, a local or general heat, and fall into sweats, others are agitated and tormented by convulsions. These convulsions are remarkable for their number, duration, and force, and have been known to persist for more than three hours ...
>
> This convulsive state is termed the *crisis*. It has been observed that many women and few men are subject to such crises; that they are only established after the lapse of 2 or 3 hours, and that when one is established, others soon and successively begin. (op. cit., pp.9-10)

Mesmer himself:

> wearing a coat of lilac silk walked up and down amid this palpitating crowd, together with Deslon [i.e. d'Eslon - MBM] and his associates, whom he chose for their youth and comeliness. Mesmer carried a long iron wand, with which he touched the bodies of the patients and especially those parts which were diseased; often, laying aside the wand, he magnetized them with his eyes, fixing his gaze on theirs, or applying his hands to the hypochondriac region [i.e. upper abdomen - MBM] and to the lower part of the abdomen. This application was often continued for hours, and at other times the master made use of *passes*. He began by placing himself *en*

> *rapport* with his subject. Seated opposite to him, foot against foot, knee against knee, he laid his fingers on the hypochondriac region, and moved them to and fro, lightly touching the ribs. Magnetization with strong currents were substituted for these manipulations when more energetic results were to be produced. (op. cit., pp.10-11)

Quoting from Figuier, Binet and Féré added:

> The master, erecting his fingers in a pyramid, passed his hands all over the patient's body, beginning with the head, and going down over the shoulders to the feet. He then returned again, to the head, both back and front, to the belly and the back; he renewed the process again and again, until the magnetized person was saturated with the healing fluid, and was transported with pain or pleasure, both sensations being equally salutary. (op. cit., p.11)

Binet and Féré continued:

> Young women were so much gratified by the crisis, that they begged to be thrown into it anew; they followed Mesmer through the hall, and confessed that it was impossible not to be warmly attached to the magnetizer's person. (ibid)

Mesmer's universal therapeutic principle was nothing more than the re-establishing of the magnetic balance by passes or by the use of the *baquet.*

Mesmer regarded the cures produced (or apparently produced) as proof of the correctness of his theory of the effects of the fluid on the body. Had he made only therapeutic claims, the validity of the theory would have been a relatively easy matter to establish. But Mesmer went further: he claimed the all-pervasive magnetic fluid manifested itself *only* through its effects upon the body and that it was too subtle for detection by any other means. Such subtlety did not seem surprising at the time. As Darnton has shown in his analysis of the social role of mesmeric ideas in eighteenth century France:

> science had captivated Mesmer's contemporaries by revealing to them that they were surrounded by wonderful, invisible forces: Newton's gravity, made intelligible by Voltaire; Franklin's electricity, popularised by a fad for lightning rods and by demonstrations in the fashionable lyceums and museums of Paris; and the miraculous gases of the Charlieres and Montgolfieres that astonished Europe by lifting man into the air for the first time in 1783. Mesmer's invisible fluid seemed no more miraculous. (Darnton, 1968, p.10)

But it was precisely because of Mesmer's supposing the fluid to have its effects *only* upon the body, that it seemed more wonderful than any of these other forces. It was also precisely this that created a special problem for investigating the truth of the fluidic theory and it was the solution to that problem that marked the beginning of scientific enquiry into hypnosis.

EVALUATING MESMER'S THEORY

In the most important of the many controversies about animal magnetism that broke out soon after Mesmer's arrival in Paris two central issues may be discerned. First, was the theory true? That is, did the all pervading fluid actually exist? Second, were the behavioural changes, including the crises and cures, genuine? Initially, most interest attached to the investigation of the validity of Mesmer's theory. But, if the only effects of the fluid were to produce changes in the subjects, how could the investigation proceed? The first of the several Commissions of the French Academy of Sciences or the Royal Society of Medicine of Paris directed part of their enquiries to the *baquet* the function of which was said to concentrate the magnetic fluid:

> The commissioners in the progress of their examination discovered, by means of an electrometer and a needle of iron ... that the *baquet* contained no substance either electric or magnetic; and from ... the interior construction ... they cannot infer any physical agent, capable of contributing to the imputed effects of the magnetism. (Bailly, 1784, p.86)

The Mesmerists, claiming of course the fluid to be 'too subtle' to be measured with such crude devices as electrometers or magnetic needles, rejected this conclusion. The Commissioners were thus forced to find a method by which they could test the hypothesis that the magnetic fluid by itself produced the phenomena. What they did was to vary either the subject's belief about what was happening or the method of magnetization and then observe the effects of those variations on the subject's behaviour.

An experiment upon the subject referred to as Mme.B___ illustrates the procedure. Mme.B___ fell into a magnetic crisis when, while her eyes were bandaged, she was told d'Eslon was magnetizing her. In fact he was not. On a later occasion a magnetizer, concealed behind a screen, went through the motions of magnetizing her. Mme.B___ was initially unaware of his presence and no effects were produced. However, within minutes of being told she was to be magnetized from behind the screen the crisis came on. However the 'magnetism' was produced contrary to the rules said to govern it; in this instance with a 'reversal' of the magnetic polarity. As a consequence of many ''decisive experiments'' of this type the Commissioners concluded that:

> the imagination without the magnetism produces convulsions, and that the magnetism without the imagination produces nothing,... They have concluded with a unanimous voice ... that the existence of the fluid is absolutely destitute of proof. (op. cit., p.126)

These experiments of the Commissioners illustrate one of the principal features of scientific enquiry: the systematic variation of one kind of factor while its effects upon another are observed. In the Commissioners experiments, the subject's beliefs and the actions of the mesmerist were *independent variables*, manipulated at will by the experimenters; the

behaviour observed in the mesmerised subject was the *dependent variable*, being the consequence of the particular manipulations. Very probably the Commissioner's experiments are the earliest application of what we now think of as "the scientific method" to a psychological problem and it was its application that enabled them to answer so decisively the question of whether or not the theory was true.

Indeed, for the Commissioners the truth of the theory was the only question:

> The animal magnetism may indeed exist without being useful, but it cannot be useful if it does not exist. (op. cit.)

Critics were quick to point out that the behavioural changes nevertheless required explanation: how could imagination and belief give rise to the profound physical changes of the crisis? And, even if only some of the cures were genuine, how had *they* been produced? In the absence of definite answers to these questions the theory of magnetic influence retained some credibility. Amongst Mesmer's followers this was bolstered by various criticisms of the Commission: they had preferred to investigate d'Eslon's practice rather than Mesmer's; they had been lax in their attendance at the demonstrations; they had failed to question the patients; and had made their observations too casually (Elliotson, 1843/1970a, pp.199-200, citing an 1825 summary).

One further aspect of scientific theorising may be briefly considered here in connection with the criticism of Mesmer's theory. The theory was not being criticised simply because Mesmer had postulated a fluid that could not be sensed directly. Developed scientific theories typically include in their explanations of observable facts propositions about entities and processes that cannot be perceived directly. Take for example the observable 'fact' of the sun travelling around the earth: the scientific theory about this is a re-statement of the 'fact' in terms of processes that not only cannot be directly observed but that are contrary to direct observation. Other examples spring readily to mind: the facts of the chemical combination of elements are re-stated as theoretical propositions about valency bonding processes, the facts of illness are discussed in terms of sub-microscopic entities such as viruses, etc. However, when a theory contains unsensed processes and entities it is proper to ask for evidence of them other than the very same facts the entities or processes were originally put forward to explain.

According to its protagonists, the truth of Mesmer's theory was to be found in the relation between the facts of planetary motion, the facts of illness, and the effects of magnets or their equivalents. However, this same pattern of facts was also said to be explained by the fluid. Evidence was lacking for the existence of the fluid independent of those facts. Further, the validity of the theory could not be impugned by the failure to detect electrical or magnetic forces: the proposition that the fluid exerted only

subtle influence blocked that line of enquiry. On the one hand the theory was an inference from certain facts and, on the other hand, it was only those facts, and no others, that were 'explained' by it. Nevertheless the Commissioners were able to devise logically satisfactory tests. Had that not been possible, the theory would have been untestable - there would have been no way in principle through which its falseness could have been demonstrated. An important requirement of a scientific theory is that it be formulated in such a way that its disproof is possible.

Despite the entirely correct conclusion of the first Commission, the criticisms of their work and the continued demonstration of mesmeric effects had the effect of keeping some form of the doctrine of animal magnetism alive. By default, the credulous view had managed to survive its first sceptical scrutiny.

HYPNOSIS AFTER MESMER

Little progress was made toward understanding the basis of hypnosis after Mesmer. Few scientists of repute cared or dared to investigate it. If no contribution to a scientific theory of hypnosis could come from the orthodox, little could be expected from the odd assortment of suspect medical men, lay practitioners, charlatans, and quacks who maintained the interest. In the confusion of post-mesmeric developments, three trends can be identified: the failure of Mesmer's direct followers to investigate or establish the properties of the fluid, the failure of those sceptical of the existence of the fluid to develop an alternative theory, and the inadequacy of the primitive physiological explanations that were proposed. Within each trend sceptical and credulous attitudes were mixed in varying proportions.

Among those whom Mesmer personally initiated into the practice of animal magnetism was Armond de Chastenet, the Marquis de Puységur. At the beginning of his mesmeric practice de Puységur's belief in the power of the fluid was conventional if somewhat extreme - he was so able to impregnate trees on his estate with magnetic fluid that hundreds of people were cured of their illnesses simply by touching them. About Mesmer's demonstrations he seems to have been less enthusiastic:

> From the first he viewed with dislike and suspicion the crisis attended with violent convulsions which he had witnessed at Mesmer's establishment. (Podmore, 1909, p.71)

Although de Puységur used Mesmer's method of passes to magnetize his subjects, it did not produce the usual convulsive crises but, quite unexpectedly according to Elliotson (1843/1970a, p.200), a state resembling sleep in which conversation with the subject could be maintained. de Puységur's production of this condition of artificial somnambulism, or magnetic sleep as it was sometimes called, seems to

have resulted as much from his aristocratic distaste for the crises as from his aristocratic status relative to his subjects. Elliotson observed that de Puységur's first three subjects were the daughter of his steward, the wife of his game-keeper, and a female peasant from the estate. The passivity of these subjects in the sleep-like state may well have reflected, as Ellenberger (1965) has argued analogously in discussing de Puységur's effect upon his famous subject Victor, the subordination of their menial, female selves to his aristocratic, male domination. Victor himself was a young peasant from the estate who, like so many of the other subjects of that period when they passed into artificial somnambulism, seemed to become clairvoyant. To the existing problems of assessing the validity of the fluidic theory (which he himself eventually rejected) and the genuineness of the cures, de Puységur's work added the extra difficulty of establishing the truth of the claim that artificially somnambulistic subjects possessed parapsychological powers.

J.P.F. Deleuze, a follower of both Mesmer and de Puységur, went further in the parapsychological aspects. His belief in the existence of the fluid was sustained in part by the claims of the somnambules that they could *see* the magnetizer shining with a brilliant glow (Dingwall, 1967, p.14). Deleuze did provide an important new defence of the fluidic theory: he argued that, however incredible the reported phenomena were, the very uniformity of the descriptions provided some proof they were real and not imaginary. This argument stood him in good stead when the effects of Perkins' metallic tractors (two pieces of metal, usually drawn lightly over the affected part of the body) were shown to be constant even when imitation metals were substituted. Since thousands of cures had been testified to, including, somewhat peculiarly, the saddle boils of horses, the cures must therefore have a real basis. Deleuze ruled out hopeful imagination on the part of the patient: first of all, imagination was too vague a concept to explain the effects; second, it was notoriously the case that hopeful patients were not always cured by orthodox physicians; third, it had been repeatedly observed that scoffers had been cured by magnetic practices (op. cit., pp.16-20).

Baron Du Potet de Sennevoy was the third of the great believers in the fluidic theory (Du Potet de Sennevoy, 1834/1970). He was the outstanding magnetizer of his day, being selected for study by the second Commission of the French Academy of Science, 1825-1831 (Colquhoun, 1833). Not only did he believe in the reality of the fluid but he invented a 'magic mirror' capable of producing convulsions in the user and linked mesmerism with magic in other ways (Dingwall, 1967, p.54; Tinterow, 1970, p.175). His performances before that Commission were sufficiently convincing for them to conclude that the phenomena of magnetism were real and not due to imagination. On a visit to England in 1837, he succeeded in reviving Elliotson's interest in mesmerism to such an extent that Elliotson introduced it into English medical and hospital practice (Elliotson, 1843, 1843/1970a, 1843/1970b; G. Rosen, 1946).

Elliotson was a medical innovator of some distinction, a Fellow of the Royal Society, President of the Royal Medical and Chirurgical Society, Censor [Examiner] of the Royal College of Physicians, and Professor of the Practice of Medicine at University College, London. Nevertheless in his mesmeric work Elliotson was credulous enough not to have guarded against the effects of suggestion. He was continuously abused and harassed by his colleagues from the time that Wakley, the then editor of *Lancet*, secretly substituted lead bars for the nickel ones used in one of his demonstrations, without thereby affecting the phenomena produced (Bramwell, 1903, pp.5-14; G. Rosen, 1946). Elliotson founded a movement for popularising mesmerism and developed reliable methods for using magnetic sleep as an anaesthetic. Through that popularisation, Esdaile (1846) was prompted to extend its application in a variety of surgical conditions, but with his death the more or less unmodified belief in the fluidic theory seems also to have died. Whatever other positive contributions they may have made, and those of Elliotson and Esdaile to psychotherapy and surgery were particularly important, none of the believers advanced the understanding of mesmerism much beyond that of its founder. Characteristically they were content to describe one unusual effect after another, and to attribute them, usually quite vaguely, to the imponderable fluid. They neither investigated the nature of the 'fluid' nor its mode of action.

Curiously enough, the sceptical tradition that emerged while Mesmer was still alive was no more productive theoretically. A peripatetic Portugese prior, the Abbé de Faria, was the first practitioner in this tradition. Initially an orthodox follower of Mesmer, he published in 1814 a work in which he denied the existence of the fluid and attributed mesmeric effects to the *impressionability* of the subject (Bramwell, 1903, pp.3, 280; Dingwall, 1967, pp.34-39). De Faria's scepticism was confined to the truth of the fluidic theory. He was quite credulous about some of the effects. For example, through some poorly contrived experiments he became convinced that mesmerized subjects acquired parapsychological powers enabling them to see and hear over vast distances and to transmit their thoughts. Credulity of this kind about the effects mixed with scepticism concerning the fluid also characterised the beliefs and work of other anti-fluidists of the period.

The work of Alexandre Bertrand is similar. Introduced to animal magnetism through a series of public lectures given by Deleuze in 1819 (Dingwall, 1967, p.46), he had produced within four years a cautiously approving text. Over the next three years he conducted a series of experiments, rather like those of the first Commissioners, leading him to confirm their conclusion that most of the effects were due to the imagination (op. cit., pp.74-76). Bertrand was credulous about other things. He defended, for example, so-called eye-less vision - the apparent ability to see even though the eyes were bandaged. Effectively controlled

experiments, even at that time, should have suggested more caution. He also believed patients could have pre-vision about their illnesses. His own patients as well as others announced in advance the time and duration of various modifications of the diseases from which they suffered, including when and how they were to be cured. Bertrand made no real effort to explain such unusual events; possibly this indefiniteness led the second Commission to neglect his evidence. Thus de Faria's and Bertrand's scepticism had little impact upon those wanting to believe; the fluidic theory was virtually unaffected by their work.

One of the early attempts to provide a physiological explanation of magnetic effects foundered similarly. Amédée Dupau, a physician of considerable repute who wrote at about the same time as de Faria and Bertrand, appears to have been the most consistently sceptical of all of the early writers. He restricted his consideration to normal effects, excluding the paranormal altogether, and he proposed a physiological explanation for them. Just before the second Commission began its work, he published a critical analysis of previous experiments on the paranormal powers of the somnambules in which, again and again, he pointed to the magnetist's failures to exclude normal sensory cues to the phenomena: experiments on eyeless vision had not always ensured the exclusion of the subject's normal vision by adequate (or any) bandages; in experiments on magnetization at a distance the conversations of others often served as cues to the presence of the allegedly unknown magnetizer; ordinary sources of knowledge were not excluded in experiments on thought transference, and so on.

Dupau accepted that diseases were often cured by magnetization, but his analysis led him to reject the claims that the somnambules had unerring powers of diagnosis and prescription. The diseases most readily cured were those nervous and chronic disorders ("*affections nerveuses ou chroniques*") he knew from history and from his own experience to respond to all kinds of irrational procedures, including miracle healing (Dingwall, 1967, pp.58-74). Dupau's own theory seems not to have been spelled out in much detail. Analysis of the conditions for producing the magnetic state led him to implicate the superior social rank and knowledge of the magnetizer, the monotony of the induction procedure that caused the subject to lose interest in the external world, and the temperamental susceptibility of the subject, often revealed by an imaginative constitution or the presence of spontaneous magnetic states. Once the magnetizer had set the imagination to work physiological modifications to the nervous system took place, manifesting themselves in such physical symptoms as catalepsy and heightened sensory awareness and such mental changes as greater suggestibility. On his view, the phenomena and physiology of magnetic sleep were simply extensions of normal sleep. No one wished to know this - at the time Dupau wrote the fashion was for belief. Dupau's alternative explanation was then lost until it was unknowingly revived by James Braid.

Braid, who is rightly regarded as the father of the scientific study of hypnosis (Binet and Féré, 1887a/1887; Waite, 1899; Tinterow, 1970), seemed to have established that all that was necessary to induce hypnosis was visual fixation producing muscle strain. Neither a magnetic fluid nor a personal or social relation with the hypnotist was involved. Following his observations at a demonstration by Lafontaine in 1841 (Bramwell, 1903, Appendix A), Braid had tentatively concluded that the inability of the subject to open her eyes after 'fixation' was due to a paralysis of the nervous centres controlling eye movements caused by the induction procedure. His first three attempts at induction were carried out successively upon a younger male friend, his own wife, and a male servant, and all succeeded. Each had been instructed to fixate an object visually above and in front of the eyes, with the head held normally. Within minutes the eyes had closed and an hypnotic state produced.

It seemed to Braid that the changes in reflex function, respiration, and circulation that followed his induction procedure were consonant with a profound physiological change. Apparently he ruled out his personal qualities and the demands he created in his subjects as factors in producing hypnosis simply because the effects had not been anticipated by them and because his method of induction seemed a physiological one (Braid, 1843/1970b). In these respects he was almost certainly incorrect. However, by denuding the induction procedure of its mystery, he provided a basis for scientific enquiry into the phenomena of hypnosis and maintained a running battle with Elliotson and the other mesmerists about this. More positively, Braid developed psychotherapeutic procedures and techniques of anaesthesia. It was he who coined the term hypnotism (Braid, 1842/1970a). His work directly influenced the formation of the two great French schools of hypnosis associated with the names of Bernheim and Charcot respectively.

Whatever might have been the direction of Braid's later theorising (Bramwell, 1903, pp.278-294), his earlier views do show two important signs of credulity: the physiological explanation itself, and a supposed connection of Gall's phrenology with hypnosis. Braid considered it possible that pressure on some part of the skull of an hypnotized subject would bring about the activity the phrenologists had supposedly correlated with that part. For example, if the 'bump' of musicality was pressed the hypnotized subject would begin to sing. Frederick Engels, not usually first thought of in connection with hypnosis, provided an interesting and very penetrating eye-witness account of a stage performance carried out by a man claiming to be one of Braid's disciples:

> Now it happens that I also saw this Mr. Spencer-Hall in the winter of 1843-44 in Manchester. He was a very mediocre charlatan, who travelled the country under the patronage of some parsons and undertook magnetico-phrenological performances with a young woman in order to prove thereby the existence of God, the immortality of the soul, and the incorrectness of

> the materialism that was being preached at that time by the Owenites in all big towns. The lady was sent into a magnetic sleep and then, as soon as the operator touched any part of the skull corresponding to one of Gall's organs, she gave a bountiful display of theatrical, demonstrative gestures and poses representing the activity of the organ concerned; for instance, for the organ of philoprogenitiveness she fondled and kissed an imaginary baby, etc. Moreover, the good Mr. Hall had enriched Gall's geography of the skull with a new island of Barataria: right at the top of the skull he had discovered an organ of veneration, on touching which his hypnotic miss sank on to her knees, folded her hands in prayer, and depicted to the astonished, philistine audience an angel wrapt in veneration. That was the climax and conclusion of the exhibition. The existence of God had been proved. (Engels, 1898/1982, pp.51-52)

Engels went on to describe his own experiments:

> Apart from muscular rigidity and loss of sensation, which were easy to produce, we found also a state of complete passivity of the will bound up with a peculiar hypersensitivity of sensation. The patient, when aroused from his lethargy by external stimulus, exhibited very much greater liveliness than in the waking condition. There was no trace of any mysterious relation to the operator: anyone else could just as easily set the sleeper into activity. To put Gall's cranial organs into operation was a mere trifle for us; we went much further, we could not only exchange them for one another, or make their seat anywhere in the whole body, but we also fabricated any amount of other organs, organs of singing, whistling, piping, dancing, boxing, sewing, cobbling, tobacco-smoking, etc., and we could make their seat wherever we wanted. Wallace made his patients drunk on water, but we discovered in the great toe an organ of drunkenness which only had to be touched in order to cause the finest drunken comedy to be enacted. But it must be well understood, no organ showed a trace of action until the patient was given to understand what was expected of him; the boy soon perfected himself by practice to such an extent that the merest indication sufficed. (op. cit., p.52)

If an amateur at the art could so easily make such shrewd observations, it is hardly necessary to enlarge upon Braid's failure to investigate the supposed phrenological aspects of the topic in a scientific manner. Although Braid was to complain that phrenological views had been wrongly attributed to him, and although he advanced possible alternative explanations for the behaviour (Bramwell, 1903, p.290), he undertook no experimental programme to dispel the earlier impression.

During his lifetime Braid's influence upon the study of hypnosis was minimal. His work is important, not for what it established, but for what it led to: the eventual disposal of the fluidic theory. And, even though Braid did not provide a scientifically acceptable alternative, his work showed how the phenomena of hypnosis might be encompassed within a scientific framework.

Two cautions emerge clearly from this brief survey of the early work on hypnosis - a methodological caution about the conduct of experiments

involving behaviour and a logical caution about the drawing of theoretical and empirical conclusions. The best of the later work followed the experimental method proposed by the first Commission. Facts established by it were eventually to show that the hypnotized subject had interesting but not especially unusual powers. However, little theoretical caution was observed by either the fluidists or the animists. For the former, the fluid somehow explained everything, while for many of the latter vaguely defined notions of imagination or suggestion were acceptable, all-pervasive explanatory principles. With some license, Charcot can be thought of as extending the fluidic tradition and Bernheim the animist.

CHARCOT AND BERNHEIM ON HYPNOSIS

By the last quarter of the nineteenth century, post-mesmeric developments in France led to the emergence of two opposed schools of hypnosis, one at the Salpêtrière in Paris led by Jean-Martin Charcot and the other at Nancy acknowledging Hippolyte Bernheim as its leader. Charcot, the most eminent clinical neurologist of his day, had from about 1880 turned to the scientific study of hypnosis and hysteria. Initially he had held a chair of pathological anatomy and during that appointment he established the histopathology of amyotrophic lateral sclerosis, tabes dorsalis, poliomyelitis, multiple sclerosis, and the muscular dystrophies (McHenry, 1969). In 1882 Charcot became the first person anywhere in the world to be appointed to a chair of neurology, and it can even be said that with him neurology was founded as a medical speciality. Charcot established the first of the three outstanding groups of neurological investigators - the others being those of Erb and Oppenheim in Germany and Jackson and Gowers in London (op. cit., pp.270-271). His new interest in hypnosis was of fundamental importance. The various scandals associated with the Mesmerists, the critical exposures, and the opposition of orthodox medical men had left hypnosis largely to quacks, showmen, and charlatans. If it really were the case that only someone of considerable eminence could establish hypnosis as a respectable area of study, Charcot was the person to do it.

Social considerations had also set the stage for a Charcot, as G. R. McGuire (1986a) has shown. During the late 1800's, the wide-spread sentiment that France was in a period of social decline led many intellectuals to become interested in what they saw as the basis of the decay: psychological dissolution in the individual. Social dissolution was to be understood through individual pathology. As well, normal processes were to approached through the abnormal. Hypnosis, hysteria, and the dissociated states observable in mediums as well as other spiritist phenomena were the exemplars, *par excellence*, of individual dissolution. Not only was Charcot eminent enough for the time - the time itself was ripe for him.

Charcot's entry into the area of hypnotic studies was preceded by a revival of interest among the orthodox. A reading of Braid's major work had led Azam, a physician at Bordeaux, to conduct his own experiments and publish his conclusions the following year. These findings were communicated to the Academie de Médecine by Velpeau in 1860 (Bramwell, 1903, pp.27, 30. Cf. Chertok and Saussure, 1973/1979, pp.38-42). Broca, an important figure in the study of the cortical localisation of speech, also read Braid at about the same time and his outline of Braid's theory, together with a report on his own experiments, were communicated to the prestigious Academie de Sciences, which then set up a committee to report on the subject (Philips, 1860/1970; Bramwell, 1903, p.27). Shortly afterwards, Charles Richet, then a young physiologist who was later to become famous in that field, began his work on hypnosis. Although Charcot's interest in mesmerism may then have already been aroused through his work evaluating Burq's 'metallotherapy' (Harrington, 1988), it was Richet's series of papers in well established journals that appears to have prompted Charcot into actively investigating the subject (Guillain, 1955/1959; Levin, 1978, p.50 and n.24; G. R. McGuire, 1986a). By that time, hypnosis had also come to interest Taine, the philosophical inspirer of French psychology, and Ribot, the director of the first French psychological laboratory.

When supporting his candidacy for the Academie des Sciences Charcot outlined how he had begun his work on hypnosis. Writing in the third person he said:

> From the very beginning a prudent and conservative orientation was developed and applied to these investigations. This approach was only slightly influenced by the purely arbitrary skepticism practiced by those who, under the pretext of 'purely scientific orientation', concealed a prejudice to see nothing and to hear nothing in these matters. At the same time every attempt was made to avoid being attracted by the esoteric or the extraordinary, a peril which in this scientifically unexplored field was encountered, so to speak, at every step of the way. Briefly, the method Charcot adopted for these intense physiologic, and neuropathologic studies can be summarised very simply; instead of allowing himself to be led into a pursuit of the unexpected and the mystic, he decided for the time being to attempt to analyze the meaning of the clinical signs and physiologic characteristics that can be identified among various conditions and phenomena caused by nervous reactions. He further decided to confine himself at first to an examination of the most simple and constant factors, the validity of which was the most easy to demonstrate, and only to investigate later and still with caution the more complex or evasive phenomena; and finally to omit studying systematically, except in a provisional way, those phenomena which are of a much more obscure nature and which for the moment do not appear to correlate with any known physiologic mechanisms. According to Charcot, it is largely because these very simple precautions have often been overlooked that studies of hypnotism as an experimental neurosis, which previously had been almost inaccessible, have not until now borne fruit to the extent

> anticipated and have not enjoyed everywhere the favorable reception that such studies should merit. Such studies, when properly prosecuted, are certainly destined to bring eventual light to a whole host of questions, not only from a pathologic standpoint but also from the standpoint of physiology and psychology. (Cited in Guillain, 1955/1959, p.167)

Charcot obviously believed his systematic, physiologically based method of inquiry would by itself guarantee that the errors of the past would be avoided.

Charcot began by systematising existing observations about hypnosis, extending them where necessary by controlled experiments. From this work, which lasted over a number of years, he arrived at a description of various "symptoms" characterising the states of hypnosis (cited in Binet and Féré, 1887a/1887, pp.154-159). Charcot distinguished three fundamental states:

(1) *Catalepsy*, ordinarily produced by the subject visually fixating a given object. It might also be produced by opening the eyes while in the lethargic state. The major feature of the condition was that the limbs and other parts of the body would retain for a considerable period the position in which they were placed. Certain reflexes disappeared, anaesthesia was pronounced, but some sensory functions were unimpaired.

(2) *Lethargy*, produced by visual fixation or, if the subject was already in the cataleptic state, by closing the eyes. The main feature was a peculiarity of the musculature such that the limbs, after being raised and freed of support, dropped flaccidly back to their original positions. Reflexes were altered and the pupils were permanently contracted. Contractures of the limbs were exhibited upon percussion of the tendons or upon mechanical stimulation of the fibres innervating the muscles of those limbs.

(3) *Artificial somnambulism*, produced by fixation of the gaze or by simple pressure or slight friction on the scalp while the subject was in the lethargic or cataleptic states. The main features were that while the subject appeared to be asleep, the limbs offered resistance to change, and the various methods for producing contracture were ineffective. Sensory functioning was often enhanced with especially marked increases in visual and auditory acuity.

Charcot's physiological approach seemed well justified: the states were clearly separated, subjects passed from one to the other in a predictable manner, and the identifying features of each state were physiological. For example, hypnosis seemed to produce such a degree of muscular and nervous hyperaesthesia that pressure on the muscles or nerves alone brought about the same contractions as direct electrical stimulation of them

usually produced. As Fig. 2.1 illustrates, pressure on the cubital nerve of the forearm caused:

> the contracture of all fingers on the hand except the second and third, the same result as is given by electrical stimulation and which explains the distribution of nerves. (Charcot, 1879-1880, p.171)

Similar contractures could be produced in the facial muscles and even tetanisation of the whole musculature was possible (Charcot, op. cit., pp.171-173). These and other apparently regular changes in physiological functioning produced under hypnosis could be subsumed under a theory that the changes were based on alterations in neuromuscular excitability.

Figure 2.1. Mechanical stimulation under hypnosis of cubital nerve producing finger contractures identical to those produced by electrical stimulation in the waking state (Charcot, 1879-80, Fig. 7).

Several other observations seemed consistent with this view. For example, Charcot noted his best hypnotic subjects were young hysterical women (op. cit., pp.162-163) and actually held that hypnosis was an artificial hysteria. Convinced that hysteria was a physiological disorder, it was then natural for Charcot to make the further supposition that hypnosis was also physiologically based. His physiological theory thus had a degree of generality that linked hypnosis and hysteria with established neurological disorders.

Hippolyte Bernheim's approach to hypnosis was very different. His introduction to the subject had come from following up his criticism of the therapeutic work of an obscure Nancy physician, Ambroise Liébeault. Liébeault's own interest is often said to have begun two years before he graduated when, after reading a traditional book on animal magnetism and practicing the methods described in it, he found himself able to mesmerise

others. Chertok and Saussure (1973/1979, p.42), however, attribute Liebault's interest to the same paper of Velpeau's that eventually influenced Charcot. He incorporated hypnosis into his medical practice from the beginning, offering it as an alternative, free treatment to his predominantly peasant patients. In 1864 he retired from active fee charging practice, devoting himself to writing a book (of which only one copy was sold!) and to the free clinic he had established for the poor. His methods for inducing hypnosis were apparently those of the mesmerists, but he eschewed the fluidic theory. In his view hypnosis came about because the subjects thought or concentrated their attention upon sleep or the idea of sleep. After *rapport* had been established between the subject and the hypnotist, the hypnotist's direct or indirect suggestions produced the phenomena (Bramwell, 1903, pp.340-344). Liébeault's theory was, therefore, a psychological one.

Bernheim's approach differed only in minor details from Liébeault's. He distinguished various stages of hypnosis solely in terms of the degree to which suggestibility was increased: six stages for which there was memory in the waking state and three in which memory of things taking place under hypnosis was lost in the waking state. Suggestibility increased in stages 1 to 6 but it was not possible to suggest illusions or hallucinations. Stages 7 to 9 were marked by such increased suggestibility that the subject became susceptible to various types of hallucinations or other powerful suggestions. Stages 7 and 9 were also distinguished by what Bernheim called somnambulism. Stage 7, for example, was somnambulism without susceptibility to hallucination and Stage 9 was somnambulism with susceptibility to hallucination (Bernheim, 1887/1888a, pp.10-15).

Despite repeated attempts, Bernheim failed to confirm Charcot's observations concerning the progression through the states of lethargy, catalepsy, and artificial somnambulism. He also failed to repeat Charcot's observations of certain phenomena being invariably associated with the three states. Finally, he demonstrated the phenomena were not necessarily the result of changes in physiological functioning or neuromuscular hyperexcitability but were due entirely to suggestion. What accounts for the difference between the findings of Charcot and Bernheim?

It was clear to many contemporary observers, and not only to Bernheim, that the apparent lawfulness of the behaviour of Charcot's subjects was brought about by a combination of the expectations of the experimenter with what the subjects believed was required of them (''demand characteristics'' as Orne has termed them) and the effect of repeated practice. The functional changes characteristic of each stage resulted primarily from the drilling of the subject. Some rather more esoteric effects, like the production of hemi-catalepsy after only one eye was opened in the lethargic state, show quite clearly the effects of expectations, demand characteristics, and practice. Bernheim (op. cit., pp.91-104) detailed many instances where the phenomena were dependent

upon what subjects knew or thought they knew what was demanded of them. With respect to the Salpêtrière experiments using magnets to transfer sensibility from one part of the body to another, he warned:

> It is well to add that many somnambulists possess extremely acute perception. The slightest indication guides them. Knowing that they should carry out the hypnotizer's thought, they make an effort to divine it. If the transfer experiments have been repeated many times with the same subject, he readily guesses that he should transfer such and such a phenomenon, and without anything being said before him, he can divine whether the transfer should occur or not, by the expectant attitude of the operator or by some other indication. (op. cit., p.95)

He concluded his criticisms by saying that two things were necessary before valid conclusions could be drawn from experiments on hypnosis:

> 1st Take inexperienced subjects who have not been used in this kind of experiment, who have not assisted in such experiments made upon others and have not heard them talked about;
>
> 2d Make the experiment without speaking a single word before the subject, even in a low voice, because in all degrees of hypnotism he hears and notes everything with a sharpness of perception which is often quite remarkable. (op. cit., p.104)

Charcot himself had seen that practice was needed to produce some hypnotic effects but he failed to attach significance to it. For example, of some of the stranger phenomena, he remarked they were "demonstrated well only by subjects who were already accustomed to hypnosis" (Charcot, 1879-1880, pp.176-177). It would be hard to imagine a clearer instance of a psychological set preventing the recognition of the obvious. As a neurologist Charcot interpreted the regularities as lawfully produced alterations in physiological functioning rather than as the consequence of psychological demands.

Charcot might also have been the victim of some deliberate deception. Dingwall (1967, Vol 1, pp.256-257) cites the revelations of one of Charcot's former hypnotic subjects to the effect that she had deliberately tried to fool Charcot with her performances. Even Guillain (1955/1959, p.174, n.11), to whom his own accusation of bias against Munthe's (1929/1945) similar charge can hardly apply, described how, six years after Charcot's death, he had seen Charcot's patients "if offered a slight pecuniary remuneration" imitate other allegedly physiologically determined conditions, the major hysterical crises.

Except for the completely credulous, almost all of the early workers in the field of hypnotism were aware of the need for methodological precautions of the kind Bernheim described. De Faria, Bertrand, and Dupau had been quite conscious of these requirements - their own criticisms of the fluidic view had been based upon them. And so also was

Braid, despite his credulity in other matters (Braid, 1846). Even closer in time to Charcot were the warnings of the methodologically sophisticated André Morin (cited in Dingwall, 1967, pp.231-254). Disregard of these earlier cautions was the source of Charcot's errors.

The phenomena that Charcot so confidently asserted demonstrated the essential physiological basis of hypnosis were actually the same phenomena that had been demonstrated by Mesmer's 'descendants' in the French travelling shows, music halls, and theatres. For example, because it seemed to be a physiological change, Charcot attributed special significance to hyper-excitability in the lethargic state. Altered excitability of this kind had already been observed by Braid when he produced paralyses and the like by mechanical pressure or the mere blowing of air upon selected parts of the body (Braid, 1843/1970b, pp.288-290), and it is more than possible he first saw these 'symptoms' at Lafontaine's demonstration. Hyper-excitability may well have reached Charcot through Braid or others who had inadvertently reproduced the same mesmeric phenomenon.

Much later, Charcot's favourite pupil, Pierre Janet, eventually analysed the depths of Charcot's methodological failings and reconstructed the process by which he had been deceived. Charcot's assistants rather than Charcot himself had hypnotized the subjects and their instruction in the techniques of hypnosis had been obtained from current practitioners of animal magnetism; some of the magnetisers even worked in Charcot's clinic! [2] Janet further demonstrated that the various features of the states of hypnosis exhibited by Charcot's subjects were virtually identical with those described previously in the mesmeric literature. And, as he concluded:

> Is it not rather quaint to find that during the years 1878 to 1882 Charcot was presenting to the Academy of Sciences what he believed to be fresh physiological discoveries destined to discredit for ever the claims of the magnetisers, when in reality he was merely reproducing the century-old teaching of these same magnetisers? (Janet, P., 1919/1925, Vol.1, pp.191-192) [3]

Toward the end of his life there is some evidence that Charcot's doubts about the defects of his work on hypnosis crystallised into certainty and that he proposed a complete revision of it (Guillain, 1955/1959). It was barely begun before he died in 1893.

2. Harrington (1988) has drawn attention to an inaccuracy in this account of Charcot's involvement. She is able to cite the report of Gamgee (1878) who visited the Salpêtrètrie and saw Charcot induce hypnosis.

3. Ellenberger (1970, p.98) attributes Janet's criticism to a much earlier paper that is, in fact, almost entirely laudatory of Charcot (Janet, 1895) and G. R. McGuire (1986b) cites Barrucand's *Histoire de l'hypnose en France* for a date of 1923. However, the quotation shows Janet's criticisms date from 1919.

DELBOEUF'S SCEPTICISM

In December 1885, at the very time Freud was at Charcot's clinic, the Belgian psychologist Delboeuf arrived to study transfer and polarization, the latest hypnotic phenomena to have been produced there. As Wolf (1973) has so well brought out, a major controversy soon erupted between Delboeuf and the workers at the Salpêtrière. A series of experiments by Binet and Féré (1885a, 1885b, 1885c) seemed to show that perceptions and actions could be transferred from one side of the body to the other by use of magnets. For example, an image in the subject's right eye was transferred to the left when a magnet was placed near to the left side of the head. Polarization referred to a complex series of visual effects produced in complementary colours of hallucinatory coloured images. Emotions could also be changed into their complements; for example, under the influence of the magnet, love would change into hate. As Delboeuf described it later, he attempted to repeat the experiments, and being only partly successful, looked for reasons. He became convinced transfer took place only when subjects knew what was being demanded of them (Delboeuf, 1886a). On his return to Liege, and in a somewhat more sceptical frame of mind, he attempted to reproduce Charcot's three hypnotic states as well as the phenomenon of transfer. His results, which were published only four to six months after Freud left Paris, were mixed: transfer could not be demonstrated when conditions were properly controlled, and the three states did not seem to be necessary components of hypnosis (Delboeuf, 1886b, 1886c).

Binet (1886) replied to Delboeuf's criticism by pointing to a number of procedural errors and defects in his understanding of the phenomena being investigated. Quite reasonably he argued that even if Delboeuf had produced all the phenomena in his cases by suggestion alone, that demonstration did not preclude their production by physiological means as well. While Delboeuf did not formally accept Binet's counter (Delboeuf, 1886d), he did go on to make another and more careful attempt to produce the phenomena by suggestion (Delboeuf, 1888-1889, 1889). To his own satisfaction and that of his colleagues he was completely successful. Two years later Binet surrendered. In discussing multiple personality he said that Charcot's work on hypnosis presented:

> a host of causes of error, which very often falsify the results without the knowledge of the most careful and prudent experimenter .... One of the principal and constant causes of error ... is suggestion, that is, the influence of the operator by his words, gestures, attitudes and even silences, on the subtle and alert intelligence of the person whom he has put in the somnambulistic state. (Binet, 1892/1896, p.76. Partly retranslated, MBM)

While Binet was critical of the incompleteness of the explanation offered by Bernheim's suggestion theory (op. cit., pp.78-79, 300), he went on to say that "*all*" that had been written about the physiological basis of hypnosis "seems to me to be fanciful" (op. cit., p.79). He wryly concluded

that one had to be content with what were admittedly unsatisfactory psychological hypotheses because:

> all things considered, they are worth more than false notions, and we do not hesitate to prefer them to physiological hypotheses, which while seeming more exact are really much more hypothetical. (op. cit., p.80)

So as to leave no doubt about the shift in his position, he added a footnote:

> Readers of my earlier works will see that I have altered my view on this important point. (ibid)

Delboeuf had taken just five years to force this admission of the correctness of his criticism from one of the leading representatives of the Charcot school. His doubts, first aroused within days of his arrival in 1885 at the Salpêtrière, had turned him into a sceptic within a few months. Now it was accepted that the so regularly observed characteristics of hypnosis really were due to influences unconsciously transmitted by Charcot (Cf. Apfelbaum and G. R. McGuire, 1985).

Delboeuf's major conclusion about the role of suggestion was also consistent with many of the early observations made at Nancy. During a discussion of hypnotic phenomena demonstrated at the Nancy Medical Society in 1882 it was remarked that Charcot's three states of hypnosis could be produced by a variety of means (Hillman, 1965). One year later Bernheim and Charpentier reported an experiment showing the effect of a magnetic field upon an hysterical patient's vision depended upon the subject knowing whether or not the apparatus was switched on (Bernheim, 1887/1888a, pp.260-266; Hillman, 1965).

FREUD AND THE CHARCOT-BERNHEIM CONTROVERSY

Freud studied with Charcot at the Salpêtrière between mid-October, 1885 and February, 1886 (Freud, 1886a). Soon after the visit he translated some of Charcot's *Lectures* into German (Charcot, 1887/1886, 1888/1894) and defended Charcot's views at, as he saw it, some cost to his own reputation. Two years after his visit to Charcot, Freud translated Bernheim's major work into German (Bernheim, 1887, 1887/1888b) and visited Nancy in the summer of 1889. In 1892 he translated a second book of Bernheim's (Bernheim, 1891, 1891/1892). He was therefore thoroughly familiar with the work of both Charcot and Bernheim.

Some vacillation is evident in Freud's attitudes to Charcot's and Bernheim's work (Editorial Note, *Standard Edition*, *1*, pp.67-69). Bernheim's mastery of induction techniques and the varied types of disorders he treated impressed him a great deal. However, he remained critical of Bernheim's theory that hypnosis was only suggestion and about this he was inclined to repeat Delboeuf's inference: "That being so, there is no such thing as hypnotism" (Breuer and Freud, 1895, p.101. Cf.

Delboeuf, 1891). In Freud's view, Bernheim had simply replaced the undefined concept of hypnosis with the even more poorly defined concept of suggestion. While at one time he had had doubts about the comprehensiveness of Charcot's theory, and clearly rejected the master's proscription of hypnotic therapy, like Deleuze he just as clearly accepted that the uniformity and apparent lawfulness of the phenomena attested their validity. Freud discounted unconscious influences in producing the phenomena and held with Charcot that the essential nature of hypnosis consisted of alterations in the excitability of the nervous system.

Freud's position derived from the particular view of determinism he held. Defined simply, determinism is the idea that all things and events have their determinants, or causes. Freud took this to mean, in part, that the causes of hypnosis had to be sought in the causes of those features of hypnosis that were invariably present. Suggestion would be incapable of producing uniform phenomena because suggestions from different hypnotists would vary. Systematic observation of the kind practised by Charcot guaranteed the identification of the idiosyncratic characteristics defining hypnosis and only a physiological theory provided a satisfactory basis for explaining hypnotic phenomena because only that kind of theory could invoke processes unaffected by suggestion.

Freud was not alone in taking the above view of determinism. Ironically enough, Janet had also once used this very argument for favouring Charcot's theory over Bernheim's. After describing himself at the 1889 International Congress on Hypnotism as adhering to a psychological viewpoint, Janet characterised Bernheim's opinions as "dangerous" because they would "lead to the suppression of all forms of determinism", and added:

> for my part I do not hesitate to affirm that these interpretations are anti-psychological, because psychology, like physiology, has laws which suggestion is incapable of bending. (Bérillon, 1889, p.109. Cf. Janet, P., 1895)

This rather curious view of determinism was hardly defensible at the time, and psychologists would now reject it entirely - the lawful determinants of many kinds of behaviours include suggestion. In any case, it led Freud as well as Charcot (and Janet at that time) to a totally incorrect view of hypnosis. But, as Delboeuf's contemporaneous experience showed, that error could have been avoided by a little more scepticism.

CONCLUSION

From the beginning of its study, sharp observations and properly conducted experiments had shown unconscious influences to play a major role in determining the phenomena of hypnosis. The investigations by Delboeuf and the experiments at Nancy further confirmed what had long been established. While Binet and Janet and others of the Charcot school came

to accept that the phenomena resulted from the unconscious transmission of the experimenter's intentions to his subjects, Freud never did. In 1888 Freud was prepared to concede that some unspecified parts of the work of Binet and Féré might be methodologically suspect. But the physiological symptoms of hypnosis were "most definitely" not to be regarded as due to suggestion and transfer in hysterical subjects was "indubitably a genuine process" (Freud, 1888c, pp.78-80). Except, perhaps, for an obscure hint in his obituary of Charcot (Freud, 1893c, pp.22-23), nowhere did Freud concede any other deficiencies in the Salpêtrière investigations. He never really modified his position and never recognised the extent to which investigators could unconsciously influence their subjects.

Freud's errors in his evaluation of Charcot's work become a little more comprehensible in the context of the other points of difference between the Salpêtrière and Nancy schools. Charcot's experiments had convinced him only hysterical subjects showed the three states of hypnosis and that hypnosis was best regarded as an artificial hysteria. Bernheim correctly rejected the experiments as having been poorly conducted. On the other hand, on the basis of equally bad experiments (which Charcot correctly rejected), Bernheim had concluded that serious crime could be induced under hypnosis. The frontier between the credulous and the sceptical was not clearly located between Paris and Nancy, nor was all scientific virtue only in the one place and all error in the other. A newcomer such as Freud was to psychological exploration, especially using a compass as faulty as his in this admittedly confusingly mapped territory, might well have become lost.

3 FREUD, DETERMINISM AND HYSTERIA

> Those voices from the past, which ... still resounded in the disturbed mind of that wretched old woman, were all that survived of the glory that had been Charcot's Salpêtrière.
>
> Ellenberger: *The Discovery of the Unconscious.*

It was only with the work of Charcot about one hundred years ago that hysteria began to be understood. Charcot's essential point was that hysterical symptoms were based on 'lesions' produced by the action of unconscious mental processes rather than on physical alterations to the nervous system. Part of the impact of his work on Freud seems to have been due to the similarity between his and Breuer's conceptions of how symptoms formed.

Hysteria itself had been known well before Charcot, of course. From the very beginning of medicine many attempts had been made to understand it (Veith, 1965). The searchers had not lacked diligence. Diseases and misplacements of the womb, blasphemous demoniacal possession, and unbridled or frustrated sexual passions had all been proposed as its cause. During those years hysteria was not even a disease for many investigators; rather it was a deceitful malingering. When Charcot began his work possession was no longer believed in but all of the other causes were still clamouring for attention. Charcot's great merit was to take hysteria seriously and reject the view that it was malingering. He thought of hysteria as a disease of the nervous system and began a comprehensive programme of clinical and experimental enquiry. What he brought to the study of hysteria were detailed examinations of its symptoms, methodical comparisons of hysteria with other conditions, and, most important of all, a physiological theory which seemed to encompass its determinants. Freud accepted Charcot's theses completely, particularly his emphasis on the lawful way in which the phenomena of hysteria were governed.

Part of Charcot's impact on Freud came from the way his deterministic and physiological thinking, with its rejection of unconscious influences as determinants of psychological phenomena, matched Freud's own. In this Chapter, I shall show how the particular view of determinism Freud and Charcot shared led them to make the most profound of errors. We shall see that it is also the view of determinism that underlies Freud's later work.

HYSTERIA AND ORGANIC DISORDERS

In Charcot's time, the term hysteria referred to a striking set of physical and mental symptoms that more or less successfully mimicked organically caused disorders but seemed not to be based on changes in the structure of the nervous system. Among the physical symptoms were convulsions, contractures, and paralyses, disorders of the special senses such as blindness and deafness, and disorders of sensations such as anaesthesias and hyperaesthesias. Mental symptoms included complete or partial losses of memory or consciousness, hallucinations, and the complex of behaviours referred to as multiple personality. Few patients exhibited all of these symptoms. In some the combination of symptoms was present permanently, while in others convulsions and hallucinations appeared episodically as hysterical attacks.

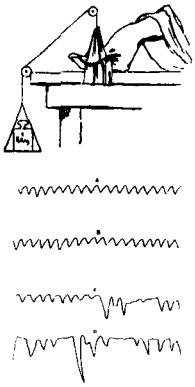

Figure 3.1. Upper drawing shows force being applied to an hysterically contracted thumb and to a simulation. Lower are respiration recordings of the hysteric (A and B) and the simulator (C and D) (Charcot, 1887/1889, Figure 19, p.96 and Figure 20, p.97).

For Charcot, the first thing to be disposed of conceptually was the notion that hysterics were malingerers, able to produce their symptoms intentionally. Charcot conducted many experiments to establish the differences between hysterical symptoms and their simulated equivalents. These seemed to show hysterics did not need to exert themselves to maintain their symptoms. For example, the respiration of an hysterical subject with a permanent contracture of the thumb to which force had been applied differed from that of a normal subject attempting to simulate the contraction. In Figure 3.1 the simulator's respiration shows greater signs of effort than the patient's. Whatever else hysteria might be, it could not be an intentional simulation of organic disorder.

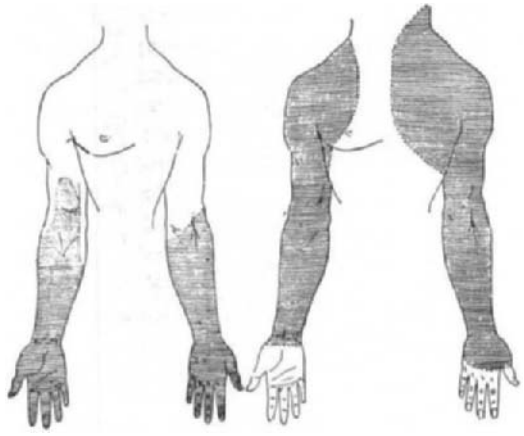

Figure 3.2. Left an organic anaesthesia (Deb___) and right an hysterical (Porcz___) (Charcot, 1887/1889, Figures 54 and 55 p.268, and Figures 56 and 57, p.269 respectively).

Not unnaturally, Charcot used the same method of studying hysteria as had been so successful in his study of organically based conditions: close systematic observation of the signs was combined with attempts to localise the site of the lesion in the nervous system. In this way the essential symptoms could be identified, related to one another, and characterised as fully as possible. Noting with great care the details of the permanent symptoms and the episodic attacks, Charcot convinced himself that hysterical symptoms reflected changes in the functions of the organs rather than in their anatomical structure. His point may be illustrated with the two of his cases known as Deb___ and Porcz___. Each had developed a paralysis and loss of sensation in an arm after an injury to the shoulder. Figure 3.2 is Charcot's representation of the anaesthesias. While in Deb___ the loss of function is exactly as expected from a rupture of the main nerve trunk in the arm carrying the motor and cutaneous nerve fibres, in Porcz___ the whole limb, excluding the fingers, is affected and cannot possibly be accounted for by the rupture of a major nerve supply (Charcot, 1887/1889, pp.262-273). Not only is it the case that the nerve fibres to the shoulder area do not have their endings so precisely distributed, but the nerve supply to the arm has a different origin from the shoulder girdle. Further, because the nerve supply to the arm includes the fibres terminating in the fingers, the arm *alone* cannot be affected; the fingers would have also to be involved. No single injury could give rise to the pattern of sensory loss shown by Porcz___. Neither could it cause the anaesthesias that were almost pathognomic of hysteria, those in which the boundaries of

the sensory loss ran perpendicularly to the axis of the limb (Charcot and Marie, 1892).

Organic disorders were caused by alterations to the structure of some part of the nervous system - structural or anatomical lesions - occurring at any level up to and including the cerebral cortex. Charcot assumed that in hysteria there were similar lesions but that they were always located in the cerebral cortex and were functional rather than structural. For example, in considering the details of an hysterical paralysis, Charcot proposed:

> There is without doubt a lesion of the nervous centres .... It is, I opine, in the grey matter of the cerebral hemisphere on the side opposite the paralysis, and more precisely in the motor zone of the arm .... we may believe ... it is not strictly limited to the motor zone, and that it extends behind the median convolution to the adjacent parts of the parietal lobe. But certainly it is not of the nature of a circumscribed organic lesion .... We have here unquestionably one of those lesions which escape our present means of anatomical investigation, and which, for want of a better term, we designate *dynamic* or *functional* lesions. (op. cit., p.278)

Dynamic or functional lesions were clearly thought of as analogous to structural or anatomical lesions. But, although Charcot often alluded to dynamic lesions, he was unable to characterise them in any way, to spell out in any detail how they differed from the structural lesions they resembled, or to be very specific about how they were formed.

MAJOR HYSTERIA

Charcot differentiated two types of hysteria: traumatic and major. Major hysteria was characterised by changes in consciousness and by convulsions. A typical attack proceeded through four successive stages:

i. The epileptoid or convulsive stage

ii. Stage of clownism or major movements

iii. *Attitudes passionelles* [hallucinatory stage]

iv. *Decline terminal* [terminal confusion].

The spontaneous attack would frequently be signalled by some unusual sensation or aura, such as a constriction in the throat, a throbbing in the temple, or a ringing in the ears. It could also be triggered by pressure applied to certain parts of the body, on what Charcot called the hysterogenic zones (Fig. 3.3). Oddly enough, pressure on these zones could sometimes also terminate a spontaneous attack once it had begun. In females an especially important hysterogenic zone was located on an area of the abdominal wall corresponding to the ovaries. So regularly did pressure there terminate attacks that some patients wore an 'ovarian compressor' in order to inhibit them.

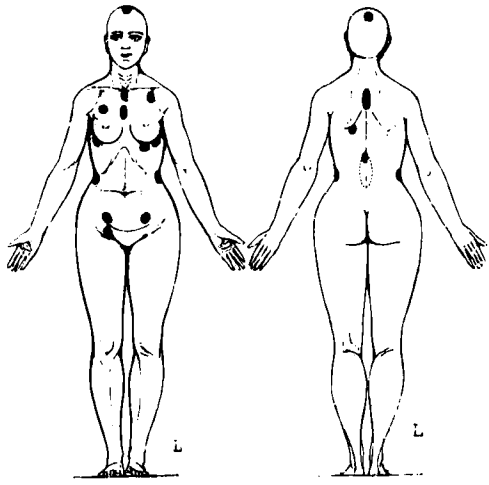

Figure 3.3. Hysterogenic zones in a female patient (Bourneville and Regnard, 1876-1880, Vol. III, pp.48-49).

The characteristic features of what Charcot called major hysteria are well illustrated in case of Ly___ (Charcot, 1887/1889, pp.248-251). As was usually the case, Charcot examined this patient in front of his students, commented to them on the diverse features of the case, and here went so far as to provoke an hysterical attack for them to see. While all four of the stages were present, the ritual non-convulsive movements of Stage 2 were not marked. It is also of interest that Ly___ was male. Two hysterogenic zones were present, and:

> Moderate pressure exercised on the last point immediately determines an attack, which we are thus able to study in all its details.
>
> The attack is preceded by the characteristic aura - epigastric constriction, a feeling of a ball in the throat, &c. At this moment, and even before the patient loses consciousness, his tongue becomes stiffened, and is retracted in his mouth towards the left side. It is found by aid of the finger that its point is carried behind the molars of that side. The mouth, half opened, is likewise deviated towards the left side. All the left side of the face shares in the deviation. The head itself is strongly drawn toward the left. The patient then becomes unconscious. The upper extremities are extended, first the right and then the left. The lower extremities remain flaccid, or at least they are very little stiffened. The movement of torsion towards the left, at first limited to the face, soon becomes general, and rolling over, the patient lies on his left side. Next, clonic convulsions replace the tonic spasm. The extremities are agitated by frequent vibrations, but of limited extent. The face is the seat of rapid tremblings, and then follows a stage of complete relaxation without stertor. But at this moment the patient seems

> tormented by horrible visions. He mentally sees again, without doubt, the scene of his quarrel ... and utters reproachful words: ''Scoundrel...., Prussian...., struck with a stone, he is trying to kill me''. The words are spoken in a perfectly distinct manner. Then, all of a sudden, he changes his attitude. Seated on his bed he is observed to pass his hand over one of his legs in such a manner as to disengage some reptile which encircles the limb, and during that time he mutters something about the worm. The scene at Sceaux comes back to him ''I will kill you ...., a gun-shot ...., you will see''. After that period, signalised by delirium and corresponding passionate attitudes, the epileptoid stage is spontaneously produced, thus inaugurating a new attack which can in no wise be distinguished from the first, and which may be followed by many others. Pressure on the hysterogenic points interrupts the evolution of the different phases. On wakening, Ly___ appears dazed and stupefied, and he states that he remembers nothing which has transpired. (Charcot, 1887/1889, pp.249-250)

The content of the hallucinations seemed related to incidents dating from three years before:

> he was engaged in efforts to rid himself of a tapeworm, from which he suffered, and for which purpose he took pomegranate bark, which had the desired effect. At first, fragments of the worm were voided, and then the whole. The sight of the taenia in his excreta so struck him, that for several days he suffered from slight nervous complications, such as colics, pains and tremblings of the limbs, &c.
>
> A year ago, while working at his trade at Sceaux, he witnessed one of his comrades violently strike his son. Ly___ desired to interpose, but his comrade turned furiously upon him, and while Ly___ was fleeing hurled a stone at him. Fortunately, the stone did not strike him; but the fright experienced by Ly___ was very severe. Immediately he was seized by trembling of the limbs .... He fancied every moment that he saw the tapeworm, or that he was again engaged in the strife with his comrade. (op. cit., pp.248-249)

However, other data in the history were taken as evidence for the hysteria having an hereditary basis: hysterical attacks in the mother, alcoholism in the father, hysteria in the grandmother and aunts.

Note that Charcot fails to comment on the possible *causal* connection between the earlier incidents and the content of the hallucinations even though he granted that, while hallucinating, the patient ''mentally sees again ... the scene of his quarrel''. The failure is consistent with his estimate of external events as mere precipitators of major hysteria. Elsewhere Charcot said the hallucinations showed how:

> the psychical element begins to play the first part in these morbid phenomena.

But, then added that the:

> vivid impression or ... emotion formerly experienced by the patient ... often has played a part in the *explosion* of hysterical symptoms. (Charcot and Marie, 1892. My emphasis, MBM)

From this perspective, Ly___ would have developed hysterical attacks irrespective of the action of external factors. His hereditary weakness would have seen to that.

Freud was to differ from Charcot about the causes of major hysteria. For him (and Breuer) it was to be explained in the same way as traumatic hysteria, the second type of hysteria that Charcot had distinguished.

TRAUMATIC HYSTERIA

What Charcot called traumatic hysteria had a more definite onset than major hysteria. While it usually followed a physical injury, it had no physical basis. Its symptoms included paralyses, losses or augmentations of sensation (anaesthesias or hyperaesthesias), and the special sense disturbances previously mentioned. Although some or all of these symptoms were also to be found in major hysteria they were there not preceded by a definite trauma. These points become clearer in the following typical case:

> The man named Le Log___ [1] was born in a little village of Brittany, and he is now twenty-nine years of age .... By occupation he was formerly a cook's assistant, but lately, for want of better work, he went in to the service of a florist in the market .... every second or third day, he went to a horticulturist at St. Cloud to fetch plants. These he brought back on a little hand-barrow, which he drew, while his master's son, young Conr___, helped by pushing behind.
>
> It was on returning from St. Cloud in this fashion on October 21st, 1885, about 6 o'clock in the evening, that the accident happened which was the cause of all his troubles. On this evening, when it was very nearly dark, Le Log___ was dragging his barrow along the road beside the Seine. He had arrived at the top of the Pont des Invalides, when all of a sudden, a heavily laden laundryman's van, driven by some drunken men at railway speed, charged into him. The wheel of the hand-barrow was struck, and Le Log___ was violently thrown on to the footpath, from which he was picked up absolutely unconscious. The horse of the laundryman's van did not touch Le Log___, and its wheels *did not pass over him*. There was no apparent wound, nor was any blood discovered about his person. Le Log___ was placed upon his own barrow and was taken in the first place to a chemist's shop, where he remained for about twenty minutes, and was then carried, still unconscious, to the Beaujon Hospital.
>
> The preceding details were given by young Conr___, and confirmed,

1. Le Log___ arrived at the Salpêtrière about a month after Freud had left Paris and the case does not appear in Freud's translation of Charcot's *Lectures*. However, similar features had been described in a less concentrated form in other cases, especially in the case of Pin___, whom Freud probably did see (Chertok and Saussure, 1973/1979, pp.72-83), and similar theoretical comments made about them.

> moreover, by a man named L___, a post-office official at the Palais de l'Industrie, who was present during the collision. The account which Le Log___ himself gives of the affair when he is questioned is a very different one. *He has made out a long history of the accident* in which he firmly believes, and of which the circumstances appear to him from time to time *in his dreams*. The laundryman's van came charging along with much noise; the horse fell right upon him, and struck him in the breast with its head. He fell down, struck his head violently on the ground, and finally the heavy van *passed completely over his body, across the upper part of the thighs*. Generally, when his dream arrives at this point, the patient wakes up suddenly screaming. At the Hôtel Dieu, and here also at the Salpêtrière, he has often been heard to cry out 'Stop! don't drive on, the horse is going to crush me'.
>
> As a matter of fact, the patient has completely lost all recollection of what passed at the moment of the accident. It is very probable that he was affected at the time by an *intense cerebral commotion*, followed by a form rcof amnesia which MM. Ribot and Azam have described under the name of *traumatic retrograde amnesia*.
>
> He was transported to the Beaujon Hospital, where he remained during five or six days without consciousness .... When his consciousness returned he was very surprised to find himself in the hospital; he remembered absolutely nothing of what had taken place. It was only after he heard the history from those around, as he himself confesses, that the circumstances of the accident as he narrates them occurred to his mind.
>
> Several facts relative to his state in the Beaujon Hospital are worthy of being mentioned. (1) *His lower extremities seemed to him as though they were dead*. At first he was unable to lift them from the bed, except with the aid of his hands, but at the end of a few days he was able to leave his bed, go out of the hospital, and walk part of the way home on foot. (2) He had several large bruises on the hip, the right groin, and over the lower abdomen. (3) He suffered with his head in the same way he does now. (Charcot, 1887/1889, pp.374-376)

Le Log___'s anaesthesia and hyperaesthesia are represented in Figure 3.4.

Two important features in Le Log___'s case history should be noted: the first is the interval between the traumatic event and the appearance of the symptoms and the second is the marked discrepancy between what had actually happened and what he recalled as having happened. With respect to the interval Charcot hypothesised:

> We have here a phenomenon of unconscious or sub-conscious cerebration, mentation or ideation. The patient, in a case of this sort, is aware of the result, but he does not preserve any recollection, or he only preserves it in a vague manner, of the different phases of the phenomenon. (op. cit., p.387, n.1)

The unconscious process took time - there was therefore a period of incubation before the symptoms appeared. In one respect Le Log___'s history

was atypical: patients were not always unconscious between the trauma and the symptom even though they were always unaware of how their symptoms had been elaborated.

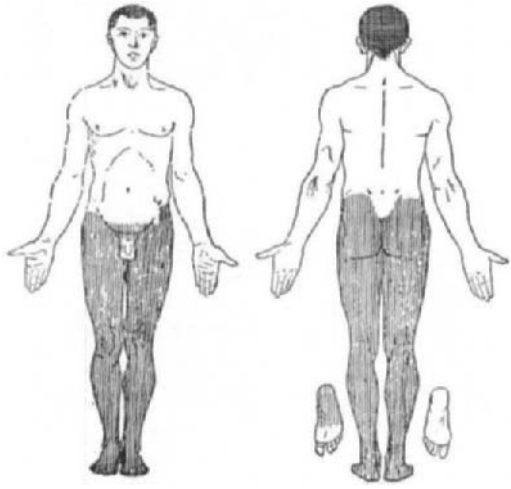

Figure 3.4. Distribution of anaesthesia in Le Log___. A patch of hyperaesthesia is on the head (Charcot, 1887/1889, Fig. 84, p.380).

However, with respect to the discrepancy between the reality and his recollection Le Log___'s history was very typical: his memory was of having been knocked down by the horse and run over by the van whereas in reality neither had touched him. As Janet was to put it in some later comments, possibly about this very case:

> Charcot, studying the paralyses, had shown that the disease is not produced by a real accident, but by the *idea* of this accident. It is not necessary that the carriage wheel should really have passed over the patient; it is enough if he has the idea that the wheel passed over his legs. (Janet, P., 1920, p.324. My emphasis, MBM)

Charcot's explanation was that the traumatic event gave rise to an idea that then overpowered the mind and realised or expressed itself in physical form as a symptom. *Realisation* was the unconscious mental mechanism by which Charcot believed ideas were turned into hysterical symptoms.

Charcot's explanation was not only based on the analysis of case histories: symptoms produced experimentally under hypnosis seemed also to implicate realisation. Charcot showed that verbal suggestion under hypnosis could create the paralyses, the anaesthesias, and the other sensory disabilities found in hysteria and that these experimentally produced symptoms reproduced in minute detail the characteristics of hysterical

symptoms. For example, the zones of anaesthesia produced by hypnotic suggestion were separated from each other by the same well defined boundaries as in hysteria, being arranged in the same geometrical segments. How striking the similarity was may be gauged in part by comparing Charcot's representation of hypnotically created anaesthetic zones (Fig. 3.5) with Le Log___'s (Fig. 3.4). In neither instance did the zones correspond to the anatomical distribution of the nerve fibres.

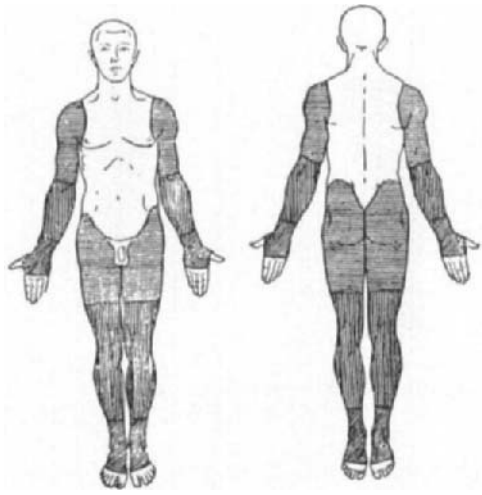

Figure 3.5. Distribution of anaesthesia in a partial paralysis suggested in hypnosis (Charcot, 1887/1889, Fig. 85, p.381).

That symptoms could be produced at all under hypnosis was due to the peculiar consequences of suggestion in that state. Under hypnosis it was possible Charcot said:

> to bring forth by suggestion or intimation, an idea, or a coherent group of associated ideas, which become lodged in the mind in the manner of a parasite, remaining isolated from all the rest and interpreted outwardly by corresponding motor phenomena. If such is the case one can conceive that an inculcated *idea* of paralysis, being of this type, results in an *actual* paralysis; and we shall see that in such cases it will frequently appear with as distinct clinical characteristics as a destructive lesion of the cerebral substance. (Charcot, 1887/1889, p.289. Retranslated, my emphasis, MBM)

For an idea to be realised it had to be isolated from the rest of consciousness. Otherwise it and its symptom would have been modifiable by an act of will. But modifications of that type were demonstrably not possible - patients could not will their symptoms away. The isolated idea or group of ideas were:

> screened from the control of that large collection of personal ideas long accumulated and organized which constitute the conscience properly so-called, the *ego*. (op. cit., p.290. Retranslated)

Ideas were transformed into symptoms, that is, realised as symptoms, by unconscious mental processes outside of the control of the ego.

Further experiments seemed to identify a source of ideas even more relevant for understanding traumatic hysteria than direct verbal suggestion. Charcot had sometimes hit some of his hypnotized subjects unexpectedly on the arm or leg thereby causing:

> a total or partial paralysis of the lower limb. When partial paralysis of the movements of the joint ... occur the loss of motor power of that joint carries with it almost necessarily ... cutaneous and deep anaesthesia of the corresponding segment of the limb. (op. cit., p.382, n.1. Cf. pp.304-305)

In a later *Dictionary* article, Charcot described the process very succinctly:

> A man predisposed to hysteria has received a blow on the shoulder. This slight traumatism or local shock has sufficed to produce in this nervous individual a sense of numbness extending over the whole of the limb and a slight indication of paralysis; in consequence of this sensation the idea arises in the patient's mind that he might become paralysed; in one word, through autosuggestion, the rudimentary paralysis becomes real. In other words, the phenomenon is brought about in the cerebral cortex, the seat of all psychical operations. (Charcot and Marie, 1892)

The idea from which the hysterical symptom developed was the idea generated during the trauma, either in the manner supposed by Janet or through the sensations produced during the trauma itself.

Charcot's chain of reasoning also required there to be a similarity between the hypnotic and traumatic states. Speaking of two of his male patients he said:

> Without doubt the two men were not at the moment of their fall in a hypnotic sleep, nor subsequently, when the paralysis was definitely established. But in this respect it may be inquired whether the mental condition occasioned by the emotion, by the Nervous Shock experienced at the moment of the accident and for some time after, is equivalent in a certain measure in predisposed subjects ... to the cerebral condition which is determined in "hysterics" by hypnotism. Upon the assumption of this hypothesis, the peculiar sensation ... which we may suppose to have been produced in ... our two male patients by a fall on the shoulder, that sensation, I say, may be considered as having originated, in the former as in the latter, the idea of motor paralysis of the member. But because of the annihilation of the *ego* produced by the hypnotism in the one case and ... by the nervous shock in the other, that idea once brought to the surface would become lodged in the brain of the patient, in that very place, be removed from every influence, be strengthened, and finally become powerful enough to realize itself objectively through a paralysis. In both cases, the

> sensation in question plays the part of a veritable *suggestion*. (Charcot, 1887/1889, p.305. Partly retranslated, MBM)

In 'mental conditions' like hypnosis and nervous shock, ideas created unconsciously from the sensations experienced in the traumatic situation spread unchecked to produce symptoms.

Although realisation was described vaguely it seemed to Charcot to be consistent with other observations. [2] Reynolds (1869) had long since seen a paralysis develop in a patient seized by the idea that she *might* become paralysed following a long and fatiguing walk. After becoming aware of a progressive weakness in her legs she was finally unable to walk at all. The idea of paralysis had been transformed into a real inability to move. Möbius (1888, cited in Decker, 1977, p.83) had similarly hypothesised that hysterical symptoms formed when ideas arising in the "void" created in consciousness by strong affects acted like hypnotic suggestions.

Experiments on the relation between emotional expression and posture or muscular activity seemed also to support the concept of realisation. According to Binet and Féré, Braid had observed that the posture assumed by hypnotized subjects affected their emotional expression. For example, when a subject was placed in a posture expressing anger and the fists clenched, the face then expressed menace (Binet and Féré, 1887/1887a, p.71). After confirming that observation Charcot and Richer (1883) went on to explore the contrary possibility that modifications of facial expression would produce changes in bodily attitudes or position. Using mild electrical stimulation of the muscles of the face to produce a particular facial expression they found the rest of the body then took up an attitude consistent with it. For example, when stimulation of the occipito-frontal muscles had produced the facial expression of astonishment (raised eyelids, wrinkled forehead, eye fixation, and enlargement of the palpebral opening) the mouth also opened slightly and the arms raised themselves in semiflexion with the palms turned forward. Bodily attitudes expressive of aggression and defence, pain, fear, and laughter were also observed following stimulation of the appropriate facial muscles. Charcot and Richer concluded that ideas operated unconsciously through the muscle sense; stimulation of the muscles suggested the corresponding posture to the appropriate centres of the brain. Other workers seemed to find that a given postural attitude produced an appropriate and real idea or emotion.

2. But not, as Havens (1966) suggested, in an otherwise excellent article, in a less formal and less organised manner than Charcot described the theoretical concepts used in his neurological work. Nor, as Havens implied, are these remarks of Charcot to be attributed to "the memory of a biographer or the footnotes to the appendices of his lectures". All of the theory is to be found in the translations he overlooked personally and, in condensed form, in the *Dictionary* article (Charcot and Marie, 1892). Although Havens (1973) later seems to have withdrawn his charge, denigration and neglect of Charcot's theoretical contribution is very common (see, for example, Veith, 1965; Miller, Sabshin, Gedo, Pollock, Sadow, and Schlessinger, 1969). Only Owen (1971) gives a reasonable account of it, although his appreciation is uncritical.

Azam, for example, had found an hypnotized subject to be *thinking* of prayer after her arms and hands had been placed in a prayerful attitude (cited in Binet and Féré, 1887/1887a, pp.76-77). It seemed reasonable to conclude there was a two way connection between ideas or feelings and bodily attitudes or postures such that one could give rise to the other.

A two way connection between ideas and postures was consistent with a then very influential theory according to which one had to have an idea of a movement before it could be carried out. Several other observations seemed to support it. Some neurological disorders were marked by an inability of the patients to discern the position of their limbs after they had been moved passively. Although not lacking in muscle power, these patients were usually also unable to move their limbs voluntarily. It was argued that, lacking the idea of the existing position, the patients could not move their limbs to a new position; or, if they could, they were unable to judge when it had been reached. But perhaps the strongest argument was the introspective observation advanced by William James (1890b) and others. When learning a new movement one had to have a more or less precise idea of it at first but, with repeated execution, it was performed automatically whenever willed. James pointed out that one was aware only of the consequences of an act of will, not of the willing itself, and that the only consequences of willing were movements. Familiar everyday movements seemed also to be based on the unconscious transformation or realisation of ideas.

BREUER AND CHARCOT

According to Freud, Charcot reacted with reserve when he told him about Breuer's treatment of Anna O. If Charcot did, it was probably not simply because he was sceptical about the possibility of treating hysteria. First, Freud could have told Charcot only that Breuer's 'talking cure' had produced a temporary remission. Freud must have known of her relapses. He certainly knew that in mid-1883 Breuer had regarded Anna O. as "quite unhinged" and even hoped she would die and "be released from her suffering" (E. Jones, 1953-1957, *1*, p.225) and it is very difficult to imagine Freud being unaware of what his fiancee knew about Anna O. at the beginning of 1887. Given Charcot's extensive knowledge of the history of attempts to treat hysteria, it is unlikely Breuer's ephemeral success would have interested him much at all. Second, had Freud described the classic magnetic-illness features of the case, especially the spontaneous somnambulistic state that allowed the therapy to take place, Charcot would have been justified in displaying even less interest. We have noted Ellenberger's (1970, pp.121-123, 484; 1972) opinion that Anna O.'s history was too much like the earlier cases for late-nineteenth century workers to take her treatment seriously as a model and Charcot himself may certainly be included among those doubters (Charcot, 1879-1880).

In getting us to react to his story of Charcot's disinterest as one of a great opportunity missed by the leading student of hysteria, Freud diverts our attention from something of rather more interest: the very considerable similarity between the theoretical ideas of Breuer and Charcot. According to Breuer, the memories of perceptions and sensations occurring in hypnoid states behaved like foreign bodies, connected with but dissociated from the normal consciousness. When those memories forcibly intruded into the normal consciousness they now brought with them the original perceptions and sensations as symptoms. Patients had no control over their symptoms and could not understand where they had come from because they had not experienced the events in a normal, primary consciousness. Freud was therefore more than a little justified in allowing himself the anachronistic expression that Breuer had *followed* Charcot in making assumptions about the hypnoid state (Freud, 1896c, pp.194-195).

By the time Breuer came to prepare Anna O.'s case for publication, dissociation theory, from which Charcot's notions formally derive, had been fully established by French workers in the field of psychopathology, mainly by Charcot's colleagues. Dissociation theory had developed from the study of the many cases of alternating states of consciousness and multiple personality reported between 1816, when the most famous of the early cases appeared (Mitchill, 1817; Carlson 1984, 1989), and 1858 when Azam began his observations on Felida X. (Azam, 1876; Taylor and Martin, 1944). What makes Felida X. important is not only that in her primary state of consciousness she had no knowledge of what transpired in the secondary state: hers was the first case in which it was noted that symptoms present in one state were absent in the other. In her primary state, Felida X. was morose and afflicted with hysterical deliria, convulsions, paralyses, and contractures but bright, affectionate, and symptom-free in the second. Later workers like Mesnet (1874, cited by Taine, 1873), Dufay (1876), Camuset (1882), and Bourru and Burot (1885) showed how the different states could be brought about and demonstrated, just as in Anna O.'s case, that the kinds of symptoms displayed, or whether they were present at all, depended upon which of the states was manifest.

Breuer further believed that the first hypnoid state acted as a focus: experiences occurring in other hypnoid states tended to become connected with it. In the *Studies on Hysteria*, which contained the later published account of Anna O., Breuer used the French term *condition seconde* for the totality of this group of memories. Its fully developed form was a dual consciousness, or *double conscience*, and it was a secondary consciousness of this kind which was the repository of all of Anna O.'s symptoms.

Breuer proposed that Anna O.'s habitual day-dreaming and reveries were predisposing causes. By themselves neither they nor their contents were abnormal - the states were readily interrupted and there was no amnesia for their content. The illness proper began with the "root cause" of the snake hallucination and arm paralysis. Both were experienced in an

hypnoid state and both recurred momentarily the next day when, in an *absence*, Anna O. saw a snake-like object. The same day she was in such a "state of abstraction" while waiting for the surgeon that she failed to perceive him entering the room. The reveries and day-dreams, the hallucination and paralysis, and the unresponsiveness to the surgeon, now formed a *condition seconde* and subsequent experiences incorporated into it. For example, her eyes filled with tears when her father had asked her the time. In trying to read the watch face, she had brought it close to her eyes and produced an enlarged image and a convergent squint. These visual peculiarities were added to the *condition seconde*; when they recurred they produced a squint and a general tendency to see things as enlarged (macropsia). The strengthening of the secondary consciousness until it was permanently present, brought into being a set of more or less perfectly understandable daytime symptoms. Just as gradually as the complex secondary state of consciousness was elaborated so a more complex set of symptoms was also being elaborated. Not until December 1880 were anything other than the *absences* manifest and initially they were so fleeting as to have been unnoticed. But by the time Anna O. was herself fatigued enough to be confined to bed, the *condition seconde* was strong enough to be present during most of the day. Breuer, it will be remembered, emphasised that the 'newness' of her symptoms was only apparent - according to him all of them had been present soon after the original foundation hallucination.

Some of the other theoretical constructs upon which Breuer drew were formulated originally by two of Charcot's close colleagues, Binet and Pierre Janet. Both Binet and Janet had described a secondary consciousness forming from the remnants of experiences occurring in spontaneous somnambulistic states and both had offered explanations of how it had come about. While their major theoretical accounts were published relatively late (Binet, 1889-90, 1892; Janet, P., 1889), most of the theory was contained in papers appearing shortly after 1886 (Binet and Féré, 1887b; Binet, 1889; Janet, P., 1886, 1887, 1888).

That Breuer eventually came to owe the French school of psychopathologists an extensive theoretical debt is shown by comparing his original case notes on Anna O., which contain practically no theory, with his discussion of her in the *Studies on Hysteria*. Nevertheless, even his initial conception of her hysteria was probably not far removed from that of Charcot and his school. In attributing the origins of symptoms to ideas arising in a state resembling hypnosis, and in characterising them as psychical foreign bodies, Breuer's views were at one with the conclusions Charcot had drawn from his case analyses and from his experiments with hypnosis. We can take Freud's partial anachronism further and say that in introducing the concept of hypnoid hysteria Breuer extended the views of Charcot and his colleagues on the origins of traumatic hysteria and the mechanism of hysterical symptom formation. It is not surprising Freud responded so positively to Charcot's teachings or that, with Breuer, he

would come to insist that what Charcot had termed traumatic hysteria should be the model for all hysteria (Breuer and Freud, 1893, pp.5-6. Cf. Freud, 1893a, pp.30-31).

CHARCOT, FREUD, AND DETERMINISM

Charcot's observations seemed to him to be consistent with hysterical symptoms being produced by lawful alterations in physiological functioning. The phenomena themselves, the pattern in the attacks of major hysteria, and the similarity of the symptoms across cultures and historical periods, all seemed to be part of such a precise determination that suggestion could be ruled out as a cause (Charcot, 1887-1888, p.305). Yet unconscious influences had undoubtedly contributed to the symptoms of Charcot's hysterical patients. Unconscious imitation of organic disorders was an always present possibility. Who, even today, has not seen a spastic contracture, a post-stroke paralysis or aphasia, or an epileptic or febrile convulsion? The relatively poorer health services of an earlier era probably made such models even more readily available. Some patients learned the phenomena through unconscious imitation of other hysterical patients. This was especially so at the Salpêtrière. Practically nowhere else were stages of major hysteria observed that corresponded to those Charcot had so meticulously described. Other patients learned by divining what the physician wanted to see. For example, in Brouillet's well-known painting *Une leçon clinique à la Salpêtrière*, the hysterical patient Charcot is demonstrating is at the point of passing from the first stage of her attack into the stage of 'clownism' or major movements in which she will display the famous *arc de cercle*. Ellenberger has pointed out her model. On the wall of the lecture theatre, in full view of the patient and her audience, hangs the 1878 drawing by Charcot's colleague Paul Richer of another patient who has completed her arc (Ellenberger, 1970, between pp.330-331. Caption to his reproduction of the painting). Charcot's patient can use that model to to complete her own.

There is an important paradox in these kinds of unconscious learning. None is in opposition to Charcot's theses, each is actually consistent with them. *The idea* in the mind of *the patient* causes the symptom. Even when the idea originates with the experimenter or physician, it is the unconscious transformation of that idea *by the patient* that brings the symptom into being. Transformation may be accepted, indeed it must be, even though Charcot's false physiological theory has to be rejected.

Another, even stronger reason than these general considerations for including suggested ideas among the lawful determinants of hysterical symptoms is the implication of the fact that the detailed characteristics of symptoms are completely determined by them. I am not aware that this implication, which follows directly from an observation of Janet's reported 1892, has previously been drawn. In that year, Janet began an over-all

analysis of hysterical symptoms with a consideration of the peculiarities of hysterical anaesthesias. Like Charcot, Janet was also impressed by the impossibility of the pathology of the ''lesion'' corresponding with what was known from anatomy. A single whole finger might be anaesthetic from its junction with the hand or the whole hand below the wrist might have lost sensation just as if it had been covered by a glove. As Janet put it:

> These distributions of anaesthesia obviously do not correspond to anatomical regions, it is not the region innervated by the cubital or the median nerve that is anaesthetic - it is *the* hand or *the* wrist. (Janet, P., 1892a. My emphasis, MBM)

He then drew the following extraordinarily novel conclusion:

> the localization is not anatomical, it is physiological, as M. Charcot rightly says. But I would like to add a word, this distribution corresponds to a very *crude*, very *common, physiology*. When an hysterical patient has her hand paralysed, where should the insensitive area be? On the muscles that are not functioning, that is on the forearm. And yet, the anaesthesia is nearly always confined to the hand itself and to the wrist. In hysterical blindness, anaesthesia bears not only on the retina, but on the conjunctiva, and even on the eyelids: the amaurotic hysterical has spectacles of anaesthesia on the face. She has *lost her eye*, not only in the physiological sense, but in the *popular sense* of the word, namely all that fills the orbit. It would seem that, even in these localized anaesthesias, the habitual associations of our sensations, *the ideas* we conceive *of our organs*, play an important role and determine these distributions. (ibid., My emphasis, MBM)

What was lost to normal consciousness was idea of the organ or its function, as Le Log___'s feeling that his legs were 'dead' illustrate.

From Charcot's discussion, it is clear he did not differentiate sharply between the idea of organ dysfunction and the idea of the organ itself (Charcot, 1887/1889, pp.254-256, 264-273, 297-302; Charcot and Marie, 1892). What Janet was proposing, in effect, was that the latter gave rise to the former. The fact that the patient's ordinary, everyday idea of an organ or function was reflected in the symptom had another radical consequence:

> hysterical anaesthesia is not for us an organic malady, it is a mental malady, a psychological malady. It exists not in the limbs, nor in the medulla, but is represented in the mind, if you like, in the cortical regions of the brain. (Janet, P., 1892a)

Consequently, the understanding of hysterical anaesthesia had to be based on what the psychologist could contribute about the effect of ideas rather than on what the anatomist knew about the distribution of nerves or what the physiologist understood of physical function.

We now see, immediately as it were, why hysterical symptoms *have* to be uniform across cultures and historical epochs. To the extent that the

ideas people have of their organs and their functions are uniform, so the hysterical symptoms those ideas generate will also be uniform. Had none of Charcot's hysterical patients ever seen an organic symptom or any other hysterical patient, or been present at his demonstrations, the details of their symptoms would have been exactly the same. The similarity of their ideas would have proved a more than adequate determinant.

Determinism in the formation of hysterical symptoms reveals itself in the lawfulness and regularity with which ideas cause symptoms, whatever their source, and especially in the lawful, regular way in which they determine the detailed characteristics of the symptoms. Consequently, it is irrelevant to object to Charcot's work, as Page, Babinski, and Hurst did (Hurst, 1920, pp.6-7), on the grounds that such pathognomonic signs of hysteria as hemi-anaesthesia or restriction of the visual field are produced by the physician's examination of the patient. In supporting his objection, Hurst also showed that when even medical students were asked to indicate how organs would be affected were they to become anaesthetic or paralysed they also drew on the "crude", "common", or "popular" physiology which Janet had described (op. cit., pp.8-11). When Hurst tended to dismiss hysterical symptoms as due to the imagination, he therefore did not see how his demonstration of Janet's point actually supported determinism.

CONCLUSION

As with hypnosis, Freud never saw that the role of unconscious influences in hysteria could be acknowledged within a deterministic framework. The key to his views is to be found in some comments he made on hypnosis and hysteria in the Preface to his translation of Bernheim's *De La Suggestion*:

> If the supporters of the suggestion theory are right, all the observations made at the Salpêtrière are worthless; indeed, they become errors in observation. The hypnosis of hysterical patients would have no characteristics of its own; but every physician would be free to produce any symptomatology that he liked in the patients he hypnotised. We should not learn from the study of major hypnotism what alterations in excitability succeed one another in the nervous system of hysterical patients in response to certain kinds of interventions; we should merely learn what intentions Charcot suggested (in a manner of which he himself was unconscious) to the subjects of his experiments - a thing entirely irrelevant to our understanding alike of hypnosis and of hysteria. (Freud, 1888c, pp.77-78)

Freud was to be as wrong about hysteria as he had been about hypnosis.

Suggestion can be given its proper role and the three most valid and important aspects of Charcot's theory accepted: the role of ideas in producing symptoms, the notion of an active but unconscious mental process elaborating the symptoms, and the concept of hysterical symptoms being

maintained by a set of isolated or dissociated mental processes. What needs to be rejected is Charcot's narrow, mistaken view of determinism, the one adopted by Freud.

When Freud came to treat his own patients, he never accepted that influences transmitted unconsciously from him to them had important effects upon what they claimed to recall about the origins of their symptoms. His view was that the important determinants of remembering were internal, part of the very fabric of the patient's thoughts, and as impervious to outside influence as the processes determining the phenomena of hypnosis and hysteria at the Salpêtrière were supposed to have been. While it is true that his adaptation of 'Breuer's method' for use in the waking state led him to develop a set of explanatory notions very different from the dissociation concepts Breuer and Charcot had favoured, it did not cause him to give up his mistaken view of the determinants of psychological phenomena.

4 FREUD'S ADAPTATION OF BREUER'S TREATMENT METHOD

Macbeth: Can'st thou not Minister to a minde diseas'd,
Pluck from the Memory a rooted Sorrow,
Raze out the written troubles of the Braine.

Shakespeare: *Macbeth*, Act V, iii.

Between 1886 and 1895, after returning from the Salpêtrière, Freud devised a method for removing the symptoms of hysteria in the waking state. Important as this was in its own right, what was more fundamental was the complementary set of waking-state theoretical concepts by which Freud tried to explain how symptoms were acquired in normal consciousness. For him, symptoms were determined by the memories of affectively charged events which patients could recall having consciously tried to rid themselves. As Freud came to see it, mental life largely reflected the difficulty the nervous system had in dealing with the effects of unpleasant emotion. The focus of this chapter is the gradual emergence over the nine-year period of the new therapy, which was based on Freud's new method of retrieving memories in the waking-state, and the theoretical concepts that went with it.

In this Chapter, I take up the treatment methods and the more or less immediate conceptual issues to which they are related. I begin with Freud's use of Bernheim's method of direct hypnotic suggestion and the variant of it pioneered by Delboeuf and Janet, showing affect not to have been involved in either. Freud's dissatisfaction with hypnosis is seen as a preliminary to his developing a waking-state therapy and I argue that consistency required him to explain how symptoms could also be acquired in the waking state. I demonstrate that it was via some concepts of Janet's and Hughlings Jackson's that affect first came into Freud's conceptualisation of symptom formation before tracing the very gradual emergence of his concepts of repression and conversion after he had recognised it. Finally, I show how Freud then built up a model deriving from Meynert, Charcot, and Jackson, from which he explained how the memories recalled by the patient were related to the symptom. I examine the deterministic assumptions underlying the model and illustrate Freud's use of them in guiding his therapy.

FREUD'S USE OF HYPNOSIS

As a preliminary to considering Freud's therapy, I would stress that in tracing how the most distinctive of the methods by which Freud treated psycho-neurotic conditions developed, we need to be very clear what 'Breuer's method' was. As Breuer practised it with Anna O., it had three components: auto-hypnosis, the retrieval of the causal memory, and talking about the original event. A deliberately induced hypnosis and an insistence on the expression of emotion during the verbal utterance were later components. Freud appears to have begun treating nervous disorders soon after he returned from Paris in 1886 but nearly seven years were to elapse before the expression of emotion became basic to his therapy. In the first six-and-a-half years of the nine, Freud either used ordinary hypnotic suggestion or he used hypnosis to recover the causal memory before modifying it by direct hypnotic suggestion. Although he said he first used 'Breuer's method' in mid-1889, all the evidence is against his version of the cathartic method then being one that required the patient to express the emotion associated with the causal event. Not until mid-1892 did affective concepts find their way into Freud's theory and it was only after that they came into his practise. Consequently, not until the end of that period could Freud begin to develop what is usually thought of as his most distinctive contribution to psychotherapy: abreaction of previously unexpressed emotion.

From the middle of 1885 Freud utilised the traditional methods of deep and whole body massage for paralyses and contractures, baths for relaxation, and both baths and mild electrical stimulation for the restoration of lost sensibilities and functions (Masson, 1985, Letters of 24.11.87, 4.2.88, and 28.5.88; E. Jones, 1953-1957, *1*, pp.234-235). At the same time, he occasionally used direct hypnotic suggestion; Jones mentions, for example, its use in June, 1886 (op. cit., p.235). But, as a letter to Fliess indicates, it was not until late in 1887 he even began using hypnosis in a systematic way, obtaining "all sorts of small but noteworthy successes" (Masson, 1985, Letter of 28.12.87).

It was not to Charcot that Freud was indebted for the use of hypnosis in therapy. While he accepted and developed Charcot's propositions about traumatic hysteria, he disagreed with Charcot almost from the beginning on the usefulness of hypnotic suggestion as a therapy. Contrary to many assertions (for example, Wollheim's, 1971, p.24), Charcot was quite wary of using hypnosis for treating hysteria. The testimony of Charcot's colleagues (Freud, 1892-94, pp.140-141; Janet, P., 1919/1925, Vol.1, pp.326-327), and the judgement of historians (Owen, 1971, pp.127-133; Schneck, 1961; Veith, 1965, p.236), shows Charcot made little therapeutic use of hypnosis.

A careful reading of Charcot's own works bears out his negative evaluation of the therapeutic use of hypnosis. Thus, Charcot extols the

virtues of hypnosis as a means of *studying* hysteria before remarking:

> from a *therapeutical point of view*, for example, hypnotism has not so far given all the results that we were justified in expecting from it. Its scope of action is limited. Contrary to what might have been expected *à priori*, its action on ... hysteria... is restricted. (Charcot and de la Tourette, 1892)

Again specifically about the treatment of hysteria:

> *Hypnotism* may be of some service, but not so much as one might *à priori* expect; it may be employed against some local symptoms. Although it may be true that in hysteria as such hypnotism prudently applied has not any injurious effects, it is quite certain that in the majority of cases the inconsiderate use or abuse of hypnotism has been followed by very serious complications. *Suggestion* may be employed without hypnotism, and may be quite as effective as in hypnotic sleep. (Charcot and Marie, 1892.)

Charcot largely restricted hypnosis to experiments and demonstrations.

Freud nevertheless added hypnotic suggestion to his treatment methods almost immediately he returned to Vienna. The step was probably provoked by his reading Bernheim's book on hypnotic suggestion which had appeared the year before. In the same letter to Fliess announcing his "successes" he explained he was "already bound by a contract" to translate it (Masson, 1985, Letter of 28.12.87).

Bernheim's influence is to be seen very clearly in Freud's use of the Nancy school procedures for inducing hypnosis. In Bum's *Lexicon* Freud described only two induction procedures: training subjects to imitate at a signal those other patients who had already been hypnotized, and techniques "which have in common the fact that they recall falling asleep through certain physical sensations" suggested directly to them (Freud, 1891b, pp.107-108. Cf. Freud, 1890/1905c, p.294). Freud recalled seeing the first procedure in Bernheim's clinic, while the second is, even down to the visual fixation on the fingers, the only method described at length in Bernheim's book (Bernheim, 1887/1888a, pp.1-4. Cf. Liébeault cited in Chertok and Saussure, 1973/1979, p.42). Neither of these methods resemble those of Charcot who required the hypnotiser to stare fixedly at the subject, to apply light pressure to the subject's closed eyes (Charcot, 1879-80, pp.162-167), or to use Braid's method of inducing fatigue in the muscles of the eye (Charcot and de la Tourette, 1892). Suggestions like those used by Bernheim were rarely if ever employed at the Salpêtrière and suggestion was not regarded as an important aspect of the induction. G. M. Robertson's (1892) contemporary account confirms these differences.

As for treatment, Freud quite specifically adopted the second of the two methods used by Bernheim whose patients were either reassured they would be well on waking from their hypnotic sleep or given direct suggestions that their symptoms would disappear (Bernheim, 1887/1888a, Part II, Chapter 1). In Villaret's *Handwörterbuch* Freud described the

"direct treatment" of the psychological sources of hysterical symptoms as consisting of:

> giving the patient under hypnosis a suggestion which contains the removal of the disorder in question. Thus, for instance, we cure a *tussis nervosa hysterica* by pressing on the larynx of the hypnotized patient and assuring him that the stimulus to coughing has been removed. (Freud, 1888a, p.56. Emphasis altered, MBM)

G. M. Robertson (1892) also mentioned how Bernheim had similarly added to the verbal suggestion that a sciatic pain would disappear by passing his hand along the patient's leg "to suggest that some active measures were being used".

Freud's other references to hypnotic treatment - the brief one in the review of Forel's *Hypnotism* (Freud, 1889, p.100), the somewhat longer discussions in the *Die Gesundheit* chapter written in 1890 (Freud, 1890/1905c. Cf. Editor's Introduction, *Standard Edition, 1*, pp.63-64), and Bum's *Lexicon* (Freud, 1891b, pp.111-112) - similarly stress the importance of removing the symptom by direct suggestion. Indeed, in Bum he went so far as to state:

> The true therapeutic value of hypnosis lies in the *suggestions* made during it. These suggestions consist in an energetic denial of the ailments of which the patient has complained, or in an assurance that he can do something, or in a command to perform it. (Freud, 1891b, p.111)

This whole-hearted endorsement of Bernheim's technique was also implicit in Freud's use of it in the first of the two treatments of the patient described in the paper on hysterical counter-will, a treatment which cannot have taken place later than mid-1890 (Freud, 1892-1893, pp.118-120). Further, as late as April and May of 1892, Freud gave two expository lectures in which he again chose to speak of Bernheim's suggestion method rather than any other (Ellenberger, 1970, pp.443-444). It was also then that the counter-will patient probably had her last hypnotic suggestion session. Later in that year, in the autumn, Freud began treating Elisabeth von R. without using suggestion at all (Breuer and Freud, 1895, p.135).

Freud seems to have abandoned Bernheim's direct hypnotic suggestion during a short period late in the summer of 1892. Until then his therapy relied on it even when he first started to use Breuer's "method" or "technique".

ADDING SUGGESTION TO BREUER'S TECHNIQUE

Freud was quite specific that he first drew on "Breuer's technique" with the patient known as Emmy von N. in May of 1889 (Breuer and Freud, 1895, p.48). Since then, Swales has advanced Freud's treatment of Frau Cäcilie, which he dates from the same period, as based on reliving emotion-

ally charged memories under hypnosis (Swales, 1986b). However, in neither case was any kind of cathartic method used.

Emmy von N.

Freud said it was because Emmy von N. was an hysteric who could easily be put into the somnambulistic state that he had decided to use Breuer's method (Breuer and Freud, 1895, p.48). We note, however, that Freud calls it a technique of "*investigation* under hypnosis" and not a *therapy* (ibid. My emphasis, MBM). What Freud did use hypnosis with her for was to *locate* the incidents that seemed to have caused her symptoms but his *treatment* did not emphasise either talking or emotional expression. We shall see that while the therapy did have a new element to it, it was essentially a variant of Bernheim's technique of direct suggestion.

Freud differentiated three components in Emmy von N.'s treatment. The first was the usual Bernheim method of direct hypnotic suggestion. Freud suggested to Emmy von N. that "all her symptoms should get better" (op. cit., p.51), her gastric pains would disappear (op. cit., p.54), her menstruation would resume a 28-day periodicity (op. cit., p.57) etc.

The second component was an investigation of the origins of the symptoms under hypnosis and then the use of suggestion to weaken or remove the pathogenic ideas on which they were based. That this was a variant of the method of direct hypnotic suggestion is clear from Freud's description. Thus Freud's suggestions included wiping away or effacing the mental images "so that she is no longer able to see them before her" (op. cit., p.53), suggestions that the images "would only appear to her again indistinctly and without strength" (op. cit., p.55), and suggestions to remove "her whole recollection of them, as though they had never been present in her mind" (op. cit., p.61).

Freud used the variant in treating some of Emmy von N.'s motor symptoms which were based on incidents in which she had been frightened. For example, she frequently broke off her conversation, contorted her face into an expression of hatred and disgust, stretched out her hand and exclaimed anxiously: "Keep still! - Don't say anything! - Don't touch me!" [1] Just as suddenly as she had broken off her conversation, she would then resume it (op. cit., p.49). When questioned under hypnosis about the origin of this symptom, she described four separate frightening events, widely separated in time, "in a single sentence and in such rapid succession that they might have been a single episode in four acts" (op. cit., p.57). Freud had concluded that her "protective formula" defended her from the recurrence of the frightening experiences but he restricted

1. Pappenheim (1980) has claimed that Emmy von N. suffered from Gilles de la Tourette's syndrome but the changes in the symptoms are not consistent with that organic condition. In any case, Pappenheim does not make a detailed enough comparision of Emmy von N.'s symptoms with their alleged organic counterparts.

himself to using direct hypnotic suggestion to remove the anticipatory fear (ibid). The traumatic memories sustaining other motor symptoms, a stammer and a vocal tic, were similarly modified (op. cit., pp.54, 57-58).

Emmy von N.'s talking about the traumatic events was the third component. About a week after the treatment began she seems to have calmed herself by recounting a series of frightening experiences:

> At the end of each separate story she twitched all over and took on a look of fear and horror. At the end of the last one she opened her mouth wide and panted for breath. The words in which she described the terrifying subject-matter of her experience were pronounced with difficulty and between gasps. Afterwards her features became peaceful. (op. cit., p.53)

Even so, more than talking was involved:

> My therapy consists in wiping away these pictures, so that she is no longer able to see them before her. To give support to my suggestion I stroked her several times over the eyes. (ibid)

Freud noted this meant that the effect of talking - especially of the emotional expression which accompanied it - could not be separated from that due to hypnotic suggestion (ibid.).

Frau Cäcilie

Swales (1986b) dates the beginning of the treatment of Frau Cäcilie (Anna von Lieben) as possibly in July of 1889 and describes it as involving recollection and emotional expression under hypnosis. By themselves those facts would run against my thesis and would do so even more strongly if Masson's 1888 date is correct (Masson, 1985, p.20).

Freud mentions Frau Cäcilie in passing to illustrate his theses on (1) the formation of false connections in consciousness, (2) the way a premonition of an unconscious process might reveal itself in consciousness, (3) the possibility of hysterical symptoms existing in the gifted, and (4) the "weakness" Janet placed centrally as a consequence of the domination of unconscious ideas rather than a cause of them (Breuer and Freud, 1895, pp.67, n.1, 76, n.1, 103, 238). Freud classed the "several hundreds of ... cycles" he witnessed with Frau Cäcilie as providing him with the information that gave the direct impetus "to the publication of [the] 'Preliminary Communication'" (op. cit., 1895, p.178). Because by far the largest part of Freud's use of her case was to *illustrate* his views on the formation of symptoms by symbolisation (op. cit., pp.175-181 and p.181, n1) I believe Freud's reference is to that role rather than to the results of her treatment which, according to Swales, was not successful.

Actually, neither Freud nor Breuer say very much about Frau Cäcilie's treatment. Freud treated her attacks of pain by laying "energetic prohibitions" on them under hypnosis, that is, by the usual Bernheim

method (op. cit., p.177). About a year later he added to this therapy. Frau Cäcilie then began to have hysterical attacks in which the hallucinations pointed to an earlier causal experience:

> I was sent for at the climax of the attack, induced a state of hypnosis, called up the reproduction of the traumatic experience and hastened the end of the attack by *artificial means*. (op. cit., p.178. My emphasis, MBM)

What were Freud's "artificial means"? Why does he not mention or allude to talking about origins either in this general description or when he discusses the treatment of those specific symptoms, such as facial neuralgia, heel pain, head pain, various miscellaneous pains, and *globus hystericus*, that did require her to reproduce the circumstances of their origin? (op. cit., pp.178-180).

Breuer does mention what seems to be a 'talking cure' with emotional expression but what he says is not without its peculiarities:

> She would ... be relieved of the unconscious idea (the memory of a psychical trauma, *often* belonging to the remote past), *either* by the physician under hypnosis *or* by her suddenly describing the event in a state of agitation and to the accompaniment of a lively emotion. (op. cit., p.231. My emphasis, MBM)

Why does Breuer confine emotional expression to the effects of what seem to be Frau Cäcilie's spontaneous utterance and not include it as part of an active treatment? Was the physician's contribution simply one of removing the pathogenic idea under hypnosis?

As with Emmy von N., there were clearly different components to Frau Cäcilie's treatment. How did they evolve? Despite Swales' (usual) assiduous search, the record is silent. The one very important clue he does provide supports my thesis: Freud wrote to Minna Bernays in July of 1889 advising her to read Edward Bellamy's (1880/1969) novel *Dr. Heidenhoff's Process* if she wished to understand his treatment of Frau Cäcilie. The fictional doctor Heidenhoff had invented a technique, which he called "thought extirpation" for wiping out troublesome memories. It involved passing a non-convulsive electric current through the patient's head while the patient concentrated on the reminiscence. Because Heidenhoff recommended it for any kind of troublesome memory, not just for traumatic ones, and because neither narration nor emotional expression was required, Freud was alluding to no cathartic method. What "Heidenhoff's process" most resembles, indeed surprisingly so as we shall now see, is a variant of the method of suggestion apparently pioneered by Delboeuf and Janet. The difference is that Bellamy's character did it electrically while Delboeuf and Janet did it hypnotically. Frau Cäcilie's treatment does not therefore conflict with my argument that prior to 1892 or 1893 Freud's therapy was based on direct hypnotic suggestion.

FREUD AND JANET'S AND DELBOEUF'S THERAPY

When Freud summarised his treatment of Emmy von N. he brought out its three components very clearly:

> As is *the usual practice* in hypnotic psychotherapy, I fought against the patient's pathological ideas by means of assurances and prohibitions, and by putting forward opposing ideas of every sort. *But I did not content myself with this.* I investigated the genesis of the individual symptoms so as to be able to combat the premises on which the pathological ideas were erected. In the course of such analysis it habitually happened that the patient gave verbal utterance with the most violent agitation to matters whose accompanying affect had hitherto only found outlet as a motor expression of emotion. I cannot say how much of the therapeutic success each time was due to my suggesting the symptom away *in statu nascendi* and how much to my resolving the affect. (Breuer and Freud, 1895, p.101. Emphasis altered, MBM)

Freud's contrasting the second component where he "investigated the genesis" of the symptom with "the usual practice" of direct suggestion tells us, I believe, that the hypnotic attack on the pathogenic memory was a new therapeutic tactic.

Where did the new technique come from? It was not part of Breuer's or Bernheim's repertoire. However, only a few months before Emmy von N.'s treatment began, both Delboeuf and Janet had used almost exactly the same technique, which they described in almost the same words as Freud, for the treatment of recurrent hallucinations similar to those exhibited by Emmy von N. I shall now show that Freud almost certainly knew of Delboeuf's use of the method, and probably knew of Janet's, before his own use of it.

Delboeuf's therapy

Early in 1888, Delboeuf visited Bernheim's clinic at Nancy and his long, four part account of the visit was liberally illustrated with summary case descriptions and contained a comprehensive discussion of the theoretical differences between the Nancy and Salpêtrière schools (Delboeuf, 1888-1889, Parts 1-4). In the third installment of the paper Delboeuf described a case of his own in which the treatment clearly went far beyond the simple, direct suggestions and reassurances used by Bernheim:

> Here is a poor mother. Her room was next to that of her son who was sick and dying. One day, about six in the morning, as she sleeps, she thinks she hears this cry: Mama! Half awake, she thinks she has been dreaming and falls asleep again. A half-hour later, she enters, as was her practice, the room of her son and finds him stretched out on the ground, dead and covered with the blood that had come out of his mouth. At the sight, her reason flees, she is assailed by remorse, and from that day, a cry resounds incessantly in her ears: Mama! This cry, she ends by producing it herself, both at home, in the presence of her family and in the presence of strangers,

> on the street, on the train, at every instant the bloody image of her son presents itself in front of her, and the cry: Mama! bursts from her breast.(Delboeuf, 1888-1889, Part 3, pp.8-9. My emphasis, MBM. Cf. Delboeuf, 1889, pp.51-52)

The patient was brought to Delboeuf because her own doctor had not been able to hypnotize her:

> She is put to sleep under his eyes after a few minutes. At my voice *the vision pales, effaces itself, disappears*. I dare defy her to see it. I go as far as making a dramatic description of the scene. It is all over, no more bloody phantom arising unexpectedly, no more cry: the patient can smile. (ibid. My emphasis, MBM.)

An hour later the patient accidentally saw a dying dog and relapsed. "A few" more sessions of treatment spread over several weeks brought about a permanent cure.

Later in the same paper Delboeuf referred to the case, this time in the context of the variability in susceptibility of patients to hypnotic suggestion. After proposing that susceptibility fluctuated throughout the day, and that the art of the hypnotist consisted in bringing about the moment of maximum susceptibility and prolonging it, Delboeuf added:

> My observations lead me even further. They tend to make me believe that many nervous states or mental illnesses have as their origin a natural suggestion which acted as this special moment. Let us recall the case of the mother whose lamentable story I recounted above. Consequently one can understand how the hypnotist assists the cure. *He puts the subject back into the state in which his trouble manifested itself and combats with the spoken word the same trouble, but in a state of rebirth.* [Il remet le sujet dans l'état où le mal s'est manifeste et combat par la parole le même mal, mais renaissant.] (Delboeuf, 1888-1889, Part 3, p.28. Cf. Delboeuf, 1889, p.71. My emphasis, MBM)

By suggesting the vision would pale and disappear, and by defying the patient to see it, Delboeuf was combating the premise on which the pathological fixed idea was erected; by returning the subject to the earlier state he was "suggesting the symptom away *in statu nascendi*".

Janet's therapy

Janet's patient, Marie, suffered from recurrent hysterical crises with deliria, hallucinations, and violent bodily contortions beginning two days after the onset of each of her menstrual periods. Concurrently with the attack, menstruation was suppressed. In the intervals between attacks limited and variable contractures and anaesthesias were present together with a total blindness of the left eye. Minor hallucinatory terror attacks, independent of the other two groups of symptoms, were sometimes observed. After seven months of unsuccessful conventional treatment Janet hypnotized her and asked her about the first menstruation. She

recalled it to have been an entirely unexpected event when she was thirteen. Reacting with shame, she had tried to stop the menstrual flow by immersing herself in cold water. Menstruation ceased, but she then had a severe attack of shivering followed by several days of delirium. Menstruation did not recur until five years later. When it did, the symptoms present on her admission to hospital came with it. Janet decided to modify the memory of the immersion:

> I tried to take away from the somnambulistic consciousness this fixed and absurd idea that menstruation had been arrested by a cold bath. I was unable to do this at the first attempt; the fixed idea persisted and the menstrual period which commenced two days later was almost the same as the preceding ones. But, with more time at my disposal, I tried once more: I was able to efface the idea only by *a singular means*. It was necessary to bring her back by suggestion to the age of thirteen years, to put her back *into the initial conditions* of the delirium, and then to convince her that her period had lasted three days and had not been interrupted by any unfortunate accident. Now, when this was done, the succeeding period arrived on the due date and lasted for three days, without bringing with it any pain, any convulsion nor any delirium. (Janet, P., 1889, pp.438-439. My emphasis, MBM)

Janet treated two other groups of symptoms similarly. The minor hallucinatory attacks of terror were repetitions of feelings experienced when she had seen an old woman fall down some stairs and kill herself. After bringing Marie back:

> *to the moment of the accident*, I was able, not without difficulty, *to change the mental image*, to show her that the old woman had only stumbled and had not killed herself, and efface the terrifying idea: the attacks of terror did not recur. (op. cit., p.439)

After having established the left-sided facial anaesthesia and left eye blindness had appeared at the age of six years after Marie had been forced to sleep with another child who had impetigo on the whole of the left side of her face, Janet attempted a similar cure. Under hypnosis:

> I put her back with the child of whom she had had such horror, I make her believe the child is very nice and does not have impetigo, she is half convinced. After two repetitions of the scene, I get the best of it and she caresses the imaginary child without fear. The sensitivity of the left side reappears without difficulty and, when I wake her, Marie sees clearly with the left eye. (op. cit., p.440)

All three groups of symptoms had been removed by modifying or effacing the mental images associated with their onset. Like Delboeuf, Janet was returning the subject to the moment of maximum susceptibility, there combating the symptom as it was reborn.

Because Janet's treatment of L., or Lucie, has been proposed by Ellenberger as the first instance of a cathartic cure (Ellenberger, 1970,

p.413, n.84) we might note how it differs from Marie's. Lucie's case is described in *L'Automatisme Psychologique* (Janet, P., 1889) but Janet had reported it previously (Janet, P., 1886, 1887, 1888). It is clear it does not qualify for the distinction Ellenberger gives it. Lucie's hallucinatory terrors were traced to a sudden fright at the age of nine years; the symptoms reappeared with the emergence of a second personality (Adrienne) that had begun to form at the time. Janet described the treatment only briefly because it was to have been reported fully in another paper by himself and Powilewicz in *Revue Scientifique* (Janet, P., 1886). However, from my own search of the literature, as well as that of van der Hart and Braun (1986), it seems that Janet did not publish any more details.

What Janet did was to command the secondary personality not to have hallucinatory attacks. Lucie, the primary personality, was thereupon freed of the symptom. Janet's treatment of Lucie was clearly ordinary direct suggestion, as Myers', F. W. H. (1886-87, p.245) contemporaneous comment confirms. Although the explanation of the symptom is similar to that advanced by Breuer, the method of treatment has nothing in common with his or with what Janet did with Marie. Myers, F. W. H. (op. cit., pp.240-241) saw clearly how it hinged on a direct suggestion to the secondary consciousness, as did William James (1890a; 1890b, pp.386-387) a little later. The only feature resembling Breuer's cathartic method is Janet's use of hypnosis to locate the memory of the original event.

Dating the treatments

Direct hypnotic suggestion was Freud's "usual practice" and his attack on Emmy von N.'s pathogenic memories was novel. Delboeuf and Janet had also used the new therapy. Can the three uses be dated?

Freud and Emmy von N. Freud gave the 1st May, 1889 as the date on which he began Emmy von N.'s treatment (Breuer and Freud, 1895, p.77) and, although the Editors of the *Standard Edition* (*2*, pp.307-309) did suggest it began a year earlier and the problem of the chronology had to remain "an open one", Andersson's investigations leave no doubt that Freud's dating is correct (Andersson, 1962, p.74, n.1). The memoirs of Emmy von N.'s daughter confirm that treatment began in the spring of 1889 (Andersson, 1979). Other unpublished documents also collected by Andersson are, he tells me, consistent with that dating.

Delboeuf's paper and book The date printed on the first part of Delboeuf's article in the volume of *Revue de Belgique* in which it appeared is 15th November, 1888. The succeeding parts are dated 15th December, 1888, 15th January, 1889, and 15th March, 1889. The four parts, virtually unchanged, were published almost immediately as a book (Delboeuf, 1889). The book must have been in circulation by at least May of 1889, because it was listed as awaiting review in *Revue Philosophique* for June of that year (Vol. *27*, p.651). Thus the crucial third part of Delboeuf's paper

appeared well before Freud began treating Emmy von N. and the book itself appeared at about the time the treatment actually started. Delboeuf's description could well have been one of the influences leading Freud to the particular technique he used with Emmy von N.

In this connection it is worth noting that, although Delboeuf's general remarks on variations in susceptibility and the role of the hypnotist occupy less than one page of over one hundred pages of text, they were regarded by G. C. Robertson, the then editor of *Mind*, as sufficiently novel to single them out for special mention in his July, 1889 review of Delboeuf's book (*Mind*, *14*, pp.470-471.) One may assume other readers would have placed the same importance upon them.

Janet's thesis The description of Marie's treatment first appeared in Janet's thesis which, as the preface indicates, was completed by December, 1888 and defended on 21st June, 1889 (Ellenberger, 1970, p.339). Professor Henri Faure, Directeur de Laboratoire de Psychologie Pathologique de la Sorbonne, has told me that Alcan's publication of the thesis as *L'Automatisme Psychologique* took place between Janet's completing it and April or early May, 1889. On publication twelve copies would have gone immediately to members of the Jury, some would have been retained by Janet, while the rest would have been kept by the publisher. Of these, one hundred would have been later deposited for distribution to University libraries and the remainder would have gone on sale immediately after the defence. Thus it is possible that printed copies of the thesis were in circulation even before the defence. Further it is known that news of Janet's work was widespread before the defence. In England, for example, Myers knew of Janet's thesis before April, 1889, since in a discussion of the then recent work on consciousness, memory, and alterations of personality he remarked that "Prof. Janet, of Havre, has a considerable book in preparation" (Myers, F. W. H. 1889-90, p.63). Freud could have known of Janet's treatment of Marie before treating Emmy von N.

Origins Two possibilities about the origins of the new treatment may be discounted because they are so unlikely. First, the method was developed by some unknown therapist and adopted almost simultaneously by Delboeuf, Janet, and Freud without any of them acknowledging the fact. Second, each arrived at the method independently. I think the evidence shows the more probable explanation to be that Freud adopted it after learning of its use by either Delboeuf or Janet. The dates leave us in no doubt that Freud could have known of Delbouef's treatment before beginning his and it is just possible that he could have known of Janet's.

An influence of Janet upon Freud may be contained in the use by both of such terms as "effacing" or "wiping out" to describe the alteration produced in the pathogenic memories. On the other hand, an influence of Delboeuf may be present in Freud's use of the word combat (*Kampfe* v. *combat*) to describe the attack on the pathogenic memory, and in the notion the suggestion was to be directed to the birth or rebirth of the symptom (*in*

the suggestion was to be directed to the birth or rebirth of the symptom (*in statu nascendi* v. *mais renaissant*). Neither of these quite central ideas is to be found in Janet's account.

There may be another clue in a curious feature of Breuer's and Freud's acknowledgment of Delboeuf's treatment. Breuer and Freud cited Janet as having produced a cure, presumably of Marie, by a method analogous to their own; however, Delboeuf was quoted, together with Binet, as having recognised only *the possibility* of such a therapy (Breuer and Freud, 1893, p.7, n.1). While this correctly represents Binet's proposal, it is obviously a totally incorrect account of what Delboeuf had done. Breuer's and Freud's quotation comes from precisely that part of Delboeuf's paper containing the speculation that cures were produced by returning the patient to the earlier state of susceptibility and actually begins with the sentence which immediately *follows* the one in which Delboeuf asked his readers to "recall the case of the mother whose lamentable story I recounted above". That is, Delboeuf mentioned a specific cure - a cure, at that, of a case identical to Freud's earliest using the same method - in the very sentence before the quotation which Breuer and Freud used to represent him as having seen only "the possibility" of a therapy. Does this oversight of Delboeuf's treatment while acknowledging Janet's signify a reversal of the real source of influence on the treatment of Emmy von N.?

Incidentally, if we assume that Emmy von N.'s talking and expressing emotion did have some effect, we seem to be faced with the problem of expectations. Why did Janet and Delboeuf not make observations similar to Freud's? Emmy von N. and Anna O. resembled each other in a number of important ways: both had recurrent and terrifying hallucinations intruding from a *condition seconde*, both had the ability to recall rapidly complete and complex sets of memories, and both had a facility in describing traumatic events. These similarities became apparent almost immediately and could well have created in Freud the expectation that Emmy von N.'s talking about the origins of her symptoms would result in the same kind of emotional calm for her as it had for Anna O. Were he to have transmitted such an expectation to her early in the treatment, the fact that the effects occurred earlier with her than with Anna O. (if they occurred at all) would be accounted for. Janet and Delboeuf, of course, had no such expectation and could have had no observations to make about the effect of talking.

FREUD'S THERAPY BETWEEN 1889 AND 1892

In the case of Emmy von N. Freud did not set talking about the origins of symptoms or expressing the original emotion as the *aim* of any part of Emmy von N.'s treatment. Indeed, as Levin (1978, p.85) has pointed out, Freud did not even expect with her "that the mere recollection of the relevant memories would induce a spontaneous cure". At this time Freud's "tracing of hysterical symptoms to initiating events" was simply

"a device for rendering hypnotic suggestion more effective" (op. cit., p.86). As for emotion, Nichols and Zax (1977, p.32) have noted that Freud's treatment of Emmy von N. actually seemed to *discourage* affective expression and his descriptions read as if it were an afterthought. The same seems to be true of Frau Cäcilie.

If the role given affect in psycho-analysis does not come directly from the treatments of Emmy von N. and Frau Cäcilie (or Anna O.'s "talking cure") what is its source? Is it in the observations Freud made in the therapy he practised after those cases and before 1893? This seems unlikely. Whenever Freud does refer or allude to 'Breuer's method' during that time it is almost always conjoined with direct suggestion and never with emotion.

In the Villaret article of 1888, Freud described Bernheim's therapy as being more effective by combining it with Breuer's method "and lead the patient under hypnosis back to the psychical prehistory of the ailment and compel him to acknowledge the psychical occasion on which the disorder in question originated" (Freud, 1888a, p.56). Neither that 'acknowledgement' nor the other features described by Freud correspond to talking or to reacting with emotion. A notable feature of the slightly later *Die Gesundheit* chapter is that it is confined to direct hypnotic suggestion and completely lacks even a hint of any version of 'Breuer's method' (Freud, 1890/1905c). What the Editor of the *Standard Edition* (*1*, p.100, n.2) describes as "a probable reference to Breuer's technique" in Freud's review of Forel's *Hypnotism* is not. In the review Freud insisted that hypnosis satisfied all the requirements of a causal treatment for those hysterical disorders which were:

> the direct result of a pathogenic idea or the deposit of a shattering experience. *If that idea is got rid of or that memory weakened* - which is what suggestion brings about - the disorder too is usually overcome. (Freud, 1889, p.100. My emphasis, MBM)

This description of the attack on the pathogenic memory exactly matches what Freud had done with Emmy von N. in the same year and the allusion, if it is one at all, is to the Delboeuf-Janet-Freud extension of the suggestion method and not to a technique based on provoking emotional reactions. Similarly, the allusion to Breuer's method in Bum's *Lexicon* is only in the possibility of obtaining "the most far-reaching psychical influence over [patients] by *questioning* them under hypnosis about their symptoms and the origin of these" (Freud, 1891b, p.112. My emphasis, MBM). Although Freud referred or alluded to 'Breuer's method' in his paper on hysterical counter-will he said nothing at all there about emotion and the treatment was not even based on modifying or altering the patient's memories (Freud, 1892-1893, pp.117-121). Freud was also quite explicit that what he could say about the origins of her symptoms was based on inference rather than on direct investigation under hypnosis (op.cit., pp.121, 123).

Contrariwise, where Freud did describe how strong emotion sometimes caused hysterical symptoms to vanish, he either did not link that fact closely to 'Breuer's method' or made no connection with it at all. Thus, in Villaret emotion is mentioned only in passing (Freud, 1888a, p.56). In the *Die Gesundheit* chapter on *Psychical (or mental) treatment*, which was probably written *after* the first seven-week phase of Emmy von N.'s treatment had been concluded, Freud made much of the general effects of emotions on normal bodily states and their curative role in illnesses generally but nowhere even hinted at the possibility of emotional discharge as the basis for treating functional disorders like hysteria (Freud, 1890/1905c. Cf. Editor's Introduction, *Standard Edition*, *1*, pp.63-64).

During 1890-1891 Freud had recommended using hypnosis as follows:

> In a number of cases - namely where the symptoms are of purely psychical origin - hypnosis fulfils *all* the demands that can be made of a causal treatment, and in that case questioning and calming the patient in deep hypnosis is as a rule accompanied by the most brilliant success. (Freud, 1891b, p.113. My emphasis, MBM)

As far as I can determine, this reference to "calming" is the only one Freud makes to emotion in his therapy prior to 1892.

It is clear therefore that until the spring of 1889 the only therapy Freud used was Bernheim's method of direct hypnotic suggestion. By that time he had had eighteen months experience with it. If patients then came to him whose central symptoms and susceptibility to hypnosis were identical to those of a patient (or patients) recently described in the literature, what would have been more natural than for him to adopt the extension of the hypnotic suggestion method reported as successful with them? If Freud used 'Breuer's method' before then it could have been only to investigate the *mechanism* of symptom formation and not as a therapy.

My dating is consistent with Freud's own. It was "during the early nineties" that he had "confirmed Breuer's results in a considerable number of patients" (Freud, 1924c, p.194). For some time after 1889 Freud's use of 'Breuer's method' seems to have been limited to locating the memory of the traumatic event as a preliminary to modifying it by direct suggestion. While this did focus on the role of the memory, nothing in what Freud wrote between then and 1891 expressed the view that hysterical patients had anything at all to remove or work off. Until he had adopted that view, it would have been impossible for him to have practiced a therapy based on the discharge of emotion.

HYPNOTIC RECALL AND THE PRESSURE METHOD

By the end of 1892 Freud began to give up using hypnosis for locating symptom-producing memories and to modify them by suggestion. These steps toward developing new methods for investigating and treating

hysteria are important because they laid the basis for an entirely new kind of theory of neuroses, one that did not draw on French concepts of psychopathology.

The chronology of Freud's abandoning direct suggestion under hypnosis is reasonably certain but there is less certainty about his reasons for giving it up. I begin with his references to his and his patients' dissatisfaction with the unreality created by hypnotic suggestions.

As early as 1891 Freud complained that:

> with hypnotic treatment both physician and patient grow tired far sooner, as a result of the contrast between the deliberately rosy colouring of the suggestions and the cheerless truth. (Freud, 1891b, p.113)

Sometime later, probably in the first half of 1893, Freud expanded on this sentiment. Commenting upon some advice of Charcot's regarding the use of suggestion, Freud declared Charcot's reservations about hypnotic therapy revealed:

> one of the greatest inconveniences with which the practical use of suggestion in the waking state and under light hypnosis has to reckon. In the long run neither the doctor nor the patient can tolerate the contradiction between the decided denial of the ailment in the suggestion and the necessary recognition of it outside the suggestion. (Freud, 1892-94, pp.141-142, Note to Charcot's p.286)

It seems to me that more than a mere dissatisfaction with the practical limitations of hypnotic suggestion may be discerned in this last passage. I believe it indicates the desirability of developing a treatment that could be used *in the waking state*.

Whether so or not, the immediate reason for Freud giving up hypnosis was the quite mundane one of encountering patients who were not hypnotisable. When Freud found such a patient he stopped the standard induction:

> and only asked for 'concentration'; and I ordered the patient to lie down and deliberately shut his eyes as a means of achieving this 'concentration'. (Breuer and Freud, 1895, p.109)

Concentration produced, Freud believed, the deepest hypnosis of which the patient was capable. But was concentration hypnosis deep enough?

Freud was at this time still so wedded to the notion that access to the memories of the *condition seconde* could be obtained only through hypnosis, he wondered if "I might be depriving myself of a precondition without which the cathartic method seemed unusable" (ibid). Faced with this seemingly insoluble problem, Freud recalled Bernheim's demonstration of the relative nature of the subject's amnesia for the events of the hypnotic state and deduced a new technical procedure from it.

Like the older magnetists, who already knew that in the waking state subjects could be got to recall what had happened when they had been magnetized, Bernheim insisted his waking-state subjects could remember the events of the hypnotic trance (Ellenberger, 1970, pp.113-114). Bernheim placed his hand on his subjects' foreheads and, while exerting light pressure with it, urged them to recall what had happened while they had been hypnotized. The memories were eventually retrieved. While Freud might have remembered Bernheim's actual demonstration, which may well have taken place during his 1889 visit to Nancy, Binet's 1892 republication of it seems to me to be a more probable source.

Bernheim described a patient who had been given the post-hypnotic suggestion that she would be unable to see him:

> It was useless to tell her that I was there and that I was talking to her. She was convinced that they were simply making fun at her expense. I gazed at her obstinately and said: 'You see me well enough, but you act as if you did not see me. You are a humbug, you are playing a part!' She did not stir and continued to talk to other people. I added, with a confident manner: 'However, I know all about it. You can not deceive me! It is only two years since you had a child and you made away with it! Is that true? I have been told so.' She did not move, her face remained peaceful ... I roughly raised her dress and skirt. Although naturally very modest, she allowed this without a blush. I pinched the calf of her leg and her thigh. She made absolutely no sign whatever. (Bernheim, 1888-1889. Cf. Binet, 1892/1896, pp.307-308)

Bernheim induced hypnosis again and suggested that on awakening she would be able to see him. In the conversation that followed, she maintained she had not seen him. After challenging her denials, Bernheim went on:

> 'you remember everything that happened while I was not here - all that I said and did to you' ... I insisted; speaking seriously and looking her in the face I laid stress on every word: 'It is true I was not there, but you remember just the same.' I put my hand on her forehead and declared: 'You remember everything, absolutely everything. There speak out! What did I say to you?' After a moment's concentrated thought she blushed and said. 'Oh no, it is not possible you were not there. I must have dreamed it.' 'Very well; what did I say to you in this dream?' She was ashamed and did not want to say. I insisted. At last she said, 'You said that I had had a child.' 'And what did I do to you?' 'You pricked me with a pin.' 'And then?' After a few minutes she said: 'Oh no, I would not have allowed you to do it; it is a dream.' 'What did you dream?' 'That you exposed me'. (ibid.)

The subject's inability to remember could be overcome by combining verbal insistence with physical pressure.

Elisabeth von R., whose treatment began "in the autumn of 1892", seems to have been the first patient with whom Freud used his new technique (Breuer and Freud, 1895, pp.110 n.1, 135, 145). As Breuer had

done with Anna O., Freud had formed the opinion that her illness was due to some secret she had deliberately concealed from him (op.cit., pp.138-139). He thought hypnosis would be necessary only to aid her recall at select points in her narrative. But, when hypnosis was called for, Freud found she could not be hypnotized. It was then that he seems to have recalled Bernheim's demonstration:

> In this extremity the idea occurred to me of resorting to the device of applying pressure to the head .... I carried this out by instructing the patient to report to me faithfully whatever appeared before her inner eye or passed through her memory at the moment of pressure. She remained silent for a long time and then, on my insistence, admitted that she had thought of an evening ... (op. cit., p.145)

The memory recovered was of an event linked by similar content to the memory Freud eventually held responsible for the symptoms. His repeated use of pressure recovered other seemingly relevant memories.

Hypnotically produced extensions of consciousness were therefore not pre-conditions for the recovery of traumatic memories. Nor was it the case that all memories could be retrieved. Freud noted several instances in which patients failed to recall therapeutically significant material, even when hypnotized quite profoundly (op. cit., pp.284-285), and, as early as Emmy von N.'s treatment, he had found hypnosis unable to assist in the recovery of temporarily forgotten and more-or-less trivial information (op. cit., pp.97-98). Hypnosis was no more omniscient than it was necessary.

A therapy based on a *condition seconde* rested partly on the assumption that the secondary consciousness could be fully studied only under hypnosis. Once Freud ceased to believe that hypnosis of any kind - even concentration - was required for the recovery of memories, he could move to a normal or waking-state treatment method. Despite the new modes of thought these observations presage, Freud was slow to abandon recall under hypnosis altogether. Elisabeth von R. was treated without hypnosis toward the end of 1892, as were Lucy R. at the end of that year and Katharina in August of 1893 (Masson, 1985, Letter of 20.8.1893; Swales, 1988), but hypnosis was not given up completely until between January and March 1895. 'Concentration' was still in use in the former month (Masson, 1985, Draft H of 24.1.95) and was replaced with the waking-state pressure method only at the time the psychotherapy chapter of the *Studies on Hysteria* was completed in the March (op. cit., Letters of 8. & 13.3.95; Breuer and Freud, 1895, pp.267-272).

SYMPTOM FORMATION IN THE WAKING STATE

Freud was not much quicker to move to a waking-state conceptualisation of symptom formation than he had been to give up an hypnotically based therapy. For consistency with his therapy he needed to abandon the notion

that the innate tendency to the formation of a *condition seconde* was the *sine qua non* for the production of symptoms.

Freud considered two other mechanisms before arriving at repression, the one he finally settled on. Both were all but abandoned immediately. Because each was firmly based on a secondary consciousness, they illustrate the difficulties Freud had in making the theoretical move toward congruence. They were different in that the one called counter-will had no connection with th emotion whereas the other, associative inaccessibility, brought affect into Freud's theory for the first time through its interaction with the *condition seconde*.

Counter-will

By counter-will Freud referred to the temporary domination of the *opposite* of an intention or act of will the subject was trying to carry out. Thus a mother who intended to breast feed her infant child had:

> a poor flow of milk, pains were brought on when the baby was put to the breast, the mother lost appetite and showed an alarming unwillingness to take nourishment, her nights were agitated and sleepless. (Freud, 1892-1893, p.118)

Freud hypothesised that the idea of every intention was accompanied by the idea of the intention not succeeding. Sometimes, as in the mother's case, the counter-intention overwhelmed the primary intent. What Freud called a counter-will was a collection of these "distressing antithetic ideas".

How Freud came upon this seemingly *ad hoc* concept of counter-will is a mystery. Ritvo, L. (1972, pp.249-254) has pointed to Darwin's *The Expression of the Emotions in Man and Animals* as a possible source. Darwin had explained what seemed to be the otherwise purposeless signs of affection in animals (e.g. the body crouch, lowered ears, and tail wagging of the dog or the upright stance, erect tail, and arched back of the cat) as being movements and postures that were the antithesis of their aggressive, attacking behaviours. Heredity had linked purposeful signs of hostility with their purposeless opposites (Darwin, 1872, Chapter 2).

When introducing the concept of counter-will Freud did not acknowledge Darwin's principle of *antithesis*. It is of some interest, though, that when he did use another of Darwin's principles, that of the *overflow of excitation*, to explain some of Emmy von N.'s symptoms he incorrectly cited Darwin as using *it* to explain tail-wagging. For Darwin tail-wagging was not overflow but antithesis. And, in the very next paragraph, Freud went on to say that some of Emmy von N.'s other symptoms were due to counter-will (Breuer and Freud, 1895, pp.91-92).

Counter-intentions were stored at an unconscious level. They enjoyed:

> an unsuspected existence in a sort of shadow kingdom, till they emerge like bad spirits and take control of the body, which is as a rule under the orders of the predominant ego-consciousness. (Freud, 1892-1893, p.127)

Ordinarily the counter-will was suppressed and inhibited by normal consciousness. Each intention and counter-intention had its separate physiological substrate in the nervous system and under conditions of exhaustion, such as occurred in nursing, there could be a relatively greater exhaustion of:

> those elements of the nervous system which form the material foundation of the ideas associated with the primary consciousness; the ideas that are excluded from that chain of associations ... of the normal ego - the inhibited and suppressed ideas, are *not* exhausted, and they consequently predominate at the moment of disposition to hysteria. (op. cit., p.126)

Once the substrate of the intention became relatively weak, the counter-intention necessarily predominated. However, if there was present a:

> tendency to a *dissociation of consciousness* ... the distressing antithetic idea, which seems to be inhibited, is removed from association with the intention and continues to exist as a disconnected idea .... [which] can put itself into effect [realise itself] by innervation of the body just as easily as does a volitional idea under normal circumstances. (op. cit., p.122)

Counter-will therefore required the same *sine qua non* as hypnoid hysteria.

Associative inaccessibility

By *associative inaccessibility* Freud referred to the way in which some of the ideas involved in the production of hysterical symptoms became inaccessible to normal consciousness. The concept resulted from his exploiting Janet's proposal that the details of hysterical symptoms were determined by the idea of the organ or its function. Freud's immediate use of Janet's concept was to solve a theoretical problem that had dogged him for years - the means by which hysterical and organic symptoms could be differentiated from one another - and we need to begin with it.

Janet and associative inaccessibility When Freud reported to the College of Professors in the Faculty of Medicine of the University of Vienna on his 1885-1886 visits to Paris and Berlin he said his discussions and correspondence with Charcot:

> led to my preparing a paper which is to appear in the *Archive de Neurologie* and is entitled 'Vergleichung der hysterischen mit der organischen Symptomatologie'. (Freud, 1886a, p.12)

However, as became clear later, at the time Freud wrote his report the paper did not exist.

Over the next two years Freud's letters to Fliess contain what seem to be references to his working on the paper and in May of 1888, two years

after the report, the title Freud gave Fliess of "the first draft" implied he was making a rather more restricted comparison of hysterical and organic *paralyses* (Masson, 1985, Letters of 28.12.1887, 4.2.1888, 28.5.1888 and 29.8.1888). Later, in September of that year, in the preface to his translation of Bernheim's *Suggestion*, he publicly foreshadowed that the paper was "shortly to appear" (Freud, 1888c, p.80). It did not. What Freud did publish in 1888 was an entry on hysteria for Villaret's *Handwörterbuch* in which he *described* the main symptoms of hysteria and only *partially compared* them with their organic counterparts. Five years later, when the paper proper did appear, it was confined to the paralyses and made only one point additional to those made in Villaret: the "lesion" in hysteria was of the idea of the organ and the intensity of the emotion accompanying it contributed to maintaining its isolation (Freud, 1893b, pp.169-172).

Freud's entry on hysteria in Villaret's *Handwörterbuch* had begun with the assertion:

> hysteria is based wholly and entirely on physiological modifications of the nervous system and its essence should be expressed in a formula which took account of the conditions of excitability in the different parts of the nervous system. (Freud, 1888a, p.41)

There being no such "physio-pathological formula":

> we must be content ... to define the neurosis in a purely nosographical fashion by the totality of symptoms occurring in it. (ibid.)

After this apology Freud discussed the convulsive attacks, the hysterogenic zones, the disturbances of sensibility, the disturbances of sensory activity, the paralyses, and the contractures. Except for the hysterogenic zones he made some kind of comparison with the equivalent organic disorders. For the convulsions and the sensory disturbances the comparisons were minimal, implicit even, but those for the paralyses, the contractures, and the disturbances of sensibility were quite explicit as well as detailed.

Charcot had pointed out that at least some of the individual symptoms of hysteria could be characterised negatively as, for example, in his discussion of the case of Porcz__ (Charcot, 1887/1889, pp.265-273). Freud took this view further by formulating slightly broader rules although they were still negative in character:

> Hysterical paralyses take no account of the anatomical structure of the nervous system which, as is well known, shows itself most unambiguously in the distribution of organic paralyses. (Freud, 1888a, p.46)

Thus, hysterical paralyses were almost always accompanied by anaesthesia and none resembled the peripheral facial, radial, and serratus paralyses in affecting groups of muscles or muscle and skin determined by common

innervation. They were also different from the cortical paralyses - the only type with which they could otherwise be compared. In an hysterical hemiplegia, for example, the arm and leg on one side were affected but the facial muscles not. Again, instead of a paralysis extending to a whole limb, only part of it might be affected, for example, the hand or the shoulder. Further, if the leg were paralysed it was dragged along like a lifeless appendage rather than moved in a circular wheeling motion from the hip. Contractures were similarly "not explicable by the stimulation of particular nerve trunks" (op. cit., p.47).

When discussing hysterical symptoms more generally, Freud took his negative rule a little further:

> they do not in any way present a copy of the anatomical conditions of the nervous system. It may be said that hysteria is as ignorant of the science of the structure of the nervous system as we ourselves before we have learnt it. (op. cit., pp.48-49)

Expressed in almost the same words in the Preface to his translation of Bernheim's *Suggestion* (Freud, 1888c, p.80), this characterisation of hysteria-as-ignorance marked the limit of Freud's 1888 comparison.

The most noticeable feature of Freud's 1893 paper is that the first three of its four sections were probably completed within two years of his return from Paris. The central conclusion of those three had already been drawn in Charcot's own *Lectures* and Freud himself had set them out (more systematically than Charcot, it is true) in the 1888 Villaret entry. Janet's thesis and Freud's elaboration of it is all that distinguishes the long-delayed paper from the Villaret entry (Cf. Freud 1893a; Charcot, 1887/1889, Lecture 21; Freud, 1892-1894, p.140, Note to page 268 of Freud's translation, and pp.141-142, Note to page 368 of Freud's translation; Editorial Note, *Standard Edition*, *1*, pp. 158-159).

In the paper Freud argued that hysterical paralyses belonged to the group of cerebral paralyses rather than to the periphero-spinal even though they were much more precisely limited (for example, to a single limb or muscle) and were more intense (for example, complete losses of function rather than partial) than the organic cerebral forms. He then raised the problem of the nature of the "lesion" in hysteria. Although he accepted Charcot's characterisation of it as "dynamic" he rejected the possibility that it might be a transitory organic affection. Were it such, the characteristics of the two kinds of paralyses would be the same. Since they were not, an emphatic restatement of his 1888 "hysteria-as-ignorance" proposition was called for:

> the lesion in hysterical paralyses must be completely independent of the anatomy of the nervous system, since *in its paralyses and other manifestations hysteria behaves as though anatomy did not exist or as though it had no knowledge of it.* (Freud, 1893a, p.169)

Then, for the first time, he said that hysteria:

> takes the organs in the ordinary, popular sense of the names they bear: the leg is the leg as far up as its insertion into the hip, the arm is the upper limb as it is visible under the clothing. (ibid.)

Freud brought this section of the paper to a close by "fully" associating himself with Janet's views as they had been presented at the Salpêtrière, adding:

> they are confirmed as much by hysterical paralyses as by anaesthesia and psychical symptoms. (ibid)

As if echoing Janet's conclusion that hysterical anaesthesia was a psychological malady Freud began the next section of the paper by requesting the permission of his readers "to move on to psychological ground":

> I follow M. Janet in saying that what is in question in hysterical paralysis, just as in anaesthesia, etc., is the everyday, popular conception of the organs and of the body in general. That conception is not founded on a deep knowledge of neuro-anatomy but on our tactile and above all our visual perceptions. (op. cit., p.170)

In the very next sentence Freud went on to say that if the popular concept:

> is what determines the characteristics of hysterical paralysis, the latter *must* naturally show itself ignorant and independent of any notion of the anatomy of the nervous system. (ibid. My emphasis, MBM)

Freud thus explicitly explained his hysteria-as-ignorance characterisation with Janet's thesis.

Freud's use of Janet's thesis The major point of difference between Freud's 1888 and 1893 works is in a new, fourth section where Freud used Janet's thesis as the basis for a physio-pathological formula in order to explain the isolation of the symptom.

Freud began the 1893 paper by embracing the specificity of Janet's thesis. He then extended it with some emphasis:

> the paralysis of the arm consists in the fact that the conception of the arm cannot enter into association with the other ideas constituting the ego of which the subject's body forms an important part. The lesion would therefore be *the abolition of the associative accessibility of the conception of the arm.* (Freud, 1893b, p.170)

What caused the loss of accessibility? Freud called upon analogies which seemed to show that people deliberately isolated those of their memories that were affectively charged. The analogue could be comical, as with the man who refused to wash his hand because his king had touched it, or serious, as in the custom observed in some societies of burning the

possessions of a dead chieftain along with his body. The function of the behaviour was clear:

> The quota of affect which we attribute to the first association of an object has a repugnance to letting it enter into a new association with another object and consequently makes the idea of the object inaccessible to association. (op. cit., pp.170-171)

Symptoms were "almost the identical thing":

> If the conception of the arm is involved in an association with a large quota of affect, it will be inaccessible to the free play of other associations. (op. cit., p.171)

By using "the quota of affect" in this way to develop Janet's thesis into a physio-pathological formula, Freud brought affect into his explanation of symptom formation for the first time.

How could excessive affect prevent associations from forming? It had been held fairly generally in association psychology that the ease with which they formed and the extent to which ideas became conscious was proportional to the amount of accompanying affect. What Freud did was to invoke a *subconscious* association between the memory of the event that had produced the symptom and the idea of the paralysed organ or lost function. The traumatic memory provided so large a quota of affect to this subconscious association that the "associative affinity" of the concept of the organ or function was "saturated" and could not form any more associations (ibid.).

The "lesion" in an hysterical paralysis was thus "the inaccessibility of the organ or function ... to the associations of the conscious ego", an inaccessibility caused by the "fixation" of the concept of that organ or function in its "subconscious association with the memory of the trauma" (op. cit., p.172). More generally, the "lesion" was of the idea of the organ, isolated from the ego by the emotional charge of the traumatic memory with which it was subconsciously associated.

Freud immediately emphasised the therapeutic aspects of this view:

> the paralysed organ or the lost function is involved in a subconscious association which is provided with a large quota of affect *and it can be shown that the arm is liberated as soon as this quota is wiped out.* (op. cit., p.171. Emphasis altered, MBM)

It was in this way, again by developing Janet's thesis, that Freud first brought affect into an explanation of therapy. However, although we can see how Freud first used the concept of affect, we have yet to find its source. I believe it to be the work of Hughlings Jackson, the eminent British neurologist, whose papers on organic speech disorders Freud had read when preparing his own monograph on that subject (Freud, 1891/1953).

Jackson, affect, and associative inaccessibility Since I set out a detailed argument about Jackson's influence on Freud in Chapter 7 I confine myself here to a few of its points. Jackson argued that all actions, especially speech ejaculations and emotional reactions, resulted from the equilibrium of the nervous system being restored (Jackson, 1879-1880a). Freud explicitly endorsed Jackson's ideas and only after absorbing them did he begin to consider the possibility that the nervous system had the purpose of maintaining its quantity of excitation at a fixed level, that it achieved its aim by disposing of surplus excitation, especially increases brought about by emotion, and that symptoms were a consequence of abnormalities in the disposal of the excess. In the sketches for the *Preliminary Communication* he sent to Breuer in November, 1892, Freud described the symptoms of hysteria as attempts to dispose of excess excitation. The physical were abnormal motor reactions and the mental were abnormal associations (Freud, 1892, pp.153-154). In the 1893 paper on paralyses he put these propositions more generally:

> Every event is provided with a certain quota of affect of which the ego divests itself *either* by means of a motor reaction *or* by associative psychical activity. If the subject is unable or unwilling to get rid of this surplus, the memory of the impression attains the importance of a trauma .... The impossibility of elimination becomes evident when the impression remains in the subconscious. (Freud, 1893b, pp.171-172. My emphasis, MBM)

Disposing of the affect through speech involved associations. Freud did not have this and the other concepts to which it is related before 1892. Jackson's concepts therefore provided part of the basis for Freud emphasising the *real* expression of emotion in the 'talking cure'.

Jackson may have contributed more than the notions of equilibrium and discharge to Freud's concept of associative inaccessibility. He used words strikingly foreshadowing Janet's to describe the language difficulties of aphasic patients. Thus, in speaking of the "dissassociation" of the objective "nervous arrangements" for words and actions from their subjective counterparts, Jackson wrote:

> were we *to use popular language*, and to say of a patient who "tries" to put out his tongue, and fails, or who tries to say "no" and fails that *he has lost part of his volition*, we should only mean that *he had lost ... the very objective actions [of speech] themselves*, their nervous arrangements being broken up; *he has lost a part of himself.* (Jackson, 1879-1880b. My emphasis, MBM)

It does not seem to me to be unreasonable to suppose that Freud recalled these remarks of Jackson's when he read Janet's thesis and linked the two to form his own concept of associative inaccessibility.

The significance of 1892 By returning to the Salpêtrière we not only find out why Freud's paper was completed when it was but we learn why 1892

was such a turning point for Freud's theorising about affect. On 11 March, 1892, at a regular clinical meeting there, Janet read the first of what was clearly to be a series of papers covering the major symptoms of hysteria. After dealing with hysterical anaesthesia, he went on to cover hysterical amnesia on 17 March and suggestibility on 1 April (Janet, P., 1892a; 1892b; 1892c. Cf. Janet, 1892d). It was in the first of these papers that he set out his thesis on the determination of hysterical anaesthesias by ideas.

We have seen how Janet's thesis allowed Freud to lift what could be said about hysterical symptoms from the level of Charcot's basically descriptive and tentatively negative characterisation and place it into a sophisticated theoretical framework. I believe we can reconstruct Freud's reaction to Janet's thesis with some certainty. Within days of the public appearance of Janet's first paper Freud suggested to Breuer they prepare their work on hysteria for publication for, on 28th. June, 1892, Freud wrote to Fliess that Breuer had agreed to publish their "detailed theory of abreaction, and our other joint jokes [Ger.: 'Witze'] on hysteria" (Masson, 1985, Letter of 28.6.1892. Translation modified, MBM). It is in this sentence that Freud uses the term 'abreaction' for the first time. The next day, on 29th June, 1892 in a letter to Breuer, Freud formulated, also for the first time, the notion that the nervous system had something to dispose of. If this sequence of events is true, I believe it solves the puzzle of Freud's delay in developing a therapy based on affective discharge better than does Friedman's proposal (Friedman, 1977, n2. Cf. Andersson, 1962, p.90).

Freud outlined two very slightly different explanations of how symptoms formed to Breuer. In both, they were due to sums of excitation that had been discharged along abnormal pathways. In the first version symptoms were attempts at reaction which had become hysterical because they persisted (Freud, 1892, p.148, n.3); in the second, the one he settled on, they were attempted reactions that displaced otherwise unreleased sums of excitation (op.cit., p.148). For both versions of his explanation he gave as the reason for the persistence of the reaction/displacement:

> The theorem which lays it down that the contents of different states of consciousness are not associated with one another. (op.cit., p.147)

Dual consciousness and dissociation, rather than affect acting by itself, were therefore responsible for the isolation.

Here, as in the later *Preliminary Communication* already quoted, Freud's affectively based therapy similarly remained within this dissociation framework:

> It will be understood that our therapy consists in removing the results of the ideas which have not been abreacted, either by *reviving the trauma in a state of somnambulism*, and then abreacting and correcting it, or by *bringing it into normal consciousness under comparatively light hypnosis*. (op.cit., p.150. My emphasis, MBM.)

This genuflection toward normal waking-state processes by allowing them to occur in the hypnotic is also present in the published explanation (Breuer and Freud, 1893, p.17).

Until 1892, when Janet extended Charcot's theses on the role of ideas, Freud had had no conceptual framework from which to describe the peculiarities of hysterical symptoms let alone explain them. Janet gave Freud exactly the positive characterisation he needed. It allowed him to describe symptoms and move from description to explanation. Janet's thesis provoked Freud into seeking publication of the *Preliminary Communication* with Breuer and then allowed him to finish his paper. We can also surmise why Freud restricted himself to the paralyses: they were the most important of the symptoms *not* included in Janet's comprehensive discussion. [2]

Not surprisingly, Freud's first explanations were couched in terms of dissociation and a secondary consciousness. In everything written between the second half of 1892 and 1894 "subconsciousness" was not due to the quota of affect itself but to the affectively charged event having been experienced in an elementary secondary consciousness. Freud thus indicated his continued debt to the French school of psychopathology, an indebtedness that also speaks to us from almost every other page of the sketches (Freud, 1892, pp.149-150, 153-154), from the finished *Preliminary Communication* with Breuer (Breuer and Freud, 1893, pp.11-17), and from his own public lecture on the new theory (Freud 1893a).

DATING FREUD'S THEORETICAL SHIFT

It took Freud about two years to move to his waking-state conceptions. In the *Preliminary Communication* of 1893 Breuer and Freud differentiated two groups of conditions in which hysterical symptoms were acquired. In the first, patients had not been able to react to a trauma because the nature of the trauma excluded a reaction or because circumstances precluded one or because there had been an intentional repression. In the second group, the lack of reaction was due to the presence of paralysing affects, such as fright, or because of abnormal states, such as day-dreaming and auto-hypnoses. Here, the nature of the state made a reaction impossible. Both kinds of conditions could be present simultaneously. A trauma might take place during the presence of a paralysing affect or a modified state of consciousness and it could also produce an abnormal state which made a reaction impossible (Breuer and Freud, 1893, pp.10-11). We know that at this time Freud himself viewed hypnoid isolation as compatible with repression because he repeated the essence of these points in the lecture he delivered when the *Preliminary Communication* was published (Freud, 1893a, p.38).

2. See also the note on Freud's claim to priority on Page 120.

Now, whether acquired by repression or in an hypnoid state, all symptoms were based upon a split in consciousness:

> the *splitting of consciousness* which is so striking in the well-known classical cases under the form of '*double conscience*' is present to a rudimentary degree in every hysteria, and ... is *the basic phenomenon* of this neurosis. In these views we concur with Binet and the two Janets. (Breuer and Freud, 1893, p.12. Emphasis altered, MBM)

Freud also repeated this proposition in his own lecture (Freud, 1893a, pp.38-39).

Within a year of the *Preliminary Communication* Freud proposed the capacity for conversion as the basic predisposition to hysteria, although in a way that maintained a kind of compatibility with Breuer's view (Freud, 1894, pp.46-47, 50-51). But, a year later again, by the time of the *Studies on Hysteria*, he denied that dissociation was fundamental and had come to view all so-called hypnoid symptoms as really caused by repression (Breuer and Freud, 1895, pp.285-286). Freud had paid off his indebtedness to Breuer and the French and severed his conceptual ties to them. Breuer's theoretical chapter in the *Studies on Hysteria* written two years later shows practically no shift from this 'French' mode of theorising. True, Breuer did prefer to speak of a splitting of the mind rather than of consciousness (Breuer and Freud, 1895, p.225) but this distinction, or one very like it, was clear also to Binet who nevertheless continued using words like "split", "division", and "doubling" to describe alterations of *consciousness* (Breuer and Freud, 1895, p.225; Binet, 1892/1896, pp.90, 257-258). But Breuer's insistence that the tendency to splitting was basic to hysteria and that a rudimentary dual consciousness was present in every hysteria was unchanged (Breuer and Freud, 1895, pp.226-227).

THE SCOPE OF FREUD'S NEW CONCEPTUALISATION

Freud's new theory went beyond hysteria. The older theories were so tied to concepts like the *condition seconde* or the hypnoid state that they had little application to other neuroses. In contrast, repression and conversion could be easily generalised.

By the beginning of 1894, Freud made the first extension of his new theory. He had initially used 'the cathartic method' simply to see how obsessional symptoms differed from the hysterical but he found they had also been initiated by repression (Breuer and Freud, 1895, p.256). One patient provided direct evidence:

> Something very disagreeable happened to me once and I tried very hard to put it away from me and not to think about it any more. I succeeded at last; but then I got this other thing, which I have not been able to get rid of since. (Freud, 1894, pp.52-53)

And, from the less clear recollection of a patient whose attention was drawn directly to the cause of his obsession:

> It can't come from that. I didn't think at all much about that. For a moment I was frightened, but I turned my mind away from it and I haven't been troubled by it since. (op. cit., p.53)

Freud thought these "most unambiguous statements" proved the existence of defensive willing in obsessional neuroses (op. cit., p.52). [3]

It seemed to Freud that obsessional symptoms were formed when the unconscious process subsequent to the willing tore the affect from the incompatible idea and attached it to a previously innocuous idea. Freud's analysis of one of his earliest obsessional patients provides an illustration:

> A girl suffered from obsessional self-reproaches. If she read something in the papers about coiners, the thought would occur to her that she, too, had made counterfeit money; if a murder had been committed by an unknown person, she would ask herself anxiously whether it was not she who had done the deed. At the same time she was perfectly conscious of the absurdity of these obsessional reproaches .... Close questioning then revealed the source from which her sense of guilt arose. Stimulated by a chance voluptuous sensation, she had allowed herself to be led astray by a woman friend into masturbating, and had practised it for years, fully conscious of her wrong-doing and to the accompaniment of the most violent, but, as usual, ineffective self-reproaches. (Freud, 1894, p.55)

A process, which at that time Freud termed transposition or "displacement", and similar to conversion, had attached the affect of self-reproach over masturbation to the idea of criminality.

At the beginning of 1894, therefore, Freud was ready to formulate his waking-state concepts, to extend his conceptualisations of symptom formation, and to lay the foundations of a unified theory of neuroses. By 1895 he had broken altogether with the traditional concepts derived from studies of hypnosis and hysteria.

REPRESSION AND PATHOGENIC MEMORIES

The normal waking-state process Freud put forward as producing symptoms was *repression*. He supposed it to start with a defensive "act of will" by which patients attempted to forget thoughts having large quotas of affect associated with them and which were incompatible with their egos

3. Statements like these, which are among many, and which are exactly the same as those of the patients with hysterical symptoms already quoted, also show very plainly that patients consciously attempted to rid themselves of unwanted ideas. They therefore contradict the Editorial Note to the *Studies on Hysteria* in which it is argued that *both* the intention and the repression were unconscious (Breuer and Freud, 1895, p.10).

(Freud, 1894, pp.46-47). Emotion came into the process in a second way: repression had the unintended consequence of "converting" or channelling the affect into bodily processes thereby producing such symptoms as paralyses, contractures, and anaesthesias. At the same time the idea itself was forced into the unconscious (op. cit., pp.48-50).

The initial act of will that brought repression into operation was readily remembered:

> the most unambiguous statements by the patients give proof of the effort of will, the attempt at defence, upon which the theory lays emphasis. (op. cit., p.52)

For example, Lucy R., seen at about the same time as Elisabeth von R., had said:

> I wanted to drive it out of my head and not think of it again; and I believe latterly I have succeeded. (Breuer and Freud, 1895, p.117)

Freud said his patients:

> had enjoyed good mental health up to the moment at which an *occurrence of incompatibility took place in their ideational life* - that is to say, until their ego was faced with an experience, an idea or a feeling which aroused such a distressing affect that the subject decided to forget about it ... patients can recollect as precisely as could be desired their efforts at defence, their intention of 'pushing the thing away', of not thinking of it, of suppressing it. (Freud, 1894, p.47)

Patients acted only to rid themselves of unwanted ideas; causing symptoms was not part of their intentions. Repressive processes therefore occurred:

> without consciousness. Their existence can only be presumed, but cannot be proved by any clinico-psychological analysis. (op. cit., p.53)

Repression was thus partly inferred from the gap between the willing and the appearance of the symptom.

Freud conceived of repression as a psychological force because the pressure method had required him to insist repeatedly that his patients could retrieve memories of the causes of their symptoms:

> this insistence involved effort on my part and so suggested the idea that I had to overcome a resistance, the situation led me *at once* to the theory that by means of *my psychical work* I had to overcome *a psychical force* in the patients which was opposed to the pathogenic ideas becoming conscious (being remembered). (Breuer and Freud, 1895, p.268. Emphasis altered, MBM)

These curious kinds of objectifications of subjective feelings have been referred to as 'logophania' by Ellenberger (1956) and, although the term is not very satisfactory, I have adopted it because there seems to be no other.

Once Freud had made this logophanic transformation he said:

> A new understanding seemed to open before my eyes when it occurred to me that this must no doubt be the same psychical force that had played a part in the generating of the hysterical symptom and had at that time prevented the pathogenic idea from becoming conscious. (Breuer and Freud, 1895, p.268)

Repression was therefore also partly an inference from the patient's resistance.

After observing that all the repressed ideas aroused distressing affects like shame, self-reproaches, and feelings of being harmed, affects, in short, of a kind one would prefer not to have experienced and would rather forget:

> From all this there arose, as it were automatically, the thought of *defence* .... The patient's ego had been approached by an idea which proved to be incompatible, which provoked on the part of the ego a repelling force of which the purpose was defence against this incompatible idea. (op. cit., p.269)

The long time it took Freud to arrive at this conceptualisation rather belies his use of "automatically".

Although forced out of consciousness, the idea had not been destroyed:

> If I endeavoured to direct the patient's attention to it, I became aware, in the form of a *resistance*, of the same *force* as had shown itself in the form of a *repulsion* when the symptom was generated .... Thus a psychical *force*, aversion on the part of the ego, had originally driven the pathogenic idea out of association and was now opposing its return to memory. (ibid. Emphasis altered, MBM)

Concurrently with the increase in his use of the waking-state pressure method Freud based more of his theorising on waking-state processes that were of a normal type. Even though conversion was as mysterious as any dissociation state concept, the defensive act of will antecedent to it was easily understood by analogy with the everyday experience of trying to forget unpleasant thoughts and feelings.

THE STRUCTURE OF PATHOGENIC MEMORIES

On what we might call the simple view of symptom formation, each symptom was produced by a single memory. Separate acts of will were followed by discrete repressions after which single conversions led to separate and single symptoms. While Freud sometimes set out his expositions like this it is clear he never held to the simple view in understanding actual cases (Freud, 1893a, p.32; 1894, pp.49-50). Real symptom formation required repressed and non-repressed events to interact with one

another in much more complex ways. Freud attempted to summarise those interactions in a causal model of the memory structure.

Breuer had observed that any one of Anna O.'s symptoms seemed to be related to whole sets of memories and that it was always the earliest in the set that seemed to be the most important. When Freud first used hypnosis to recover traumatic memories he also found that even a relatively simple symptom rarely had a single determinant. For example, when Emmy von N. was asked why she was so readily frightened, she recalled four separate sets of memories, each in chronological order, each containing an element of being frightened (Breuer and Freud, 1895, pp.52-55, 58-62).

Like Breuer, Freud also found:

> the patient's communications are given in a reverse chronological order, beginning with the most recent and least important impressions and connections of thought and only at the end reaching the primary impression, which is in all probability the most important one causally. (op. cit., p.75, n.1)

A chronological ordering also seemed to be true of memories recovered by the pressure method. On its first use, Elisabeth von R. similarly produced a series of recollections rather than the memory of a single traumatic event (op. cit., p.110, n.1 and p.145).

In discussing this multiplicity of memories Freud observed:

> We must not expect to meet with a *single* traumatic memory and a *single* pathogenic idea as its nucleus; we must be prepared for *successions* of *partial* traumas and *concatenations* of pathogenic trains of thought. (op. cit., pp.287-288)

To explain how the memories of these partial traumas succeeded one another and how the causal trains of thought were interconnected Freud formulated one of his most important concepts, that of the pathogenic memory structure. Because the concept is essentially implicit in his work and commented on only infrequently it requires explication.

The concept of a memory structure

The *pathogenic memory structure* was built up around a nucleus of:

> memories of events or trains of thought in which the traumatic factor has culminated or the pathogenic idea had found its purest manifestation. (op. cit., p.288)

The other memories were arranged around the nucleus in a tri-dimensional way. The first dimension was chronological: memories closest to the nucleus were the earliest laid down, the later material being furthest away. A concentric stratification in terms of the degree of consciousness, or of

resistance to retrieval, characterised the second dimension: memories nearest the nucleus were the most difficult to recover and recognise, those more peripherally organised were more readily recalled. The most important was the third dimension. It was:

> an arrangement according to thought-content, the linkage made by a logical thread which reaches as far as the nucleus and tends to take an irregular and twisting path, different in every case .... The logical chain corresponds ... to a ramifying system of lines and more particularly to a converging one. It contains nodal points at which two or more threads meet and thereafter proceed as one; and as a rule several threads which run independently, or which are connected at various points by side-paths, debouch into the nucleus. (op. cit., pp.289-290)

While, in general, the logical chain ran from periphery to nucleus, the actual pathway was much more complex: around the strata in terms of the degree of consciousness, across them chronologically, and from surface to deeper layers and back.

Connecting assumptions

In any given set of memories Freud found that there were incidents that, although not pushed out of mind, had been followed by symptoms. There were also incidents that had been repressed but which did not seem to have caused symptoms. Further, some symptoms seemed to have manifested themselves only after a delay. Freud made three assumptions about the way in which these memories were connected to one another. First, at least one memory in a set had to have been repressed. Once repressed it served as a nucleus around which other ideas similar in content might be grouped even when they themselves had not been repressed. The second assumption was that affects from successively occurring traumas could summate, eventually becoming strong enough to be converted into a symptom. A given idea which lacked strong affect could produce a symptom because of its contribution to a pool of affect. The third was that an incubation period of the type described by Charcot might occur between the trauma and the symptom, even an intense trauma.

Freud's use of the first two assumptions is most clearly illustrated in his account of the case of Lucy R. (Breuer and Freud, 1895, pp.106-124). This patient had complained of a smell of burnt pudding being continuously present in her nostrils. She recovered memories of four events. The earliest was of a conversation in which she realised she was falling in love with her employer. The second occurred a little later when he had criticised her severely for allowing a female guest to kiss the children for whom she was governess. She then felt her employer could never have loved her and she banished the memories of the earlier conversation, the memories of her feelings of love, and the negative feelings she had felt in response to his criticism. A third event almost repeated the second: her employer had

railed against a male guest for also kissing the children. Cigar smoke had been in the air and she was troubled afterwards by a persistent smell of cigar smoke, that is, a symptom had formed. The fourth and last event was one during which she had received a letter that revived her thoughts of leaving her employment. The letter had led to a game with the children and, in the distraction of the game, a pudding they had been cooking had been burned. The presenting symptom was its odour.

The earliest event Lucy R. recalled had not been repressed. Nor had it led to a symptom. It was the second event that had been repressed and Freud thought it had taken the memory of the female guest with it. Together the two memories formed a nucleus to which the memory of the third scene, the one of the second, male guest and the smell of cigar-smoke, had attached itself. The affect of that idea added to the pool and the whole was converted into the mnemic symbol of a cigar smell. The memory of that scene then became unconscious even though it had not been actively banished. Neither had Lucy R. tried to put the memory of the burned pudding of the fourth scene out of mind. Its disappearance, if it had disappeared at all, was due to its connection with the earliest repressed ideas. It had produced the second symptom of the smell of burning because its affect also added to the pool (op. cit., pp.121-124).

Freud's use of the assumption that symptoms might incubate is illustrated in the case of Katharina. Freud recovered the memory of a traumatic incident that had been followed three days later by prolonged vomiting (Breuer and Freud, 1895, pp.126-129). He believed the symptom resulted from a conversion of the affect of disgust which, after summing with the affects of similar previous incidents, had required that time to incubate.

Together the connecting assumptions allowed memories to be related even if they had not been directly and explicitly connected to the symptom in time or in affective force or by intentional banishment. Rather than these kinds of observations being in conflict with Freud's theory, the assumptions maintained their consistency with his explanation of how repression converted affect into symptoms.

Because every memory recovered could be given its place in the pathogenic structure through its direct connection to the symptom or its presumed connections to other memories or through its connection to the pool of affect, Freud was able to argue that no memory recovered during treatment was unimportant:

> It may be asserted that every single reminiscence which emerges during an analysis of this kind has significance. An intrusion of *irrelevant* mnemic images ... in fact never occurs. An exception which does not contradict this rule may be postulated for memories which, unimportant in themselves, are nevertheless indispensable as a bridge, in the sense that the association between two important memories can only be made through them. (Breuer and Freud, 1895, pp.295-296)

While this might have been true in the abstract, Freud's use of the connecting assumptions were to lead to dubious reconstructions of the ways in which the hysterias were supposed to have developed.

Using the structure

The relations between the nucleus, the other memories, and the symptoms defined more than an abstract organisation from which only a history might be reconstructed or an intellectual understanding of the case be gained. One recollection led to another because of their causal contribution to the symptom, not just because they were related in content. Patients could only be cured by systematically working through the pathogenic structure and discharging the affect of the successive memories. Consequently, the concept of the structure and the assumptions about the connections between the memories and the symptom had the important practical use of guiding therapy.

In the case of Lucy R., for example, Freud rejected her first recollection as being the memory at the nucleus because it had not been intentionally repressed:

> It all sounded highly plausible, but there was something that I missed, some adequate reason why these agitations and this conflict of affects should have led to hysteria rather than anything else. Why had not the whole thing remained on the level of normal psychical life?..
>
> Now I already knew from the analysis of similar cases that before hysteria can be acquired for the first time one essential condition must be fulfilled: an idea must be *intentionally repressed from consciousness* and excluded from associative modification...
>
> I accordingly inferred ... that among the determinants of the trauma there must have been one which she had sought intentionally to leave in obscurity and had made efforts to forget. (Breuer and Freud, 1895, pp.116-117)

From the assumption of an earlier repression and from his inferences about the content of the repressed memory, Freud made a guess about the memory and her second recollection seemed to confirm it.

Other deficiencies could be similarly filled out. Critical examination of patients' accounts would "quite infallibly discover gaps and imperfections" such as inadequate explanations of interruptions to a train of thought or the attribution of feeble motivation to an action (Breuer and Freud, 1895, p.293). Because "the same demands for logical connection and sufficient motivation in a train of thought" (ibid.) could be made of the hysteric as of the normal, the patient was told:

> You are mistaken; what you are putting forward can have nothing to do with the present subject. We must expect to come upon something else here, and this will occur to you. (ibid.)

Access to successively deeper layers of the pathogenic structure was gradually gained. Provided sufficient force was exerted, the therapist's insistence would make it possible to:

> penetrate by a main path straight to the nucleus of the pathogenic organization ... now the patient helps us energetically. His resistance for the most part is broken. (op. cit., p.295)

Despite the energetic direction Freud obviously gave to the search for the nucleus, he rejected the possibility he had influenced the content of the recollections:

> we are not in a position to force anything on the patient about the things of which he is ostensibly ignorant or to influence the products of the analysis by arousing an expectation. I have never once succeeded, by foretelling something, in altering or falsifying the reproduction of memories or the connection of events. (ibid. Emphasis removed, MBM)

Why did Freud believe that the memories recovered by the pressure method were always of events connected to the symptoms, that nothing accidental was ever brought to mind, and that it was not possible to force memories on to the patient with it? The answers to these questions are found in other assumptions Freud made about the continuity of psychological processes and the relation between associations and symptoms.

PSYCHOLOGICAL CONTINUITY

It seems to have been as a consequence of the pressure method that Freud came to assume mental processes were continuous. In essence he believed that gaps in a sequence of conscious mental processes marked changes of psychological state. Gaps were simply points at which conscious processes became unconscious. The processes nevertheless remained psychological and had not become or turned into physiological processes. In other words, Freud rejected the equation of 'unconscious' with 'physiological'.

Freud had not always held that psychological processes were continuous in this way. As Hering's lecture on memory and the famous debate between Mill and Hamilton show, the better established tradition was that 'conscious' was equivalent to 'psychical' and 'unconscious' to 'physiological' (Hering, 1870/1913, p.10; Mill, 1878, pp.354-358). Originally Freud had also adopted it. For example, at the end of the Preface to his translation of Bernheim, he assumed that gaps in a chain of conscious psychical processes were bridged by physical processes (Freud, 1888c, pp.84-85). But, by the time of the *Studies on Hysteria*, impressed as he then was by the way the lost memories retrieved by the pressure method eventually filled the gaps in the patient's account of the illness, he saw no need to continue with it.

Freud explicitly based his assumption of the continuity of psycho-

Freud explicitly based his assumption of the continuity of psychological processes on Jackson's version of psychophysical parallelism. Jackson argued strongly that physiological processes and their dependent psychical processes should not be confused with each other, warning especially against:

> the fallacy that what are physical states in lower centres fine away *into* psychical states in higher centres; that, for example, vibrations of sensory nerves *become* sensations, or that somehow or another an idea produces a movement. (Jackson, 1878-1879)

Freud first used Jackson's thesis in 1891, in his *On Aphasia*, as a basis for his attack on the localisationist views of Meynert and Wernicke (Freud, 1891/1953, pp.54-56). Although in his adaptation of Jackson's position, Freud made the mental a 'dependent' concomitant of the physical (and not just a concomitant), that change made his position somewhat stronger than Jackson's.

Freud described how he could follow a train of thought from:

> the conscious into the unconscious (i.e. into something that is absolutely not recognized as a memory), ... we can trace it from there for some distance through consciousness once more and ... we can see it terminate in the unconscious again, without this alteration of 'psychical illumination' making any change in the train of thought itself, in its logical consistency and in the interconnection between its various parts. Once this train of thought was before me as a whole I should not be able to guess which part of it was recognized by the patient as a memory and which was not. I only, as it were, see the peaks of the train of thought dipping down into the unconscious - the reverse of what has been asserted of our normal processes. (Breuer and Freud, 1895, pp.300-301. Cf. pp.269, 293)

It was precisely the gaps in the patient's account of his or her symptoms that indicated the presence of "secret motives"; it was there that Freud looked for the connecting threads (op. cit., p.293). Toward the end of the treatment, he said later, the facts given by the patient enabled the construction of an "intelligible, consistent, and unbroken case history" (Freud, 1905a, pp.17-18).

The assumption of continuity was central. Eventually he was to say of the observations he had made while filling the gaps that they became "*the determining factor of his entire theory*" (Freud, 1904, p.251. My emphasis, MBM). He held to this position until the very end:

> We make our observations ... precisely with the help of the breaks in the sequence of 'psychical' events: we fill in what is omitted by making plausible inferences and translating it into conscious material. In this way we construct, as it were, a sequence of conscious events complementary to the unconscious psychical processes. (Freud, 1940a, p.159)

For Freud, the only alternative to assuming that conscious processes were not unbroken sequences, complete in themselves, remained that of the con-

comitant "physical or somatic processes" being "more complete" than the psychological (op. cit., p.157).

SYMPTOMS AS ASSOCIATIONS

In the *Studies on Hysteria* both Breuer and Freud proposed associations between an intense affect and a simultaneously occurring pattern of muscular or sensory innervation as the most important immediate basis for the formation of symptoms. For example, Anna O.'s convergent squint formed when an intense emotional upset about her father had coincided with the pattern of innervation of the extra-ocular muscles produced as she moved a watch closer to her eyes, thereby resulting in a real squint (Breuer and Freud, 1895, pp.39-40, p.208). Elisabeth von R.'s leg pains resulted from the association between her erotic feelings for her brother-in-law and the real leg pains she was presumed to be experiencing simultaneously (op. cit., p.165). An association between a real olfactory sensation and a simultaneous feeling of love for her employer was said to explain the peculiarities of Lucy R.'s disturbed sense of smell (op. cit., pp.118-119). Associations might also create symbolic symptoms. For example, Cäcilie M.'s facial neuralgia was said to symbolise a self-reproach because insults experienced figuratively as slaps in the face had supposedly occurred at the same time as a slight toothache or facial pains (op. cit., pp.176-179). More extreme forms of symbolism were also observed. In Cäcilie M., for example, penetrating forehead pains had formed when she had received a suspicious look she had experienced as penetrating at the very time she had been afraid (op. cit., pp.179-180).

Meynert's physiological associationism

There can be little doubt that it was Meynert's particular brand of physiological associationism on which Breuer and Freud drew. In 1865 Meynert had published a paper which he claimed had for the first time established a physiological basis for the formation of associations (Meynert, 1884/1885, p.153). According to him, an association was a linkage between the cells of the cortex in which the neural representations of the elements of the association were stored.

Meynert and his work were well known to Breuer and Freud. He was the Professor of Psychiatry at the University of Vienna, the author of a widely read textbook on psychiatry, and it was in his laboratory that Freud worked for some years on brain anatomy. Even though Freud himself rejected a number of Meynert's anatomical and functional concepts (Freud, 1891/1953, pp.44-54) he, as well as Breuer, drew on other aspects of Meynert's work. For example, Freud fully accepted Meynert's associationism in referring to the associational pathways in the same terms as Meynert (Meynert, 1884/1885, p.154). Again, when Breuer explained some symptoms as associations formed by simultaneity, he illustrated how one

element of such an association recalled the other with what was obviously Meynert's example of the formation and revival of the association between the sight and sound of a lamb (although he called it a sheep) (op. cit., p.208. Cf. Meynert, 1884/1885, p.154).

Induction, cause, and association

Breuer and Freud also accepted Meynert's view of an identity between an association, a logical relation, and a causal connection. Meynert argued:

> Inferring one attribute of a phenomenon from the presentation of another attribute, constitutes an induction; it is a recognition *in the direction of causality*, for the bleating sound is taken to be the result of the presence of the lamb. (Meynert, 1884/1885, p.154. My emphasis, MBM)

Meynert added:

> The anatomy of the cerebral structure, and the proof of the presence everywhere in the brain of an induction apparatus, render it highly probable that all perceptions received simultaneously or in continuous succession become correlated with one other. Such connections explain the relations of successively and separately received impressions to one another. Our methods of thought and of speech have designated this relation as one of causality; but this is a purely cerebral function, for there need be no bond in the outside world corresponding to these cerebral relations of causality. (op. cit., p.177)

However:

> as soon as the subjective bond of causality represents an actual union of things, the re-occurrence of external stimuli will establish a permanent association within the brain. Thus by the renewal of perception, such *associations* are turned into the elements of *inductive logical thought*. (ibid. My emphasis, MBM)

In Meynert's view then, an association was a logical induction from which causal relations could be inferred.

Freud implicitly equated the logical dimension of his pathogenic memory structure with a set of associations: he described the dimension as "an arrangement of memories according to *thought-content*, the linkage made by a *logical thread* which reaches as far as the nucleus" (Breuer and Freud, 1895, p.289. My emphasis, MBM). Both the arrangement of associations and the logical thread were causal connections for it was in the nucleus that the causal "traumatic factor ... culminated or the pathogenic idea ... found its purest expression" (op. cit., p.288). Freud also used these ideas when describing chains of associations: interruptions were breaks in the thread (op. cit., pp.271-272), and ideas "closely linked in thought", i.e., in content, led to the pathogenic idea, that is, the causal idea (op. cit., p.276). Only by formally identifying associations with logical relations and causal connections were these descriptions possible and only then was

Freud justified in using the connecting thread to guide therapy. Following a train of associations in the way Freud did was equivalent to unravelling a chain of causes and so revealing the internal logic of hysteria.

Determinism and trains of associations

Although Meynert's conception of the revival of one element of an association by another was a physiological one, past experience controlled the revival of both single associations and whole trains of thought and completely determined which associations could be revived:

> certain obstacles which impede the excitation of cells in full repose are *very much lessened* after a single, and particularly after repeated, identical excitations of association-bundles uniting the cells of two distinct areas of the cortex; while the transmission of such stimuli to association-tracts, which have been called upon to unite other, previously established groups of associations, *becomes well nigh impossible.* (Meynert, 1884/1885, p.155. My emphasis, MBM)

A pathway between two cortical "images" was provided by association-tracts excited by both:

> A train of thought starts from a residual image in the cortex. All the associations connected with this image are, as it were, ready for action. (op. cit., p.253)

The associations available were those experienced previously. When two images were present:

> the associations connecting these two images are under double attraction, and will consequently be more intensely excited than any others. (ibid.)

Common associations provided the pathways that allowed thinking to move from one image to another:

> The orderly evolution of any one thought implies a starting-point and a goal between which it runs its course. The two images are at either end of this course; and through a strict observance of this course a firm union is established between them. Just as a marksman, in spite of numberless objects around him, establishes a direct relation between his finger on the trigger and the bull's-eye which he is to hit, so a similar relation is established between the two terminal images, which controls the direction of the play of association. (ibid.)

This bullet like determinism meant that a train of thought could terminate only in an idea that shared experiences and pathways with the starting idea.

Freud's specific acceptance of the importance Meynert placed on common experience is evident in the 1888 article on the brain he wrote for Villaret's *Handwörterbuch* (Freud, 1888d) in which, as Amacher (1965, pp.58-59) pointed out, he also alluded to the lamb. Later, in the unpublished *Project*, written not long after the *Studies on Hysteria*, Freud included

the pleasurable and unpleasurable affects of earlier experiences among the factors determining non-critical, reproductive thought along with inborn pathways of transmission and facilitations due to previous common excitation (Freud, 1950/1954, *Project*, Part I, Section 16 and Part III, Section 1). Other than by helping the patient to overcome the resistance caused by unpleasurable affects, no action of Freud's could influence the recollective process. Once begun, recall determined its own paths.

Of course, in Freud's view the pathway between the symptom and the memory of the traumatic event was not single. Multiple pathways intersected in complex ways before coming together to join the pathogenic nucleus. Nevertheless, the pathways were only laid down from sets of associations common to adjacent elements and it was still only past experience that guided the present train to its end.

Freud's concept of a pathogenic memory structure and related assumptions had been arrived at before the *Studies on Hysteria* was written. The concept later came to fruition as the primary method of psycho-analysis, that of free association. Although discussion of the rule takes us out of the historical period I have been considering, it is appropriate to examine it here because it is based on essentially the same deterministic assumptions as the pathogenic memory structure.

THE PRIMARY METHOD OF PSYCHO-ANALYSIS

The basic method by which psychological phenomena are investigated in psycho-analysis is *free association*. It is also the basis of psycho-analytic therapy. Free association consists of adjuring the patient to follow what Freud termed "the fundamental rule of psycho-analysis": while focussing attention on the symptom, the dream, or the parapraxis (a faulty action such as a slip of the tongue) being analysed the patient has to suspend his or her critical attitude and report all the ideas that then come to mind. More correctly, given the meaning of the German 'Einfall', [4] the patient has to report those ideas that suddenly and involuntarily irrupt into consciousness.

Freud believed the ideas reported under the rule were no more random than those which came to mind in response to the pressure method. The various inadequacies in the patient's account so revealed had been met with a demand for connection and motivation "even if [the train of thought] extends into the unconscious" (Breuer and Freud, 1895, p.293). When successfully met, the indistinct recollections became clearer, the effect of

4. For the considerable difficulties in translating the German 'Einfall' and the expression 'freier Einfall', difficulties which are compounded by Freud sometimes using the latter as synonymous with 'freie Assoziation' (free association) and sometimes not, see the discussions by the Editor of the *Standard Edition* (*11*, p.29, n.1 and *15*, p.47, n.1).

the mutilated manner in which the scenes emerged was overcome, and the elements missing from the broken connections were filled in. Similarly with the rule: despite the gaps, the confused order, and the broken causal connections, trains of complete associations could be followed to their causes. The 'fundamental rule' was obviously implied in the *Studies on Hysteria* and was much used before Freud gave it that name in 1912, in the *Dynamics of transference* (Breuer and Freud, 1895, pp.270-272, 276, 280-281, 293-296; Freud, 1912a, p.107 and n.2. Cf. Freud, 1896c, pp.198-199; 1900, pp.101, 523; 1901b, pp.9, 80; 1904, pp.251-252; 1910a, pp.33, 35; 1916-1917, p.287 and n.1; 1925a, pp.40-42).

The 'fundamental rule' was largely a consequence of Freud's belief in psychic determinism. Ideas reported under it were determined or caused by the chains of ideas to which they were connected and the chains themselves were also completely determined. Application of the rule depended on the twin notions of a train of thought being continuous and of it being guided by an unconscious idea. Freud assumed that ideas produced during free association were under the guidance of an unconscious idea connected with the one being focussed upon. The thesis was proposed explicitly by von Hartmann, although Freud came across it much later than when preparing the *Interpretation of Dreams*, where he first mentioned von Hartmann (Freud, 1900, p.528, n.1 of 1914). It was, however, close to one of Meynert's. According to Meynert, a train of thought beginning "from a residual image in the cortex" led inevitably to another with which it had previously been connected. Because Meynert allowed that the connections of this "initiatory image" might be unconscious, in the sense of not being stimulated sufficiently to rise above the threshold for consciousness, a train of associations could occur even when all of the linkages were unconscious (Meynert, 1884/1885, pp.246-247, 252-253). von Hartmann also had an idea evoking a train of thought, but in his case it was a motive or a special interest. Ordinarily the motive or interest was a conscious purpose but, "if one in appearance completely abandons his train of thought to accident", some other but unconscious special motive then directed "the train of thought to its particular goal" (E. von Hartmann, 1882/1931, *1*, Section B, Chapter 5, pp.283-284. Cf. Freud, 1900, p.528, n.1 of 1914). Freud put it the same way in one of his earliest implicit references to the rule, it was "demonstrably untrue" that trains of thought were purposeless; when conscious purposive ideas were abandoned, unconscious ones simply took their place (Freud, 1900, pp.526-529). The train of thought remained psychically determined.

What, then, did Freud mean by psychic determinism? Simply 'psychic causation'. No idea became conscious unless it was logically connected with another and ideas came or went from consciousness because of the kinds of causal links or associations they had with other ideas. These are the psychological assumptions underlying the pathogenic memory structure and the use of the pressure method. Free association stood in the same relation to psychic determinism as had the pressure method. As Brill

(1938-1939) put it, free association was used "to find the origins of symptoms", a search that was "equivalent to a search for ... determinants".

Meynert's speculative physiological associationism provided Freud with the basis for treating associations as causal connections. If unconscious purposive ideas or motives set the train of associations in motion, and if only ideas already present in the mind could be incorporated into it, as von Hartmann and Meynert assumed, associations evoked by the starting idea could not but be other than causes or links in a causal chain which had to terminate with the purposeful, causal idea (E. von Hartmann, 1882/1931, *1*, Section B, Chapter 5, pp.276-277).

Freud clearly believed the pressure method, based as it was on robust deterministic assumptions, to be strong enough to resist his attempts to force memories on to his patients. However, the only mode of influence he considered, and rejected, was the same one he had previously considered and naively rejected in relation to Charcot, that of direct suggestion from therapist to patient (Breuer and Freud, 1893, p.7; 1895, p.295). [5] Obviously Freud believed that in ruling out direct influence he could be sure that the memories would always be of events that had actually happened to the patient. Similarly, the fundamental rule guaranteed "to a great extent ... that nothing will be introduced into [the structure of the neurosis] by the expectations of the analyst" (Freud, 1925a, p.41).

RECONSTRUCTING HYSTERIA

Freud used the assumptions underlying the pathogenic memory structure as an aid in reconstructing the way the symptoms had developed. Thus, with Lucy R., he not only assumed an earlier event; he guessed at its content and suggested to her she was in love with her employer before she had revealed that fact. It was the discussion of those feelings that led to the recollection of unrequited love. Here Freud had evidently guessed correctly.

In other cases the matter is not so clear. Freud supposed Katharina had felt disgust prior to her vomiting. Her rather reflective response to that suggestion is not very compelling: "Yes, I'm sure I felt disgusted, but disgusted at what?" (op. cit., p.129). Later Freud suggested directly what it was that had disgusted her but she replied only, "It may well be that that was what I was disgusted at and that that was what I thought" (op. cit., p.131). Despite the lack of conviction conveyed by these rejoinders, Freud made an incident in which she had experienced disgust central to his reconstruction of the history of her vomiting.

5. The reports of Breuer's defence of Freud's views on the sexual causes of hysteria in October, 1895 show Breuer also considered only direct suggestion (Sulloway, 1979, Appendix A).

In the case of Elisabeth von R. it is very evident that Freud used the assumptions to create a *plausible* account of the development of her symptoms rather than to re-create a *real* history, a plausibility achieved in part by disregarding observations conflicting with the assumptions. It is also evident in that case that he preferred the plausible reconstruction to the observable or ascertainable facts. Elisabeth von R. developed very severe hysterical leg pains two years after the death of her father whom she had nursed for the last eighteen months of his illness (op. cit., pp.140-141). While nursing him she had been persuaded to go to a party where she had met a man of whom she was fond and had stayed later than she had intended. Her father's condition being much worse on her return, she reproached herself for having enjoyed herself so much. This pattern of events suggested to Freud that he "could look for the causes of her first hysterical pains" in her relation with the young man and the conversion of the affect of the "erotic" ideas aroused by meeting him (op. cit., p.146). However, the pains had not commenced "at the moment of her return home", or even at about that time, and he was unable to identify "any psychical cause" for them (op. cit., p.147). Freud thereupon *assumed* a pain had been produced but that it "was certainly not perceived at the time in question or remembered afterwards" (op. cit., p.148).

In bringing the case history to a close, Freud summarised what was supposed to have happened as follows:

> While she was nursing her father, *as we have seen*, she for the first time developed a hysterical symptom - a pain in a particular area of her right thigh .... It happened at a moment when the circle of ideas embracing her duties to her sick father came into conflict with the content of the erotic desire she was feeling at the time. Under the pressure of lively self-reproaches she decided in favour of the former, and in doing so *brought about her hysterical pain.* (op. cit., p.164. My emphasis, MBM)

Freud acknowledged there was a conflict between his suppositional reconstruction and the patient's recollection:

> On the evidence of the analysis, I *assumed* that a first conversion took place while the patient was nursing her father....
>
> But it appeared from the patient's account that while ... nursing ... and during the time that followed ... *she had no pains whatever and no locomotor weakness....*
>
> the patient had behaved differently in reality from what she seemed to indicate in the analysis. (op. cit., p.168. Emphasis altered, MBM)

Freud then drew on the connecting assumptions to suppose that a second event occurring two years later and really involving leg pains had converted the affect belonging to the first memory (ibid.). He concluded:

> Stated in terms of the conversion theory, this incontrovertible fact of the summation of traumas and of the preliminary latency of symptoms tells us

> that conversion can result equally from fresh affects and from recollected ones. (op. cit., pp.173-174) [6]

Summation of traumas and the latency or incubation of symptoms are never facts, of course. They can only ever be implications from the assumptions about the ways in which symptoms, traumas, and affects might be connected. In Elisabeth von R.'s case, the 'fact' of a delayed conversion of the affect of the first memory was nothing more than an implication of the way Freud's assumptions *demanded* the memories be connected. Freud's acceptance here of his reconstruction over what he was told shows a rather ominous preference for the plausible but theoretically neat over the factually uncomfortable.

CONCLUSIONS

Although Freud initially accepted splitting, hypnoid states, and secondary consciousness as fundamental to symptom formation, his observation that some symptoms could be removed in the waking state led him to the view that they could also be acquired in it. While the intermediate mechanisms of counter-will and associative inaccessiblity show only too clearly the burden of the earlier heritage, Freud eventually overcame the inconsistency of being able to remove symptoms in a state different from that in which he had previously assumed they had been formed. Abnormalities in mental life resulted from the difficulty which psychological forces in the ego had in coping with the affect that accompanied an idea incompatible with it. An affectively based cathartic method was now central to his therapy and with his new concepts, Freud was able to suggest how symptoms other than those of hysteria had been acquired.

Some of the psychological forces Freud described were familiar from everyday mental life. For example, the act with which repression began was like any other attempt to fend off an unwanted idea and the continuous pressure confining the repressed idea to its unconscious exile was similar to other prolonged attempts to forget unpleasantly toned memories, and so on. Nothing at all resembling this view of psychological processes was to be found in the theories of Freud's predecessors in the fields of philosophy, psychiatry, or abnormal psychology.

Freud's contention that trains of association had their own internal determinants, and could not be influenced directly, extended but did not go beyond the view of determinism he first expressed in relation to Charcot's work. Freud believed the ideas recovered by his patients were no more influenced by his expectations or suggestions than the phenomena at the Salpêtrière. Apart from helping to overcome temporary resistances the therapist could not influence the process leading from one idea to another.

6. Not as the *Standard Edition* has it "fresh symptoms". Compare Freud's *Gesammelte Werke*, Vol. *1*, p.242.

Freud insisted that nothing without significance appeared in the patient's consciousness - everything had a place in the pathogenic memory structure. Together with the three assumptions about interconnection, the structure purported to allow an accurate reconstruction of the history of the symptom. Freud's method of memory retrieval was objective and his assumptions about the structure supposedly wove each memory correctly into the tapestry of causal relations.

Although Freud thought his method of determining the causes of symptoms was as objective as any other scientific technique, his connecting assumptions meant that more-or-less direct tests of his causal hypotheses and explanations were not possible. Complex reconstructions are only needed if symptoms do not manifest themselves fairly soon after banishing the incompatible idea. On the simple view of symptom formation anyone mastering the pressure technique or the method of free association could test Freud's explanations for themselves. It would then have been as reasonable for Freud to demand that his critics adopt his techniques as was for Galileo to demand his look through his telescope. Indeed, on the simple view any such demand would have actually been more reasonable. Evaluating the causal significance of a single remembered idea did not rest upon even as complex a web of theoretical presuppositions as did Galileo's use of the telescope (Lakatos, 1970, p.98). Freud's assumptions put paid to the reasonableness of any demand that others repeat his observations. His hypotheses and explanations were soon made even more difficult to test. He allowed that the patient might even not remember the act of will. There was then nothing to which the appearance of the symptom could be related. The scope Freud gave for apparently contradictory observations to be explained away and for plausible reconstructions to be preferred to fact meant that almost from its beginning a very heavy burden was placed on psycho-analysis in establishing the adequacy of its method and its theory.

A NOTE ON PRIORITY

Nearly thirty years after 1892, in *An Autobiographical Study*, Freud denied Janet any credit at all for the notion that the popular idea of the organ determined the details of hysterical symptoms affecting it:

> Before leaving Paris I discussed with [Charcot] a plan for a comparative study of hysterical and organic paralyses. I wished to establish the thesis that in hysteria paralyses and anaesthesias of the various parts of the body are demarcated according to the popular idea of their limits and not according to anatomical facts. (Freud, 1925a, pp.13-14)

No contemporaneous evidence exists of Freud's so discussing the role of "the popular idea" with Charcot. About six months after returning from the Salpêtrière, Freud presented a case of hysterical hemi-anaesthesia to the Vienna Society of Medicine in which the limits and the distribution of the anaesthesia could have been most usefully summarised with that concept, but Freud does not even hint at it (Freud, 1886b).

How Freud conceptualised the paralyses at that time is revealed with especial clarity in the footnote he appended to that part of his translation of Charcot's *Leçons du Mardi* where Charcot was distinguishing between organic and hysterical aphasias. Charcot had drawn attention to the intense or absolute nature of hysterical aphasia - usually all speech was lost - and its precise delimitation or isolation - it affected spoken language only, the patient usually being still able to understand and use language in reading and writing (Charcot, 1887, pp.362-363). In his footnote, Freud described how Charcot had set him the task of comparing organic and hysterical paralyses but said only that its outcome was "a further extension of the thesis laid down here by Charcot", that is, hysterical paralyses were like the aphasias in being characterised by intensity and isolation. He made no reference at all to "the popular concept" (Freud, 1892-1894, p.140. Note to p.268 of Freud's translation). Unfortunately the footnote cannot be dated accurately but there is some probability that it was written before June 1892 (Editor's Note, *Standard Edition, 1*, p.131).

The validity of Freud's 1925 claim to originality seems not to have been much discussed. Ernest Jones (1953-1957, *III*, pp.233-234) appears to accept it. But he clearly mixes Freud's propositions of 1888 with Janet's of 1892 and others have followed him. Thus Levin (1978, p.76) discussed Freud's characterisation of hysterical aphasia which appeared in another of Freud's (1888b, p.89) contributions to Villaret in a context where he interprets Freud's 1888 hysteria-as-ignorant thesis as if it included Janet's of 1892 on ideas. However, in the companion-piece in Villaret Freud only *described* hysterical aphasia and said nothing about its relation to anatomy. Levin therefore conveys the quite incorrect impression that Freud explicitly proposed the loss of the common-sense notion of speech as the basis of hysterical aphasia.

One can make similar objection to the argument of Chertok and Saussure (1973/1979, pp.76-77) that Freud had no need to acknowledge Janet at all. According to them, the essential points of Janet's 1892 concept are summed up in Freud's two 1888 propositions about hysterical disorders not copying anatomical conditions and their 'ignorance' of the structure of the nervous system. However, neither of those propositions is as positive and specific as Janet's. And, as we have seen, even with them Freud was unable to finish his paper.

More recently, Masson (1985, p.22, n.2) has said Charcot marked his own copy of the journal in which Freud's paper appeared at exactly the point where Freud states the 1893 version of the hysteria-as-ignorance view. Since Masson's discussion is also in a context which represents Freud as the originator of Janet's thesis, he thereby implies Charcot's recognition of Freud's originality. Charcot's markings cannot have that significance. Charcot himself had stated the ignorance view, at least in part, and Freud had set it out formally in 1888. For Charcot to have recognised Freud's originality, he would have had to have marked the passages where Freud extended and transformed Janet's well-known thesis.

5 CAUSES AND THE ACTUAL NEUROSES

Despina: Saper bisognami pria la cagione
E quinci l'indole della pozione...

Mozart: *Cosi Fan Tutte*, Act I, iv.

Why did Freud think that the symptoms of hysteria were caused by memories? For the most part he and Breuer first implicated them because the symptom was removed by bringing the memory into normal consciousness: "Remove the cause and the effect will cease". We have also seen that as time went on the way the core memory could be placed in the pathogenic structure also became important. But a more sophisticated method of establishing and evaluating causes was required. In this Chapter I show how the method that Freud developed waited on his work on neurasthenia and anxiety neurosis.

The main relevance of my evaluation of Freud's causal analysis is for his conclusion that hysteria had an exclusive sexual etiology. In neither the *Preliminary Communication* with Breuer nor in his own contemporaneous lecture on hysteria did Freud describe the repressed incompatible idea as having any particular content let alone that it was sexual (Breuer and Freud, 1893; Freud, 1893a). Moreover, in the *Preliminary Communication* the conclusions from the patients who were to be reported on in more detail in the *Studies on Hysteria* were summarised without sexual factors being singled out. However, by the time the *Studies on Hysteria* was written both Breuer and Freud had come to believe that in most instances the idea was a sexual one. As Breuer put it: "The most numerous and important of the ideas that are fended off and converted have a sexual content" (Breuer and Freud, 1895, p.245. Cf. Freud, 1894, pp.47, 52). Freud's investigations over the next few months convinced him repression in obsessional neuroses was always of a sexual idea and he was soon to extend that conviction publicly to hysteria (Freud, 1895c, p.75, 1896a, 1896b).

Were simple observations, untrammelled by theoretical considerations or other expectations, responsible for Freud's adopting this exclusively sexual etiology? My answer is "No". For the most part it resulted from expectations generated by his false conclusion that the causes of neurasthenia and anxiety neurosis were also sexual. That is why in this Chapter I concentrate on the inadequacies of Freud's methods of evaluating data about presumptive causes.

I begin by showing neurasthenia to have been the most important of the wide range of nervous diseases and disorders Freud investigated and treated at the beginning of his medical practice. I argue that it was largely through a faulty adaptation of Koch's postulates for determining whether a particular bacterium was the cause of a given disease that Freud concluded that neurasthenia, together with the entity of anxiety neurosis he detached from it, had an exclusively sexual etiology. I show this mistaken conclusion to have been well-established by the time Freud started his extensive studies of hysteria and that from the time he began them he consciously attempted to establish a sexual etiology for it. When he did so, he used the same faulty principles for weighing the evidence as he had for neurasthenia and anxiety neurosis.

THE PRECEDENCE OF NEURASTHENIA

Freud himself tells us that neurasthenia and not hysteria was the most important of the wide range of nervous diseases and disorders he saw in the first five or six years of his medical practice between 1886 and 1892 (Freud, 1887, 1892-1893). We also knew that during that period Freud wrote to Fliess about only two cases of neuroses. One, a neurasthenic, was discussed in some detail but the other, an hysteric, although a very important one, was mentioned only in passing (Masson, 1985, Letters of 24.11.87, 4.2.88, 28.5.88, 12.7.92; Swales, 1986b). Further, in the extensive discussions about neuroses which he initiated with Fliess at the end of 1892, Freud only ever mentioned hysteria in passing, and then in a fairly elementary way. What dominated his correspondence until the end of 1893 were the discussions of neurasthenia and anxiety neurosis, what he came to call the actual neuroses. [1]

It is also clear from the correspondence that it was for neurasthenia and not for hysteria that Freud first proposed an exclusively sexual etiology. He had attempted to construct a general theory of the neuroses on a sexual basis from at least early 1894, possibly from as early as twelve to eighteen months before that. Thus, the list of potential causes of neurasthenia he drew up at the end of 1892 were all sexual, by the February of 1893 he appears to have concluded that they were indeed all sexual, in the May he remarked that his explanation of anxiety attacks in virginal females was capable "of filling yet another gap in the sexual etiology of the neuroses", and a little later again he spoke as if his sexual 'etiological formula' had been firmly established. Hysteria is barely mentioned in passing in the

1. Mr. Peter Swales has suggested to me that some caution needs to be exercised in so interpreting this peculiarity of the Freud-Fliess correspondence. He thinks it possible Freud divided his interest in the neuroses between Fliess and Breuer, restricting his discussions about the actual neuroses to the former and sharing his ideas about hysteria only with the latter. While this could have been the case, I believe the detailed analyses of Freud's early theoretical concepts about hysteria in the last chapter show them not to be consonant with an exclusively sexual etiology either.

correspondence (Masson, 1985, Draft A, possibly of December, 1892, Draft B of 8.2.1893, Draft C of between February and May, 1893, and Letter of 30.5.93).

ESTABLISHING CAUSES

Freud's conclusion that each of the actual neuroses resulted from a specific sexual practice is a statement of an invariant connection between the sexual practice as cause and the neurosis as effect. Generalisations of this kind set out either the necessary or the sufficient condition of some effect. When an effect Y is observed and a presumed cause X is also always present, then X is said to be necessary to cause Y or to be a necessary condition of Y. On the other hand, if when X is present, Y also always occurs, then X is said to suffice to cause Y or to be a sufficient cause or condition of it. In other words, for a factor or set of factors to be the sufficient cause of some effect, it must be the case that when it or they occur(s) the effect always follows.

Analysing causes

Despite the many analyses of causality made between Aristotle's and more recent times (e.g. H. L. Hart and Honoré, 1985), it has not proved possible to arrive at a universally valid definition of cause or to schematise *the* relation between cause and effect. Nevertheless, the view that cause and effect are easily defined and stand in rigid antithesis to one another is extremely plausible. It is a plausibility which comes about, as Engels noted in the preliminaries to his critique of Duhring's 'total revolution in philosophy', because it is "the mode of thought of so-called sound commonsense". But, he went on:

> sound commonsense, respectable fellow that he is within the homely precints of his own four walls, has most wonderful adventures as soon as he ventures out into the wide world of scientific research. (Engels, 1894/1947, p.37)

In science, as Engels explained, the extent to which any rigid opposition between categories of thought, such as cause and effect, can be maintained varies with the domain and object of investigation. Sooner or later, he argued, those oppositions reach a point beyond which they become one-sided and limited, and have validity only in a particular case:

> but when we consider the particular case in its general connection ... they merge and dissolve in the conception of universal interaction, in which causes and effects are constantly changing places, and what is now and here and effect becomes there and then a cause, and *vice versa*. (op. cit., p.38)

It is "universal interaction" which makes universally valid definitions impossible and, on the other hand, the relatively circumscribed domain which makes establishing the cause of disease possible.

Germ theory, Koch's postulates and causality

According to the germ theory of disease proposed by Pasteur and Koch, diseases came about through infection by bacteria. The particular or specific organism responsible for a given disease could be identified through the three procedures enshrined in Koch's postulates. First, the suspect bacterium had to be different from any other by taking up staining material in a unique or specific way. Second, it had to be found in every instance of the given disease and not in any other. Third, inoculation of a culture from it had to produce the disease experimentally. Koch argued that only if all three conditions were satisfied could it be concluded that the bacterium was the specific cause of the disease.

When Koch's postulates are compared with the distinction between necessary and sufficient conditions, we see that the reaction to staining and the presence of the specific bacterium in every case of the disease but not in others identifies the necessary condition of that disease. Producing the disease in healthy but susceptible animals by inoculation with culture from the specific bacterium identifies the sufficient. Koch's postulates thus translate the distinction between necessary and sufficient conditions into precepts guiding research into causes.

Freud, germ theory, and Koch's postulates

We know how much importance Freud put on Koch's postulates from his discussion of them in two articles he contributed to the American medical literature some twelve years before his papers on the actual neuroses (Grinstein, 1971). The first, written early in 1883, evaluated some work questioning Koch's discovery of the tubercle bacillus. Freud summarised Koch's arguments as follows:

> By applying a new method of coloring, he succeeded in detecting what he believes to be a new specific organism, and sought to establish its relation to tubercular disease by two important facts. First, this organism - bacillus - is nowhere found but in tubercular material, from which it is never absent; and, second, when cultivated and inoculated upon hitherto healthy animals, it causes well-characterized tubercular disease. (Freud, 1883, cited in Grinstein, 1971)

The reaction to colouring (staining) distinguished between different bacteria and thereby established if the one under investigation was specific to the disease. The first of the "important facts" described by Freud is, of course, none other than that which established the bacillus as a necessary condition: it was found only "in tubercular material". The second was the evidence for it being part of the sufficient conditions: it "cause[d]" the disease. (In the necessarily brief article Freud omitted discussing the other part, the pre-condition of susceptibility). Freud's second article, written some twenty months after the first, evaluated the significance of a report that another bacterium had been found "nowhere but in syphilitic tissues".

The question was whether it was the cause of syphilis:

> The results of further experiments on the cultivation of the new bacillus, and on infection of animals by subsequent cultures of it, and further microscopical investigation, are to be expected before the last word on the new bacillus can be said. (Freud, 1884, cited in Grinstein, 1971)

Freud here recognised quite clearly that the disease had to be produced by inoculation before it could be concluded that the sufficient condition or specific cause of syphilis had been established.

The causal framework Freud used in establishing the causes of the actual neuroses is a revolutionary adaptation of Koch's postulates. Prior to Freud, no one had set out any kind of logical system by which the factors causing neuroses could be judged and no one had applied principles from physical medicine to identify their specific causes. Carter (1980) was the first to show in detail that Freud derived his methods for evaluating the causes of neuroses from germ theory although the indebtedness to Koch had been noted earlier (e.g. Macmillan, 1976). While Carter is undoubtedly correct in regarding it as a major attempt to bring some logic to the investigation of the causes of neuroses, he is wrong in thinking Freud's *initial* application of the precepts was to *hysteria*. [2]

We now need to consider Beard's concept of neurasthenia and his analysis of its basis and causes before examining Freud's use of Koch's postulates in his evaluation of its cause.

BEARD'S NEURASTHENIA

About fifteen years before Freud began his practise as a neurologist, the American neurologist George Miller Beard described a new clinical entity he called neurasthenia. In the absence of anaemia or organic disease, neurasthenia was to be diagnosed by the following symptoms:

> general malaise, debility of all the functions, poor appetite, abiding weakness in the back and spine, fugitive neuralgic pains, hysteria, insomnia, hypochondriases, disinclination for consecutive mental labor, severe and weakening attacks of sick headache, and other analogous symptoms. (Beard, 1869)

For Beard, 'neurasthenia' retained its literal meaning of a weakness of the nerves.

2. The influence of germ theory may be even more extensive than Carter (1980) suggests. Arlow has pointed out, possibly without knowing Carter's paper, that Freud's whole conception of the cause of a neurosis as a pathogenic foreign body "drawing about itself the memory of contiguous events and of associated recollections .... is not unlike the organized tissue reaction surrounding the necrotic core of a tubercle in response to the toxins of the tubercle bacillus". Arlow draws the further parallel between catharsis and "the extrusion or ejection of the noxious foreign body" (Arlow, 1981. Cf. Arlow, 1959; Waelder, 1967a).

Neither the term nor the notion that neurasthenia was a disease was original with Beard. The concept actually has a very long history in medicine (Arndt, 1892b; López Piñero, 1963/1983) and the term itself was in such common use in the second quarter of the nineteenth century that it is to be found in a number of medical dictionaries of that period. López Piñero (1963/1983, p.73) cites two such entries from the 1830's and Bunker (1944, p.214, n.54) has one for 1856. For a time 'neurasthenia' was used to describe individual symptoms but gradually evolved into a name for a disorder or disease. The first person to so use it seems to have been Van Deusen in 1867 who also provided the first outline of a method of treatment (Van Deusen, 1868-1869. Cf. Bunker, 1944, n.54 and Wiener, 1956, n.5).

Beard was unable to demonstrate any organic or physiological basis for the condition although he did think there was a dephosphorizing of the nervous system, or a loss of other solid constituents, with, perhaps, slight changes in chemical structure. Beard was led to this essentially functional view by inference, or as he put it, it was "rendered logically probable", by Du Bois-Reymond's experimental production of analogous changes in the nervous system. He added:

> We know that the intelligence of men and animals is proportioned to the quantity of the cerebral contents, that the proportions of water, of phosphorus, of fat, and of the other solid constituents of the central nervous system vary more or less with age, and with the intellectual and moral capacity, and that all forms of insanity are dependent on *some* central morbid condition. (op. cit.)

From these "facts" it followed that any disturbance in the amount or quality of the constituents of the nervous system would create the "morbid condition" of neurasthenia in it.

The causes of neurasthenia

Beard distinguished two classes of causal factor: a predisposing one of "hereditary descent" and various precipitating or "special exciting causes". He adduced no particular evidence for heredity and referred to familial tendencies in only a very general way. His clinical judgement provided the evidence for the precipitating or special exciting causes which included:

> the pressure of bereavement, business and family cares, parturition and abortion, sexual excesses, the abuse of stimulants and narcotics, and civilized starvation such as is sometimes observed even among the wealthy order of society, and sudden retirement from business. (op. cit.)

In a somewhat obscure passage, Beard foreshadowed his later and better known thesis that the disorder had a social basis:

> From statistics that I compiled and arranged a few years since, it appears that the expectation of human life or average longevity has at no time been

> greater than in the present century; that in no country is it so favourable as in our own, and that no class, on the whole, live longer than our leading brain workers, who are, of course, peculiarly liable to be affected with chronic neurasthenia. (op. cit.)

In short, by increasing life expectancy, improved social conditions increased the prevalence of neurasthenia.

Beard (1880, 1881) later announced significant alterations to his theory. By then the number of symptoms had grown enormously; even to list them in his Table of Contents took more than a page (Beard, 1881). More importantly, Beard placed much greater emphasis on the over-expenditure of the 'vital force', or nerve force, than upon changes in the nervous system, and the loss of phosphorus and other solids was barely mentioned. He assumed there was an hereditarily determined limit to the quantity of nerve force. The special exciting causes reduced even further an initially limited supply of nerve force already dangerously depleted by the pace of modern living. Neurasthenia was an *American* nervousness. American society was so advanced, technologically and socially, that the expenditure of nerve force required to adapt to it was beyond the capacity of many to restore. Any excess in life - for example, too much work, too much worry, too much alcohol, or too frequent sexual intercourse - added to the difficulties of adapting.

Treatment

Beard's treatment followed from his inferences about the underlying causal process. The nervous system had to be supplied with tonics that would replace the lost 'solids' or otherwise reverse the changes. The tonics included:

> air, sunlight, water, food, rest, diversion, muscular exercise, and the internal administration of those remedies, such as strychnine, phosphorus, arsenic, & etc., which directly affect the central nervous system. (op. cit.)

The application of mild electric current to the whole body, especially to the head and spine - what Beard called "general electrization" - had a special place in his treatment:

> it increases the appetite, promotes sleep, and develops the size and weight of the muscles - thus preparing the way for the *digestion of food*, which is itself one of the very best of tonics; for *rest*, which is really food for the nerves; for muscular exercise, which, in its turn, prepares the way for air and sunlight.
>
> In this capacity of general electrization for marshalling to its aid other tonic influences, lies, I think, the secret of its power. (op. cit.)

Beard believed that electrization might also "directly improve the quantity and quality of the vital force, in accordance with the theory of the correlation and conservation of forces". (op. cit.)

Beard's assumptions

Rosenberg (1962) has made explicit the three central ideas upon which Beard's explanatory theory rested: the law of conservation of energy, the reflex theory of illness, and the electrical nature of the nervous impulse. Like physical energy, nervous energy ('nerve force') was limited both in amount and distribution; when used in one activity little or none was available for another. According to the reflex theory of disease, reflex connections between different parts of the body enabled disease or malfunction originating at one bodily site to have pathological effects at another. The electrical nature of the nervous impulse provided a plausible analogy between the individual's store of nerve force and the limited capacity of a battery. The electrical analogy in turn justified the use of "general electrization" in treatment for, when it was allied with the law of conservation of energy, it could be supposed to restore the nerve force in much the same way that the charge of a battery could be restored.

Rosenberg may be slightly incorrect in attributing such a definite recharging notion to Beard. It is true that in the early version of Beard's and Rockwell's (1867) *The Medical Use of Electricity* and in Rockwell's edition of Beard's (1894) *Practical Treatise*, the discussion of the effects of electrization is reasonably consistent with this attribution. Nevertheless, in the last edition of Beard's and Rockwell's (1891) *The Medical and Surgical Uses of Electricity*, also edited by Rockwell, there seems to be a repudiation of the recharging analogy in that the question is raised as to whether general electrization "has any direct influence on the quantity or quality of the nervous force" (Beard and Rockwell, 1891, pp.217-225. Cf. Beard and Rockwell, 1867, pp.16-17; Beard, 1894, pp.203-204).

Beard's (1884) analysis of sexual problems in his *Sexual Neurasthenia* aptly illustrates the use to which he put these ideas. For him, 'sexual neurasthenia' included sexual problems of all kinds: impotence, premature ejaculation, frigidity, uncontrolled sexual drive, and so on. Beard suggested that excessive expenditure of nerve force at any one of the three main centres of reflex activity - the brain, the digestive system, and the sexual system - would deplete the amount of nerve force available at the other two. Sexual neurasthenia could therefore be caused by excesses of a non-sexual character as well as by sexual excesses themselves. Conversely the general or non-sexual form could be caused by sexual excess. What symptoms were produced depended more upon the reflex connections evoked than upon the particular kind of excess.

Evidence for causes

Beard's evidence that excess caused either the general or the sexual kinds of neurasthenia was not very convincing. For the most part he simply described cases in which he believed the presumed causal factor was present. In few instances did he attempt an analysis of the conditions

under which the symptoms had first appeared or give an account of how they had then developed. Nowhere did he essay a causal analysis in which the sufficient conditions were differentiated from the necessary. At the very most, Beard's evidence allowed only the conclusion that neurasthenia *might* develop in response to stresses of various kinds. And, when he claimed "hereditary descent" to be present, he did not go beyond enumerating other members of the family with the tendency. He was, of course, quite unable to express how strongly it was present in any particular case.

The central mechanism proposed by Beard is also open to objection. If neurasthenia was due to a loss of nerve force what was that force? How was it produced? What was involved in its utilisation and destruction? The later theory makes scant mention of these matters, and the earlier theory is not much more illuminating. There the functioning of the nervous system was said to depend upon the "quantity and quality" of the chemical constituents of the brain. While that might have been true, the theory gave no account of how the constituents were related to "nerve force" or how the "force" was used by the nervous system. Similarly, while it might have been true that the loss of constituents caused malfunction, the theory failed to say how the creation of nerve force was thereby impeded. Beard's concept of nerve force had only pseudo-physiological referents and the mechanism he proposed was an empty one. Nothing could be said about a basis for neurasthenia in disturbed nervous system functioning.

Beard's social theorising also rested upon very few facts. There were no data demonstrating neurasthenia to be more prevalent in America than elsewhere; Beard simply asserted it to be so. He assumed the distinguishing feature of American society was its technology and simply claimed it was twenty five years ahead in steam power, the press, the telegraph, the sciences, and the mental activity of women (Beard, 1881, p.vi). When examined, these indices of superiority have the same arbitrariness as the earlier lists of causes and remedies; whatever might have been meant by the 'mental activity of women', for example, it is obviously different from that indexed by steam power or the telegraph. The social theory was based on little more than unsupported assertions, and no detailed account was given of how social pressures caused neurasthenia. At every point Beard simply inferred causal connections from mere conjunctions.

The odd nationalism of the theory, together with the fact that so many patients did complain of the symptoms, probably explains the amount of generally favourable attention given it in the United States. However, there were some critics. Despite Beard's deserved eminence in neurological and psychiatric circles, some of his colleagues referred to him as a kind of Barnum of American medicine. Spitzka, a neuroanatomist of considerable standing, opined of *American Nervousness* that it was "not worth the ink with which it is printed, much less the paper on which this was done" (cited in Rosenberg, 1962). Although the disorder became a medical fashion for a time, its importance gradually declined and it is nowadays not

thought to be of any great interest. Some years ago, in a series of papers devoted to a comprehensive evaluation of the concept of neurasthenia, few had a good word for it. Carlson (1970-71) called it an "archaic term", Chatel and Peele (1970-71) estimated that the disease had been diagnosed in fewer than 50 of 102,000 admissions over a 114 year period at their hospital, Chrzanowski (1970-71) went so far as to entitle his contribution "An obsolete diagnosis", and Mora (1970-71), after noting that it had remained in the American Psychiatric Association's diagnostic guide until the 1951 revision, thought neurasthenia was best thought of as kind of precursor to modern conceptions of neurosis.

It is difficult to explain the demise of neurasthenia. Many psychiatrists, including Freud, recognised that the disorder was not confined to the United States - a fact which alone must have detracted somewhat from Beard's nationalistic explanatory schema. And even if we accept that there are now new terms for the symptoms originally described by Beard, or for sub-sets of them, none of the causal factors identified by Beard are peculiarly associated with them. What do not appear among the reasons for the disappearance of discussions of neurasthenia from the medical literature are the facts that Beard had merely described instances of the disease, that he had not undertaken a proper causal analysis, that his central concept was a vacuous one, and that his theory could generate only pseudo-explanations. Until now, none of these aspects of Beard's conceptualisation seem to have been scrutinised.

FREUD'S NEURASTHENIA

Beard's description of the new clinical entity generated enormous interest. By the middle 1880's a very sizeable literature of several hundred books and papers had accumulated (Levin, 1978, p.128), especially in German, including translations of Beard's own works (Morton, 1883; Beard, 1884 pp.20-22). According to López Piñero, the "decisive period in the evolution of neurasthenia" was ushered in by Charcot, mainly in his *Leçons du Mardi*. Using the same approach as he had in studying hysteria, Charcot "legitimized neurasthenia as a major neurosis, comparable only to hysteria" (López Piñero, 1963/1983, p.74).

We can be certain Freud knew of Charcot's work on neurasthenia, but we cannot date either the time he became aware of it or when he translated that part of the *Leçons* to which López Piñero refers in which it was set out. Freud at least knew of Charcot's opinions of Beard's work as well as of Beard's work itself by the middle 1880's, since Charcot cited one of Beard's publications and discussed neurasthenia in the *Lectures* Freud translated in 1886 (Charcot, 1886/1887, p.218). In that same period, Freud became familiar with the condition itself. In an 1887 review he referred to neurasthenia as "the commonest of all the diseases in our society" (Freud, 1887), and a few years later remarked he had "come across [it] repeatedly

every year in my medical practice'' (Freud, 1892-1893, p.118).

Beard's influence on Freud

Although Freud clearly knew of Beard's work, the evidence that he was directly influenced by it is rather meagre, amounting to his accepting some of the incidental opinions expressed by Beard in *Sexual Neurasthenia* antedating Freud's own. Of this evidence the strongest is Freud's repetition of Beard's assertion that agoraphobia had a sexual cause. Beard had speculated that agoraphobia might be under the influence of the genital system (Beard, 1884, p.189), and what is probably Freud's earliest comment on agoraphobia echoes this completely: ''The more frequent cause of agoraphobia as well as of most other phobias lies ... in abnormalities of sexual life'' (Freud, 1892-1894, p.139). Positing this not immediately obvious relation between sexuality and phobia is the only evidence of any direct influence of Beard on Freud.

It is more likely that Beard's claim of a relation between sexual factors and neurasthenia provided the starting point for Freud's own theses than that his work had a direct influence. When discussing ''excessively frequent'' seminal emissions, Beard said they ''may be both results and causes of disease, indicating an abnormal, usually an exhausted state of the nervous system, and in turn reacting on the nervous system, increasing the very exhaustion that causes it.'' He then added:

> Chronic neurasthenia is often accompanied, as one of its symptoms, by seminal emissions .... In almost all cases of long-standing nervous exhaustion, the reproductive system necessarily participates, sooner or later, either as cause or effect, or both. In very many cases, local disease consequent on abuse of these parts is a prominent exciting cause of general nervousness. (Beard, 1894, pp.100-101)

Although Beard cited four cases of general neurasthenia and one case (out of seven) of sexual neurasthenia, none of which had an obvious sexual basis, in support of this opinion (Beard and Rockwell, 1891, pp.426-430, 584-591), Freud was to assert that, even in cases such as these, the causes were in the patient's sexual life.

Symptoms

Which symptoms Freud first included in neurasthenia is now difficult to establish. Between late 1887 and late 1894 he mentioned the following symptoms in various letters and drafts to Fliess: feelings of tiredness, weakness, and sadness; attacks of giddiness and weakness, paraesthesias, sensations of pressure on the head and of drawing or pressing on the muscles; loss of appetite and weight, attacks of indigestion, dyspepsia and constipation; neuralgias of all types; loss of male potency (Masson, 1985, Letter of 24.11.87 and Draft B of 8.2.93). In his first paper devoted to the condition, Freud argued the time had come to restrict the term to such ''typical symptoms'' as intracranial pressure, spinal irritation, and dys-

pepsia with flatulence and constipation (Freud, 1895a). He did not mention fatigue and sexual weakness until a later paper (Freud, 1896a, p.150).

What Freud meant by intracranial pressure and spinal irritation, especially the first, is not clear to the modern reader. López Piñero's (1963/1983) discussion does not clarify the matter: in the nineteenth century the term had many different meanings. Beard's (1894, p.80) last description of spinal irritation was that it was a "tenderness either of the whole length of the spine, or, more likely, at certain points, as the nape of the neck, and between the shoulder-blades, and on the middle lumbar vertebrae". He added that "crawling, creeping, and burning sensations often accompany this tenderness". Ferenczi (1926/1955b), however, described it as "a condition varying in degree between an unpleasant sensation and a violent pain in the back".

As he described it, Freud included fewer symptoms under neurasthenia than Beard. However, Freud presented no arguments for the selection he had made nor for his characterisation of them as typical. And, unlike Arndt (1892b) for example, who had attempted a logical derivation of the symptoms from a supposed "fatigued or degenerating nerve", Freud made no attempt to account for either the origin of the symptoms or for their range.

Causes

The most obvious difference between Beard's and Freud's approach to neurasthenia is the etiological role they gave to sexual factors. For Beard, sexual excess was only one of a number of factors that produced neurasthenia whereas for Freud it was the only one.

We can be fairly certain Freud arrived at his view between the middle and end of 1892. During 1887 and 1888, in several letters to Fliess, Freud mentioned a case of neurasthenia in which the sexual agent he later proposed as cause was present but without paying any particular attention to it (Masson, 1985, Letters of 24.11.1887, 28.12.87, 4.2.1888, and 28.5.88). Further, in the paper on hysterical counter-will, probably written near the end of 1892, when discussing whether his patient's hereditary disposition was indexed by her brother's neurasthenia, Freud (1892-1893) wrote that he was "not certain whether it is not possible not to *acquire* this form of neurasthenia" without, again, laying stress on the sexual factors he mentioned in the brother's history. A schematic outline, probably sent to Fliess at the end of 1892, contains the first proposal of a sexual etiology for neurasthenia (Masson, 1985, Draft A, possibly of December, 1892). A footnote to his translation of Charcot's *Tuesday Lectures* does make the same point but, although it may be earlier than the draft, it cannot be dated with any precision (Freud, 1892-94, p.142, Note to Charcot's p.399).

Freud's very early remarks do not specify the nature of the sexual factor. Abnormal gratification (e.g. masturbation), inhibition of the sexual

function (e.g. *coitus interruptus*), "affects accompanying these practices", and early sexual trauma all seem to have been thought of as contributing equally (Masson, 1985, Draft A, possibly of December, 1892). By early 1893, Freud had narrowed the field. He argued that singly or jointly masturbation and incomplete coitus always produced neurasthenia. In males the two factors were typically held to operate at different periods:

> *Neurasthenia* in males is acquired at puberty and becomes manifest when the man is in his twenties. Its source is masturbation ....
>
> The second noxa, which affects men at a later age, makes its impact on a nervous system which is either intact or which has been predisposed ... through masturbation .... This ... is *onanismus conjugalis* - incomplete intercourse in order to prevent conception. (Masson, 1985, Draft B, February 1893)

Freud said that the various forms of incomplete coitus varied in severity: *coitus interruptus* was considered "the main noxa", being able to produce its effects even in "an individual who is not predisposed". Differing severities of incomplete coitus, combining with differing degrees of innate or acquired predisposition, resulted in different latencies with which neurasthenia was produced. Masturbation in the female produced neurasthenia in the same way as in the male but it might also be a consequence of sexual relations with a neurasthenic male. How this consequence resulted was not specified, although Freud implied that the incomplete coitus practiced by the male was responsible (Draft B of 8.2.93).

Freud adduced four observations as evidence for his contentions. First, he claimed it was "a recognized fact" that neurasthenia was "a frequent consequence" of an abnormal sexual life. His view simply extended the scope of that factor. Second, the frequency of male neurasthenia matched the frequency of masturbation in men. Third, observations among his acquaintances proved to him that men "who have been seduced by women at an early age have escaped neurasthenia". Finally, he claimed "that the sexual neurasthenic is always a general neurasthenic at the same time".

None of this is very strong evidence. The "recognized fact" is buttressed by the editors of the Fliess papers - but not by those of the *Standard Edition* nor by Masson - with a footnote of their own to a paper by Peyer (incorrectly given as Preyer) dealing only with the *sexual* form of neurasthenia (Levin, 1978, p.132). Freud's own two references to Peyer show him to have judged Peyer's work as barely relevant to the matter of the sexual etiology of the neuroses (Masson, 1985, Draft C of February or May 1893; Freud, 1895a, p.98). Masson adds to the confusion by incorrectly implicating another totally irrelevant Preyer, Wilhelm Thierry Preyer, the author of *Die Seele des Kindes*, in his notes to Freud's next draft, Draft C, written between February and May of 1893. The only author with a name like this relevant to what Freud had to say about sexual

factors *in the actual neuroses* has to be Alexander Peyer who wrote on *Congresses interruptus and onanismus conjugalis* as causes of sexual neurasthenia (Levin, 1978, p.132 and n.35). Where Freud does cite Masson's candidate, Wilhelm Thierry Preyer, it is as one of a group of writers on child psychology who, had *nothing* to say about "the erotic life of children" (Freud, 1905b, p.173, n.2).

If Beard's case summaries are typical, it is not at all obvious that sexual factors were frequently associated with the general form of neurasthenia. As he said, well before Freud made his bid:

> In regard to the relation of neurasthenia to the genital function ... two errors have prevailed: that the genital organs have nothing to do with the causation of neurasthenia and allied affections, and that they are the exclusive causes ...
>
> Without dispute ... there are some cases ... that depend entirely on genital irritation ... and entirely recover with the removal of the irritation; there are others that depend in part on irritation from this source; there are others that arise independently of all irritation of that kind. (Beard, 1880, pp.127-128)

The medical literature of the period is also best summarised as supporting the view that what Beard termed the general form of neurasthenia was only sometimes and not even frequently a consequence of an abnormal sexual life (Erb, 1878, cited in Levin, 1978, p.129; Arndt, 1892b). The situation was rather different for sexual neurasthenia in which, not surprisingly, masturbation and *coitus interruptus* were very frequently implicated. Levin, for example, cites Krafft-Ebing and Loewenfeld in addition to Peyer as supporting this conclusion (Levin, 1978, pp.130-132).

As to the relation between the prevalence of neurasthenia and masturbation, Freud seems to have been implying that the different frequencies of male and female neurasthenia matched the differences in male and female masturbatory habits. But, in Freud's time, there were no valid prevalence figures for masturbation in general and it was almost certainly underestimated in females. With masturbation among females far more common than Freud assumed, neurasthenia should have been very much more common among them.

Neither are the casual observations by Freud of his acquaintances of much significance, although such data from more extensive enquiries would have been extremely important. Freud did propose collecting one hundred cases of male and female neurasthenics for study (Masson, 1985, Draft B of 8.2.93), but this collection seems not to have been made. While his subsequent letters and papers report several cases of the related disorder of anxiety neurosis, some of which seem to be classified and numbered, neurasthenic cases were mentioned only infrequently and never numbered.

Finally, if, as may be supposed, under sexual neurasthenia Freud

included such symptoms as impotence or premature ejaculation, it was simply not true these were always found with the symptoms of general neurasthenia.

FREUD'S ANXIETY NEUROSIS

During the same period as he was codifying the symptoms he thought to be typical of neurasthenia, Freud proposed a subset of them should be recognised as forming an independent clinical entity. He suggested the term anxiety neurosis for them and first appears to have used that name, late in 1892 or early 1893 (Freud, 1895a. Cf. Masson, 1985, Draft A, possibly of December, 1892).

Symptoms

By the beginning of 1893, Freud recognised three distinct forms of anxiety neurosis: anxiety attacks, the symptoms of which he did not describe further; the chronic state of anxiety, in which hypochondriasis, phobias relating to the body, and anxiety relating to decisions and memory were the defining symptoms; and periodic depression, the symptoms of which were like those of melancholia. He began a collection of cases from which he eventually arrived at the following diagnostic criteria: general irritability, with especially marked auditory sensitivity; anxious expectation, in which the patient interpreted everyday events as presaging personal calamity; the overwhelming feeling of anxiety, with or without such physical symptoms as sweating and trembling; fearful night waking; vertigo; phobias and obsessions; digestive system disorders such as vomiting or diarrhoea; and paraesthesias (Freud, 1895a).

Causes

Whereas Freud had initially associated neurasthenia with a variety of sexual factors, from the very beginning he seems to have linked anxiety neurosis with only one - what he called the inhibition of sexual function (Masson, 1985, Draft A, possibly of December, 1892). In February of 1893 he identified the inhibition as incomplete coition, most often coitus interruptus, a sexual practice he then assumed to produce neurasthenia also (op. cit., Draft B of 8.2.93). Freud's initial evidence implicating coitus interruptus is difficult to discern but it seems to have been the occurrence of the symptoms in conjunction with the sexual practice or just after it. Freud noticed, for example, that the number and/or intensity of attacks diminished during pregnancy, when normal intercourse was possible, only to rise again after pregnancy, when withdrawal or similar birth control practices were re-adopted (Freud, 1895a, pp.103-104).

The source of the anxiety

At first Freud seems also to have been as much concerned with identifying the source of the anxiety as with establishing the sexual practice with

which it was associated. Freud saw that the anxiety might be either a prolongation of anxiety over the conception the incomplete gratification had been designed to prevent or, somehow, a direct result of the incomplete gratification itself. By October, 1893 he had seen at least one case in which anxiety neurosis had developed in a patient who had not been at all worried about becoming pregnant (Masson, 1985, Letter of 6.10.93, Case 1) and he later said the same was true of others (op. cit., Draft E probably of 6.6.94). Freud concluded that anxiety about a possible pregnancy could not be the source of the anxiety in the attack. At the end of 1893 Freud also observed an anxiety neurosis in a "totally frigid" woman (op. cit., Letter of 27.11.93). Since she had no sensations during intercourse, that seemed to mean those sensations could not be the source of anxiety either. Having ruled out worries over the possibility of pregnancy and the sensations themselves, Freud concluded that the anxiety was *solely* a consequence of physiological aspects of the sexual act. By not being allowed a natural termination in orgasmic discharge, the *physical* excitation accompanying intercourse was diverted into the autonomic nervous system and the anxiety was nothing more than the physiological reactions produced by it. From that point of view, Freud regarded the symptoms as substitutes or surrogates for the physiological reactions that should have taken place during orgasm.

THE ACTUAL NEUROSES

About two years after his first paper on anxiety neurosis and neurasthenia Freud introduced the term "actual neurosis" to designate both conditions (Freud, 1898a, p.279). The name conveyed his belief that they had their origins in current or present-day sexual problems (German "aktuelle" = present-day) unlike psychoneuroses like hysteria, obsessions, and phobias which originated from sexual traumas in the relatively distant past. Because of their different origins, the treatments differed: the former required only some adjustment to the patient's current sexual life while the latter required repressed sexual memories to be rendered ineffective.

Freud's causal analysis

It seemed to Freud (1895b, pp.135-138) that there were four different kinds of factors, or as he termed them "causes", that acted together to produce the actual neuroses: precipitating causes, concurrent (or auxiliary) causes, pre-conditions, and specific causes. Precipitating causes were those that occurred last in the sequence of factors and immediately preceded the appearance of the neurosis. Concurrent causes were not present every time and by themselves were unable to produce the neurosis. Pre-conditions were those factors whose presence was necessary but which by themselves could not bring it about. Specific causes were present in every instance and required only the additional presence of the pre-conditions to produce the

neurosis. A specific cause could be distinguished from a pre-condition by being found only in the one disorder. For example, because it was the specific cause of neurasthenia, masturbation could not also cause anxiety neurosis or hysteria. Specific causes were to be further distinguished from pre-conditions in that the latter tended to be of a general nature, varying little from one neurosis to another, and stable in having only slight apparent effect until the specific cause acted.

Freud set out an "aetiological equation" or "aetiological formula" to represent the relation between the factors. In the equation or formula the several causes were regarded as "terms" that had to be satisfied if the neurosis, as the effect, were to occur. In those instances where the specific cause seemed not to produce a neurosis, Freud believed more effort had to be made to assess the contribution of the pre-conditions and the concurrent causes. Or, if all the causes were present they might not be at a sufficiently intense level; for example, a concurrent cause might be needed to potentiate the effect of the other two.

Applied to anxiety neurosis, for example, the etiological equation identified a libidinal weakness, usually of an hereditary kind, as the pre-condition. Emotional stress, physical illness, or others of what Freud called the "stock noxae", might be concurrent causes. However, anxiety neurosis would only come about if its specific cause of incomplete sexual gratification was also present. Depending upon which acted last, the illness could be precipitated by either the specific or the concurrent cause. Because these precipitating causes were defined only by the time at which they acted, Freud eventually dropped them as a separate class (Freud, 1896a, pp.146-149).

Freud could hardly parallel exactly Koch's requirement of producing disease through inoculation. The logical equivalent can be established, however, by actively searching for instances where the presumed cause is present but does not produce its effect. Only if systematic and exhaustive enquiries of non-neurotic individuals show the presumptive pre-conditions and specific causes to be absent from their histories and sexual practices does the presence of those factors in cases of neuroses allow them to be classed as sufficing for the neuroses.

Freud made no such enquiries. He argued instead that all that was required to confirm his causal hypotheses was for the appropriate specific sexual factor be present in the history of each patient (Freud, 1895b, pp.135-139). He represented himself as following established principles of medical logic. He noted, for example, that, while not everyone infected by the bacillus suspected of being the specific cause of tuberculosis actually developed the illness, that fact did not detract from its causal significance. The plausibility of these arguments conceals two very important weaknesses: what Freud describes as the sufficient conditions are only the necessary and the "aetiological equation" is pseudo-mathematical.

Freud said the pre-conditions and specific causes of the actual neuroses were "among the '*necessary causes*'" (his emphasis), but clearly believed that together they defined the sufficient conditions because he actually went on to say of the specific cause that it:

> *suffices*, if present in the required quantity or intensity, to achieve the effect, provided only that the preconditions are also fulfilled. (Freud, 1895b, p.136. My emphasis, MBM)

Accepting for the moment that Freud represented his case material accurately, all he had done was to show the presence of pre-condition and the specific sexual factor in each case. All that meant was that they were among the necessary conditions. Nowhere had he demonstrated that by *always* causing the neurosis the specific factor and the pre-conditions were the sufficient conditions.

THE METHODOLOGICAL PRECEPTS OF KOCH AND FREUD

Koch	Freud
Specific staining of micro-organism	Specific sexual practice
Specific micro-organism found only in the one disease	Specific practice found only in the one neurosis
Inoculation to produce the disease	Practice not looked for in the healthy ("control")

Figure 5.1 Freud's adaptation of Koch's postulates

Freud did not bother with an equivalent of Koch's inoculation condition by conducting a systematic search for cases in which the presumed cause might be present but its effects absent. That is, Freud did not examine normal subjects - 'controls' if you wish - to see whether the presumed cause was at work in them (Cf. Fig. 5.1). Freud held the specific cause of anxiety neurosis to be incomplete sexual gratification primarily because he claimed it was present in every instance. Naturally, nothing solely identified in this way could be a specific cause. Freud knew this was not the right way to establish the sufficient conditions of the actual neuroses. He actually proposed studying "Men and women who have remained healthy" (Masson, 1985, Draft A, possibly of December, 1892) and in Draft B of 8.2.93 he specified *non*-neurotics as a "necessary counterpart" or complement to his investigations (op. cit. My emphasis,

MBM), but there is no evidence of any such study being conducted. He seems to have contented himself with his casual observation:

> in the circle of one's acquaintances that ... those individuals who have been seduced by women at an early age have escaped neurasthenia. (ibid.)

Presumably what this meant was that Freud had never found masturbation among this group of non-neurasthenics. These observations appear to constitute Freud's only evidence having a bearing on whether the presumed causes might be present in the healthy. Without more of it, it mattered little how many cases of *neurosis* were collected - the information critical for determining the sufficient conditions was not to be found in their histories. Kris (Freud, 1950/1954, p.64, n.1) defends Freud by arguing that to conduct such a study "obviously would be impossible without collaborators". It is a very poor excuse. Although difficult for one person to conduct, the study was obviously absolutely necessary to Freud's thesis - and he knew it. We shall see in Chapter 8 that Freud had a collaborator in Felix Gattel who spent some months investigating his causal hypotheses in a setting where there was ready access to non-neurotics.

Freud had not argued, of course, that an actual neurosis was caused by a single factor. After all his etiological equation purported to show how the different classes of cause were related to one another. However, without procedures for identifying the factors or for measuring their strengths independently of each other, the relative contribution of each cannot be established. For example, suppose incomplete gratification had failed to produce anxiety neurosis. Any one of the following meanings can be attributed to that fact: the specific cause might not be intense enough, or the pre-conditions might be of insufficient intensity, or the concurrent cause might have failed to act sufficiently strongly. Because the strengths of the factors cannot be measured, there is no way of deciding between the various possibilities. And there are other problems. For example, where the pre-condition was an hereditary one, Freud could adduce only the fact of neurotic illness in the patient's relatives. However good an index of the presence of an hereditary predisposition such a fact might be, it cannot provide a measure of the *intensity* with which it operates. The "equation" is pseudo-mathematical in that it implies the factors can be measured when that is clearly not the case. Stewart, the only psycho-analytic writer to have considered Freud's formulation in a critical way, notes that:

> Although it involves an equation with a variety of theoretical variables, none of these variables can be quantified or even distinguished except on theoretical grounds: *each is independently variable and only subjectively and post hoc estimatable.* (Stewart, 1967, p.35. My emphasis, MBM)

As a contribution to clarifying the role of the presumed causal factors, Freud's analysis, for all its apparent sophistication, is not really different from Beard's simple enumeration.

Stewart also drew attention to similar deficiencies in Freud's more

general summary statement about the relations between the terms of the etiological equation. Freud had concluded:

> (1) Whether a neurotic illness *occurs at all* depends upon a quantitative factor - upon the total load on the nervous system as compared with the latter's capacity for resistance. Everything which can keep this quantitative factor below a certain threshold-value, or can bring it back to that level, has a therapeutic effect, since by so doing it keeps the aetiological equation unsatisfied.
>
> What is to be understood by the 'total load' and by the 'capacity for resistance' of the nervous system, could no doubt be more clearly explained on the basis of certain hypotheses regarding the function of the nerves.
>
> (2) What *dimensions* the neurosis attains depends in the first instance on the amount of the hereditary taint. Heredity acts like a multiplier introduced into an electric circuit, which increases the deviation of the needle many times over.
>
> (3) But what *form* the neurosis assumes - what direction the deviation takes - is solely determined by the specific aetiological factor arising from sexual life. (Freud, 1895b, pp.138-139)

Freud's own comments recognised the vagueness of the concepts of total load and resistance, but Stewart points out there is no way they could ever be quantified:

> Since none of the individual components of the load, or the total load, or the resistance, could be measured, the 'equation' was from this point of view *worthless*. (Stewart, 1967, p.36. My emphasis, MBM)

And, since Freud's "hypotheses" that might have given meaning to these concepts are nowhere set out or defended, Freud's summary version of his etiological argument was as worthless as the original.

In the light of what is actually required to establish the cause of a neurosis, the inadequacy of the *cessante causa cessat effectus* dictum is now apparent. In no field is the observation that modifying a phenomenon significantly by removing its presumed cause ever more than marginally relevant to the confirmation of the causal presumption. In particular, the history of placebo effects in medicine and psychopathology shows that symptoms of quite serious illness (and not just minor psychological irritations) can be removed by procedures having little if anything to do with their real causes (A. K. Shapiro, 1960; A. K. Shapiro and Morris, 1978; Grünbaum, 1984; Macmillan, 1986).

Establishing how a particular therapy brings about its effects requires an analysis at least as complex as that implied in Koch's methods of identifying causes. Breuer and Freud did not do this. Largely because the symptoms of hysteria were removed by abreaction, they ruled out

expectations and suggestion as the basis of their results and concluded they were due to catharsis (Breuer and Freud, 1893, p.7; 1895, pp.255-256). Freud seems to have done no more in evaluating his treatment of the actual neuroses. He seems to have believed that his advice to adopt more normal sexual practices was therapeutically effective and that that outcome went toward proving he had correctly identified the causes of the actual neuroses. For the most part, as we shall see in Chapter 8, Freud's followers did not believe in his causal scheme and they had nothing like the same degree of therapeutic success.

CONCLUSIONS

Despite the apparent sophistication of Freud's method of assessing the roles of the factors presumed to cause the actual neuroses, he no more stepped outside of the realm of case material than had Beard. Both used basically the same methods for identifying and evaluating causal factors, both committing the error of assuming that all that was necessary to identify a cause was to extract the features common to a group of similar cases. Neither investigated whether those features might be present where the neurosis was absent. Although Freud took a view of the importance of sexual factors different from Beard there was little in Beard's approach with which he was to disagree.

Freud had not established excessive masturbation and incomplete gratification as the specific causes of the actual neuroses and in Chapter 8 we shall see that he had not even established that they were among the necessary conditions. The real outcome of his work consisted of a misleading expectation that all neuroses might be caused by sexual factors.

Previously I argued Freud did not begin investigating the causes of hysteria systematically until late 1893. Two letters among those made public for the first time in Masson's edition of the Freud-Fliess correspondence confirm this dating. More importantly, they also show Freud began his work on hysteria by attempting to extend the sexual etiology he thought he had already established for the actual neuroses.

In the first letter, written in May, 1893 Freud said of his work on neurasthenia and anxiety neurosis:

> The neuroses are somewhat at a standstill, am working more on hysteria. (Masson, 1985. Letter of 15.5.93)

In September, nearly five months later, and using the term *sexualia* to refer to his hypothesis that neurasthenia and anxiety neurosis had sexual causes, he wrote:

> I happen to have very few new sexualia. I shall soon start tackling hysteria. (op. cit. Letter of 29.9.93)

Given this expectation and his other assumptions it was not surprising he found sexual factors at work in hysteria and, given the defects of his 'sexualia', it was also not surprising that he had again confused necessary with sufficient conditions.

By mid-1896 Freud concluded that hysteria was caused by sexual trauma in childhood. By that time too, the defective causal analysis was very much in evidence:

> It does not matter if many people experience ... sexual scenes without becoming hysterics, provided only that all the people who become hysterics have experienced scenes of that kind .... Not everyone who touches or comes near a smallpox patient develops smallpox; nevertheless infection from a smallpox patient is almost the only known aetiology of the disease. (Freud, 1896c, p.209)

In bringing Part I to an end, I do not think the wide ranging effects of this and the other of Freud's defective methodological precepts and assumptions we have examined require additional comment. We must expect to find that these various deficiencies have a profound impact on the first of Freud's independent attempts to account for hysteria as well as on his more comprehensive theory of the mental apparatus. It is the examination of those effects which makes up Part II.

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PART II

FIRST THEORIES AND APPLICATIONS

Chapters 6 to 10

6 Symptom Formation

7 A Theory of the Neuroses

8 Expectations and Neuroses

9 Dreams and Symptoms

10 A Theory of Sexuality

6 MECHANISMS OF SYMPTOM FORMATION

Erskine: If you had been sent to Cambridge to study science ... you would know that a hypothesis that explains everything is a certainty.

Wilde: *The Portrait of Mr. W. H.*

Part II consists of Chapters 6 to 10. In it I attempt three things: first, an evaluation of Freud's theoretical ideas as they stood at about the end of 1895; second, an examination of some of the difficulties that arose in 1896-1897 when Freud tried to apply them; third, an assessment of the modifications to his theory made in response to those difficulties and which he published between 1900 and 1905.

1895 is an important date because it marks the end of the first distinguishable phase of Freud's work. By that time Freud had extended the sexual etiology he first proposed for the actual neuroses to include the psychoneuroses, he had postulated mechanisms to explain both kinds of symptoms, and he had constructed his first explanatory theory.

Part II begins with an evaluation of the mechanisms by which Freud attempted to explain how symptoms form. Freud's first and more general theory of psychopathology of which they were a part is then considered. Within a few months of the end of 1896 Freud's applications of these theoretical ideas led to the hypothesis that hysteria was caused by perverse sexual seduction in childhood and I discuss the decision he eventually made that the seduction hypothesis was incorrect. The difficulties in Freud's explanations of the actual neuroses are also considered. Part II concludes with an outline and evaluation of the considerable alterations Freud then made to his theory and which resulted in a general psychological theory applicable to normal and abnormal behaviour. Although the differences between this much modified theory and the one considered at the beginning of Part II are quite marked, I believe certain defects in Freud's pre-1895 work were carried over into it.

I begin Part II by focusing in this Chapter on the status of the mechanisms Freud proposed for explaining how neurotic symptoms form. I compare his explanations with those of Charcot, Breuer, and Janet and try to make explicit the bases on which choices between explanations of these kinds should be made.

A REALITY FOR EXPLANATORY MECHANISMS?

Explanations of the relations between observable causes and observable effects are usually in terms of processes or entities that cannot be sensed in any direct way. Freud supposed, for example, that repression of incompatible ideas caused the symptoms of hysteria and the deflection of somatic sexual excitation anxiety neurosis. Repression and deflection are hypothetical processes which, unlike the idea, the excitation, or the symptom cannot be sensed or observed directly. The problem I consider here is whether and how these underlying processes or entities 'exist' or are 'real'. Clearly some confirmation of psycho-analytic theory would come from showing that repression or deflection, unsensed as they are, actually existed.

It can be argued that to ask about the reality of explanatory mechanisms is to ask erroneously: mechanisms or entities are postulated or hypothesised precisely because the reality is unknown. Once the real processes linking causes with effects become known, real knowledge replaces hypothesis. On this view to ask if there really are mechanisms of repression and deflection is to confuse an hypothetical process with the reality it tries to describe. Although there is no doubt about the appeal of this argument, there is also no doubt that it should be disregarded. Galileo and his Inquisitors both thought there was a difference between asking whether the earth's movement was only an hypothesis or if it really did move. So in our own time it seems make sense to enquire if there really are genes, quarks, or any of the other unsensed entities postulated in the modern sciences. Obviously an hypothetical mechanism or entity that refers to a real process is not itself that process but recognising its hypothetical nature does not preclude asking how well it depicts the underlying reality it reflects. [1]

HYPOTHETICAL AND OTHER CONSTRUCTS

During the 1940's there was a good deal of discussion of the status of the unsensed entities and processes, like repression, that were incorporated in psychological theories. Eventually MacCorquodale and Meehl (1948) admitted two into the corpus of psychological theorising: intervening variables and hypothetical constructs. Do either of these notions describe the mechanisms hypothesised to cause symptoms?

Reflecting the fashionable positivism of the day, intervening variables and hypothetical constructs were limited to relating things that could be directly observed. An intervening variable stood for a relation that was otherwise expressed in a quantitative empirical law. For MacCorquodale

1. The argument is strengthened rather than weakened by recent reports of the "visualisation" of genes, the DNA helix, and the atom.

and Meehl the "correctness" of an intervening variable was a function of the validity of the law expressing it. For example, in his studies of motivation and learning, Hull used the concept of *drive* to relate, among other things, observed performance on various learning tasks to a preceding condition of food deprivation (Hull, 1943, pp.57, 66). MacCorquodale's and Meehl's standards had it that drive was a "correct" intervening variable to the extent that there was a valid law expressing how quantitative variations in food intake were related to time taken to learn a task. In their usage, 'drive' simply became an economical expression for a set of relatively complex experimentally determined relations. They could remain good positivists and make no reference to the bogey of non-observable processes or entities.

On the other hand, an hypothetical construct was not reducible to a quantitative law and its truth could not therefore be indexed by it. Because hypothetical constructs seemed to express more than empirical relations MacCorquodale and Meehl said they had "surplus meaning". A character trait like anxiousness illustrates the point. The term 'anxiety' may be used to relate certain stressful conditions to sweating or heart rate acceleration even though empirical laws relating the conditions to the behaviours cannot be expressed precisely. But a trait like anxiousness is thought of as a more or less permanent disposition of the individual, existing apart from any particular empirical relation. 'Anxiousness' cannot be sensed in the same way as 'anxiety' and is therefore said to have surplus meaning.

It is clear that the various symptom-producing mechanisms are neither intervening variables nor hypothetical constructs. First, none expresses more than a relation between *apparent* observables. Typically they connect two unobservable psychological states or an unobservable state and a symptom. Charcot's mechanism of realisation, for example, relates an unobservable idea to an observed symptom while Freud's repression expresses the difference between the original affect laden *percept* of the traumatic situation and its *memory*. Second, in attempting to *explain* symptom formation the mechanisms do more than *express* a relation. Maze (1954) long ago demonstrated that intervening variables cannot have an explanatory role: the very production of an effect requires the variable to do more than express a relation with its cause. A third reason rules out symptom-producing mechanisms as intervening variables: quantitative laws do not apply. If the criteria proposed by MacCorquodale and Meehl for judging the truth or correctness of intervening variables and hypothetical constructs do not apply to the mechanisms supposed to produce symptoms, can our question be answered at all?

THEORETICAL TERMS

A starting point for an alternative to the MacCorquodale and Meehl view is provided by O'Neil's (1953) proposal that different hypothetical entities or

processes might be classed according to the ways properties or qualities are attributed to them, or, as he put it, the ways in which they are characterised. I would extend his proposal with the suggestion that it is those theoretical terms whose characteristics or attributed properties can be investigated that have the potential for referring to real processes. A term has a real referent when investigation shows those properties or qualities exist. [2]

O'Neil distinguished between *hypothetical relations* and *hypothetical terms*. By the former he meant concepts that expressed lawful relations between two classes of conditions, usually observable. O'Neil's 'hypothetical relation' was broadly equivalent to an 'intervening variable'. An hypothetical term, on the other hand, was a term in the logical sense of being either the subject or predicate of a logical proposition. It was also characterised by having various qualities or properties attributed to it and it was from these that explanations could be generated.

As an example, O'Neil discussed various ways in which an hypothetical term like 'memory trace' might be characterised. Were it assumed to be a relatively persisting mental image of a word that was not ordinarily conscious, qualities or characteristics would have been attributed to it asserting, as O'Neil put it, what the trace *was* as well as what it did. It was a "characterized" theoretical term (O'Neil, 1962, p.96). On the other hand, to say that a memory trace was a relatively persisting alteration of mind said nothing and O'Neil proposed calling such terms "uncharacterized". They had only the virtue of pointing to what had yet to be explained. An uncharacterised entity or process told one only what the entity or process *did*, not what it was.

For my purposes, O'Neil's proposal requires extending. A more adequate description of characterisation is needed and a method for judging the correctness of a given characterisation is required. The first step in characterisation is to attribute properties to the term which logically entail the facts expressed in the relation. Characterising memory traces as mental images of words, for example, attributes all the properties of word images to memory traces. The ways memory traces change over time, are consolidated or forgotten, interfere with one another, and so on are explained by whatever produces changes in the images of words. Once logically suitable qualities like these have been proposed, the question of the correctness of the characterisation reduces to the questions of whether memory traces do indeed resemble images of words and whether the facts to be explained can still be arrived at from the qualities the traces are shown to possess. The process to which the hypothetical term refers exists to the extent that the characteristics exist. Characterisation thus provides a basis for establishing the reality of proposed explanatory mechanisms.

2. I had thought my extension of O'Neil's proposal to be unique, Williams (1989) has recently described Ampère's understanding of theoretical entities in a very similar way. The view attributed to Ampère does not have to derive from Kant or be tied to a religious view of the world.

Ellman and Moskowitz (1980) note a difficulty in the requirement that an independently characterised term has to enter into explanatory statements about at least two qualitatively different phenomena. They ask, what constitutes a *qualitative* difference? Ellman and Moskowitz are among the very few authors I have found in the psychological literature, and the only ones in the psycho-analytic, to discuss the question of whether theories or the non-logical terms ('entities') they contain may be judged true or false, that is, as having real referents or not.

Sometimes simple observation confirms or disconfirms the reality of an hypothetical term. When this happens, the characterisation has usually been of a single property having the potential to be observed directly. An illustration is provided by Harvey's use of the theoretical term 'pore' in explaining the circulation of the blood. On the basis of his observations of the anatomy and functioning of the heart, lungs, and blood vessels Harvey concluded the heart pumped a fixed amount of blood through the body. He had then to assume an exchange took place between the arteries and veins and proposed this took place through minute openings, or pores, too small to be seen. A logical implication of Harvey's characterisation was that the pores would be visible through microscopes more powerful than those available in his own day. Some years after his death the small vessels were actually observed and the status of the pores changed. Postulated or assumed pores had been transformed into real capillaries; a theoretical term had become a fact.

Harvey's concept possessed the most essential attribute of a well-characterised theoretical term: genuine explanatory power. It allowed him to deduce how the heart could maintain the continuous circulation of a fixed quantity of blood. The explanation was also a genuine one, not the pseudo-explanation which would have resulted had Harvey postulated the existence of pores so small and subtle they could never be seen, not even by the most powerful of microscopes. Although in Harvey's day no observational test of the reality of the pores was possible there was nothing in principle to prevent it. 'In principle' testing of the factual consequences of the properties attributed to the term is the 'testability' of the term.

What I am arguing, therefore, is that the essential features of well-characterised terms - explanatory power and testability - derive from the hypotheses that can be generated from the properties attributed to them. Therefore the other features of well-formulated hypotheses considered by O'Neil (1962, Ch. 6) - consistency of assumptions about the properties, clarity of definition, a small number of assumptions, and the greatest compatibility with other known or assumed processes - should also hold for well-characterised terms. I turn to the importance of these additional requirements.

Consistency in the properties attributed to a theoretical term corresponds to consistency in the assumptions of an hypothesis. Obviously it is

quite essential because properties inconsistent with one another lead to pseudo-explanations and reduced or absent testability. This is true even of simple theoretical terms. For example, although Mesmer's fluid was said to be 'imponderable', it was also said to be disturbed by the movement of the planets. Phlogiston, a 'substance' once thought to be released during combustion, had negative weight attributed to it after it was found that elements burned in oxygen actually increased in weight.

In practice, the most useful hypotheses are those based on the most clearly defined assumptions, involve the smallest number of assumptions, and have the greatest compatibility with other known or assumed processes. Although these requirements are worth meeting, it is worth stressing that they are not logical necessities but practical guides. The need for clear definition of qualities might seem self-evident. However, concepts are often required to represent processes that are only partly or imperfectly understood. Under those circumstances the attribution of vaguely defined properties may be justified; all that can be asked is that definitions be as clear as the stage of investigation allows. The requirement of the smallest number of assumptions applies more properly to assumptions underlying explanations than to theoretical terms proper. It derives from the nominalist philosopher William of Ockham (or Occam) who argued that the fewer assumptions made in an explanation the better. [3]

Ockham used his principle to counter a then prevalent tendency for philosophers to propose unnecessary explanations for natural and supernatural events. The principle of parsimony, or Ockham's razor as it came to be called, is a useful guide even though it does not have the force of logical necessity. Similarly, the history of scientific concepts shows there have been many valid theoretical terms incompatible with other processes. The concept of dual consciousness used by the French psychopathologists was, as Binet noted, completely at variance with traditional associationist concepts (Binet, 1892, pp.269-270, 350-352). Freud's concept of an unconscious mind containing repressed ideas was similarly at variance with every one of the many other concepts of unconscious mental functioning then in vogue. In one sense, it must always be the case that new theoretical terms require the attribution of qualities of variance with those characterising other terms. Were it not so, progressively deeper understanding of reality would not be possible.

Theoretical terms should not be discarded just because they fail to meet these practical rules-of-thumb of compatibility, parsimony, and definitional clarity. Being logically necessary, explanatory power, testability, and consistency of assumptions are more important.

3. Ockham seems *not* to have formulated this principle as "entities are not to be multiplied without necessity".

HYSTERICAL SYMPTOMS

Having clarified the standards according to which theoretical terms ought to be evaluated, we may now examine how the mechanisms of hysterical symptom formation proposed by Charcot, Janet, Breuer, and Freud meet them.

Realisation

Charcot supposed that, because normal ego control was lacking during traumatic events, the sensations generated in them called up ideas that manifested themselves as symptoms. The properties he attributed to the mechanism of realisation were essentially those required by the theory of ideo-motor action. What marked the difference between normal and abnormal realisation was the kinds of ideas involved and the presence or absence of ego control. What explanatory power Charcot's mechanism of realisation possessed derived from the more general theory of which it appeared to be a special case.

The theory of ideo-motor action postulated that a voluntary movement was always preceded by the idea or mental image of the movement (James, 1890b, Vol. 2, pp.522-528). The evidence supposedly consistent with the theory was of very doubtful validity: James' summary of it shows it to be, in almost equal proportions, a mixture of speculation, dubious clinical observation, and introspection. When, just after the turn of the century, the experiments were conducted and the developmental observations co-ordinated, it became apparent that neither kinaesthetic nor other images were among the precursors of movement (Bair, 1901; Woodworth, 1903, 1906). Especially important for this conclusion were those investigations of subjects whose muscles were stimulated electrically to give them the kinaesthetic sensations involved in movements. They gained voluntary control only slightly faster than those not stimulated. Many of the diseases in which patients were unable to sense or control the position of their limbs were not adequately described and, if as appears to have been the case, a large proportion were hysterical, the clinical observations were of doubtful value.

Further, as Woodworth (1906) observed, the sensations giving information about "the present condition of the member about to be moved" were not at all the same thing as the kinaesthetic or sensory image of the impending movement. Indeed the two were frequently opposed. For example, in alternate flexion and extension of the forearm, the sensations of extension evoke the ensuing flexion, and vice-versa. The relevance of the clinical observations was thus based on a confusion between the roles attributed to sensations in maintaining and in initiating movement. Finally, the introspective argument assumed little more than that willing was based on the idea of the desired consequence and that the only effect of willing of which one became aware was an action. James himself believed the only reason

why the theory was not self-evidently true was that ideas did not invariably cause movements. When no movement resulted, James believed one could detect on the fringe of consciousness an idea of an inhibiting action, counterposed to the original intention (James, 1890b, Vol. 2, pp.525-527).

When Woodworth (1906) investigated the role of imagery directly he used subjects well trained in introspection who had varying types of imagery of varying strengths. He obtained only a minority of reports in which the imagery was related adequately to the subsequent movement and in nearly half of the reports there was no imagery at all. In his 1903 studies Woodworth had tried to isolate the extension and flexion of his great toe from the others. He noted that attempts to prevent the others from moving were "a good means of insuring that they did move". Many of Bair's subjects had similarly noted the ineffectiveness of the idea of the inhibiting action to control the primary act. Woodworth concluded:

> I infer from the results of Bair, combined with my own, that even in first getting control over a particular movement, at least in the case of adults, the kinesthetic image of that movement is neither a necessary nor a sufficient condition. (Woodworth, 1906)

Consequently, in Charcot's day, the evidence consistent with the theory of ideo-motor action was not very strong and the evidence gathered soon after was decidedly negative.

Charcot's adaptation of the theory of ideo-motor action involved yet another difficulty and a rather special one at that. He assumed the idea of the *absence* of a movement was equivalent to the idea of a movement:

> The idea of movement, in the course of being executed, is already movement; the idea of absence of movement, if strong, is already the realisation of motor-paralysis; all this is entirely in conformity with the laws of psychology. (Charcot and Marie, 1892)

However, the assumption that the idea of movement produces an action does not logically entail a paralysis as a consequence of the idea of an absence of movement. As a result, even if the theory of ideo-motor action had been acceptable, Charcot's adaptation of it was not.

Like the mechanism that transformed normal ideas into actions, the mechanism of realisation proposed by Charcot is also uncharacterised. It therefore lacks explanatory power, and because it has no properties or qualities of its own, it cannot be tested. Nevertheless several investigations do suggest themselves. Modifications of sensory input by chemically induced anaesthesias and paralyses ought to modify the experimental production of symptoms under hypnosis. Differing degrees of hypnotic trance ought also to be associated with differing degrees of realisation and with variations in the relation between emotion and bodily expression. Investigating realisation in these ways is not inconsistent with its uncharacterised status when it is recognised that the investigations do not

concern the properties of the mechanism *per se* but only the effect of altering the sensations the mechanism is supposed to transform.

What Charcot referred to as the absence of the normal control of the ego is also a completely uncharacterised process. While it is implied that absence of control is a state opposite to that in which the controlling ego is present, the state in which control is present is not defined at all. Quite simply, what role the ego normally plays in controlling the transformation of an idea into an act is unknown. So too is what happens when those controls are in abeyance. Consequently, to the extent that the concept of normal ego control lacks explanatory power, so does its opposite. Because Charcot does not always use it, there is also some difficulty about the centrality of the notion of the absence of ego control. For example, in the last quotation from Charcot we see he derived symptoms directly from the idea of the absence of action and not from any peculiarity in the ego's control. However, there is a number of other places in which the absence of control seems central. If it is not central, the lack of characterisation poses no problem in evaluating realisation; the weakness of the ideo-motor theory itself and the lack of characterisation within it of the mechanism of realisation become the bases for rejection. On the other hand, if absence of ego control is central, its own lack of characterisation adds further to the lack of explanatory power of realisation.

The assumption that an hereditary predisposition is necessary for hysteria provides one definite point at which Charcot's theory is open to test. However, heredity seems to have been given different emphases in different phases of Charcot's work. Even quite late he proposed heredity was only a dominant factor:

> The dominant idea for us in the aetiology of hysteria is, therefore (in the widest sense), that of hereditary predisposition; although some individuals seem to be hysterical from their birth by reason of direct heredity, the *greater number* ... are simply born susceptible. (Charcot and Marie, 1892. Emphasis altered, MBM)

Elsewhere, as for example in the role he gave the number of affected relatives in his discussion of Le Log__'s traumatic hysteria, Charcot seemed to imply the necessity of an hereditary predisposition in every case. Freud later objected that this kind of evidence often confused hereditary and non-hereditary diseases, that it did not exclude the possibility that the disease had nevertheless been acquired, and went on to claim that the predisposition was not present in a number of his own cases (Freud, 1896a). To the extent that the concept of predisposition was central to Charcot's explanations of hysteria, Breuer's and Freud's arguments and observations disproved them.

Finally, there were actually many inconsistencies between Charcot's theory and the evidence he took to support it. For example, while it was true that electrical stimulation of the facial muscles could produce the

bodily attitude appropriate to the 'emotion' so expressed, it failed to do so unless there were many repetitions of the stimulation (Charcot and Richer, 1883). Although Charcot and Richer interpreted the fact differently, presumably the real function of the repetitions was to give the subject the opportunity of divining the experimenter's intentions. Charcot's conclusion that realisation took place automatically, merely because the idea was called up by the muscle sense, was a rash conclusion for its own time and is not consistent with present knowledge of hypnosis.

Restriction of consciousness

According to Janet, a consequence of the stimulation of the sense organs was the production of corresponding elementary psychological processes in the mind which he called *sensations*. More than one modality was usually excited and a large number of different sensations generated. These sensations Janet regarded as subconscious phenomena "isolated, without intervention of the idea of personality" (Janet, P., 1892a. Cf. 1892d, p.36). Usually these elementary phenomena were *synthesized* into *perceptions* and that unity might be *assimilated* into the previously existing concept of personality. "It is only after this sort of *assimilation* that we can truly say 'I feel'" (ibid.). In order to distinguish this type of perception from the recognition of external objects, Janet proposed the term *personal perception* for it. Personal perception led to a more complete consciousness than the isolated elementary sensations could produce. But not all sensations actually present were necessarily assimilated by this act of personal perception. What Janet called *the extent of the field of consciousness* was simply not wide enough to allow all the sensations to be taken in. Ordinarily this deficiency was overcome by directing attention successively to different aspects of the stimulus. But suppose a permanent narrowing of the field. Sensations from one or other modality would not be assimilated. The patient:

> neglects to perceive the tactile and muscular sensations, thinking he can do without them .... One fine day the patient, for he has truly become one now, is examined by the physician. He pinches his left arm and asks him if he feels it, and the patient, to his great surprise, affirms that he can no longer, if I can put it this way, recall as part of his personal perception the sensations so long neglected: he has become *anaesthetic*. (Janet, P., 1892a. Cf. Janet, 1892d, pp.39-40)

The sudden development of symptoms and the development of symptoms other than anaesthesia could be readily accounted for. Janet proposed that traumatic situations produced a narrowing of consciousness such that all ideas occurring outside of it were cut off from the dominant consciousness. These split-off ideas then formed a second consciousness which manifested itself in the primary consciousness as the symptoms. The tendency to a narrowing of the field of consciousness was permanent. It was based upon a "psychical insufficiency", an hereditarily determined incapacity to attend to a wide enough range of stimuli.

Although a restriction of consciousness plausibly leads to defective synthesis and assimilation and to deficits that produce symptoms, the processes themselves are quite uncharacterised. While Charcot had tried to derive the mechanism of realisation from the widely accepted and very plausible theory of ideo-motor action, Janet's proposals had no such link. Although Janet's account of normal sensation and perception was quite novel, it rested on nothing more than his own powers of description. He cited no experimental, clinical, nor other findings. In synthesis and assimilation Janet offered two uncharacterised theoretical terms completely lacking in explanatory power. Symptoms resulting from abnormalities in the processes were similarly not explained.

In any case, what explanatory power restriction might have had was weakened by its dependence upon other poorly characterised concepts. Psychological weakness, or psychological insufficiency, was supposed to produce the narrowing of consciousness in which normal synthesis and assimilation failed. On the evidence for insufficiency, Freud noted about Emmy von N.:

> I must confess, too, that I can see no sign ... of the 'psychical inefficiency' to which Janet attributes the genesis of hysteria. According to him the hysterical disposition consists in an abnormal restriction of the field of consciousness (due to hereditary degeneracy) which results in a disregard of whole groups of ideas .... If this were so, what remains of the ego after the withdrawal of the hysterically-organized psychical groups would necessarily also be less efficient than a normal ego .... Janet, I think, has made the mistake here of promoting what are after-effects of changes in consciousness due to hysteria to the rank of primary determinants of hysteria .... in Frau von N. there was no sign of any such inefficiency. During the times of her worst states she was and remained capable of playing her part in the management of a large industrial business, of keeping a constant eye on the education of her children, of carrying on her correspondence with prominent people in the intellectual world - in short, of fulfilling her obligations well enough for the fact of her illness to remain concealed. (Breuer and Freud, 1895, p.104)

Leaving to one side his strange translation of Janet's term, Freud's representation of Janet's emphasis on heredity is disputed. According to Laplanche and Pontalis (1967/1973, p.194), Janet did not believe the insufficiency to be innate. Van der Hart and Horst (1986) accept that Janet's concept of a tendency to dissociate was a congenital one but argue that for him the development of a frank dissociative disorder like hysteria depended on an interaction of the tendency with the effects of inebriation, physical illness, and "vehement emotions".

Janet thought insufficiency was evidenced by such signs of physiological malfunctioning as variability in heart rate, blushing, and sweating, as well as by such psychological states as inability to concentrate, and feelings of depression and fatigue. He believed these signs varied with the patient's condition, being present when the symptoms were

exacerbated and absent when the patient was well. However, Janet produced little evidence of the signs being present prior to the onset of the illness. More important logically is the fact that the signs from which he inferred insufficiency were the same as those from which failures in assimilation and synthesis were inferred.

Even if the mechanism proposed by Janet were genuinely explanatory, it would be difficult to test. Under what circumstances does personal perception fail? Modifications to sensory input might lead to different symptoms but that would not differentiate restriction from realisation. While Janet gives a plausible account of the symptoms of hysteria, it is clear the mechanism he proposed is almost completely uncharacterised and generates only pseudo-explanations.

Hypnoid isolation

Breuer assumed the symptom was a manifestation of an idea that properly belonged to a secondary consciousness. When the secondary consciousness appeared, the ideas belonging to it came also. Some of these ideas would be symptoms. When the secondary consciousness manifested itself intermittently, like the occasional hallucinations of Anna O., the symptoms also appeared only intermittently. If the secondary consciousness co-existed with the primary state the symptom was present permanently, appearing to belong to the primary state. In either case the primary consciousness had no access to the ideas of the second state and no knowledge of the causes of the symptoms or how to control them.

The explanatory power of the mechanism of hypnoid isolation derived from the well-known properties of hypnotic states. Although hypnosis itself is not fully explicable even now, it was known then that the hypnotized subject's behaviour was marked by increased suggestibility, amnesia for the events of the hypnotic state, and the ability to carry out post-hypnotic suggestions. That symptoms might result from ideas arising in a special state, that they might exist apart from normal consciousness without the patient's awareness, and that they might continue to have effects long after the original experience could be derived from the hypnoid state once it was assumed, as Breuer did, that the hypnoid state had properties similar to those of the hypnotic.

What Breuer did not explain so successfully was how the hypnoid state, the bearer of the symptoms, was revived. Breuer described two very different kinds of revival. In the first, the hypnoid state reappeared as a consequence of a later experience having some similarity with one first experienced in the hypnoid state. For example, Anna O.'s hallucinations and paralysis first recurred as she reached, with arm outstretched, toward a bent stick resembling the hallucinatory snake she had tried to ward off the evening before (Breuer and Freud, 1895, pp.216-217). The appeal of this explanation is to associationism of an intuitive kind for, as we have already

noted, Binet (1892) pointed out that associationist psychology proper is quite unable to account for such connections between two different states of consciousness. The second kind of revival Breuer described was when the memories of the *condition seconde* became strong enough to appear as a totality, as a *double conscience* (Breuer and Freud, 1895, pp.42-43). The difficulty of this explanation is that 'strengthening' is not defined independently of the appearance of the secondary consciousness. Strengthening *is* the reappearance of the hypnoid state. At best, what is involved in strengthening can be understood by reference to similar cases of double consciousness and multiple personality, the coherence of whose secondary states had frequently been remarked upon in the literature. The gradual strengthening in Anna O.'s case was but another instance of this well-known fact.

Breuer explained how symptoms might be present permanently in an otherwise normal state by supposing the *condition seconde*, which contained the symptoms, was able to co-exist with the normal state (Breuer and Freud, 1895, p.217). Experimental work by Binet and the two Janet's had demonstrated how simultaneous communication with the two states of consciousness was possible. One might converse with the subject verbally on one topic while at the same time conducting enquiry into other matters through automatic writing. Neither mode of communication would influence the other. Typically the subject spoken to showed extreme puzzlement on seeing the written communication, (Janet, P., 1886; Janet, J., 1888; Binet, 1889). Although the two states of consciousness were separate, they must nevertheless have been active at the same time for the two communications to have been possible. Given those demonstrations, the appearance of the symptom in Anna O.'s normal state was no more inexplicable. This interpretation stands even if we allow, as Bernheim's demonstration suggests, that there is not an absolute lack of communication between the primary and secondary states. Nemiah (1974, 1985), who has advocated the revival of Janet's theory, and Hilgard (1977), who has actually revived dissociationist thinking, both suggest ways of resolving the apparent contradiction. Van der Hart and Braun (1986) have also made some comparisons with Hilgard's neo-dissociation theory.

Although Breuer proposed no methods by which the mechanism of hypnoid isolation might be tested, the virtual identity of the hypnoid and hypnotic states indicates that hypnotic experiments could be used to test it. The kinds of experiments Charcot conducted, and those like Luria's (1932) done since in inducing conflict under hypnosis, although not without their problems (Reyher, 1962; Sheehan, 1969), go some way to testing the value of Breuer's supposition.

The major weakness of Breuer's explanation is its inability to account for symptoms that are not reproductions of hypnoid experiences. Some of Anna O.'s symptoms, including the paralytic contractures of the left extremities and the paresis of the neck muscles, were of this kind. No real

explanation was proposed for them:

> they never came up in the hypnotic analyses and were not traced back to emotional or imaginative sources. I am therefore inclined to think that their appearance was not due to the same psychical process as was that of the other symptoms, but is to be attributed to a secondary extension of that unknown condition which constitutes the somatic foundation of hysterical phenomena. (Breuer and Freud, 1895, pp.44-45)

A similar problem exists where the symptom symbolises an experience, for example, where hysterical vomiting symbolises a moral disgust. While this mechanism had not been observed in Anna O., it was sufficiently common in other patients for Breuer and Freud to note it in the *Preliminary Communication* (op. cit., p.5). Something other than simple isolation is required to explain how an experience is transformed into a symbol.

We have already noted how Breuer's later insistence that only affect-laden ideas could produce symptoms is inconsistent with certain of the facts about Anna O. Given that the hypnoid origin of the symptom explained the isolation from normal consciousness, it also seems unnecessary. Anna O.'s *caprices* had no affect directly associated with them. Neither the *caprices* nor Breuer's general explanation limited the power to produce symptoms to affect-laden ideas. Breuer may have thought the role of abreaction in removing symptoms required this recognition of the role of emotion in their formation. The surmise that only affect-laden ideas caused symptoms may have been necessary to explain the results of treatment, but it is not at all necessary to explain symptom formation.

Counter-will and associative inaccessibility

Quite apart from whether the histories of hysterics always show the presence of hypnoid states, the mechanism of hypnoid isolation is of limited generality. Similar problems are found with the 'intermediate' mechanisms of counter-will and associative inaccessibility. The explanations deriving from both are limited to fairly circumscribed situations - exhaustion in the case of counter-will and excess affect for associative inaccessibility - and both depend on an undefined tendency to dissociation.

Counter-will poses another problem. The exhaustion has to be one capable of weakening the substrate of the intention without affecting (or perhaps not affecting as much) the substrate of the counter-intention. No process of *general* exhaustion can produce a *selective* emergence of some of the host of counter-intentions - the supposition defies all logic. Freud was forced to postulate the equally unsatisfactory notion of a partial or selective exhaustion. Later, in the *Studies on Hysteria*, where counter-will is treated as a kind of supplementary mechanism, Freud seems to make the general exhaustion assumption (Breuer and Freud, 1895, pp.91-93).

With associative inaccessibility there is also the half-voiced but very doubtful implication that excessive affect alone somehow prevented associations forming. The notion that strong affect prevented associations from forming was not unknown in the psychological literature of the period. Maudsley (1867, pp.119-120) had argued that by itself strong emotion prevented "the free course of varied associations". In Maudsley's view, strong emotion was a persisting tension of the energy of a nerve cell. And, "in proportion to the degree of persistent tension must be the retardation of, or hindrance to, the process of association". While it is not known if Freud was aware of this particular proposition of Maudsley's, he did know of the work in which it appeared, if only because it had been cited by Charcot (Charcot, 1887/1889, p.308, n.2). Janet certainly knew of it as he cited it later in *Les Obsessions et la Psychasthénie* (1903). But Maudsley cited no evidence nor advanced any argument to support the proposition. Two years later Freud asserted the more usual proposition, "In general ... the part played in association by an idea increases in proportion to the amount of its affect" (Breuer and Freud, 1895, pp.165) but, oddly enough, in the unpublished *Project* of about the same period, he appealed to a self-observation in support of his original notion (Freud, 1950/1954, Part II, Section 6).

Although the concept of an affectively charged memory linked in a subconscious association could explain the isolation of the symptom, a *condition seconde* had to be present or to form with it. In that respect, associative inaccessibility had no advantage over hypnoid isolation.

Repression

The mechanism of repression that Freud proposed began when an idea incompatible with the individual's normal standards presented itself to consciousness. It was then pushed out by a deliberate act of will and lost to consciousness when repression stripped the idea of its affect. In hysteria the affect was converted into a bodily symptom and in the obsessions and phobias it was attached to another idea.

As Breuer indicated, repression was difficult to understand:

> We cannot, it is true, understand how an idea can be deliberately repressed from consciousness. But we are perfectly familiar with the corresponding positive process, that of concentrating attention on an idea, and we are just as unable to say how we effect *that*. (Breuer and Freud, 1895, p.214)

Breuer's difficulty arises because repression is uncharacterised and only expresses a relation. We know what it *does* but we do not know what it *is*.

Patients initially recall nothing of the origin of their symptoms. After resistances have been overcome, memories of traumatic events are recovered, each with its quota of affect intact. From those recovered memories the original perception of the traumatic event is reconstructed. Repression is

an inference from the difference between the reconstructed, affect-laden percept and the presumed unconscious affectless ideas. The essence of repression lies in the detachment of the affect from the idea. As has been said before, it is more correct to say that that detachment *is* repression. What Freud does not tell us is how it comes about.

As I illustrated in Chapter 4, when Freud first described repression he placed considerable emphasis upon its connections with observable behaviour. For example, patients could describe the act of will, the symptom appeared afterwards, and disappeared after abreaction. As Freud's theory developed these links became less important - patients might be unaware of their acts of will or even that they had once had an unwanted idea. Similar simple observational links were lost when it was proposed that affects might have to summate or a symptom might not appear immediately after a given trauma. The change made it very difficult to say when repression had taken place, to specify the conditions under which it occurred, or even to know if it had taken place at all.

Repression is quite different from any of the other mechanisms. Charcot and Janet proposed that symptoms formed as a consequence of a deficiency in a normal process and Breuer invoked the similarity of hypnosis with hypnoid states. In contrast, and apart from its resemblance to everyday processes which were themselves not understood, repression did not resemble or derive from any other. Its explanatory power cannot be evaluated by evaluating anything else. This is not to say repression is not consistent with any other theory. Certain of the characteristics of the memory of the traumatic event are in fact consistent with association psychology. From that theory it might be expected an idea lacking affect is neither distinguishable from other ideas in consciousness nor able to form associations with them. Depriving an idea of its affect would then account for its unconsciousness and isolation. The affect itself, lacking normal pathways of discharge persists, rather than being forgotten in the ordinary manner, and is more vivid and forceful when recalled than in normally forgotten, non-repressed idea. Some of the qualities of the traumatic memory are thus consistent with the affect having been detached from its idea but that tells us little about whether repression really takes place.

Although repression is consistent with some associationist notions there is actually a fatal inconsistency. For an affect to be converted and for its idea to become unconscious the separation of the two has to be complete or near complete. However, for abreaction to take place, the idea has to be recovered with its affect still attached. Symptom formation thus requires repression to separate the idea from its feeling but symptom removal requires they remain attached. The inconsistency remains whether the affect is supposed to be converted into a permanent somatic innervation or displaced onto another idea. It is also most improbable the idea is stored as an unchanging, permanent trace (Loftus and Loftus, 1980). However, if it

is synthesised anew on each recollection, how is it, or how does it seem to be exactly like the original.

What the mechanism of repression does seem to possess is generality. A single principle seems to explain not only the different kind of hysterical symptoms but the symptoms of other psychoneuroses as well. But, in order to explain how they came about, Freud had to invoke dispositional concepts that were as uncharacterised as repression itself. He was completely vague about how the tendency to form a *condition seconde* manifested itself other than through hysterical symptoms. The capacity for conversion was also uncharacterised, a point clear to Storring in 1900 (cited in Decker, 1977, pp.237-238 and n.4). A similar dispositional concept, one for displacing affect, was proposed to explain obsessions. None had definable properties. Each 'capacity' or 'tendency' was a *post hoc* inference from the fact that different patients undergoing similar experiences developed different symptoms. The very generality of the mechanism of repression derives in part from the completely uncharacterised dispositions upon which Freud supposed it to operate.

THE SYMPTOMS OF THE ACTUAL NEUROSES

Freud derived his explanations of the symptoms of the actual neuroses from inadequacies or abnormalities in the discharge of somatic (i.e. physical) sexual excitation. He supposed somatic sexual excitation arising in the sexual organs ordinarily formed linkages with the person's ideas of sexuality - what Freud called the psychosexual group of ideas - thereby creating libido and initiating sexual activity. In the absence of normal avenues of discharge, the somatic sexual excitation was deflected from the psychosexual group of ideas into the autonomic nervous system to produce there the physiological concomitants of orgasm, such as accelerated breathing, heart palpitations, and sweating that were experienced as anxiety. In neurasthenia, although linkage took place, the threshold for discharging somatic sexual excitation was so low that it occurred too frequently. Neurasthenic symptoms, particularly the central symptom of weakness, were the result of the draining away of somatic sexual excitation.

The mechanisms have an attractive simplicity and plausibility. Each neurosis had its own cause: incomplete gratification caused anxiety neurosis and masturbation or spontaneous emission produced neurasthenia. The general irritability in anxiety neurosis, and its periodicity seemed adequately explained. Undischarged somatic excitation produced an irritability that waxed and waned with it. The characteristic circumstances under which anxiety neurosis developed also seemed to be explained: for example, in deliberate, prolonged, abstinence, the excitation simply built up to intolerable levels, finally exhibiting itself in the attack; in virginal anxiety, on the other hand, the linkage could not take place because the

psychosexual group of ideas were too immature; again, neurasthenia in the male caused anxiety in the female because the males were unable to provide complete gratification for their partners. The reported loss of libido was also accounted for. Excitation was so regularly deflected that the normal linkage could not be reinforced by the pleasurable sensations accompanying orgasm. Gradually the link was broken, the libido declined and eventually disappeared completely. The lowered threshold for discharge appeared to explain one of the more puzzling features of neurasthenia: the excessive frequency of masturbation and spontaneous emission. Too little somatic excitation had to build up before there was a demand for another discharge - an automatic tendency to repetition had been created.

However, for each of the actual neuroses the explanations of symptom formation are inadequate and contradictory. Freud seems only to have implied there was a parallel between normal post-intercourse tiredness and neurasthenic weakness and that the latter generated the former (Masson, 1985, Draft B of 28.2.93). That hint, rather than an analysis, gives the only indication of how the central symptom formed. While it might have been possible to sustain an argument of the type Ferenczi (1912/1952b) proposed much later - neurasthenic weakness as the cumulation of successively produced feelings of tiredness - the other symptoms are not accounted for. How could digestive system upsets or sensations of pressure on the head derive from post-orgasmic feelings of weakness or tiredness? Freud does not even hint at how such symptoms are produced. As a result, neither the major nor the minor symptoms of neurasthenia are explained.

Examination of the proposal that masturbation or spontaneous emission lowers the threshold for discharge of excitation reveals a similar explanatory hiatus (Masson, 1985, Drafts E, possibly of 6.6.94, and G, possibly of 7.1.95; Freud, 1895a, pp.108-109). Too frequent discharge was said to be the consequence of a lowered threshold but, unless it can be shown how the sexual factors cause the threshold to be lowered, the concept of a lowered threshold adds nothing to the explanation. Freud assumed that which required explanation. Finally, the claim that the symptoms of *both* neurasthenia and anxiety neurosis might result from *coitus interruptus* involves a major contradiction between the two explanatory mechanisms. In neurasthenia, little or no excitation was supposed to be available for discharge. How could any anxiety be produced? On the other hand, the abnormal discharge of excitation in anxiety neurosis did not predicate either too frequent discharge or a lowered threshold.

The major inadequacy of the theory of anxiety neurosis, and one Freud recognised, is that it is not apparent why anxiety, rather than some other emotion should be experienced. Freud supposed the internal, somatic excitation was perceived by the ego as an external threat. Anxiety was the emotion appropriate to a threat (Freud, 1895a, p.112). Because the sexual excitation is not perceived by the patient, the explanation requires the

somatic sexual excitation to be intense enough to register as a danger but not intense enough to become conscious when forming its linkage with the ideas located in the ego. While such a selective mechanism might be consistent with anxiety attacks during abstinence, when sexual ideas might not be being attended to, it is hardly consistent with the attention consciously directed to sexual ideas during *coitus interruptus*. The latency of the anxiety attack poses another problem. One would expect the attack to occur when the somatic sexual excitation was maximally intense, at intercourse or soon after, rather than forty eight hours later.

An important but frequently overlooked modification to the above explanation, made within months of its publication, may be construed as Freud's attempt to overcome the failure to find a source for the anxiety. Freud now proposed that anxiety arose from the deflection, not of somatic sexual excitation, but *psychical* sexual excitation, that is, of libido itself:

> In my short paper intended to introduce anxiety neurosis I put forward the formula that anxiety is always libido which has been deflected from its [normal] employment. (Freud, 1898a, p.268)

This is not the original formula at all, but it does overcome part of the difficulty of the earlier explanation. No bodily or somatic process has to be transformed into a psychological state; both the cause and its effect are psychological. But the gain is illusory: even graver problems are created. First, if the mental effects are now accounted for, the physical accompaniments are not. As will be seen later, Freud recognised this problem by supposing incomplete gratification led to the build up of toxic substances in the blood which then produced the physiological responses of anxiety. Second, as Oerlemans (1949) has noted, the mentalistic explanation blurs the distinction between anxiety neurosis and hysteria, perhaps making it impossible to distinguish between them. Compton goes further, arguing that once:

> the differentiation between psychic and somatic libido is, in effect, lost, the whole concept of neurasthenia and anxiety neurosis requires re-evaluation. Freud did not do so. (Compton, 1972a)

Additionally, the new explanation is achieved at the expense of the very observations that led Freud to propose a non-mental basis for the anxiety, namely, the cases of anxiety neuroses in the sexually anaesthetic. Perhaps it is not surprising that, in the paper reporting the modification, such cases were not mentioned at all and that, twenty years later, Freud said *coitus interruptus* plays "a far smaller part" in such instances (Freud, 1916-17, p.402). It is difficult to resist the conclusion that the problem caused Freud to evaluate his case material in a somewhat arbitrary way. To this observation we might add Compton's (1972a) claim that Freud's "inference about the anesthetic women is obviously incorrect, and without that inference there is nothing to require the assumption of anxiety without psychic mechanism."

Both mechanisms are deficient in explanatory power, in testability, and in the consistency of their assumptions. The central all-pervading weakness of neurasthenia cannot be derived from too frequent discharge of somatic sexual excitation. In anxiety neurosis either the physical or the mental symptoms may be derived, but not both. Requiring sexual excitation to be intense enough to be perceived as a threat is inconsistent with assuming it would not then be intense enough to form a linkage. It is difficult to see how the mechanisms might have been tested. Even were it the case that the production of somatic sexual excitation and libido could be established physiologically, the core concepts of a lowered threshold and deflection would not thereby be tested.

CONCLUSIONS

In the sense that none has the potential for reflecting real processes, there is little to choose between the different mechanisms Charcot, Breuer, Janet, and Freud proposed.

Hypnoid isolation has the greatest explanatory power, is the most readily testable and has greater consistency in its assumptions than the others but it is of extremely limited generality. Realisation also explains only a limited number of symptoms and those not very satisfactorily. Even had it been possible to characterise realisation via the theory of ideo-motor action there is the fatal inconsistency of it being based on the idea of an action being equivalent to the idea of no action. Restriction of consciousness involves no inconsistencies in assumptions but, being linked with an uncharacterised theory of perception, lacks testability and generates only pseudo-explanations. Repression resembles realisation and restriction of consciousness in being uncharacterised and to that extent also untestable and non-explanatory. Like realisation it is also based on an inconsistency: it requires the assumption that affect can be detached from an idea but somehow remain linked to it. While hypnoid isolation, counter-will, and associative inaccessibility can all be rejected because they explain such a limited number of cases there is no real basis for rational choice between the other mechanisms.

Charcot and Janet did assume the presence of an hereditary predisposition which Breuer and Freud demonstrated was not present in their own cases. While that demonstration led to the rejection of Charcot's and Janet's proposals it also rather illogically led to the acceptance of the equally defective mechanisms of repression and hypnoid isolation. What seems to have been overlooked is that the tendencies to the splitting of consciousness and the capacity for conversion required by Breuer and Freud were undefined predisposing factors having exactly the same logical status as an hereditary disposition.

Freud's descriptions of psychological forces did seem close to everyday experience and the characteristics of the recovered memories also seemed to be consistent with some of the tenets of association psychology. However, the fact that the explanations which repression seemed to give of a wider range of symptoms were really pseudo-explanations seems not to have been appreciated.

The mechanisms which Freud proposed for explaining the symptoms of the actual neuroses similarly lack testability and are based on inconsistent assumptions. With respect to explanatory power, there are differences between the actual and the psychoneuroses. While the symptoms of neurasthenia are not explained at all and the central symptom of the anxiety attack only partly so, repression 'explains' too much.

Repression is an uncharacterised theoretical term which has been substituted for the relation Freud wanted to explain. It tells us only what the repression is supposed to *do*, not what it *is* and has no potential for referring to real processes. Consequently, we should not be surprised to find that its main role has been to add to the difficulties of testing psychoanalytic theory.

7 A THEORY OF THE NEUROSES

... to calm the perturbations of the mind
and set the affections in right tune.

Milton: *The Reason of Church Government.*

In this Chapter I examine the theory of which the mechanisms considered in Chapter 6 are part. It is Breuer's and Freud's theory of the neuroses as it was in 1895 and I concentrate on the ability the theory gave Freud to explain the facts of neuroses.

Ordinarily, of course, we speak of facts and theories as opposites. A fact is something that can be seen, or at least agreed on, but a theory seems to refer to something less certain, often being equated with mere speculation. Usually a theory is cast in terms that do not refer to things in the visible world and about which ready agreement is not possible. Theories therefore tend to be dismissed as having little, if anything, to do with their more robust antipodean relations. Properly regarded, however, a theory exists in order to explain the facts. We develop theories in order to understand facts and to guide us in our relationship to them.

A theory may explain its facts in different ways. Arnoult (1972) brought together a number of threads from discussions that began in the last century and suggested that theories may be classified as abstract, reductionist, analogical, or metaphorical. A theory is *abstract* if it is expressed in terms of processes, things, or events that cannot be sensed directly. The most abstract theories are those expressed solely in mathematical and logical terms but, to the extent that any theory includes unsensed processes among its propositions, it has a degree of abstractness. Further, the manner in which a theory is expressed may be more or less congruent with its subject matter as, for example, when a psychological theory draws on such concepts as habit, attitude, and motive, or when a physical theory uses such notions as particle, mass, or acceleration. However, if a theory describes these notions at what is believed to be a more basic level Arnoult classifies it as *reductionist.* In this category he would include theories of perception or learning set out in neurophysiological terms, for example. Finally, if the theory bears only a figurative relation to the subject matter, it is *analogical* or *metaphorical.* An analogical theory retains some of the essential properties of the subject matter, especially the proportionality between different components, whereas a metaphorical theory does not. A description of the transfer of excitation within the nervous system as a moving current draws on an

electrical analogy, for it assumes the same direction, regulation, and intensity in the current flow as in the neural process itself. Freud's characterisation of counter-intentions as demons inhabiting an underworld is clearly metaphorical.

Breuer's and Freud's theory purports to be psychological but it is actually a mixture of the analogical and the reductionist. The apparently neurophysiological terms used in it to describe psychological phenomena are the conventional analogues of electrical processes used at that time. However, of greater import than the type of theory of which Breuer's and Freud's instantiates is the logical relation between the facts and the statements comprising the theory. In the course of the discussion in this Chapter, I will argue that only if the facts can be deduced from the fundamental statements of the theory can we say that they are explained by it. A logical connection must therefore exist between the assumptions and the facts. Without that link, and however it is expressed, the theory will lack explanatory power. This point is true for all domains of enquiry, holding equally for the mathematical, physical, biological, social, historical, and human sciences.

The central assumption of Breuer's and Freud's theory was that the nervous system was governed by a tendency to minimize its level of excitation. The essence of their explanations was that neuroses resulted from abnormalities in the disposal of surplus excitation. I begin this chapter by examining the nature and origins of Freud's ideas about excitation within the nervous system, how Freud thought the disposal of the surplus was brought about, and how he related these notions to his ideas about emotions. The theory itself is then outlined, its logical structure exposed and its status evaluated.

EXCITATION AND ITS REDUCTION

Freud's earliest theoretical remarks on hypnosis and hysteria in the Preface to his translation of Bernheim's *Suggestion* (Freud, 1888c) and in the contemporaneous contribution to Villaret's *Handwörterbuch* (Freud, 1888a) had it that both hypnosis and hysteria were based upon changes in the excitability of the nervous system. Fragmentary as these remarks are, they are worth examining because the notion of 'changes in excitability' blossomed into two central theoretical ideas: the concept of psychic energy and the principal of constancy.

In his Preface to Bernheim, Freud said Charcot's approach to hypnosis was based upon the supposition that:

> the mechanism of some at least of the manifestations of hypnotism is based upon physiological changes - that is, upon displacements of excitability in the nervous system, occurring without the participation of those parts of it which operate with consciousness. (Freud, 1888c, p.77)

Here Freud appears to have been referring to the changes in reflex action, the contractures produced by mechanical pressure and the other signs of neuromuscular excitability. But even the less florid responses to suggestion were also based on physiological processes. The blows Charcot had given his subjects were suggestions containing:

> an objective factor, independent of the physician's will, and they reveal a connection between various conditions of innervation or excitation in the nervous system. (op. cit., p.83)

In Villaret, Freud argued that the explanation of hysteria should be similarly based:

> wholly and entirely on physiological modifications of the nervous system and its essence should be expressed in a formula which took account of the conditions of excitability in the different parts of the nervous system. (Freud, 1888a, p.41

Freud specifically mentioned such mental symptoms as the strange associations of ideas, the inhibitions of the will, and the characteristic emotional expressiveness as needing physiological explanation. He emphasised that all the symptoms showed:

> *changes in the normal distribution over the nervous system of the stable amounts of excitation.* (op. cit., p.49)

Freud did not define what was meant by a 'normal distribution' or 'stable amount' of excitation his later discussion of the increased influence of psychological processes upon the physiological in hysteria gave a clue:

> hysterical patients work with a surplus of excitation in the nervous system - a surplus which manifests itself, now as an inhibitor, now as an irritant, and is displaced within the nervous system with great freedom. (op. cit., pp.49-50)

Although, as Levin (1978, p.65) says, Freud's concept of surplus is vague and broad (as is that of its distribution) it can be seen that Freud was thinking of normal psychological life being carried out by relatively fixed or stable amounts of excitation being apportioned between such functions as willing, forming associations, and expressing emotions. Given the embryonic quality of these ideas, however, Green (1977) is clearly mistaken in thinking that Freud was at this time using the very specific and theoretically more advanced concept of a "quota of affect". As I showed in Chapter 4, the formulation of that concept was inextricably bound up with Freud's completion of his paper on the organic and hysterical paralyses. In 1888, Freud spoke only about surplus excitation, not surplus affect, and claimed only that the surplus in the hysteric inhibited or exaggerated those functions. In any case, Green cites nothing to support his claim.

At the end of his Villaret contribution, Freud introduced the notion that the distribution of the surplus was controlled by ideas:

> we may say that hysteria is an anomaly of the nervous system which is based on a different distribution of excitations, probably accompanied by a surplus of stimuli in the organ of the mind. Its symptomatology shows that this surplus is distributed by means of conscious or unconscious ideas. Anything that alters the distribution of the excitations in the nervous system may cure hysterical disorders: such effects are in part of a physical and in part of a directly psychical nature. (Freud, 1888a, p.57)

In his Preface to Bernheim, Freud gave ideas a rather more complex role than the relatively simple one Charcot had given them. In analysing the action of the ideas used in inducing hypnosis, Freud began by supposing that closing the eyes led to sleep because eye closure was linked through connections inherent in the nervous system to the idea of sleep. Once the eyes were closed, sleep could occur only if there were present:

> changes in the excitability of the relevant portions of the brain, in the innervation of the vasomotor centres, etc. (Freud, 1888c, pp.83-84)

Excitation could be transmitted in both directions. The idea of sleep led to feelings of fatigue in the eye muscles and to changes in the excitability of the relevant parts of the brain. An hypnotic suggestion had its effects, then, because the idea suggested by the hypnotist produced alterations in excitability appropriate to it. The redistribution of excitation was thus effected through ideas and, as the phenomena of hypnosis and sleep showed, this was done unconsciously.

Three important notions of Freud's can be extracted from these remarks. First, psychological processes such as willing and the formation of associations could be described in physiological terms. Second, the manifestations of hypnosis and hysteria were based on changes in the distribution of excitation. Third, echoing Breuer and Charcot, hysterical symptoms could be conceptualised as due to a surplus of stimuli or excitation which was disposed of by unconscious ideas. I turn now to an examination of the details of these three notions and their sources.

Physiological theorising

A marked tendency to try to explain mental events in physiological terms can be discerned in the physiological and psychiatric literature of the second half of the nineteenth century. The particular form this tendency took in Germany, and which provided the basis for Freud's thought, had arisen in opposition to the vitalist philosophy of the eminent physiologist Johannes Müller. Helmholtz, du Bois-Reymond, Brücke, and Ludwig, who were Müller's most distinguished pupils, formulated a distinctly materialist approach to physiological phenomena that could hardly have contrasted more with the outlook of their master (Bernfeld, 1944; Cranefield, 1957, 1966). Müller believed there was no continuity between the inorganic and

the organic worlds: organic and inorganic substances differed in material composition, in the phenomena they exhibited, and in the forces upon which they depended (Galaty, 1974). The alternative position proposed by Müller's pupils was that there was continuity and that all living phenomena were to be explained by physical or chemical forces or, were that not possible, by forces "equal in dignity", that is, forces equally material (Du Bois-Reymond cited in Galaty, 1974. Cf. Bernfeld, 1944).

By the time Freud began the study of medicine, this new biophysics movement, as it has since been called by Cranefield (1966, n.5) in opposition to the misleading term "school of Helmholtz" coined by Bernfeld (1944), was well established. For some years Freud worked under Brücke, one of its co-founders, of whom he was to say some fifty years later that he "carried more weight with me than anyone else in my whole life" (Freud, 1926b, p.253). Brücke's field was the physiology of the nervous system and his explanation of mental events was the opposite of Müller's. For example, Müller had believed voluntary acts were the result of the "spontaneous and conscious direction of the nervous principle" to the appropriate part of the brain (Müller, cited in Amacher, 1965, p.17). Brücke, on the other hand, regarded all movement as due to reflexes; if consciousness was involved it was simply because the pathways conducting the nervous impulse went through the brain. As a causal agent, mind could be eliminated from consideration (op. cit., p.18). According to Amacher, Brücke himself devoted little time to psychological matters, regarding that topic as the province of his colleague, Meynert, the brain anatomist and psychiatrist. When Brücke did discuss mental events "he assumed that they were simultaneously paralleled by physical phenomena" (op. cit., p.16). Brücke's view, which he held in common with Meynert, allowed a process to be discussed partly in physiological and partly in psychological terms. Because he also worked with Meynert for some years, Freud was exposed to a double dose of this particular form of materialist philosophy. His own description of psychological processes in physiological terms is best seen as a direct influence of the biophysics movement (op. cit., pp.58-59).

The ways Brücke and Meynert conceived of reflexes and associations are especially important for understanding Freud's theory of hysteria. Brücke's major contribution had been to extend the reflex doctrine, then recently clarified by Marshall Hall's work on spinal reflexes and endorsed by Müller, to all nervous action (Müller, 1833-1840/1833-1842). Brücke accepted the then recent histological studies of the brain as showing that the nervous system consisted of only two elements: nerve centres and nerve fibres. Some fibres connected sensory receptors to the nerve centres and others linked the centres to the muscles. Reflex action was produced when nervous excitation in the receptors was transmitted via the fibres to the centres and from thence to the muscles. Because there were only the two kinds of elements in the system, Brücke argued that a reflex mode of functioning was the only one possible (Amacher, 1965).

Meynert added his notion of the cortical reflex to Brücke's schema in order to explain how new connections or associations were formed. Laycock (1845) and Sechenov (1863) had made similar proposals at about the same time and, while Meynert very probably knew of Sechenov's formulation, we do not know if he knew of Laycock's (Amacher, 1965, p.18, n.12). Brücke had followed Marshall Hall in supposing that most reflex elements were located below the level of the cortex. But, because direct electrical stimulation of certain areas of the cortex produced movements, Meynert went on to suppose that some permanent trace of the images of the sensations and perceptions involved in reflex movement had been laid down in the cortex. Projection bundles of white matter linked the cortical cells in which the traces were registered to the sub-cortical centres and to the receptor and motor systems. Electrical stimulation of the cortical cells revived the trace and excitation was transmitted to the muscles to initiate movement (Meynert, 1884/1885, p.144).

Meynert also supposed it was the association bundles in the white matter that connected the traces in the different cortical cells with one another (op. cit., p.150). In Meynert's opinion, these anatomical and physiological facts provided the whole basis for mental life. Images were brought into association with one another via the association bundles and those associations provided the basis for the formation of new concepts and for inductive reasoning itself, which Meynert, like Wundt, regarded as the "fundamental logical function". As early as 1865 Meynert claimed his analysis of the anatomical structure of the brain provided a material basis for traditional associationist psychology (op. cit., p.153).

More than an anatomical structure was required, of course. Some means of activating it was required. The transmission of excitation through the nervous system was thought to fill that need. In the late 1840's du Bois-Reymond had demonstrated that the propagation of the nervous impulse was accompanied by detectable electrical phenomena. Bernstein's investigations in the late 1860's showed it was most probable the nerve impulse was propagated by successive local depolarizations rather than by a direct transmission or transfer of physical energy. And when Helmholtz measured the velocity of the neural impulse in 1880 it turned out to be too slow for it to be an electric current. Nevertheless, excitation of the elements of the nervous system and the transmission of excitation within it tended to be thought of as analogous to electrical processes. Some kind of electricity was thought to energise the anatomical structures Brücke and Meynert regarded as fundamental to behaviour and to mental life.

Possibly because a unitary kind of excitation and a single mode of action provided little room for any vitalist principle, Brücke assumed there was only one type of excitation within the nervous system and that only reflexes were elicited as it was transmitted from receptor to effector. For Brücke, excitation was transmitted along preformed pathways, but Meynert used the same notion to account for the opening up of new ones. He

proposed that when two cortical cells were simultaneously excited the excitation was transmitted from one to the other, along the association fibres connecting them. For example, if an animal was simultaneously seen and heard, images were simultaneously registered in cells in the visual and auditory areas of the cortex. Subsequent excitation of either cell caused excitation from it to be transmitted to the other and re-excite it. Thus, when only the sound of the animal was heard, excitation was transmitted along the previous pathway from the auditory cells to the cells in the visual area where it revived and brought to consciousness the sight of the animal.

Meynert extended his physiological associationism to provide an alternative to Müller's vitalist explanation of voluntary movement. When the limbs were first moved, innervations from the muscles were registered cortically and associations were formed with other simultaneously present cortical images. At some later time, the excitation of the images re-excited the motor registrations. Excitation was then transmitted through the sub-cortical centres to the muscles (Meynert, 1884/1885, pp.153-161).

Meynert believed that he had provided a physiological foundation for memory, inductive thinking, association by simultaneity, and voluntary movements.

Excitability and hysteria

Benedikt's *Elektrotherapie* of 1868, Oppenheim's analysis of the role of ideas in hysteria (both cited in Levin, 1978, pp.48, 67), and Donkin's article in Tuke's *Dictionary* shows the approach of Brücke and Meynert to mental activity to be consistent with a well-established mode of theorising about hysteria, one that moved easily between psychological and physiological levels of descriptions. According to Donkin the cardinal mental characteristic of the hysteric was:

> an exaggerated self-consciousness dependent on undue prominence of feelings uncontrolled by intellect - that is to say, on the physical side, an undue preponderance of general widely diffused, undirected nervous discharges, and an undue lack of determination of such discharges into definite channels. (Donkin, 1892)

Like those of Brücke and Meynert, Donkin's particular suppositions were different from Freud's, but this mixture of the physiological and psychological was basic. In the hypnosis literature there was a similar tendency to physiological theorising. Even Bernheim, who advocated a psychological explanation of hypnosis, could discuss the concentration of attention as:

> the fixing of the nervous force upon the phenomenon, - the idea or image suggested, - is what appears to dominate. (Bernheim, 1887/1888a, p.153)

Freud's theorising was therefore grounded in a well-established tradition.

Freud also seems to have built upon some ideas Charcot had derived from the use of electricity in the treatment and diagnosis of nervous system disorders. From about the middle of the 1700's electricity seems to have been used for the treatment of hysteriform disorders and in about 1850, following du Bois-Reymond's investigations of the electrical nature of the nervous impulse, the method became quite popular. Electrical stimulation was recommended for the restoration of sensibility, for overcoming paralyses, and for reducing excitability (Arndt, 1892a). Although it seems very probable that observations of diagnostic value were made in the course of these treatments, it was Duchenne's experiments that provided electrical diagnosis with a systematic basis. In 1849 Duchenne (de Boulogne) distinguished between paralyses according to the response of the muscles to electrical stimulation. Hysterical paralyses were among those in which stimulation caused muscular contraction but in which sensation was either abolished or diminished (Stainbrook, 1948).

Charcot brought the observations of muscular function made by Duchenne, and the related therapeutic applications of electricity by Vigouroux (both at the Salpêtrière), together into a systematic method for exploring muscular and sensory function. He claimed to have coined the term "electro-diagnosis" for it (Charcot, 1875-1877/1877, p.30). He used the method for characterising the level and type of excitability of disease affected muscles and nerves, combining that information with the results of traditional neurological examination. For example, he said some permanent muscle contractures were due to continuous and above normal levels of stimulation coming from a lesion in the spinal centre normally producing muscle tone (Charcot, 1876-1880/1883, p.268). Charcot interpreted the symptoms of another type of spinal lesion as showing "a sort of inertia or stupor of the electrical elements of the nervous system" (Charcot, 1887/1889, p.28). Another example is his implication of a type of hyper-excitability of the cortical motor cells in a number of organically based conditions (op. cit., pp.37-38). Stable neurological symptoms were the result of stable, organically based alterations in the excitability of the nervous system. Hysterical symptoms could therefore be described as exhibiting their own stable patterns of changed excitability (op. cit., pp.87-89), and could also be presumed to be based upon stable changes in the excitability of the nervous system.

Freud himself had used electrical methods of treatment even before he visited Charcot's clinic (Bernfeld, 1951) and electro-diagnosis seems to have been used in the examination of his first independently studied case of hysteria (Freud, 1886b). But the influence of Charcot and the biophysics movement was more important to Freud's theorising about the means by which surplus excitation was disposed of by the nervous system.

Surplus excitation and its removal

Freud's notion that the hysteric attempted to dispose of a surplus of excitation had its basis in the physiological schemata of Brücke and Meynert, in the neurological theories of Hughlings Jackson, and possibly also in the semi-mystical speculations of Fechner.

According to Ellenberger (1956), Fechner had been fortunate enough to have had revealed to him a number of the laws and principles according to which the universe functioned. One was that mental energy obeyed the general law of the conservation of energy and could neither be created nor destroyed. Mental energy could, however, be augmented but only at the expense of physical energy. Fechner also formulated a principle of stability which he believed showed that mental activity was directed towards establishing stability. If mental energy increased, some activity had to occur to restore the original stable state.

Once the hysteric had been thought of as having a surplus of excitation, the necessity for the disposal of the surplus could be deduced from this "law" of Fechner's. However, although Freud was to acknowledge the contribution of Fechner to some of his ideas, the stability principle seems not to have been among them (Levin, 1978, p.89 and n.60). Nevertheless, Fechner's ideas were so well known it would have been impossible for Freud not to be familiar with them. And, in a part of Meynert's work that was clearly known to Freud, Meynert had used Fechner's conservation law to explain, among other things, the supposed inhibition of cerebral blood flow during directed thinking (Meynert, 1884/1885, pp.248-252). Meynert's explanation is, of course, consistent with the notion that physical movement reduces the level of mental energy associated with thinking.

Brücke's and Meynert's description of the way in which the nervous system worked could also be subsumed under a principle that required surplus excitation to be reduced. Brücke believed that successive excitations from repeated stimulation might have to summate before a particular reflex was elicited. The notion was common. James (1890b, Vol. I, Ch.3) cites a very large number of authors who from 1873 on had put forward a similar concept of summation and discharge. For Brücke, food might lodge in the oesophagus for some time but produce only an occasional swallowing movement. The presence of the food:

> creates a constant stimulus. It lasts a long enough time during which the stimuli are summed so that it can release a reflex movement. (Brücke, 1876, cited in Amacher, 1965, p.14)

Summation of stimuli can be viewed as a local accumulation of a surplus of excitation and the reflex swallowing movements as an attempt to dispose of it. On this view also, even the simple law of nervous conduction from receptor to centre to muscles, which Meynert (1884/1885, p.138) had

supposed to be all that was required to account for central nervous system activity, could be regarded as a means for disposing of the local surplus of excitation created by stimulation of the receptors. Now, neither Brücke nor Meynert attributed a purpose of any kind to reflex movements. That step was Freud's and it is this attribution of purpose to the reflex that most distinguishes his approach from those of his predecessors.

As Sulloway suggests, Breuer may have been an influence in Freud's attributing purpose to reflex action. He notes that the Hering-Breuer reflex was "one of the first biological feedback mechanisms to be documented in mammals" Sulloway (1979, pp.51-52). It is now notorious how easy it is to interpret inhibitory control mechanisms as showing purpose and being directed toward teleological goals. Perhaps, as Sulloway suggests, Breuer's understanding of how the vagus nerve is involved in the self-regulation of breathing also found its reflection in the ease with which he and Freud adopted the constancy principle (op. cit., pp.64-65). It should be noted however, as Mancia (1983) points out, that Freud's own models lack a real inhibitory component. Levin (1978, pp.89-93), of course, argues that the constancy principle owes nothing to these kinds of "physiological considerations". He is able to do so by classing Freud's *Project* as a clear "exception to Freud's pattern of emphasizing psychological interpretations" (op. cit., p.7), rather than as a work that "contains within itself the nucleus of a great part of Freud's later psychological theories" and which throws light "on some of the more obscure of Freud's fundamental hypotheses" (Editorial Introduction to Freud, 1950/1954, *Standard Edition*, *1*, p.290).

I foreshadowed in Chapter 4 that the work of Hughlings Jackson, the eminent British neurologist, seemed to me to be the greatest of all the influences causing Freud to consider the possibility of the nervous system being guided by the purpose of disposing of excessive quantities of excitation. Freud had read Jackson's works when preparing his own monograph, *On Aphasia* (Freud, 1891/1953). Jackson had argued that speech ejaculations:

> are all parts of emotional language; their utterance by healthy people is on the physical side a process during which the equilibrium of a *greatly* disturbed nervous system is restored, as are also ordinary emotional manifestations. (Jackson, 1879-1880a)

He continued:

> *All actions are in one sense results of restoration of nervous equilibrium by expenditure of energy.* (ibid. My emphasis, MBM)

These excerpts come from that part of Jackson's paper devoted to a topic of especial interest to Freud, the so-called recurrent utterances or uncontrollable remnants of speech sometimes left to an otherwise speechless patient. We know Freud read this paper because he quoted from it, referred to case

material contained in it, and endorsed Jackson's opinion that the ejaculations were all part of emotional language (Jackson, 1878-1879, 1879-1880a, 1879-1880b. Cf. Freud, 1891/1953, pp.56, 61).

Freud first indicated his belief that the nervous system acted to reduce excitation within twelve months of reading Jackson's paper. Up to the first half of 1891, when *On Aphasia* appeared, he had, as we have seen, only ever spoken of hysteria as being based on an abnormal distribution of a surplus of excitation. The addition of a purpose, Freud's characterising the nervous system as acting to reduce its level of excitation, was first mentioned in the letter to Breuer on the 29th of June, 1892. After referring to "our theorem of the constancy of the sum of excitation" he set out the proposition that:

> The chronic symptoms [of hysteria] would seem to correspond to a normal mechanism. They are displacements in part along an abnormal path ... of sums of excitation which have not been released. *Reason* for the displacement: attempt at reaction. (Freud, 1892, p.148. My emphasis, MBM)

A few months later, in November, 1892, when the theorem was spelled out in a little more detail, Freud not only expressed this 'reason' or purpose more clearly, he made explicit a quite new feature, one which allowed him to explain mental symptoms:

> The nervous system endeavours to keep constant something in its functional relations that we may describe as the 'sum of excitation'. It puts this precondition of health into effect *by disposing associatively of every sensible accretion of excitation* or by discharging it by an appropriate motor reaction. (op. cit., pp.153-154. Emphasis altered, MBM)

In one sense it was easy to understand the physical symptoms. They were conversions of motor reactions that had gone awry. Only if associations were put on the same footing as movements could mental symptoms also be seen as the result of an abnormal disposal of excitation.

Association by simultaneity and symbolisation were the two associative methods which Freud thought could go wrong in disposing of increases in excitation (Breuer and Freud, 1895, pp.176-180). An innervation present simultaneously with excitation caused by a trauma could become directly associated with it (conversion). Symbolisation occurred when the innervation came to stand in a verbal sense for an essential feature of the trauma. An insult might bring about an hysterical pain if the insult was experienced as a slap in the face at the time the patient was suffering from a trigeminal neuralgia. A physical basis as direct as this was not always needed, however. A piercing look directed at a patient could result in penetrating head pains.

Sandler (1967) seems to be the only psycho-analyst to have noticed this subtle change in Freud's thinking. He does not appreciate its significance

however, remarking only that disposing of excitation by associative reaction "goes rather further" than motor reactions. But only by this associative generalisation of his reflex model could Freud allow for the formation of associations to be as effective as movements in disposing of surplus excitation.

While it may have been Breuer's and Freud's observations on Frau Cäcilie's symbolic symptoms that gave impetus to their publishing the *Preliminary Communication*, it is Jackson who seems to be the source of the theoretical underpinning. On the very page where Jackson spoke of speech ejaculations restoring an equilibrium, he quoted the opinion of an unknown author that swearing had value as a safety-valve for feelings and as a substitute for aggressive muscular action. Jackson then quoted another anonymous view that "he who was the first to abuse his fellow-man instead of knocking out his brains without a word, laid thereby the basis of civilization" (Jackson, 1879-1880a). Freud repeated both of Jackson's points exactly in the lecture he gave on his and Breuer's new theory of hysteria (Freud, 1893a).

Unlike Forrester (1980, pp.18-21), I do not think the parallel between Freud's conceptualisation of the isolation of the hysterical symptom and Jackson's of the isolation of the recurrent utterances of the aphasic was the sole or even important influence in directing Freud's attention to the role of affect in isolating the symptom. Jackson had hypothesised that the content of some of these recurrent utterances was determined by what was about to be spoken at the time the patient was taken ill. He believed a discharge of nervous arrangements formed the material substratum of speech, its physical basis, and from his observations on speech and memory he went on to infer that after one had finished speaking the:

> nervous arrangements just discharged remain for a short time in a state of slight independent organisation. (Jackson, 1879-1880b)

In the aphasic, the "nervous arrangements" were kept in a state of greater readiness to discharge, later becoming fixed by the very repetition consequent on their being the patient's only speech (Jackson, 1879-1880a, 1879-1880b). Where the utterance was jargon, Jackson supposed it to consist of elements of the real words the patient had been trying to put together prior to uttering. Strong emotion accompanying the onset of the cerebral insult caused the elements to be too hurriedly assembled so that, instead of saying "Pity me" or "Come, pity me", the patient formed such sound combinations as "committymy" or "pittymy" that could not be broken down further. These "nervous arrangements" for speech remained "permanently in a state of dischargeability far above normal".

Forrester interprets Jackson's remarks about recurrent utterances to mean that the nervous arrangements:

> retain their high level of undischarged energy in a now *permanently closed*

> and *permanently activated* circuit, separated off from the other nervous elements. (Forrester, 1980, p.18. My emphasis, MBM)

Forrester is then able to draw what I believe is a quite specious implication that functionally "isolated" aphasic utterances parallel affectively "isolated" hysterical symptoms.

Freud had concluded his remarks on recurrent utterances by offering an explanation of his own for their isolation:

> I am inclined to explain the persistence of these ... modifications by their intensity if they happen at a moment of great inner excitement. (Freud, 1891/1953, p.62)

Freud was correct to imply that Jackson had no explanation. Although Jackson had noted "the nervous arrangements" for normal speech went "out of function" soon after speaking had finished and that in the aphasic there remained "lines of less resistance than before", he was able to speak of the arrangements only as having "*somehow* achieved a degree of independent organization" (Jackson, 1879-1880b. My emphasis, MBM).

Jackson later tried to connect the fixity of the recurrent utterance to what he saw as the closely related problems of the persisting memory of the last position of an amputated hand ("phantom limb"), of epileptic automatisms and of repetitive actions carried out by unconscious head-injured patients. He had grouped these phenomena together in order to:

> make a basis for the discovery of the reason why there is a fixation of states, which are normally temporary, upon the sudden occurrence of lesions of the nervous system. (Jackson, 1889)

But, apart from showing how the cases illustrated his doctrine of evolution and dissolution, he did not achieve his goal. Indeed, he had to admit of a man whose hand had been blown off while holding a glass but whose phantom hand "retained" that position:

> The persisting memory, so to call it, of the last position of the lost hand implies a persisting state in the highest centres. Why that state remained permanently, *it is impossible for me to say.* (ibid. My emphasis, MBM)

We need, therefore, to look elsewhere than to these ideas of Jackson's for an explanation of the isolation of the symptom.

Having said that, Freud's shift to an "emotional" explanation may nevertheless owe something to Jackson. Not only was there his own explanation involving "excitement" but Jackson several times implicated strong emotion as contributing to the form as well as to the content of recurrent utterances. He also came close to suggesting that emotion contributed to their isolation as for example when he proposed that "strong emotion tends to more automatic, inferior, utterance" (Jackson, 1879-1880b). And we must remind ourselves of the way in which Jackson's

reference to popular language in discussing aphasic incapacity could well have resonated with Freud's, so evocative are they of Charcot and Janet. As we saw in Chapter 4, in both of the slightly different explanations Freud sketched of "associative inaccessibility", the connection of his concepts about isolation with those of Charcot and Janet is very clear. It was not simply because ideas were invested with an excessive quota of affect that different ideas could not be associated with one another; it was because they occurred in different states of consciousness. However, once Janet proposed that ideas determined the characteristic features of hysterical symptoms, Freud could incorporate Jackson's notions of affect and of nervous system functioning into his patho-physiological formula.

Freud's own proposal that associations were as potent as movements in disposing of excitation provided an affective basis for explaining catharsis as well as the formation of mental and physical symptoms. The *talking* cure could be reinterpreted as a *cathartic* method emphasising the expression of *real* emotion rather than mere narrative. It is precisely because Freud did not make this proposal until 1892 that I maintained in Chapters 1 and 4 that if 'Breuer's method' were based on discharge at all, it was really a 'working off' via utterance.

Giving the reflex the purpose of disposing of excitation explained symptom formation and symptom removal. None of Freud's colleagues or predecessors had found it necessary to formulate a purpose for the reflex, probably because what they needed to explain gained nothing from it. For example, although Brücke had noted that when a reflex was released by virtue of the summation of stimuli "the previous state of rest is re-established" (Brücke, 1876, cited in Amacher, 1965, p.14), he did not propose the re-creation of that state to be the *purpose* of the action. A superordinate principle of that kind would have added only a scientifically undesirable vitalist purpose, or at least a teleological goal, to his otherwise materialist explanation. The Brücke and Meynert models had movements as their sole consequences and needed nothing to "explain" them. But if it could be assumed that all the functions of the nervous system were guided by a tendency to dispose of surplus excitation, the reflex reaction could be regarded as only one of the means by which that end was achieved and others might achieve the same goal. All behaviour, normal as well as abnormal, could well be brought under rubric of this inbuilt purpose.

Freud referred to his new found canon variously as the 'theorem', the 'theory', or the 'principle' of constancy. It became the starting point for his general theory of behaviour as well as for his theory of the neuroses. The same proposition, or rather one very like it, is at the core of modern psycho-analytic theory.

EMOTIONAL EXPRESSION AND SYMPTOM FORMATION

Toward the end of the nineteenth century the work on emotion that stood above all others was Charles Darwin's *The Expression of Emotions in Animals and Man* (1872). Given Freud's theory of hysteria held in part that hysterics suffered from abnormalities of emotional expression, it was not surprising Freud should have adopted some of Darwin's ideas. What Darwin provided however was rather more than a few isolated theoretical constructs. Darwin's conceptualisation of emotional reactions as being caused by the disposal of surplus amounts of nerve force or excitation matched Freud's ideas almost exactly. Because his basic concepts fitted so well with Freud's, Darwin's influence on Freud was rather greater than the two slight references to the *Expression of Emotions* in the *Studies of Hysteria* suggest. In fact, once he had adopted Darwin's theory, all Freud had to account for was how the abnormal expression of an emotion was transformed into a symptom.

Freud had the opportunity of knowing Darwin's work simply because he lived at a time when it created considerable interest. But Freud himself tells us that he was "strongly attracted" to Darwin's theories when he enrolled in the *Gymnasium* (Freud, 1925a, p.8) and we know that his first independent scientific work was conducted under Carl Claus, the eminent German Darwinist (Ritvo, L., 1972), and that he owned a copy of Darwin's *Expression of Emotions*, probably from 1881 onwards (op. cit., pp.235-236). Ritvo also mentions Meynert as a more direct but little noted source that may have drawn Freud's attention to Darwin's work on emotions.

Over a number of years Meynert had been in profound theoretical disagreement with Darwin about the role of inheritance in expressive movements. Meynert's lengthy Appendix to his *Psychiatry* (1884/1885) contained an argument that the movements through which the emotions were expressed resulted from inbuilt connections within the nervous system which had nothing at all to do with inheritance. Although he rejected Darwin's thesis on the inheritance of modes of emotional expression, Meynert appears to have accepted the central proposition that some of the movements were activated by excess nervous excitation. It was this aspect of the theory that was to have the greatest impact on Freud.

Darwin began by supposing that nerve force selectively activated the various groups of muscles involved in emotional expression. He then drew very heavily on Herbert Spencer's essay, *The physiology of laughter* (1860) to support his contention that emotions increased the amount of nerve force and that it was the flow of nerve force to the various parts of the musculature through which an emotion was expressed. Darwin quoted and endorsed Spencer's principle:

> As Mr. Herbert Spencer remarks, it may be received as an 'unquestionable truth that, at any moment, the existing quantity of liberated nerve-force,

> which in an inscrutable way produces in us the state we call feeling, *must* expend itself in some direction - *must* generate an equivalent manifestation of force somewhere'. (Darwin, 1872, p.71)

He added:

> when the cerebro-spinal system is highly excited and nerve-force is liberated in excess, it may be expended in intense sensations, active thought, violent movements, or increased activity of the glands. (ibid.)

Darwin went on to accept a second of Spencer's principles:

> Mr. Spencer further maintains that 'an overflow of nerve-force, undirected by any motive, will manifestly take the most habitual routes; and, if these do not suffice, will next overflow into less habitual ones'. (ibid.)

Darwin used Spencer's two principles to account for normal and abnormal emotional expression. Normal expression occurred when emotional states of moderate intensity activated the habitual routes or pathways laid down by selective inheritance. The actual expression was comprehensible because those pathways were either currently appropriate (as when one ran in fear) or because they had been appropriate at some previous evolutionary stage. Abnormal emotional expression resulted from intense emotions or frustrated moderate emotions causing nerve force to flow into channels not normally used. For example, jumping for joy was an abnormal expression of happiness in which there had been overflow into motor channels.

The accumulation and transfer of excitation

Little wit is required to see how readily Darwin's theory can be integrated with Freud's thought. First, both Darwin and Spencer took a quantitative view of the distribution of nerve force - a surplus had somehow to be disposed of. Second, consistent with Jackson's remarks on the effects of action, and possibly with Brücke's on the effects of release after summation, their suppositions at least implied that a state of rest would be re-established, if not that the maintenance of a state of rest was the purpose of nervous functioning. It is consistent with this implication that Spencer placed this purpose centrally in his later *Principles of Psychology* (1873).

Amacher, among others, has argued that the analogy between the flow of a liquid and the mode of transmission of the nervous impulse, so readily apparent in both Darwin and Spencer (made quite explicit in the latter's *Principles*), is also present in the conceptualisations of Freud and his predecessors (Amacher, 1965). Were this so, Darwin (and Spencer) could be thought of as reinforcing a mode of thought already familiar to Freud, perhaps even causing him to adopt it. However, careful consideration of Amacher's quotations from Brücke and a detailed examination of Meynert's *Psychiatry* fails to support Amacher's contention. In fact, in one of the few places where Meynert discusses "nerve force" (in a comment

on Darwin's use of the phrase "the excess of nerve force"), he seems to reject the hydraulic analogy because he re-describes transmission in the then conventional physiological terms (Meynert, 1884/1885, p.275).

Nor is it the case that a fluidic model is to be found in the *Outline of a Physiological Explanation of Psychological Phenomena* by Freud's colleague Exner (1894), even though the mode of transmission considered by Exner is different from that used by Brücke and Meynert. By the time Exner's *Outline* came to be written, Cajal's histological studies had been reasonably widely accepted as showing that the elements of which the central nervous system was composed were not connected to each other structurally. Between the elements there were discernible spaces; there was discontinuity rather than continuity. As a consequence, the propagation of the nervous impulse had also to be discontinuous.

The new view of nervous system structure, or neurone theory as Waldeyer's hypothesis of 1891 came to be called, put paid to hydraulic and electrical models of neural transmission. Neither a fluid nor a current in the crude electrical analogy could 'flow' across the gaps. Even if the nerve impulse were electrical, it could not be transmitted through a discontinuous system in the same way as a current might pass through a network of connected wires. Neuronal transmission could however be pictured as involving the accumulation and transfer of quantities of some kind of energy. For example, from the facts of summation at nervous centres described by Brücke, Exner inferred that successive stimulation caused excitation to accumulate in the neurone until it reached a level where it was transferred across the gap to the next one. His view was that when two neurones were simultaneously charged with excitation, an "intercellular tetanus" was established between them, and a quantity of excitation moved from one to the other.

On Exner's interpretation of the neurone theory, a quantitative account of neural transmission was still possible, at least in principle. Spencer's and Darwin's ideas could also be adapted to an accumulator-transfer model - a name I prefer to Rosenblatt's and Thickstun's (1970) 'accumulator-discharge' - even though that model was neither hydraulic nor electrical.

Freud seems to have developed his version of the accumulator-transfer model as part of his preliminary theorising about the actual neuroses and the psychoneuroses. Until then there is no strong evidence of any such thinking. It was probably not tied to the neurone theory, for Freud not only did not foreshadow Waldeyer's hypothesis, - as has been claimed by Brun, Jeliffe, Jones, Spehlmann, Ellenberger, and Sulloway - he was actually a very late convert to it (Koppe, 1983). However, shortly after Freud's basic approach to the two kinds of neuroses had crystallised, the accumulator-transfer model is found explicitly in the speculative neurophysiological essay known as the *Project*, the first part of which he wrote in September, 1895, and the ideas for which had been germinating over at least the

previous five months (Freud, 1950/1954, Part I; Masson, 1985, Letter of 27.4.95).

An accumulation-transfer model of the kind proposed by Exner provided plausible solutions to three problems that were by then very much in the forefront of Freud's thinking and which could not be accommodated in the older neurophysiological theories. First, in hysteria the detachment of the emotion or affect from the idea and its conversion into a bodily innervation could be thought of as a quantity of excitation redistributed to some other part of the mental structures in much the same way as a charge of static electricity might be redistributed:

> in mental functions something is to be distinguished - a quota of affect or a sum of excitation - which possesses all the characteristics of a quantity ... which is capable of increase, diminution, displacement and discharge, *and which is spread over the memory-traces of ideas somewhat as an electric charge is spread over the surface of a body.* (Freud, 1894, p.60. My emphasis, MBM)

Freud went on to remark that this hypothesis (or model) underlay the theory of abreaction, describing it with an hydraulic analogy involving the "flow of electric fluid" (op. cit., p.61). This description did not mean, of course, that Freud was adopting an hydraulic model - he was simply using a conventional analogy.

The second of Freud's problems was the way in which the separate trauma of the psychoneuroses sometimes seemed to summate before producing their final result. On the accumulation-transfer conception the affects of the traumas could be thought of as building up in a common store rather as a battery might be charged. After reaching a certain level the total could be diverted onto another idea or into some part of the body. An accumulator model was also helpful in solving the third problem of the way somatic sexual excitation seemed to build up before being discharged. Here too, Freud used an hydraulic analogy to describe how excitation in neurasthenia was impoverished; it was as if the store of somatic sexual excitation had been "pumped empty" (Masson, 1985, Draft G, possibly of 7.1.1895).

Meynert pointed out that Darwin's explanation of emotions required little or no real knowledge of the nervous system. Nerve force, whatever that was, flowed along vaguely defined 'channels' or 'pathways'. Darwin's hydraulic model therefore 'overcame' gaps in scientific knowledge about real anatomical pathways, real nervous impulses, and the real ways excitation was propagated. Freud's accumulator-transfer model has the same characteristics. Solomon (1974) and Mancia have noted that Freud's 'quantum of energy' in motion is a mechanical analogy, the latter adding that, like most of his other neurophysiological concepts, it is:

> unacceptable or at least highly questionable, not only in the light of present-day knowledge of neurophysiology, *but even of knowledge in*

Freud's own time. (Mancia, 1983. My emphasis, MBM)

Why did Freud hold to a view that was so at variance with the then known facts of the nerve impulse being propagated by local depolarization? I believe it was because he had the same need as Spencer and Darwin to 'overcome' the lack of real knowledge of the physiology of the nervous system and its processes.

Precisely because it was analogical, Freud's accumulator-transfer model was able to describe the formation of complex associations like those in the pathogenic memory structure. In Meynert's theory only those sensory components registered cortically and connected by association fibres could form associations. Little more than simple associations between the sight and sound of an animal or those between a stimulus and an approach or withdrawal movement could be built up. Associations of a more general and complex kind were not possible, especially associations between memories of events having similar content or emotional quality and occurring with long intervals between them. A conception of energy accumulating in neurones before it was transferred overcame this limitation. Exner had already supposed the first transfer made subsequent transfer easier: it established what he called a *bahnung* [Ger.: pathway] or 'facilitation' between neurones. The more frequently transfers took place and the greater the quantity of excitation transferred, the easier the facilitation. Contiguous neurones could thus be linked together; two neurones had only to be excited simultaneously for an association to be formed. The more often this happened the stronger was the association and its tendency to be repeated (Exner, 1894, cited in Amacher, 1965, pp.43-47).

Freud adopted Exner's view of facilitation and the formation of associations in his *Project* (Freud, 1950/1954, Part I, Section 3). It had two advantages over the Meynert view: it enabled one to suppose that channels that had not been or could nor be specified anatomically might link the neurones in any of the ways required by the theory and it allowed one to postulate ways for quantities of excitation to be dealt with other than by motor discharge.

As well as using a version of Exner's notions to explain facilitation and the formation of associations, a version which Koppe (1983) shows clearly was an adaptation, not an adoption, and an idiosyncratic one at that, Breuer and Freud drew upon them in several other ways (Breuer and Freud, 1895, pp.193, 195, 241). They supposed that Exner's concept of "intercellular tetanus" between adjacent neurones held for all conduction pathways and that the total amount of this excitation defined the general level of excitation, or "intracerebral tonic excitation". Typically this was distributed uniformly throughout the brain but it varied with emotional state, with physiological need, and, as Exner had also thought, with attention. The principle of constancy discharged and redistributed this excitation in order to keep it low in amount and even in distribution.

Freud's theory of symptom formation drew upon the preceding ideas by supposing emotions or affects always increased intracerebral excitation in a non-uniform way. As Breuer put it:

> It may be taken as self-evident that all the disturbances of mental equilibrium which we call acute affects go along with an increase of excitation. (Breuer and Freud, 1895, p.201)

Broadly speaking, one of three things happened to the increased excitation: it found discharge in action, it found substitute discharge in speech (which involved motor innervations in any case), or it could 'wear away' in the effort involved in forming associations with other ideas or otherwise level out. As Freud put with respect to the suppressed reaction to an insult:

> a healthy psychical mechanism has other methods of dealing with the affect of a psychical trauma even if motor reaction and reaction by words are denied to it - namely by working it over associatively and by producing contrasting ideas. Even if the person who has been insulted neither hits back nor replies with abuse, he can nevertheless reduce the affect attaching to the insult by calling up such contrasting ideas as those of his own worthiness, of his enemy's worthlessness, and so on ... finally the recollection, having lost its affect, falls a victim to forgetfulness and the process of wearing-away. (Freud, 1893a, p.37. Cf. Freud, 1950/1954, Part III, Sections 1 & 3)

Breuer seems also to have believed that in those cases of fear and anxiety when motor and associative reactions were not possible the increased excitation disappeared "by a gradual levelling out" (Breuer and Freud, 1895, p.202). However:

> If ... the affect can find no discharge of excitation of any kind along these lines .... The intracerebral excitation is powerfully increased, but is employed neither in associative nor in motor activity. In normal people the disturbance is gradually levelled out. But in some, abnormal reactions appear. (op. cit., pp.202-203)

Abnormal reactions led to "the passage of cerebral excitation to the vegetative organs" or it flowed off "in primitive movements" because excessive excitation bypassed or broke through "the co-ordinative centres".

The essential characteristics of hysteria were not abnormal patterns of emotional expression, however. These lay in the vicissitudes of the original affect. An intentional failure to discharge the affective excitation along adequate pathways provided the primary motive for the formation of symptoms. Symptoms only began to form when a tendency was created for subsequent discharge to follow the same abnormal pathway. Anything threatening to revive the original affect, such as an attempted recollection of the original trauma, would lead to a discharge like the first. Through repetition, this tendency to abnormal discharge would be strengthened. Finally the affect would be discharged before the idea with which it had previously been associated could be remembered. Any stimulus tending to

revive the original idea now produced only the abnormal reaction. The affect had been converted into a pattern of discharge. At the same time the idea, being now deprived of all affect, would be indistinguishable from other affectless ideas. In other words, it would be unconscious. A similar process produced obsessions and phobias. There the separation was followed by the affect attaching itself to another idea and being discharged through the recurrence of the second idea. The first idea had, of course, become unconscious too.

Actual neuroses were also the result of abnormal responses to increases in excitation and the theory applying to the psycho-neuroses was consonant with them. All that had to be recognised was that the excitation resulted from a continuously operating, endogenous physical process rather than from an occasionally experienced affect. The physical process was assumed to produce pressure on the walls of the seminal vesicles which stimulated nerve endings there. Periodically that nervous excitation, or somatic sexual excitation, exceeded a threshold level and was transmitted to that part of the cerebral cortex where the ideas of sexuality were registered. By becoming connected to or forming a linkage with this psychical sexual group of ideas, the somatic sexual excitation charged them with energy and created psychical sexual tension or libido which raised the level of intracerebral excitation. The principle of constancy then brought a tendency for the libido to be discharged. In the mature adult, discharge was through a specific or adequate action leading to the complex spinal reflex involved in orgasm. Discharge through orgasm relieved the pressure on the seminal vesicles, removed the whole of the previously existing somatic sexual excitation, and thereby reduced the level of psychical tension or libido. The theory obviously applied to the male, but Freud believed it als applied to the female, even though he recognised there was no corresponding site where somatic sexual excitation was produced (Freud, 1895a, p.109).

The various kinds of incomplete gratification inhibited the sexual function and resulted in the somatic sexual excitation being deflected from the psychical sexual group of ideas. As was noted in Chapter 5, psychical tension or libido was then not created and normal discharge was not initiated. Somatic sexual excitation spilled over into the autonomic nervous system to produce the physiological symptoms of anxiety. The increase in excitation was concurrently sensed as a threat to the organism and generated the psychological feeling of anxiety (Freud, 1895a). In the neurasthenic, masturbation produced lowered thresholds for linkage and discharge. Since excitation was produced continuously, discharge took place too frequently. The result was that sexual substances were depleted and the symptoms of neurasthenia ensued.

According to Darwin's theory, emotional expression resulted from increased nerve force flowing into normally used pathways or into ones not usually available. Freud's theory was similar. Increases in cortical

excitation were followed by attempts to reduce the level of excitation but symptoms might result if unusual pathways were innervated. Concurrent excitation of the muscular and sensory apparatus, often present fortuitously, created a strong tendency for pathways of discharge to be opened up to them and resulted in the stable patterns of sensory and motor innervation that were the physical symptoms of hysteria. If the pathways to action were unavailable or weak some substitute figurative or symbolic expression of the affect would result. Finally, if there was little or no discharge, the affect persisted in consciousness as a state of feeling (Breuer and Freud, 1895, pp.85-90, 164-169, 173-175, 180-181). The essential difference between Freud's theory and Darwin's was that Freud had to explain how a transient abnormal emotional reaction could develop into a permanent symptom.

THE THEORY

Critical evaluation of Freud's theory of abnormal psychology requires a more formal account than I have so far provided. For this purpose I have arranged the statements constituting the theory according to content. They are of three types: statements about the creation and disposal of excitation within the nervous system, statements about affects and the consequences of their expression, and statements about the discharge of sexual excitation. After outlining the theory I attempt an evaluation of its logic.

The statements themselves are paraphrases of what appear to be the essential propositions set out by Breuer and/or Freud. However, the second part of proposition 1.3 is not in Breuer's theoretical section of the *Studies*, although it is clearly implied in his discussion (Breuer and Freud, 1895, pp.198-200). Except for proposition 7.9, which is also implied in Breuer's discussion (op. cit., pp.200-201), the whole of the third section is Freud's. It is based substantially upon his first paper on the anxiety neuroses (Freud, 1895a), although I have made some use of his correspondence with Fliess (Masson, 1985). For the sake of completeness, I have added the notion that depletion of sexual substances and sexual weakness caused neurasthenia.

1 Excitation within the nervous system

1.1 In the waking state the conduction and connection pathways of the brain are in a condition of *tonic intracerebral excitation.*

1.2 The level of this excitation is increased by stimuli impinging on the organism from within and without.

1.3 Excitation arising from within the organism has a somatic *source* and tends to be of a periodic, recurring nature.

1.4 A tendency exists within the nervous system to maintain the level of excitation at a constant, uniformly distributed optimum.

1.5 The maintenance of excitation at this optimal level is achieved either by neural discharge to the motor system or by a redistribution into other associational structures within the brain.

1.6 Neural discharge tends to take place along preformed pathways. When such a discharge is complete an *adequate reaction* is said to have occurred.

1.7 Discharge of excitation within the nervous system is controlled by resistances between its component parts. A particularly strong resistance prevents discharge through the autonomic nervous system to the organs of circulation and digestion.

1.8 The strength of these resistances varies from one individual to another depending upon innate disposition or because of long standing states of excitation having existed previously in some part of the system, for example, as a consequence of illness.

2 Affects and the discharge of excitation

2.1 The psychological side of an affect is a disturbance of the dynamic equilibrium of the nervous system. Acute affects in particular are always found to increase with increases in excitation.

2.2 The adequate reaction to affectively produced increases in excitation may be either preformed, reflex patterns of motor activity, or substitute speech reactions, or the activation of associations representing ideas. Any of these reactions restore equilibrium in the nervous system.

2.3 The strength of recollected affects in memory is a function of the adequacy of the original motor discharge or of its *abreaction* in words.

2.4 When the individual fails to react intentionally, affectively produced increases of excitation may not be dischargeable through normal motor, speech, or associative activity. In some individuals the failure leads merely to an *abnormal expression of emotion* but in those with disposition to conversion hysterical symptoms result.

2.5 Abnormal emotional expression occurs when weak internal resistances fail to prevent discharge into the autonomic nervous system and the circulatory and digestive systems are disturbed.

2.6 Symptoms of hysterical convulsion result from the excitation overwhelming or bypassing the centres co-ordinating motor behaviour; bodily symptoms result from the excitation being *converted* into a pattern of muscular and sensory innervation; mental symptoms are the continuation of the original affect.

2.7 In hysteria the selection of one pattern of discharge rather than another is *determined* by such factors as the strength of the resistances involved, the presence of simultaneous excitation elsewhere in the body (i.e., the law of association by simultaneity), and by figurative or symbolic modes of expression.

2.8 The recollection of an affect originally discharged in an abnormal way causes a repetition of the abnormal pattern of discharge. Excitation is then said to be converted into somatic phenomena, that is, to have brought about an hysterical symptom.

2.2 Frequent repetition of such abnormal patterns of discharge reduces the excitation of the affect such that the idea associated with it enters consciousness deprived of affect. The conversion is then said to be complete.

2.10 In those with the appropriate disposition the failure of affective discharge may lead to the attachment of its quota of affect to another idea. In this case the affect remains in consciousness and is experienced as an obsession.

3 Sex and the discharge of excitation

3.1 The accumulation of *sexual substances* within the sexual organs produces physical or *somatic sexual excitation*.

3.2 When the level of somatic sexual excitation increases above a certain threshold value it is represented in the brain as a *psychical stimulus* and the *psychical sexual group* of ideas (those ideas concerned with sexuality) are *supplied, invested* or *cathected* with energy.

3.3 Cathection of the psychical sexual group of ideas by somatic sexual excitation creates *libido*, or psychical libidinal tension.

3.4 The *adequate* or *specific* action that reduces this excitation is the normal sexual act culminating in orgasm.

3.5 Masturbation lowers the threshold for the discharge of somatic sexual excitation.

3.6 The continuous production of somatic sexual excitation and a lowered threshold for discharge causes frequent discharge at low levels of libidinal tension.

3.7 Too frequent discharge depletes the sexual substances and causes sexual weakness and fatigue.

3.8 Sexual weakness and fatigue cause the typical symptoms of neurasthenia: intracranial pressure, spinal irritation, and dyspepsia

with flatulence and constipation.

3.9 Somatic sexual excitation may be *deflected* from the psychical sexual group of ideas. When this happens libido does not form.

3.10 Somatic sexual excitation unable to form libido may be discharged into the autonomic nervous system producing there alterations in heart rate, breathing, and sweat gland activity. Those alterations are the *physical signs* of the anxiety attack and are *surrogates* for orgasm.

3.11 The ego perceives undischarged tension as a threat and perceived threat produces the *affect* of anxiety.

3.12 Undischarged somatic sexual excitation consequently produces the *subjective signs* of the anxiety attack.

THE LOGIC OF THE THEORY

When I set Freud's theory out it was appropriate to group the statements according to their content. In evaluation proper the logical structure has to take precedence. Its starting point is provided by the three very general propositions about constancy, affects, and sexual excitation. The other statements are either about the regulation of the discharge of excitation in the nervous system or about the psychological and physiological processes required to derive the explanations of the symptoms. Some of these latter are not particularly limited to the neuroses.

The most general of all of the propositions [1.3] expresses the principle of constancy without which the nervous system cannot function. When it is linked with any other proposition stating that the level of intracerebral excitation has increased it necessarily follows there will be an attempt to reduce the level of excitation. For example, if physiological needs such as hunger or sex increase excitation they must result in an action designed to reduce the excitation.

A general proposition like constancy may be empirical or theoretical. Acceptable explanations may begin from either. However, if the proposition is empirically based it is evident that a theory based upon it cannot claim to explain the observations from which it was itself inferred. Breuer and Freud make precisely claims of this kind. Among a number of kinds of behaviour from which they infer the principle of constancy are the need for mental activity after intellectual inactivity (when the level of excitation was presumed to rise), the similar need for movement after motor quiescence, the "torment of boredom" resulting from reduced sensory stimulation, and the various actions performed to satisfy needs. Each was said to be preceded by an increase in excitation which was experienced as unpleasure. Breuer then argued:

> Since these feelings disappear when the surplus quantity of energy which has been liberated is employed functionally, we may conclude that the removal of such surplus excitation is *a need* of the organism. And here for the first time we meet the fact that there exists in the organism a 'tendency to keep intracerebral excitation constant'. (Breuer and Freud, 1895, p.197. Original emphasis altered, MBM)

However, in the same discussion this newly formulated principle is used to *explain* such behaviours as purposeless motor activity, individual differences in the ability to tolerate mental and physical inactivity, the unco-ordinated motor behaviour of the infant in response to need, and the convulsions of the epileptic and the hysteric. Most of these behaviours are similar to the behaviours from which the principle was inferred and some are actually identical to them. The explanations generated from the principle are tautological.

Two kinds of observations would allow the principle of constancy to provide an acceptable explanatory starting point: neurophysiological measures of the level of cortical excitation and independent behavioural changes directly correlated with them. Only observations of that kind could index the level of excitation before and after discharge and independently of motor activity or rest.

The second general proposition is that affects increase the level of intracerebral excitation [2.1]. When combined with the principle of constancy it logically entails that emotional states will be followed by attempts to reduce the extra excitation. This entailment carries weight only to the extent it can be shown, either directly or indirectly, that emotions actually produce increases in excitation. It is just that which Breuer and Freud fail to do. Indeed, Breuer regarded the increase as "self-evident" (op. cit., p.201). Rather unnecessarily, then, he cited some behavioural evidence. But the behaviours (jumping for joy and crying with grief) were again of the very type requiring explanation.

The need to index the increases in intracerebral excitation caused by somatic sexual excitation [3.2] is also clear. Neither the general restlessness consequent upon sexual deprivation nor the quiescence following orgasm which Breuer cited as indexing the extremes of intracerebral excitation can be so used. It is precisely those behaviours the changed levels of excitation are supposed to explain. The point applies most forcefully to general restlessness, because that is one of the symptoms of anxiety neurosis the theory should be accounting for.

The lack of evidence for these three propositions independent of the behaviours to be explained by them is fatal to Breuer's and Freud's theory. They did not adduce acceptable independent evidence of a behavioural or neurophysiological for any of the propositions. Nor could that evidence be found today.

Defects in explanatory power resulting from the lack of independent evidence are just as marked when the propositions about the regulation of the discharge of excitation in the nervous system are considered (for example, 1.4-1.8, 2.2, 2.4-2.7, 3.4-3.5, and 3.9-3.10). The propositions about discrete pathways of discharge, resistances between sections of the nervous system, preformed actions capable of bringing discharge about, and associational structures capable of disposing of excitation are all used in conjunction with one or other of the three general propositions to explain the *direction* of discharge. For example, given an increase in somatic sexual excitation, the knowledge that linkage was unavailable and the resistance of the autonomic pathway weak would explain why the discharge produced the physiological responses characteristic of anxiety. However, none of the concepts referred to in the propositions were defined other than by the gross behaviours they are supposed to explain. Thus the autonomic reactions occurring in the anxiety attack were the sole indicators of weak resistance.

Similarly, even though the absence of an adequate motor response might be inferred from the patient's recollection of the traumatic event, the failure of an associative reaction to occur was actually indexed by the conversion itself. Again, while hysterical convulsions were said to result from excitation breaking through the resistances surrounding the motor co-ordinating centre, the presence in the convulsions of so-called primitive movements was the sole basis for inferring that the resistances had been broken (op. cit., p.204). The lowered threshold for the discharge of somatic sexual excitation in neurasthenia was also inferred from the very increased frequency the changed threshold was invoked to explain. In addition to non-independence the many concepts incorporated into this group of propositions which seem to refer to physiological processes are actually pseudo-characterised terms. Their referents are the very relations they are supposed to explain.

In contrast, the remaining propositions are capable of independent investigation and would, if true, add to the completeness and power of the explanations. Thus, if it were true that the strength of an affectively toned memory was a function of the adequacy of the original affective discharge [2.3], explanations of the formation of hysterical symptoms by conversion through successively repeated abnormal discharges would be strengthened. Again, if depletion of sexual substances did cause sexual weakness and fatigue [3.7], neurasthenic symptoms would be explicable. However, even though it is possible to investigate these propositions without any commitment to Freud's methods, or even to the study of abnormal conditions, it is not in fact known if they are true or false.

The following schematic outlines of the explanations of hysteria and the actual neuroses show the explanations generated by Freud's theory to consist of assumption piled upon assumption rather than a linked set of propositions entailing the facts. They lack genuine logic.

The assumption that the nervous system operates according to the principle of constancy, together with the presumed regulating structures, implies that if increases in intracerebral excitation occur, the disposal of that excitation will be regulated by the structures. Assuming now that emotions raise the level of excitation, it follows of necessity that those structures will similarly regulate discharge consequent upon emotional experience. By further assuming that particular types of regulatory defect, such as the unavailability of a motor response or normal associative connections, direct the excitation to the muscles of the body concurrently excited, hysterical symptoms like paralysis and contractures are initiated. Two further assumptions account for the formation of permanent symptoms: attempted revivals of the idea originally associated with the affective experience repeating the abnormal discharge and the frequent repetition of such discharge leading to the idea being completely deprived of its affect.

The actual neuroses are 'explained' similarly. Assume that as sexual substances accumulate they produce increases in somatic sexual excitation. Assume the excitation has to reach an optimum intensity before its assumed connection with the psychical sexual group of ideas results in adequate discharge of excitation. Assuming that masturbation lowers the threshold for discharge, assuming that repeated discharge at low levels of intensity depletes the sexual substances, and assuming that those consequences cause the symptoms of neurasthenia, it then follows that masturbation causes neurasthenia. Again, assuming that somatic sexual excitation not discharged via linkage and orgasm is directed into the autonomic nervous system, assuming that such discharge produces the physical signs of anxiety and that the undischarged excitation is perceived as a threat, and assuming that perceptions of threat generate the affect of anxiety, it follows that deflected excitation causes the objective and subjective symptoms of anxiety neurosis.

Stated like this the weaknesses of the explanations are almost self evident. Assumptions are simply piled on top of one another until a pseudo-logical chain has been constructed between the starting propositions and the clinical facts. If the worth of a theory is proportional to the evidence supporting its assumptions then Freud's theory is very unsatisfactory indeed. The theory is quite unable to explain either the psychoneuroses or the actual neuroses. It is not so much that the propositions are incorrect or that they lack direct or indirect empirical support. Rather the inadequacies arise because for too many of the propositions it is impossible to imagine what sort of evidence could show them to be correct or incorrect.

THE FREUD-BREUER DIFFERENCE

Before concluding, I must defend my characterisation of the particular combination I have presented here of Breuer's theoretical chapter of the

Studies and Freud's theory of the actual neuroses as *Freud's* theory. Freud "wholly" dissociated himself from Breuer's chapter (Masson, 1985, Letter of 22.6.94) but it has not been possible until recently to say why he so sharply separated it from his own contribution.

Differences did exist over the relevance of the French concepts of psychopathology. By 1895 Freud had abandoned all notions that symptoms formed in other than the waking state or that treatment ought to be carried out other than in it and his theoretical concepts had changed accordingly. Breuer's theorising had not at all kept pace, as Levin (1978, p.111) has also noted. But the discussions in the *Studies on Hysteria* show it was possible to reconcile the two approaches (Freud, 1893a. Cf. Levin, 1978, p.117).

A second source of disagreement was Breuer's fairly conservative theorising as compared with Freud's tendencies to speculation. Even before the *Preliminary Communication* had been published, Freud complained to Fliess:

> My hysteria has, in Breuer's hands, become transformed, broadened, restricted, and in the process has partially evaporated. (Masson, 1985, Letter of 12.7.1892)

The difference hinted at here became marked by the time of the *Studies*. For a start, and although he believed hysterical symptoms were built on a physiological foundation, Breuer attempted a purely psychological mode of explanation of them. As he put it at the beginning of his theoretical chapter:

> In what follows little mention will be made of the brain and none whatever of molecules. Psychical processes will be dealt with in the language of psychology; and, indeed, it cannot possibly be otherwise. If instead of 'idea' we chose to speak of 'excitation of the cortex', the latter term would only have any meaning for us in so far as we recognized an old friend under that cloak and tacitly reinstated the 'idea'. For while ideas are ... familiar to us ... 'cortical excitations' are on the contrary rather in the nature of a postulate, objects which we hope to be able to identify in the future .... Accordingly, I may perhaps be forgiven if I make almost exclusive use of psychological terms. (Breuer and Freud, 1895, p.185)

At almost the same time as Breuer was refusing to speculate, Freud was attempting just the opposite. Nothing could contrast more with Breuer's stance than the physiological and reductionist position with which Freud began his *Project*:

> The intention is to furnish a psychology that shall be a natural science: that is, *to represent psychical processes as quantitatively determinate states of specifiable material particles*. (Freud, 1950/1954, Part I, Introduction. My emphasis, MBM)

Given the ignorance of neural processes at the time, Breuer had warned against exactly this line of approach:

> The substitution of one term for another would seem to be no more than a pointless disguise. (Breuer and Freud, 1895, p.185)

Freud was prepared to risk that result but Breuer was not.

Breuer largely restricted himself to using such descriptive physiological terms from the contemporary scientific vocabulary as excitation, transmission, and resistance without trying to explain or characterise them further. Thus, precisely while Breuer was restricting his use of these physiological terms, treating them as givens, Freud began his speculative and pseudo-quantitative characterisation of them. His effort was to prove as unsuccessful as Breuer's comments implied it would be (Solomon, 1974; Mancia, 1983; Koppe, 1983). [1]

In his introduction to the *Studies on Hysteria*, the Editor argues that Breuer's theoretical chapter shows the same neurological bias as Freud's *Project* (*Standard Edition*, 2, pp.xxiii-xxv). If this were so, the above argument, which is based on the proposition that precisely this "bias" was a major point of disagreement between Breuer and Freud, would have to be rejected. What the Editor seems not to have appreciated is that Breuer simply used neurological terms in a conventional and almost descriptive way whereas Freud was attempting a further speculative characterisation of them.

It has also been argued that Freud was the conservative theorist and that it was really Breuer who possessed the truly speculative mind (Gedo, Sabshin, Sadow, and Schlessinger, 1964; Schlessinger, Gedo, Miller, Pollock, Sabshin, and Sadow, 1967). Breuer is therefore supposed to have been the one who made unwarranted leaps from concrete observation to remote theoretical construct. The argument is based on a comparison of statements by Breuer and Freud in the *Studies on Hysteria* and judgments about the magnitude of the gap between the empirical and theoretical levels those statements reveal. The conclusion of Gedo, *et al.* and Schlessinger, *et al.* cannot be accepted. It is based on a number of methodological deficiencies of which the most gross concerns the material compared. Breuer's one case observation and *the* theoretical chapter are contrasted with Freud's four case observations and his chapter on psychotherapy, a

1. Pribram is one of the few neurophysiologists who has argued that the concepts of the *Project* relate positively to modern neurophysiology (Pribram, 1962 and 1965; Pribram and Gill, 1976). I find his argument unconvincing. Not only is the central notion of cathexis not at all like the graded electrotonic excitations with which Pribram identifies it (Swanson, 1977), but neural transmission does not involve transfer of anything like the quanta of energy Freud postulated (Grenell, 1977). Neither does Pribram mention the lacuna of an inhibitory process. I would add further that Pribram's thesis that the *Project* is based on a binary model of processing (from which he argues for Freud's anticipating modern information theory) is simply not supported by anything that Freud wrote - in the *Project* or elsewhere.

chapter which contains almost no theory. Breuer had virtually unlimited opportunity to theorise but Freud was quite restricted. It would be surprising had the analysis shown a difference other than the one Gedo, *et al.* and Schlessinger, *et al.* report. Before it can be concluded that one was more speculative than the other, equivalent kinds of material from Breuer and Freud have to be compared. Either the case history material alone from the *Studies on Hysteria* ought to be analysed or, since the interest is in how theoretical statements are used, Breuer's theoretical chapter should be contrasted with Freud's *Project*. If that were done there is no doubt whatever Freud was the more speculative of the two, an opinion which Friedman (1977) also holds, although on somewhat different grounds. But, at the time *Studies on Hysteria* was begun, the difference was not marked.

By maintaining that hysterical symptoms were intrusions from a secondary consciousness, Breuer also avoided completely Freud's concept of an unconscious mind as a kind of repository for repressed ideas. He clearly disapproved of thinking of unconscious processes in a way that gave them substantive properties (Breuer and Freud, 1895, pp.227-228). Possibly this is why he placed the phrase 'in the unconscious' in quotation marks when he introduced that concept to his theoretical chapter in the *Studies* (op. cit., p.45, n.1). Among a number of psycho-analytic writers, Abrams (1971a) has especially noted how Breuer's conception of mental life was fundamentally different from Freud's.

Third, it has frequently been remarked that disagreements about sexual factors *per se* were not responsible for Freud's disapproval. At the time of the *Studies* Breuer seems to have been prepared to place at least as much stress as Freud on sexual factors (Breuer and Freud, 1895, pp.245-247. Cf. Sulloway, 1979, Ch. 3 and Appendix A). However, only recently has an important difference in the way they thought about them come to light. In my view it explains two things: why Freud dissociated himself so strongly from Breuer's chapter and Freud's puzzling later characterisation of Breuer's hypnoid explanation as based on "a theory which was to some extent still physiological" (Freud, 1914a, p.11).

For Breuer psychologically determined symptoms were erected on the foundation of "an idiosyncracy of the whole nervous system". It was, he emphasised, "a building *of several storeys*" [Ger.: es ist ein mehrstöckiges Gebäude]. Developing the analogy, he went on:

> Just as it is only possible to understand the structure of such a building if we distinguish the different plans of the different floors [Ger.: Stockwerke], it is, I think, necessary ... for us to pay attention to the various kinds of complication in the causation of symptoms. If we disregard them and try to carry through an explanation by employing a single causal nexus, we shall always find a very large residue of unexplained phenomena. (Breuer and Freud, 1895, pp.244-245)

Hysterical stigmata and such nervous symptoms as some of the pains and vasomotor phenomena, even perhaps the pure motor convulsions, "were

not caused by ideas'' but resulted from the fundamental abnormality of the nervous system. To try to attribute them to psychological causes was, he concluded:

> just as though we tried to insert the different rooms of a many-storied house into the plan of a single story [Ger.: eines mehrstöckigen Hauses auf dem Grundrisse eines Stockwerkes eintragen]. (ibid.)

Now, the most interesting thing about Breuer's argument is its placement. It occurs half-way into the very last section of his theoretical chapter, just after his discussion of those symptoms and characteristics he saw as resulting from innate and abnormal excitability and just before his discussion and endorsement of sexual factors as ''the most numerous and important of the ideas that are fended off and converted''.

We can be fairly certain what Freud thought of Breuer's attack on single nexus causation. From a previously unpublished portion of a letter to Fliess we learn that one of Freud's case histories was not to be:

> included in the collection with Breuer because the second level ['Stockwerkes'] that of the sexual factor, is not supposed to be disclosed there. (Masson, 1985. Letter of 21.5.1894)

Hence, although both agreed on the importance of the nexus of sexual factors, Breuer's interpolation clearly showed his unwillingness to derive all the symptoms from them.

Even with these two last very considerable differences between Breuer and Freud, it is nevertheless possible to accept the theory Breuer outlined in the *Studies on Hysteria* as reflecting his own and Freud's views prior to Freud's speculative theoretical tendencies becoming prominent. Many years ago Bernfeld concluded from his analysis that the theory presented by Breuer:

> is conceived in the spirit of the 'physicalist' physiology, in complete accordance with Freud's earlier thinking, and represents Freud's ideas at the time of his collaboration with Breuer .... It is Freud's as well as Breuer's. (Bernfeld, 1944)

Nothing in the *Project* nor the correspondence with Fliess, neither of which were known to Bernfeld, requires this conclusion to be modified (Amacher, 1974; Waelder, 1956b). Freud's own much later praise and endorsement of Breuer's theoretical contribution to the *Studies* may even support it (Freud, 1923a, p.236, 1925e, p.280).

CONCLUSION

My evaluation has concentrated on the formal or logical characteristics of Breuer's and Freud's early theory rather than upon its content. In this respect it is unlike those made by Thompson (1957) and Stewart (1967). In the final analysis, it is the logic of the relations between the propositions and between the propositions and the facts that determines whether a theory is worthwhile or not. Only when a theory implies its facts can it be said to explain them, and it is only when factual consequences can be deduced from a theory that it can be said to be testable (Nagel, 1959; O'Neil, 1969, pp.67-84). Unless the logical structure is present, to carry the content as it were, the content can have only limited meaning. This is why the language in which a theory is expressed is of such little moment. Ever since Braithwaite's (1953, pp.88-114; 1960/1962) analysis of the relation between a theory and its model it has been clear, irrespective of the type of model, that a model can be adequate for its theory only if it has an identical logical structure. The types of model are defined by their content; it follows the logical structure is the important component of the theory rather than its content.

Many models are of a visual kind and, although visual imagery may be useful in thinking about structures and processes that cannot be seen, whether or not a structure or process resembles some picture of it visually or not is not the issue, as Ellman and Moskowitz (1980) point out. Sometimes it turns out that there is a resemblance - as with Harvey's pores. As often as not, however, as with sub-atomic particles or multi-dimensional space for example, it does not make sense even to ask about the similarity. What does make sense is to make the conceptual link between that which we cannot see and the model explicit. And this means arriving at an independent characterisation of that which we cannot see. To take Ellman's and Moskowitz's example, the link between the DNA molecule and the picture of it as a double helix is given by its characterisation "in purely geometric terms, without reference to its visual appearance".

An equally important reason for concentrating upon the logical structure of Freud's early theory is that the logical deficiencies revealed in it are very like those of the later theory (Nagel, 1959). Deficiencies such as the lack of explanatory power and testability, the use of assumptions having no empirical referents, and the reliance on uncharacterised theoretical terms seem to reflect Freud's *style* of theory construction as much as the *standards* by which he judged the adequacy of his explanations. A connection of this sort between his early and late theories would not have been revealed if content had been the main consideration. We shall expect, of course, that these deficiencies will also show up in the applications of the theory to be considered in Chapter 8 and in the later alterations to it outlined and evaluated in Chapters 9 and 10.

8 EXPECTATIONS, ACTUAL NEUROSES AND CHILDHOOD SEDUCTION

Miss Prism: Memory, my dear Cecily, is the diary we all carry about with us.
Cecily: Yes, but it usually chronicles the things that have never happened, and couldn't possibly have happened.

Wilde: *The Importance of Being Earnest*, Act II.

In this Chapter I examine Freud's investigations of hysteria and the actual neuroses. For both disorders I shall bring out how Freud's expectations about the kinds of causes likely to be at work created his clinical "facts" and how his explanations further misinterpreted them. The context of the discussion is partly provided by the evaluation of the theoretical ideas considered in Chapters 6 & 7 of Part II and partly by the analyses of the etiological equation and the assumptions underlying the pathogenic memory structure in Part I. It will become apparent that Freud's approaches to understanding the causes of hysteria and the causes of the actual neuroses were identical.

It is well-known that Freud's first attempt at explaining hysteria independent of Breuer and Charcot collapsed over a relatively short and clearly defined period. After gradually building up a thesis that hysteria was caused by parental seduction, Freud suddenly made an about-face. That Freud's theory of the actual neuroses was also abandoned is much less commonly remarked. Partly the reason is that it subsided gradually, rather as an old balloon gradually shrivels and deflates. For the psychoneuroses the alternative explanation which was found appeared to overcome the deficiencies of the earlier theory and it was eventually easy for Freud to acknowledge that his seduction explanation really had been incorrect. Without a happy outcome of that kind, the theory of the actual neuroses eked out an existence of sorts in a shadow world, being neither definitely accepted nor rejected. Despite a number of modifications, it never became satisfactory and, toward the end, Freud virtually granted the actual neuroses could not be explained. A number of his co-workers went even further: they doubted there were or ever had been any such disorders.

Because it is evident that the expectations generated about the actual neuroses were simply transferred to hysteria, I begin with the former before considering the seduction theory.

THE ACTUAL NEUROSES

I deal successively with the problems of Freud's theory of the actual neuroses by discussing his causal analysis, his explanations of the symptoms of neurasthenia and anxiety, and the role suggestion played in his gathering of facts and in his treatment.

The specific causes

Freud consistently maintained it was easy to show *coitus interruptus* and masturbation caused anxiety neurosis and neurasthenia respectively. At about the time he first began to attract pupils and followers, he stressed his initial discovery had been "easy to make and could be confirmed as often as one liked" (Freud, 1906a, p.272). Some eight years later Freud described the cause-effect relations as:

> a crude fact that springs to the observer's eyes .... I have no doubt that I could repeat the same observations to-day if similar pathological material were still at my disposal. (Freud, 1916-17, pp.385-386)

Ten years later again Freud said "The observations which I made at the time still hold good" (Freud, 1926a, p.110).

Freud's certainty was echoed by Ernest Jones (1911a) who, after his extensive review of the mainly German evidence, concluded that incomplete sexual satisfaction, especially coitus interruptus, had been clearly established as the cause of anxiety neurosis and that that conclusion was "not a matter of psycho-analysis ... it can at any time be tested by means of direct clinical investigation". Despite this confidence, few psycho-analysts since about 1950 have been able to agree with Freud and Jones. Blau (1952), who attempted to resurrect the clinical concept of an actual anxiety neurosis, side-stepped the issue of sexual etiology and cited a number of cases in which sexual factors could not be found. Even more equivocality was expressed by Gediman (1984) who, after accepting that there was a clinical entity entitled to be called actual neurosis, implicitly rejected the sexual etiology, going on to say that it was due to factors in "the broader arena of excitability and frustration in the face of *any kind* of internal and external stimulation of traumatic intensity". Brenner (1953) found no evidence of a relation between anxiety and abnormalities of sexual life. He also observed that most authors who had written on the subject assumed that there was a relation "without offering any independent evidence ... to corroborate it." Among the notable psycho-analytic personalities to whom Brenner directed this reproof were Fenichel and W. Reich. It would seem also to apply to Ikonen and Rechardt (1978) who cite no observations to support their view that deficiently bound libido is the cause of anxiety in actual neurosis. Zetzel (1955a), Rangell (1955), and Waelder (1967b) are among other psycho-analysts who have failed to confirm incomplete sexual gratification as a cause. Concentrating on anxiety neurosis but speaking of actual neurosis generally, Compton

concluded the basis of the whole concept "was spurious to begin with". There is, he added, "no good evidence" for the existence of the actual neuroses as a clinical entity (Compton, 1972b). Holt (1965) remarked, "Today very few clinicians indeed can be found whose experience confirms the etiological sequences Freud thought he saw." Holt also wondered "whether the whole concept was not based on the coincidence of a few chance clinical observations." We shall see that these failures to confirm Freud's observations have rather more methodological significance than Holt allows.

Neurasthenia was a similar failure. Stekel, who seems to have been one of the first of Freud's pupils to disagree with his master on this matter, denied completely any connection between it and masturbation (cited by Federn, 1930 and by Lampl-de Groot, 1950). He took the view that if masturbation was harmful at all it was because of the guilt feelings the attitudes of social disapproval engendered. During the 1910 and 1912 discussions arranged by the Vienna Psycho-analytical Society, a substantial number of those contributing took the same view or one very like it (Ferenczi, 1912/1952b; Tausk, 1912/1951). Freud himself acknowledged there were "significant uncertainties" among the participants in the discussion about the causal role of masturbation (Freud, 1912b). A little later, Brill (1916), one of Freud's earliest translators and his pioneer and major publicist in North America, concluded that his own clinical impression had been confirmed by several investigations showing the physical effects of masturbation to be minimal, even among young children.

Neither has later psycho-analytic opinion supported Freud. Federn (1930) was prepared to confirm what he called Freud's view that cumulative masturbation played a role in "the great majority" of cases but claimed neurasthenia could also have other "exhausting moments" as causes. A. Stern (1930), Fenichel (1945b, p.188), Frankley (1950), Lampl-de Groot (1950), A. Reich (1951), and Hojer-Pedersen (1956) all denied a direct or simple causal link or claimed it was yet to be proved. Psycho-analytic thought had at last caught up with the doubts expressed earlier by such non-analysts as Edes (1904), Meagher (1924, 1936, pp.99-103, 139), and MacCurdy (1923), each of whom had been sympathetic to Freud's views (the last two especially so), but none of whom were convinced by his arguments about neurasthenia.

Two reasons account for the conflict between Freud and his followers. The first is the indeterminacy of the etiological equation. There were no methods for measuring the strengths of any of the factors in the equation; disagreements were bound to occur, especially when the factors were present at low levels of intensity and had to sum with one another to have an effect. Given the widespread prevalence in the community of the specific factors of masturbation and *coitus interruptus*, of the stock noxae, and, presumably, of any pre-condition produced by masturbation, even less

agreement must have occurred. The etiological equation virtually guaranteed dissension among those using it.

Second, not only had Freud's causal analysis confused sufficient and necessary conditions, but it is doubtful he had even identified correctly the necessary conditions of either form of the neurosis. Scrutinising what has survived of Freud's case material shows at least some of the directness and simplicity of his initial causal formulations was based upon the exclusion of cases running counter to the causal rule. Between late 1893 and mid-1894, Freud twice mentioned or implied that incomplete coition could be a cause of *neurasthenia* or a closely related condition in which there was also no anxiety (Masson, 1985, Letter of 6.10.93, Case 4, and Draft F of 18 & 23.8.94, Case 2). In the 1895 paper that relation was denied. It is true that Freud did refer in that paper to *coitus interruptus* as *always* producing in men an admixture of anxiety neurosis with neurasthenia but, unfortunately, that particular qualification overlooked at least one of his male cases of pure anxiety neurosis, that is, one *without* neurasthenic symptoms (Freud, 1895a, pp.113-114. Cf. Masson, 1985, Letter of 6.10.93, Case 3). Much the same point can be made about E. Jones' (1911a) review: he overlooked important papers like that, for example, by Booth (1906), who implicated *coitus interruptus* as causing *neurasthenia* and, as Levin's (1978, pp.131-132) discussion indicates, several of the authorities Jones cited (including Loewenfeld and Krafft-Ebing) had only gone as far as claiming incomplete gratification *sometimes* led to anxiety attacks (Cf. Decker 1977, pp.136-139 for further consideration of Loewenfeld and other similar views). Since the causal scheme had never been fully congruent with the case material, it is hardly surprising that later workers were unable to confirm it.

The central symptom of neurasthenia

There was also disagreement about how neurasthenic symptoms were produced. Ferenczi (1908/1950) seems to have accepted implicitly the inadequacy of Freud's explanation in his attributing the effects to the strain successful masturbation put on the sources of neuro-psychic energy: "It is comprehensible that such a willed gratification requires a greater consumption of energy than the almost unconscious act of coitus" (ibid). Weakness after masturbation was thus to be expected. Ferenczi also noted the weakness was sometimes most marked the day following masturbation. He thought a summation of that weakness with others that had been produced earlier had occurred. Were the process to be continued over a long enough period a chronic weakness would result (Ferenczi, 1912/1952b). The symptoms of general irritability, paraesthesias, and other sensitivities were to be accounted for by supposing masturbation did not discharge all the excitation. What was left behind kept the nervous system in a state of heightened excitability. Ingenious as this explanation was, it was bound to fail. Freud did not restrict the effects to "excessive onanism mostly continued long after puberty" as Ferenczi (1908/1950) had and, as A. Stern (1930) noted, Ferenczi did not define what was to be

meant by excessive. Neither had Freud argued neurasthenia was to be found only in those currently practising masturbation, which was all Ferenczi's explanation really accounted for. Finally, the notion that the effects of repeated masturbation somehow summed to cause neurasthenia merely described what had to be explained.

Among the "significant uncertainties" that he saw as having emerged from the 1912 symposium, Freud included the manner in which masturbation produced its effects and its etiological relation to the actual neuroses. He concluded that the mechanism by which it produced organic damage was "unknown" (Freud, 1912b, pp.246, 251). In the years that followed he never returned to the problem and left it unsolved.

The sources of anxiety

We have already noted Freud revised his ideas about the source of the anxiety in anxiety neurosis almost as soon as his original explanation had been put forward. That revision was but the first of three. According to the first, *psychical* sexual excitation, or libido, rather than *somatic* sexual excitation, was the source of anxiety. We have seen the only advantage of this mentalistic re-conceptualisation was to enable an 'in principle' explanation of the psychological symptoms and that it is less successful with the physical symptoms. Proposing psychological causes for *both* the psychoneuroses and the actual neuroses also blurs one of the previously important distinctions between them. A partial solution to some of these difficulties, one which Freud seems to have considered, is to think of anxiety and libido having quite different sources and being quite different from each other. During the late 1890's Freud tried hard to work into his theory the notion that repression was based on an organic process capable of producing repression automatically, without the subject's awareness. He thought this might occur if an attempted revival of the memory of an abandoned childhood sexual activity produced a feeling of disgust rather than the original pleasurable libidinal affect. Disgust would then prevent the memory of the earlier activity from becoming conscious. The mechanism required there to be a strong associative connection between libido and disgust. Freud thought the link lay in the phylogenetic history of mankind: after man adopted the erect posture, the readily obtained pleasures of the sight and smell of the sexual and excretory organs of the opposite sex were abandoned and disgust substituted. Toward the end of 1897 Freud therefore wrote to Fliess:

> I have resolved, then, henceforth to regard as separate factors what causes libido and what causes anxiety. (Masson, 1985, Letter of 14.11.97)

Because most of the rest of this letter deals with the organic basis of repression Freud's proposal is hardly "without any apparent connection" with it, as the Editor's Introduction to *Inhibitions, symptoms, and anxiety* states (Freud, 1926a, p.79). In fact, in order to explain some of the symptoms of the hysterical patient known as Dora, Freud very soon went

on to postulate a connection between libido and disgust which did not involve anxiety. Dora had reacted to an adolescent sexual experience with disgust. Freud assumed the experience had begun to revive the memory of a childhood sexual activity she had given up and the attempted revival had produced the disgust (Freud, 1905a, pp.28-32, 46-55, 85-86). But, in the letter to Fliess, Freud complained there was an essential obscurity "in the nature of the change by which the internal sensation of need becomes the sensation of disgust". We will see in Chapters 9 and 10 that this continuing obscurity in the concept prevented him from pursuing the idea fully and applying it - at least in that form - to cases other than Dora's.

Freud's second revision was based on the supposition that anxiety was produced by the build up of toxic substances in the bloodstream. It tended to be directed toward explaining the physical symptoms of the anxiety attack rather than the mental. Chemical substances distributed throughout the body were assumed to decompose and produce libido when the individual was sexually aroused. When libidinal discharge through orgasm was inadequate, the decomposed substances acted as toxins. Anxiety was now not so much a psychical transformation of libido, as the original and first revision had held, but a poisoning brought about by a faulty metabolism of the chemical elements of sexual life.

While this second revision was developed between 1900 and 1906, the basic ideas on which it was based had been expressed in the correspondence with Fliess during 1896 (Masson, 1985, Letters of 1.3.96, 2.4.96, 30.6.96, 6.12.96 & 17.12.96). Taken together, the correspondence reveals that Freud had "always conceived of the processes in anxiety neuroses, as in the neuroses in general, as an intoxication"; that he believed the differences between the symptoms of anxiety neurosis and neurasthenia required the postulation of two different toxic substances; and that the two hypothetical substances proposed by Fliess to explain the periodicities of male and female sexuality might be what he was searching for. The comparison of the neuroses with auto-intoxications then lapsed until 1905. Freud then made it public in the course of a theoretical digression about the patient Dora:

> No one, probably, will be inclined to deny the sexual function the character of an organic factor, and it is the sexual function that I look upon as the foundation of hysteria and of the psychoneuroses in general. No theory of sexual life will, I suspect, be able to avoid assuming the existence of some definite sexual substances having an excitant action. Indeed, of all the clinical pictures which we meet with in clinical medicine, it is the phenomena of intoxication and abstinence in connection with the use of certain chronic poisons that most closely resemble the genuine psychoneuroses. (Freud, 1905a, p.113)

This rather cryptic remark, which may date from 1901, the year in which a manuscript of the Dora case was prepared, was expanded in the general theory of sexuality outlined in the first edition of the *Three Essays*:

> It may be supposed that, as a result of an appropriate stimulation of erotogenic zones, or in other circumstances that are accompanied by an onset of sexual excitation, some substance that is disseminated generally throughout the organism becomes decomposed and the products of its decomposition give rise to a specific stimulus which acts on the reproductive organs or upon a spinal centre related to them. (We are already familiar with the fact that other toxic substances, introduced into the body from outside, can bring about a similar transformation of a toxic condition into a stimulus acting on a particular organ.) The question of what interplay arises in the course of the sexual processes between the effects of purely toxic stimuli and of physiological ones cannot be treated, even hypothetically, in the present state of our knowledge. I may add that I attach no importance to this particular hypothesis and should be ready to abandon it at once in favour of another, provided that its fundamental nature remained unchanged - that is, the emphasis which it lays upon sexual chemistry. For this apparently arbitrary supposition is supported by a fact which has received little attention but deserves the closest consideration. The neuroses, which can be derived only from disturbances of sexual life, show the greatest clinical similarity to the phenomena of intoxication and abstinence that arise from the habitual use of toxic, pleasure-producing substances (alkaloids). (Freud, 1905b, p.216 and n.1. This passage is a reconstruction of that which appeared in the first edition, MBM)

A subtle change to the role attributed to sexual discharge was now required and Freud made it without hesitation.

In a discussion of the actual neuroses contemporaneous with this chemical theory of sexuality, Freud repeated that specific sexual practices were associated with the two neuroses but modified their supposed effects by saying they resulted in "insufficient discharge of the libido" (Freud, 1906a, p.272). The notion that there was an *insufficient* libidinal discharge was quite new. It is, of course, a necessary consequence of regarding libido as the product of the *breakdown* of sexual chemicals. Were all the libido discharged there would be no by-products left behind to act as toxins. This new process contradicts the thesis that masturbation caused neurasthenia by discharging the libido *too fully*. But, for neither neurosis was there empirical evidence about the effectiveness of the discharge.

Another quite unsatisfactory aspect of Freud's revision is the alleged similarity between the neuroses, auto-intoxications, and states of alkaloid withdrawal. By the end of the last century experimental physiological work had established the role of the thyroid gland and it appeared to support the view that Basedow's disease, or thyrotoxicosis, was due to excessive secretion of thyroid toxins (Levin, 1978, pp.184-189). However, Freud's parallel between Basedow's disease and the neuroses was based on a superficial resemblance between the psychological symptoms of fatigue, irritability, and emotionality found in the two conditions. And whereas the substances in Basedow's disease were identifiable, the two sexual substances Freud's revision required were, are, and seem likely forever to remain entirely hypothetical.

Freud's third revision formed part of a more general rethinking of the relation between anxiety and repression. Its major consequence was to declare the matter a non-problem. Originally Freud had supposed that when repression had been unsuccessful, and the impulse attempted re-entry to consciousness, the attempt would be accompanied by the affect of anxiety. What Freud was now to propose was a reversal of this relation: repression was the attempt to do away with the affect of anxiety that had been produced by feelings of helplessness, excessive libidinal tension, and like threats to the ego. After announcing the new theory Freud remarked:

> It is still an undeniable fact that in sexual abstinence, in improper interference with the course of sexual excitation or if the latter is diverted from being worked over psychically, anxiety arises directly out of libido; in other words, that the ego is reduced to a state of helplessness in the face of an excessive tension due to need ... *though the matter is of little importance*, it is very possible that what finds discharge in the generating of anxiety is precisely the surplus of unutilized libido. (Freud, 1926a, p.141. My emphasis, MBM)

Some years later, in his final comments on anxiety neurosis, Freud reaffirmed this point: "the question of what the material is out of which anxiety is made loses interest" (Freud, 1933b, p.85). After having formed part of a very central topic for over thirty years, one of consuming theoretical interest, Freud finally left the source of anxiety unexplained. Further, he remarked, "we now understand the apparently complicated cases of the generation of anxiety better than those [like anxiety neurosis] which were considered simple" (ibid). This extraordinary conclusion, perhaps better than any other single remark of Freud's, reflects the problems anxiety posed for his theory.

No better summing up can be given than that provided by Kaplan's pithy comment. Freud's work on the actual neuroses:

> began in certainty but went on to become elusive to observation and something of a trial to theory. (Kaplan, D. M. 1984)

Eventually it had expired, becoming:

> one might say, a ghost of its former self, a haunting rather than a palpable concern about which it no longer seems necessary to have views. (op. cit)

Another point should be added to Kaplan's *Decline and Fall.* Freud neither investigated nor treated any cases of actual neuroses after about 1900 (Freud, 1925a, pp.25-26). New clinical observations on the actual neuroses formed no part of the basis for the second and third revisions: they resulted from changed interpretations of the original stock of facts and were motivated by attempts to produce consistency, either between the explanations of the two neuroses, or between the mechanisms of the actual neuroses and the psychoneuroses.

Suggestion, facts, and treatment

There is clear evidence that suggestion played a major role in Freud's identifying sexual practices as the causes of neurasthenia. He put his neurasthenic patients under a good deal of pressure to admit to masturbation:

> Having diagnosed a case of neurasthenic neurosis with certainty and having classified its symptoms correctly, we are in a position to translate the symptomatology into aetiology; and we may then *boldly demand confirmation* of our suspicions from the patient. We must not be led astray by initial denials. If we keep firmly to what we have inferred, we shall in the end *conquer every resistance* by emphasizing the *unshakeable* nature of our *convictions*. (Freud, 1898a, p.269. My emphasis, MBM)

Freud rejected the possibility of so obtaining false information:

> Moreover, the idea that one might, by one's insistence, cause a patient who is psychically normal to accuse himself falsely of sexual misdemeanours - such an idea may safely be disregarded as an imaginary danger. If one proceeds in this manner with one's patients, one also gains the conviction that, so far as the theory of the sexual aetiology of neurasthenia is concerned, there are *no negative cases*. (ibid. My emphasis, MBM)

His own colleagues as well as many non-analysts had no such success - they found plenty of negative cases. What Freud called 'false accusation' readily accounts for the discrepancy.

It is also possible Freud misinterpreted the disappearance of symptoms after the patient had adopted a normal sexual life. He simply took it to be a natural consequence of the removal of the cause. Against this Oerlemans (1949, pp.23-24) has argued that the failure of other analysts to obtain cures using the same 'therapy' is consistent with Freud's having been produced by suggestion. Freud's authority and conviction played a much greater role in the investigation and treatment of neurasthenia than he was prepared to grant and very probably the same was true of anxiety neurosis.

CHILDHOOD SEDUCTION AND PSYCHONEUROSES

During 1896 Freud put forward a causal hypothesis which, together with its associated concepts, became known as the theory of childhood seduction. According to it, the trauma causing psychoneurotic symptoms were always sexual, always occurred in childhood, and always involved perverse sexual activities, usually forced on the child by an adult (Freud, 1896c). In private correspondence to Fliess, Freud frequently implicated the patient's father as the seducer (Masson, 1985, Letters of 6.12.96, 3.1.97, 12.1.97, 24.1.97), including his own father (op. cit., & 8.2.97), eventually going so far as to speak directly of a "paternal etiology" for hysteria (op. cit., Letters of 28.4.97, 2.5.97, 31.5.97, & 22.6.97) and of his own father as "one of these perverts" (op. cit., Letters of 28.4.97). The theory had a short life. By

August, Freud was "tormented by grave doubts" (op. cit., Letter of 14.8.97) and, terming the theory his 'neurotica', he wrote to Fliess five weeks later:

> And now I want to confide in you immediately the great secret that has been slowly dawning on me in the last few months. I no longer believe in my *neurotica*. (op. cit., Letter of 21.9.97)

The first of the reasons Freud gave Fliess for doubting the "memories" were of real events was that even when all the memories had been recovered and abreacted it had not been possible to bring "a single analysis" to a definite and successful conclusion (ibid.). The second reason was that a specific causal factor needing the cooperation of pre-conditions for its effect had to be more widespread than those effects themselves. Therefore there would have to be very many more cases of childhood seduction than of hysteria, a fact that would hardly have escaped public notice. Freud's third reason was that there was no indication of reality in the unconscious. The unconscious that had mistaken the recollection for the "memory" of a real event. The fourth was that even in the uncontrolled thinking of the most severely psychotic patients, when the mechanism of repression was assumed to be in abeyance, no traces of seduction memories were found.

Although the patient's associations seemed to have led to the origins of the neurosis, to what Freud referred to metaphorically as a veritable source of the Nile, the memories were false. There had been no seduction at all, let alone by a sexually perverse father.

In the years following the collapse of the seduction theory Freud came to the view that the seduction "memory" had not been derived from the experiences of the child in the external world and eventually placed its origins within the child. He supposed that what had been recalled was a fantasy expressing the wish for the satisfaction of a perverse childhood sexual impulse. With this emphasis on the role of inner impulses and wished for gratifications Freud broke with *all* those of his predecessors who had sought for the origins of symptoms in the real experiences of the patient. The step is such a momentous one that it is appropriate to consider whether it was justified. Had Freud completely excluded the possibility that the fantasy had an external basis? As with the actual neuroses there is little point in an appeal to the facts. Levin observes that three of Freud's four reasons for giving up the theory:

> could have been cited months earlier, while the fourth bears no relation to any comments previously made ... and is not pursued in subsequent discussions so that there is no clear indication of why the rejection of the seduction theory came at this particular time. (Levin, 1978, p.200)

It is pretty obvious, as M. I. Klein (1981) notes, that Freud was merely reinterpreting the data he already had.

Now, it is important to note that what was recalled from childhood appeared to be the memory of a real *sexual* experience. No mere erotic feeling or pleasurable sensation had come to mind. What the child seemed to remember was sexual and perverse. Each of the three papers in which some part of the seduction theory was outlined used phrases such as "stimulation of the genitals, coitus-like acts, and so on" (Freud, 1896c, p.206; Cf. 1896a, p.152 and 1896b, p.163). The first two of the papers implied the activity was perverse, using words like "repulsive", "brute abuse", and "positively revolting" to characterise it. The last of the papers was quite explicit. The acts had included:

> all the abuses known to debauched and impotent persons, among whom the buccal cavity and the rectum are misused for sexual purposes. (Freud, 1896c, p.214)

Within the twelve months following this paper Freud claimed perverse sexual activity initiated by the patient's father was always involved.

How Freud came to attribute the role of seducer to the father is not at all clear. His published evidence does not support the thesis. In the first two papers, seven of the thirteen seductions reported were of *children by children* (Freud, 1896a, p.152; 1896b, p.164). Most of the remainder were said to have been initiated by *adults unrelated to the child*: servants, tutors, governesses, and nursemaids. About four months later, in the third paper, *adults, including close relatives*, were said to comprise "the much more numerous" group of seducers (Freud, 1896c, p.208). Now, since the sample reported on in the third paper was the original thirteen cases together with five new ones, the number of adult seducers could not have totalled more than eleven of the eighteen cases, even if an adult had been involved in all five of the new ones. A theoretical maximum of eleven adult seducers as compared with seven child-child seductions hardly warrants referring to them as "much more numerous".

Within about the next twelve months the early cases must have been re-interpreted or treated further for Freud to have been able to write to Fliess about the paternal etiology of the condition. In telling Fliess of the collapse of the theory, he remarked that his patients had blamed their fathers "in all cases" (Masson, 1985, Letter of 21.9.97). Like his conclusions about the 'causes' of the actual neuroses, he retained this impression, at least about female hysterical patients, for many years (Freud, 1925a, pp.33-34; 1933b, p.120). That there had been successive transformations of children into adults unrelated to them, of those adults into relatives, and of those relatives into fathers seemed to have been forgotten. Only the image of the fantastic seducer father remained, a transformation that raises serious questions about the accuracy of Freud's reporting (Cioffi, 1974; Schimek, 1987).

When publicly announcing the childhood seduction theory, Freud considered and rejected the possibility that he had influenced the

recollections of his patients:

> It is less easy to refute the idea that the doctor forces reminiscences of this sort on the patient, that he influences him by suggestion to imagine and reproduce them. Nevertheless it appears to me ... untenable. I have never yet succeeded in forcing on a patient a scene I was expecting to find, in such a way that he seemed to be living through it with all the appropriate feelings. (Freud, 1896c, pp.204-205)

Here Freud was repeating the essence of the claim, previously made in the *Studies on Hysteria*, that the content of the recollections could not be influenced (Breuer and Freud, 1895, p.295). And, as in that instance, it can be seen the only mode of influence Freud considered was direct suggestion. He seems not to have thought indirect, unconscious suggestive influences might be important.

Freud's failure to consider these subtle unconscious factors is, of course, consistent with his view of determinism and, as we shall now see, with his notion of a neurosis having a logical and associative structure. The concept of a logical and associative structure is a development of the pathogenic memory structure considered in Chapter 4. Like the earlier concept, it too is based firmly on Meynert's physiological associationism.

Although Freud nowhere formally defined or described what he meant by the logical and associative structure a neurosis had, his usage makes it clear it encompassed the totality of associations between the symptoms and the causal memories. He appears to have first used the concept when describing the memories of childhood seduction as being represented in the cases of hysteria by:

> a host of symptoms and of special features which could be accounted for in no other way; it is peremptorily called for [governed] by the subtle but solid interconnections of the intrinsic structure of the neurosis. (Freud, 1896a, p.153)

The "solid interconnections" were associations, that is, connections of content, because in:

> the relationship of the infantile scenes to the content of the whole of the rest of the case history ... the contents of the infantile scenes turn out to be indispensable supplements to the associative and logical framework of the neurosis. (Freud, 1896c, p.205)

Again the interconnections were also causal connections:

> the aetiological pretensions of the infantile scenes rest ... above all, on the evidence of there being associative and logical ties between those scenes and the hysterical symptoms. (op. cit., p.210)

Subtle peculiarities of the content of the symptoms were explained by the content of the infantile memories demanded by the intrinsic structure. That

content completed the logical and associative structure and the causal role of the memory was inferred from its logical and associative ties with the symptom.

Of the memories in the logical structure, the most important was that at the nucleus. Freud described it as having two attributes: suitable determining quality and appropriate traumatic force. Neither was defined clearly or positively. An unsuitable determinant was one having a content bearing "no relation to the nature of the symptom" and traumatic force was defined negatively, as the opposite of "an impression which is normally innocuous and incapable as a rule of producing any effect" (op. cit., p.194). Freud gave an hypothetical example that expanded on these definitions only by implication:

> Let us suppose that the symptom under consideration is hysterical vomiting; in that case we shall feel that we have been able to understand its causation (except for a certain residue) if the analysis traces the symptom back to an experience which *justifiably produced a high amount of disgust* - for instance, the sight of a decomposing dead body. But if, instead of this, the analysis shows us that the vomiting arose from a great fright, e.g. from a railway accident, we shall feel dissatisfied and will have to ask ourselves how it is that the fright has led to the particular symptom of vomiting. This derivation lacks *suitability as a determinant.* We shall have another instance of an insufficient explanation if the vomiting is supposed to have arisen from, let us say, eating a fruit which had partly gone bad. Here, it is true, the vomiting *is* determined by disgust, but we cannot understand how, in this instance, the disgust could have become so powerful as to be perpetuated in a hysterical symptom; the experience lacks *traumatic force.* (op. cit., pp.193-194)

The difference in traumatic force between the eating of a bad apple and the viewing of a decomposing corpse is readily sensed even if it cannot be expressed with any precision.

Similarly, the determining quality is quite apparent: seeing a decomposing corpse might well produce a tendency to vomit that could be perpetuated as a symptom. Freud's use of this resemblance in content to index the determining quality of the scene, that is, its causal relevance, is a development of the point implicit in Charcot that the sensory content of the symptom is based on and reflects the sensations experienced in the trauma. It is also based entirely on the formal identity of an association and a causal connection.

In summary, Freud used the concept of the logical and associative structure of a neurosis to refer to the complex of associational pathways running from the symptom to the memory of the core trauma to related memories. Since any one idea in the pathway implied the presence of another with which it had been previously experienced, the associative paths revealed the logical relations between the ideas and the symptoms and between the ideas themselves. In turn, the logical relations revealed

the causal or deterministic connections. Associating to the ideas generated by the symptom necessarily led to the traumatic memory at the core of the structure where the memory was recognisable by its traumatic force and determining quality. The process leading from one idea to another had its own determinants and could not be influenced by the therapist (apart from helping to overcome resistances temporarily impeding the process). The memories recovered during treatment necessarily allowed an authentic reconstruction of the development of the neurosis. If a memory of a childhood seduction was found at the core of the memory structure a real seduction must have occurred (Cf. Jacobsen and Steele, 1979).

UNINTENTIONAL INFLUENCES IN PSYCHOTHERAPY

Experimental and observational evidence about psychotherapy shows the therapist can unintentionally influence the extent to which the patient talks, the extent to which emotional topics will be explored, the kinds of problems discussed, and the content of any memories recalled. This evidence is consistent with Freud having unconsciously influenced the recollections of his patients and that that influence created the "memories" of a seduction in childhood.

The work of Matarazzo and his colleagues, usefully summarised in Matarazzo (1962), shows the formal characteristics of psychotherapeutic interviews are determined by the interviewer. These characteristics, which include whether subjects say anything or not and, if they do speak, the length of their remarks, are functions of the verbalisations of the interviewer. Matarazzo devised a standardised interview in which subjects were free to talk about anything they chose but in which interviewers varied their remarks according to a pre-arranged schedule. During one part of the interview they always responded to the subject's remarks, in another they remained silent, while in yet another they interrupted. Across all the groups of normal and abnormal subjects studied, interviewer silence increased the subject's silence and decreased the average length of each remark for all but schizophrenic subjects. The decrease in length of remarks was greater in the interruption period than for the silence condition. The findings are highly reliable: the extent to which the subject talks is a function of the interviewer.

Colby (1960, 1961) has shown much the same effects as Matarazzo even when the subject is only free associating: how much activity takes place and what its content is, is a function of the mere presence and sex of the experimenter. Although in their study of the psychotherapeutic interview Lennard and Bernstein (1960) stress the contribution the patient makes to the pattern of interaction that develops with the therapist, their findings are generally consistent with those of Matarazzo - the formal aspects of psychotherapy are very much a function of the therapist.

Direction may also be given to the patient's behaviour through the therapist responding differently to different aspects of it. In an experimental analogue of a client-centred interview, Truax and Carkhuff (1965) demonstrated the therapist could control the extent to which the patient explored his own problems. In the middle third of the interview the therapist simply refrained from making whatever was the best response the therapeutic rules would otherwise have dictated and within that period self-exploration declined. Truax (1968) later analysed tape recordings of group psychotherapy sessions and showed self-exploration increased when the therapist differentially responded to it. Isaacs and Haggard (1966) have similarly noted the extent to which patients discuss an affect-laden topic increases when therapists comment affectively upon patients' verbalisations. As compared with non-affective comments, the patients' subsequent verbalisations were more affective and contained many more references to the factors presumed to have influenced their lives and to have caused their problems. What is especially important is that delayed returns to the topics mentioned by the interviewer, although small in number, were much greater when those mentions were affective. Consequently the patient's discussion of some topics appeared 'spontaneous' and unconnected with the therapist's intervention. The more the therapist encourages self-exploration and the discussion of affective topics, the more probable it is that topics of the kind Freud regarded as traumatic will come to dominate the therapeutic interchange.

Particular kinds of content can also come to be emphasised by the therapist. The early work of Greenspoon (1955) on what came to be called verbal conditioning shows how it may be given. Greenspoon's experiment required the subject to utter single words over a fifty minute period. During the first twenty-five minutes each plural noun was 'reinforced' by the experimenter murmuring "mmm-hmm". As compared with the second half, in which the experimenter remained silent, the first period produced many more plural nouns. The narrow class of plural nouns rather than the wide class of non-plurals were found to be more easily influenced. The simplicity of Greenspoon's findings are deceptive. Later work, reviewed extensively by Krasner (1958), Salzinger (1959), Greenspoon (1962) and Kanfer (1968), showed the extent of the influence to be dependent upon such complex conditions as the social setting of the experiment, the personality characteristics of subject and experimenter, the expectations and awareness of the subject, and the particular response made by the experimenter. Nevertheless the fact of influence is reliably established.

Simple verbal content may be similarly influenced in clinical-like settings. Beginning with the work of Fahmy (1953, cited in Greenspoon, 1962) a number of studies has shown responses equivalent to "mmm-hmm" influence the names given ink blot shapes and the content of stories the subject makes up in response to pictures shown him. Subtle influences from the therapist may thus determine the content of a number of types of simple verbal responses given by patients.

Although these effects on simple responses is of interest it is more important to know if they can also occur with more complex psychological functions in clinical and quasi-clinical settings. Quay's (1959) demonstration that the content of recalled childhood memories could be influenced by the experimenter's verbalisations is the most relevant of such investigations. Two groups of subjects were seen for two one half hour periods during which each subject was asked to recall events from his childhood. In the first ten minutes of the first half hour, the experimenter said nothing. Then, for one group, he murmured "uh-huh" each time the subject recollected an event involving his family. For the other group, the verbalisation was made after each recollection that did not involve the subject's family. As compared with the first ten-minute period, both groups increased the number of memories of the type that had been followed by the experimenter's "uh-huh". The proportion of memories for family events was, however, higher in the first group than in the second.

In many verbal conditioning experiments proper, as well as in the other investigations I have mentioned, subjects appeared either to be unaware that cues from the experimenter or interviewer shaped their responses or, if they became aware, they rarely detected the direction the cues were meant to give. To them, their verbal productions seemed spontaneous. It was also noted by Matarazzo that the observers of his standardised interview were unable to detect the fairly gross changes in the interviewers' behaviour.

Therapists themselves may not realise they are guiding the patient's thoughts in a particular direction. Murray (1956) and Truax (1966) have separately analysed individual therapy sessions conducted by Carl Rogers, the founder of the client-centred method of therapy, who believes therapists should neither approve nor disapprove of the topics the client (patient) raises for discussion. Both Murray and Truax found Rogers exerted considerable selective influence without his apparently being aware of it. Murray categorised Rogers' statements in response to the client's remarks as approval, disapproval, or neutral. The proportions of these responses for each of the four main categories of problems first mentioned by the patient (sex, affection, dependence, and independence) was then examined. Rather than Rogers' responses being equally represented for each problem, Murray found Rogers had disapproved of nearly all the sexual discussion and approved all of that in which the patient indicated his need for independence. As therapy progressed sexual matters were raised less frequently and moves toward independence more frequently. Other problem areas were affected similarly: those eliciting Rogers' approval were discussed more frequently than those of which he disapproved. Truax examined a random selection of verbal interactions between Rogers and another client. Each interaction consisted of a therapist statement followed by a client statement and a second therapist statement. Rogers' statements were evaluated according to the degree to which they possessed the qualities regarded by Rogers himself as facilitating therapeutic progress:

empathic understanding, acceptance, and non-directiveness. For seven of the nine classes of patient behaviour studied, changes in the predicted direction were brought about by Rogers' selective responding. Thus the clarity of the client's expression and the similarity of his style of expression to that of Rogers, the degree of insight he showed and the extent to which he discriminated among his earlier experiences and feelings were functions of differences in Rogers' responses. Rogers' qualities functioned, therefore, as reinforcers. From these two studies it is clear that what patients talk about, how they talk about it, and the degree of understanding that develops are functions of therapists' differential responsiveness, even when therapists are unaware of responding differently.

The possibility that the differential responsiveness of the therapist is conditioned by the characteristics of the patient may be ruled out. Truax's and Mitchell's (1971) review shows the therapist, not the patient, to be the primary determinant of therapeutic interactions. One of the strongest lines of evidence is that therapists are remarkably consistent in their behaviour from one patient to another and do not modify their behaviour as patient characteristics vary. While experimental investigations do show that pseudoclients can manipulate the behaviours of therapists, the effects are less marked on experienced and skilled therapists, that is, on the kinds of therapists actually in practice.

Another important type of evidence is suggested by Cartwright's (1966) study comparing patient changes in client-centred and psychoanalytic therapy. One member of each of two matched pairs of patients was treated by client-centred methods, the others by psycho-analytic or psycho-analytically oriented methods. If the patient determined the therapeutic interactions one would expect to find similar interactions for the matched patients in the two different treatment situations. In fact, with the partial exception of some of the behaviours of the psycho-analytically oriented therapist who actually used some client-centred methods as well, the therapists behaved in accord with the tenets of their schools. And so did the patients. Patients thus behave differently according to the different influences to which they are exposed. Although Cartwright's study is frequently cited as showing little or no difference in the patients' behaviours, it is clear if the data from a single *ad hoc* and unvalidated scale is disregarded, the patients behaved as their therapists would have wished them to. Heine's (1953) earlier and more extensive comparison of psychoanalytic, client centered, and Adlerian therapists had also shown the patient's experiencing of therapy to be a function of the therapist's orientation. Clearly the therapist, not the client, is the primary influence on the therapeutic process. Similarly, the psycho-analyst Marmor has remarked of the depth psychology therapies:

> depending upon the point of view of the analyst, the patients of each school seem to bring up precisely the kind of phenomenological data which confirm the theories and interpretations of their analysts! Thus each theory

> tends to be self-validating. Freudians elicit material about the Oedipus Complex and castration anxiety, Jungians about archetypes, Rankians about separation anxiety, Adlerians about masculine strivings and feelings of inferiority, Horneyites about idealised images, Sullivanians about disturbed inter-personal relationships, etc ... What the analyst shows interest in, the kinds of questions he asks, the kind of data he chooses to react to or ignore, and the interpretations he makes, all exert a subtle but significant suggestive impact upon the patient to bring forth certain kinds of data in preference to others. (Marmor, 1962)

However, this important psycho-analytic opinion and the supporting research evidence does not go as far as showing that that impact results in new creations such as fabricated or pseudo-memories.

Fortunately, some instances are known of situations not unlike those of psychotherapy in which false memories have been created by the dominant partner. Perhaps the best documented is that of Virginia Tighe (''Ruth Simmons'') who recalled a number of incidents in her previous incarnation as one Bridey Murphy, an Irish colleen. The recollections were fabricated during sessions with an amateur hypnotist attempting to get her to recall the earliest possible events in her life (M. Bernstein, 1956). The false recollections were based on events in her own childhood, such as her training in Irish dancing and ''stage-Irish'' monologues, and on her elementary knowledge of some events in Irish history (W. White, Hartzell, and Smith, 1956). Bernstein's insistence led to the memories of these real events being worked up into pseudo-memories of a previous incarnation. Although the content of the pseudo-memories was old, their elaboration was as recent as Bernstein's hypnosis. How subtle the process can be has been shown by Kampman's and Hirvenoja's (1978) study of two cases of hypnotically recalled earlier incarnations ('previous lives'). Seven years after the incarnation stories, the subjects were re-hypnotised and details known only to the 'incarnation' were traced to information acquired in the subject's actual childhood. Much of the acquisition had been casual or incidental. For example, one subject had learned the melody and modernised medieval English words of a song while ruffling through the pages of book in a library.

Memories of real past events are not the only sources of false recollections. For example, in the 'auditing' or treatment offered in the pseudo-science of scientology the subject produces ''memories'' that draw on a content not known prior to contact with scientology. Beginners in scientology are treated by those more knowledgeable. Adepts believe the individual to have had a continuous spiritual existence over [US] seventy trillion years and to be currently inhabiting a body that is the end result of a long biological evolutionary process. During auditing certain 'traumatic' memories are recalled some of events having happened to the individual as a spiritual being and others to the precursors of the body in which the spirit now resides. Among the former are recollections of unusual, extreme, and quite specific forms of electronic torture, among the latter are recollections

of life as an atom, a unicellular organism, a clam, an ape, a Piltdown man (sic), other human beings in previous lives, and a prenatal being. Scientologists expect the recollection of these events will relieve various problems and complaints. After many hours of "auditing", neophytes characteristically recall having had just these experiences, at similar periods, and in the same way (Hubbard, 1952a, 1952b). The very peculiar content of the experiences speaks against their having been known beforehand. They are recent elaborations of newly acquired content.

The Bridey Murphy and scientology cases also illustrate two different modes of action of demand characteristics. Virginia Tighe had to discern what was required of her from the not-very-well-disguised expectations contained in Bernstein's suggestions. The neophyte scientologist's task is easier for it is probable 'auditors' do not disguise their expectations at all. Additionally, through study of scientological doctrines, the subject has direct knowledge of what the recollections should be. Once subjects learn what is being demanded of them, their 'recollections' adapt accordingly.

Freud's treatment has some important similarities with scientology auditing and the investigation of Mrs. Tighe's early life. All three involve the emotional and intellectual dependence of a subject upon a mentor and an interaction allowing the transmission of the beliefs and expectations of the investigator. Like Bernstein and the scientology auditors, Freud had expectations about the content of the memories to be recovered and the way they were related to one another. There is no reason why the content acquired from these expectations could not have been elaborated into pseudo-memories and integrated with existing memories of real events. While Mrs Tighe's "memories" do not seem to have been particularly vivid, well-structured, or tightly integrated with her real memories, this is not so with the "memories" fabricated in scientology auditing; those memories are of complexly elaborated events, recalled in extremely vivid detail. They are also integrated with each other and with the subject's real memories. Although it may not be possible to explain how this happens it obviously occurs. It is also evident that an essential pre-condition for integration is that the subject divine the investigator's expectations.

FREUD'S EXPECTATIONS AND THE CAUSES OF HYSTERIA

Only at the end of the period that began in late 1892 and ended in late 1896 did Freud's patients come to report their nuclear traumatic memories as being of sexual seductions in childhood. At the beginning of the period Freud had three specific expectations: first, all neuroses might be caused by sexual factors; second, a given disorder would always be caused by the one specific factor; third, a traumatic memory would be recognisable by the effects of its abreaction. These expectations affected the recollections of his patients in different ways. The first determined the type of content the memory of a traumatic event had to have, while the second and third set the

standards by which the relevance of a given memory was judged.

I have already shown in Chapter 5 how, before he began work on hysteria, Freud convinced himself he had established an exclusively sexual etiology for the actual neuroses. That work was so methodologically defective, however, it resulted only in an unjustified expectation that all neuroses would have sexual causes. Although Freud several times denied he had expected the memories of trauma in hysteria to be of sexual experiences (for example, in Freud, 1896c, p.199) it is clear from the correspondence with Fliess cited in Chapter 5 that he had been attempting to construct a general theory of the neuroses on a sexual basis from at least early 1894 and possibly from as early as twelve to eighteen months before. During the same period Freud concerned himself with the possibility that the psychoneuroses might also have sexual causes. For example, that hysteria was sometimes found together with neurasthenia was attributed to a suppression of accompanying sexual affects (op. cit., Drafts A and B).

That Freud expected similar kinds of phenomena to have similar causes is apparent in his 1888 defence of Charcot against the imputation that the phenomena of hypnosis and hysteria produced at the Salpêtrière had resulted from unconscious suggestion. Unconscious influences necessarily varied from one investigator to another and could produce only variable phenomena (Freud, 1888c, pp.77-80). Freud's argument is the obverse of the deterministic view that all things and events are the regular and lawful consequence of their causes or determinants. Freud's search for the causes of the actual neuroses was clearly based on this same deterministic view: the two kinds of symptoms required two specific causes (Freud, 1895a, pp.91, 106, 109, 113). One of Freud's later discussions of the causes of hysteria is consistent with his also looking for uniform causes of it from the beginning. Freud asked where the chains of associations terminated:

> Do they perhaps lead to experiences which are in some way alike, either in their content or the time of life at which they occur, so that we may discern *in these universally similar factors* the aetiology of hysteria of which we are in search? (Freud, 1896c, pp.197-198. My emphasis, MBM)

Now, as early as January, 1894 Freud had concluded that the chief causes of hysteria and the only causes of obsessions were sexual traumas (Freud, 1894, pp.47, 52) and by the May had listed "The thesis of specificity" [of etiology] as a topic in the outline of a projected major work that was to cover all the neuroses (Masson, 1985, Draft D, possibly of 21.5.94). From the beginning Freud may well have expected similarities in the content and/or time of occurrence of the sexual traumas reported by his patients.

Freud's expectation that the effect of the abreaction of a given memory might be used to assess its contribution to the symptom also appears to have developed early and to have grown out of the conviction that the abreaction of a memory would always remove any symptom it maintained. Already in the case of Anna O. Breuer had stressed that whenever the

accumulated stimuli had been given verbal utterance the symptom disappeared. In the *Preliminary Communication*, Breuer and Freud (1893, pp.6-7) claimed the symptom would be untouched if the recollection was without affect. Freud's earliest lecture on hysteria endorsed this view (Freud, 1893a, p.35). About a year later (Freud, 1894, p.47), and again in the opening paragraphs of his chapter on psychotherapy in the *Studies on Hysteria*, he quoted those parts of the *Preliminary Communication* that had stressed the effectiveness of abreaction (Breuer and Freud, 1895, p.255). Later in the chapter he placed various qualifications on the scope of the cathartic method (op. cit., pp.261-265, 301-304) but, since these were of an essentially practical nature (the patient being too resistant, or the phase of the illness being too acute, for example), they constituted exceptions that confirmed Freud's assertion the method was "as a matter of theory ... very well able to get rid of any hysterical symptom" (op. cit., p.261).

Only a small step separates the belief that abreaction can remove any symptom from the practice of using the effect of abreaction to index the contribution of a given memory to it. When Freud took that step is not known. It was implied in one of the criteria he used for the differential diagnosis of actual neurosis: if the therapeutic results of abreaction were scanty the disorder was probably an actual neurosis. Conversely, a therapeutic effect tended to indicate a psychoneurosis (Breuer and Freud, 1895, pp.256-259). Since the actual neuroses predominated among the cases Freud treated early in his medical career, it is possible he developed this differential diagnostic criterion fairly soon after he began investigating hysteria. Freud assumed that abreaction rightly used would always succeed. It is not at all unfair to say this expectation was based on the presumption that abreactive therapy was infallible.

With these three expectations Freud therefore had a mental set to search for memories of a uniformly sexual kind the abreaction of which would relieve the patient's symptoms.

GUIDING THE RECOVERY OF CAUSAL MEMORIES

According to the concept of the logical and associative structure of the neurosis, there had to be associative linkages between the ideas recalled in treatment and the symptom. The linkages or logical threads had to be followed if important traumatic memories were to be found. In the *Studies on Hysteria*, Freud described how, in using the model of the pathogenic memory structure to guide his work, he had had to persevere with the connections suggested by the content of associations. In recovering the childhood seduction scenes he described the same practice:

> Travelling backwards into the patient's past, step by step, and always guided by the organic train of symptoms and of memories and thoughts aroused, I finally reached the starting-point of the pathological process. (Freud, 1896a, p.151)

This starting point was not reached automatically. Usually the first memories recovered were of fairly recent traumatic events. Freud argued these could not be the causes because they varied:

> in their intensity and nature, from actual sexual violation to mere sexual overtures or the witnessing of sexual acts in other people, or receiving information about sexual processes. (Freud, 1896b, p.166)

These later traumas also lacked determining quality and traumatic force (Freud, 1896c, p.193). Nor was there therapeutic gain:

> If the memory which we have uncovered does not answer our expectations, it may be that we ought to pursue the same path a little further; perhaps behind the first traumatic scene there may be concealed the memory of a second, which satisfies our requirements better and whose reproduction has a greater therapeutic effect. (op. cit., p.195)

Insistence might then be increased:

> If the first-discovered scene is unsatisfactory, we tell our patient that this experience explains nothing, but behind it there must be hidden a more significant, earlier, experience. (op. cit., pp.195-196)

Patients treated after 1893 were thus put under pressure similar to that to which Lucy R., Katharina, and Elisabeth von R. had been exposed. Each of them had also been told that various of their memories did not explain their symptoms and each had been forced to try again.

The first consequence of Freud's insistence was that the chains of associations were pushed back from recent events to things supposed to have happened in puberty. But the sexual experiences recalled from that time also differed from one another in kind and in importance, for example, between an obscene answer to a riddle, an attempted rape, and a surreptitious hand stroking combined with knee pressing. Serious and trifling events seemed to be involved equally. One further deficiency was evident: the content of the experiences had no sensory connection with the symptoms, that is, they lacked determining quality. Symptoms such as painful genital sensations, for example, could hardly have been produced by surreptitious caresses or answers to riddles. Freud extended the search:

> I was unable to find indications that they had been determined either by the scenes at puberty or by later scenes ... It seemed an obvious thing, then, to say to ourselves that we must look for the determinants of these symptoms in yet other experiences, in experiences which went still further back - and that we must, for the second time, follow the saving notion which had earlier led us from the first traumatic scenes to the chains of memories behind them. In doing so ... we arrive at the period of earliest childhood ... and here we find the fulfilment of all the claims and expectations upon which we have so far insisted. (op. cit., pp.201-202)

The three expectations were met: the experience was sexual, it always had

the same content and time of occurrence, and its abreaction seemed to produce a cure.

Within the logical structure, the childhood "memory" served as a focus for drawing other memories to it as in the same way as the repressed memory in the pathogenic memory structure:

> All the events subsequent to puberty ... are ... only concurrent causes ... they enjoy a pathogenic influence for hysteria only owing to their faculty for awakening the unconscious psychical trace of the childhood event. It is also thanks to their connection with the primary pathogenic impression ... that their memories will become unconscious in their turn and will be able to assist in the growth of a psychical activity withdrawn from the power of the conscious functions. (Freud, 1896a, pp.154-155. Cf. Freud, 1896b, pp.165-166)

This connecting principle supplanted or incorporated the previous principle of the summation of trauma and also explained the latency of the effect of the childhood experience. Through it, all the memories recovered in treatment could be linked with one another and to the symptom.

So well did the various aspects of the theory fit together that Freud proposed coherence itself as "another and stronger proof" of the reality of the infantile memories. The proof was:

> furnished by the relationship of the infantile scenes to the content of the whole of the rest of the case history. It is exactly like putting together a child's picture-puzzle: after many attempts, we become absolutely certain in the end which piece belongs in the empty gap; for only that one piece fills out the picture and at the same time allows its irregular edges to be fitted into the edges of the other pieces in such a manner as to leave no free space and to entail no overlapping. In the same way, the contents of the infantile scenes turn out to be indispensable supplements to the associative and logical framework of the neurosis, whose insertion makes its course of development for the first time evident, or even, as we might often say, self-evident. (Freud, 1896c, p.205)

What Freud is proposing here as a standard by which the reality of the recalled event can be judged is his subjective feeling of certainty about the patient's recollection.

Were these standards to be used in an ambiguous situation we can be sure that any hint from Freud about his expectations would have potent effects. But, while some of Freud's expectations were transmitted indirectly, as when the subject was led back to an earlier period by being told that a particular memory explained nothing, others were transmitted very directly. There is no doubt that before and during treatment Freud gave explicit information about the content of the memory being sought:

> Before they come for analysis the patients know nothing about these scenes [of childhood seduction]. They are indignant as a rule *if we warn them* that

such scenes are going to emerge. (op. cit., p.204. My emphasis, MBM)

Patients' recollections were guided from the non-significant late traumas to the sexual traumas of puberty by Freud's informing them of the required linking *content*. When telling the patient to try again, Freud would:

> direct his attention ... to *the associative thread* which connects the two memories - the one that has been discovered and the one that has still to be discovered. A continuation of the analysis then leads in every instance to the reproduction of new scenes of the character we expect. (op. cit., p.196. My emphasis, MBM)

Guidance could hardly have been more specific. There was also a lot of it, because, at about this time Freud told Fliess he was "almost hoarse" from working at full pressure ten to eleven hours a day (Masson, 1985, Letter of 6.12.96). Over at least the next two years his therapy continued in the same way: it gave him "occasion enough" for talking (op. cit., Letter of 18.6.97), he was "speechless" from too much therapeutic work (op. cit., Letter of 9.10.98), and incapable of writing "after ten hours of talking" (op. cit., Letter of 16.1.99).

Should Freud's patients have adapted their recollections to his demands their nuclear memories would have lacked reality. And that was what Freud found. Telling of their childhood seductions, patients would:

> suffer under the most violent sensations, of which they are ashamed and which they try to conceal; and, even after they have gone through them once more in such a convincing manner, they still attempt to withhold belief from them, by emphasizing the fact that, unlike what happens in the case of other forgotten material, *they have no feeling of remembering the scenes*. (Freud, 1896c, p.204. My emphasis, MBM)

Freud took this inability to remember as evidence of how basic and real the "memory" was rather than that it might be false.

Here Freud was, in effect, repeating an argument first put forward in the *Studies on Hysteria* about the memory in the nucleus (which was not, of course, a "memory" of childhood seduction). There he noted the patient might never remember the events "although he admits that the context calls for them inexorably" (Breuer and Freud, 1895, p.272). Those patients had needed the specific hypothesis:

> Not at all infrequently the patient begins by saying: 'It's possible that I thought this, but I can't remember having done so.' And it is not until he has been familiar with the hypothesis for some time that he comes to recognize it as well; he remembers - and confirms the fact, too, by subsidiary links - that he really did once have the thought. (op. cit., p.299)

But when treatment had ended there might still be no recollection:

> Even when everything is finished and the patients have been overborne by the force of logic and have been convinced by the therapeutic effect

> accompanying the emergence of precisely these ideas - when, I say, the patients themselves accept the fact that they thought this or that, they often add: 'But *I can't remember* having thought it'. It is easy to come to terms with them by telling them that *the thoughts were unconscious.* (op. cit., p.300. My emphasis, MBM)

Why should Freud have not put this same argument to those patients who could not remember their seductions? What was recalled, or indeed whether anything was recalled at all, did not matter - the inexorable demands of context, that is, the need to arrive at a plausible reconstruction of the patient's past, took precedence (Cioffi, 1972; Schimek, 1987).

Whatever logical and theoretical implications the concept of a totally unconscious memory might have, one practical implication is obvious: allowing there did not need to be a recollection at all made it that much easier for Freud to believe in his theory and for his patients to fall in with his demands. I have spoken, of course, as if most of Freud's patient's really did recall a "memory" of seduction and that where they did not they eventually came around to accepting one which Freud felt they ought to have. The detailed re-analysis by Schimek (1987) establishes, fairly conclusively in my view, that most of the patients did *not* so report seduction "memories". What Freud really describes is his foisting his reconstructions on to them, a fact which, when added to those I have so far considered, makes the rise and fall of the seduction theory even more comprehensible.

Each of Freud's reasons for giving up the seduction theory had the happy consequence of allowing him to continue to believe the psychoneuroses were caused by uniformly operating sexual factors and the process of recollection unaffected by unconscious influences. Freud's failure to bring any analysis to a conclusion did not question the infallibility of the cathartic method; the most that could be doubted was whether it had been used with suitable material. Freud could grant the logical point of a specific sexual factor having to be more prevalent than its effects without questioning the omnipresence of sexual factors themselves. By making the unconscious responsible for being unable to tell whether a "memory" was of a real event or not, Freud absolved himself (and his patients) for any error of judgement. Although when viewed in retrospect the fourth reason (Freud's failure to observe seduction "memories" in psychoses) is not without its peculiarities, it did not require him to abandon or alter his expectations. One would really have anticipated the fantasies Freud later proposed as substitutes for the pseudo-memories to have been present. In essence Freud treats the failure as if it were irrelevant.

Freud could therefore continue to believe the process of recollection was uninfluenced by his expectations and suggestions and, in fact as time went on, Freud recommended giving:

> the patient the conscious anticipatory idea [the idea of what he may expect to find] and he then finds the repressed unconscious idea in himself on the

basis of its similarity to the anticipatory one. (Freud, 1910c, p.142)

One could even unfold "the reconstruction of the genesis of his disorder as deduced from material brought up in [the first phase] of the analysis" before reaching the phase in which the patient began recalling what repressed memories he could (Freud, 1920b, p.152).

OTHER ACCOUNTS OF THE COLLAPSE

The seduction theory seems better accounted for by Freud's expectations than in any other way. Here I consider only those alternatives most relevant to my argument. A completely comprehensive analysis has recently been made by Vetter (1988).

The pressure method (Schusdek)

The seduction theory is most unlikely to have been a simple consequence of the pressure technique, as Schusdek (1966) has argued. Schusdek contends that the error of mistaking a fantasy for reality would not have occurred had the memories been obtained by the method of free association. The contention must be rejected. Free association is based on the same assumptions about the determinants of the associational train as is the pressure technique. Freud and his patients would have been led just as readily by it to a "memory" of a childhood seduction.

Non-repressed memories (Klein and Tribich)

On the basis of a very idiosyncratic reading of Freud's letter to Fliess, M. I. Klein and Tribich (1982a, 1982b) argue that the reason Freud gave up the seduction theory was the *failure* of his patients to report seduction "memories" or, if they did, that those "memories" had not been repressed. Freud's failing to complete any analysis is interpreted by them as really meaning that Freud had not been able to recover a seduction "memory". The absence of the "memories" in deliria is similarly interpreted: they make it refer to Freud's failure to retrieve "memories" during treatment rather than to a failure to observe their spontaneous occurrence during the deliria themselves. Then, while they attribute Freud's third reason - the lack of differentiation between truth and fiction in the unconscious - to the forcefulness of his technique, they also want to argue that despite that same forcefulness, Freud did not retrieve "memories" in enough of his patients.

Freud's lack of courage (Masson)

Masson (1984) has recently argued the seduction theory was abandoned because of "a personal failure of courage" (op. cit., p.189) on Freud's part in refusing to face the very unpleasant consequences of Fliess' near-fatal carelessness in leaving half a metre of surgical gauze in a patient's nasal cavity during an operation. The victim, Emma Eckstein, was also a

patient of Freud's who was at the same time conducting psycho-analyses of her own. Freud had referred her to Fliess because of stomach and menstrual pains. Under the influence of his nasal-reflex theory Fliess had been treating similar cases, probably quite successfully (Sulloway, 1979, p.152 and n.13), by cocainization and cauterization of particular areas or "spots" on the turbinate bones of the nose. With Emma Eckstein, Fliess appears to have experimented with a more radical surgical procedure in which part of the bone was removed. The operation was carried out in February, 1895. The carelessness over the gauze then produced a major focus of infection, a near bleeding to death, a prolonged convalescence, and the permanent facial disfigurement of a lively and attractive young single woman.

Fliess' reputation was at stake and Freud, undoubtedly feeling very guilty about the whole incident, came to Fliess' defence. During the convalescence Emma had repeated episodes of bleeding from the nose and she seems not to have recovered properly until late in May, some four months after the operation. About a year later Fliess seems to have suggested to Freud that the episodic haemorrhaging was hysterical. [1] Freud quickly agreed (Masson, 1985, Letters of 16.4.96, 26/28.4.96, 4.5.96, and 4.6.96). Emma, he claimed, had a history of nose-bleeding and headaches - the latter thought to be due to malingering. She had had erotic thoughts about a young doctor whom she had wanted to treat her nose-bleeding and she had welcomed her severe and painful menstruation as showing to others that she really was ill. Freud had been present when the gauze was discovered and the near-fatal haemorrhage began. He had then fainted. According to Freud, Emma experienced his reaction "as the realization of an old wish to be loved in her illness" (Masson, 1985, Letter of 4.5.96), an interpretation which Freud held to despite not being able to get the dates of the haemorrhages "because they were not recorded at the sanatorium" (Masson, 1985, Letter of 4.6.96). Emma's bleeding was thus caused by sexual longing rather than by the trauma of the operation and the gauze.

Masson argues that by the middle of 1896 Freud was in a conflict:

1. As to responsibility for the "longing" explanation, my reading of the correspondence leads me to conclude that the first explanation was formulated by Freud in terms of Fliess's periodicity theory. I think Fliess then questioned his interpretation and proposed the alternative of the bleeding as "sexual longing". Then, in so far as Freud could check any facts, he did so and, after taking them as confirming Fliess's thesis, he accepted it. As evidence, consider the following points. First, Freud wrote on 16.4.1896 that he had "a completely surprising explanation of Eckstein's hemorrhages - *which will give you much pleasure*". I take this to mean that Freud's explanation drew on Fliess's periodicity theory. Second, on 26/28.4.1896 Freud clearly attributes the longing thesis to Fliess by saying "I shall be able to prove to you that *you were right*, that her episodes of bleeding were hysterical, were occasioned by longing, and probably occurred at the sexually relevant times" (My emphasis, MBM). He then indicates he is seeking more data and, a week later, on 4.5.1896, says he knows "only that she bled out of longing" before summarising what he has found. A month later again, on 4.6.1896, he writes that, although the dates cannot be obtained from the sanatorium, the story "is becoming even clearer; there is no doubt that her hemorrhages were due to wishes" and concludes with an unmistakable acknowledgment of Fliess as source: "Your nose has again smelled things correctly".

> On the one hand, his patients told him their memories of traumas from their childhood; these he had no reason to disbelieve ... On the other hand, one of the patients ... had been severely injured by an operation that Freud had recommended and which was carried out by his closest personal friend and scientific colleague. The tension between these two sets of events ... was bound to reach a breaking point. Freud would be forced to make a choice (Masson, 1984, p.100)

Masson has it that Freud chose Fliess and fantasy over Eckstein and experience.

What is critical to Masson's argument is whether Freud really had "no reason" to think the seduction stories were of other than real events. According to Masson, during Freud's visit to Paris in 1885-1886, when he had frequently attended the lectures and forensic post-mortems conducted by Brouardel at the morgue, he had become all-too-well aware of the frequency of sexual assaults on children. His own investigations into the nucleus of hysteria pointed, Masson claims, to the same brutal and perverse assaults. From these two facts Masson concludes that Freud should have believed his patients. He nowhere considers the arguments actually proposed by Freud in announcing the collapse of the theory. He simply dismisses them as "the very objections" raised earlier by Freud's own critics to Freud's belief "in the reality of childhood seduction" (op. cit., p.110), and more than adequately answered by Freud at that time.

Now, it is just not the case that Freud's arguments had *ever* been aired in public. No one other than Freud makes the points and they had not then been made in any place other than his letter to Fliess. That is, the points about psychotic thinking, about not bringing any treatment to a real end, about the lack of reality in the unconscious, and the prevalence of a specific cause that had to co-operate with a pre-condition were Freud's 'objections' and not at all those of his critics. True, Freud had discussed this last methodological requirement in public but not as an objection. His discussion was positive, so to speak, being in order to explain how the prevalence of childhood seductions had to be *greater* than the prevalence of hysteria. All that he had done in public in anticipating objections was to advance his arguments about the determining quality and traumatic force of the scenes, to stress the coherence of the scenes with the logical structure, to point to the uniformity of the material apparently recalled, and to cite one or two instances where he seemed to have independent evidence of seduction having taken place. Whether the private or public arguments are considered we might note M. I. Klein's (1981) recent point that Freud had "little scientific reason" for so reinterpreting his data.

Holt (1982), in his review of Balmary's *Freud and the Hidden Fault of the Father*, says that Balmary has Freud pulling back from the seduction theory because it "would have implicated his [Freud's] father as a child molester". If this is what she says, it is a reason similar to Masson's 'lack

of courage'. However, it conflicts with Freud twice so accusing his father (Masson, 1985, Letters of 8.2.1897 and 21.9.87).

Critics like Masson, Balmary, and Klein and Tribich confuse the issue. As Vetter (1988) has so tellingly brought out, the question is not whether seductions really occur in childhood. Nor, I would add, is it whether they cause hysteria. The issue is whether there are seductions of the type that cause the kinds of symptoms Freud wanted to explain.

Critics from within psycho-analysis are also unable to view the psycho-analytic method of investigation as other than an objective one. Because they have to leave free association untouched, they can offer only the most simplistic of choices: real seduction versus perverse fantasy. If Masson, for example, were right, all that had happened in the Eckstein case was that Freud had deliberately disregarded some of the facts revealed by the method. For Masson and his co-critics, the facts obtained by free association are not affected by the analyst's expectations or pre-conceptions. For those who do not hold the method sacrosanct and are able to doubt the objectivity of data gathered in the psycho-analytical treatment situation there are three choices, not two. The third is that the "memories" were fabricated during the course of treatment (Cf. Cioffi, 1972, 1974). Pre-existing fantasies and real seductions both have to be ruled out by the failure to observe seduction "memories" during psychotic deliria. Real seductions may also be dismissed because of the strength of Freud's own methodological point about prevalence.

Gattel's data on prevalence (Sulloway)

Sulloway has pointed out that there were data on prevalence and, what is more, they were data gathered specifically for Freud (1979, Appendix C, pp.513-515). During the second half of 1897, Felix Gattel, one of Freud's earliest pupils and followers, worked at Krafft-Ebing's Psychiatric Clinic at the Vienna General Hospital investigating the role of sexual factors in the actual neuroses. Gattel (1898) collected data on one hundred consecutive out-patients with neurasthenia or anxiety neurosis. Although he had initially excluded patients with severe hysterical symptoms, on closer examination he found four cases of pure hysteria among the one hundred. Because there were thirty cases of pure neurasthenia in the sample, the number of hysterics was about thirteen percent of that of the neurasthenics. Sulloway's point is that if we accept Gattel's claim to have established masturbation as the specific cause of the neurasthenia of his thirty cases and if we assume that seduction had taken place in all four of the hysterics, it followed that paternal seduction had to have a prevalence of at least about one eighth that of masturbation. Gattel also found cases in which the symptoms of hysteria were present in other disorders as well as cases in which neurasthenic were mixed with others. The ratio of all the hysterias, the pure cases as well as mixed, to all the neurasthenias was 17:30. On that basis, it followed that seduction had to be even more widespread, having a

prevalence of about *half* that of masturbation.

Sulloway suggested this improbably high upper limit helped undermine Freud's seduction etiology (Sulloway, 1979, p.515). Too high a prevalence of seduction was among the reasons Freud gave Fliess for giving up the hypothesis. Sulloway also observes that Gattel had probably collected at least half his data before interrupting his work to join the Freud family for three weeks in Italy, early in September, 1897, and that Freud's letter announcing his suspicions about the seduction theory was written almost immediately he returned to Vienna on 21 September. It was also then, as we find from a previously suppressed passage in that same letter, that Freud's attitude to Gattel underwent a significant change. Gattel, Freud then wrote:

> is something of a disappointment. Very gifted and clever, he must nevertheless, owing to his own nervousness and several unfavorable character traits, be described as unplatable. (Masson, 1985, Letter of 21.9.87)

Although Freud's previous references to Gattel had mentioned his neurotic traits, Freud had then been rather more positive overall: Gattel "greatly pleases me", "is becoming much attached to me and my theories", and Freud even considered he might have the potential to work as Fliess' neurological assistant (op.cit. Letters of 18.6.87 and 7.7.87). I am very tempted to think that the data Gattel gathered were partly responsible for Freud's change of heart.

Making Gattel's work partly responsible for the collapse of the seduction theory gives a chronology that is rather more pleasing than Masson's. Rather than having the theory abandoned as the result of developments that took place within a period of a few months, developments which perhaps culminated with Gattel's findings, Masson first has to assume that it took about a year for Fliess' and Freud's thinking about the consequences of the operation to generate the hypothesis of bleeding as a form of sexual longing and then for Freud to take a further twelve months to decide against real seduction.

CONCLUSION

The theory of the actual neuroses was abandoned because no satisfactory explanation of the symptoms could be formulated and because the empirical generalisations upon which it was based could not be confirmed. Neither Freud nor his colleagues ever arrived at a satisfactory explanation of the central symptoms of neurasthenia. Freud persisted for a long time in trying to account for anxiety attacks, making three distinct revisions of his theory before giving up altogether. The curious thing about these explanations was that there was really nothing to explain: excessive masturbation and incomplete gratification are not among the necessary conditions of

neurasthenia and anxiety neurosis, let alone their specific causes. Neither the explanations nor the mechanisms were required. With regard to treatment, few of Freud's colleagues found the actual neuroses to respond to his simple therapeutic manoeuvres. Here Freud appears to have underestimated the power his suggestions had both to produce therapeutic effects and to elicit the kind of evidence he sought.

The relatively sudden collapse of the childhood seduction theory was not due to any disconfirming observation or conflict with the facts. The answer I have given to the question of why Freud came to believe that hysteria was caused by perverse sexual seductions in childhood is the most substantial influence of his three expectations that sexual factors were all-important, that the causes of neuroses were uniform, and that abreaction adequately indexed the importance of a memory. Freud's concept of psychic determinism effectively prevented him from considering how his conscious suggestions and unconscious influences determined the content of the pseudo-memories of the events his patients reported as having had or agreed with him they must have had. The pressure exerted on patients, subjective standards, and an elastic criterion by which a total failure to recall was allowed to count as a memory all helped create the false recollections.

Freud's proposal that the recollections were of fantasies had several consequences. One was to reduce the importance of the cathartic method. Abreaction might still be used to reduce the effect of traumatic memories that had become linked with the fantasy. But it no longer had a role in identifying significant memories and it could not be central to therapy. Indeed, within ten years Ferenczi (1908/1950) classed the "old Breuer-Freud 'catharsis' or 'abreaction'" with such simple methods as suggestion and reassurance.

It might also be thought that another consequence of Freud's abandoning the seduction theory would be for him to neglect the effects of real trauma in favour of the analyses of fantasies. In fact, this alleged outcome has been discussed recently by a number of different writers (e.g. M. I. Klein and Tribich, 1982a, 1982b; Masson, 1984). Apart from some points I shall make shortly, I would say that while it is true that Freud did come to place the primary emphasis on psychic reality - what was real for the patient - the analysis of Dora as well as analyses I have not so far considered (Little Hans, Wolf Man, and Rat Man) show that this supposed neglect was far from being total.

Even though the seduction theory had gone, Freud retained his expectations virtually intact, now incorporated into the concept of the logical and associative structure of the neurosis. All he had to do was to give a new account of the origins of the seduction "memory". But that required explaining how sexual wishes created fantasies in childhood as well as explaining how memories were laid down, how some of them were

repressed, and how all of them retained some potential to become conscious. In short, what Freud needed was a comprehensive theory of the mind, one concerned with the relation of drives to mental processes generally and not simply with the regulation and discharge of excess excitation. This new theory was largely set out in *The Interpretation of Dreams* and the *Three Essays on the Theory of Sexuality* and it is to those works we now turn. However, we can anticipate difficulties for the new theory to the extent that it invokes omnipresent sexual factors, is based on facts gathered and evaluated by a defective method, and is framed in such a way that the explanations it generates are similar to the earlier ones.

It is, of course, impossible to distinguish expectations from hypotheses in an absolute way. Both serve equally well or badly in guiding an investigation. An hypothesis will usually have the advantage over an expectation because it will most often have been consciously formulated and carefully scrutinised. It is because the notions on which Freud based his work had not been so dissected that they are better designated as expectations than hypotheses. For example, Freud's expecting the causes of all neuroses to be sexual could be regarded as an hypothesis only if one were to overlook the fact that it had not been established for the actual neuroses. Again, it is reasonable to search for uniformity in causes but to think that the causes of hysteria might be discovered without influencing the trains of associations is not. Although the belief is most obviously false, it is the one that has persisted longest in psycho-analytic theory and practice.

9 DREAMS AND SYMPTOMS

> ... the fingers ... beat gently ... as if her memory had set them going mechanically with the remembrance of a favourite tune.
>
> Collins: *The Woman in White*, Gilmore's story.

In forming his new theory of the mind, Freud brought together two reasonably independent lines of thinking about symptoms and dreams. He proposed that both were produced by similar kinds of repressed wishes coming into conflict with the same regulatory process. Because his explanations were not limited to symptoms and dreams, Freud saw his new theory as a general psychology, or at least as providing the basis for one. Before setting out the theory itself and evaluating it, I give an account of the two strands of thought and how Freud brought them together.

Why Freud first evinced an interest in dreams, which he did over a number of years, is not entirely clear. At the time he began his work on hysteria his attention was directed to the manner in which the dream was constructed. Not long after that he identified a motive behind the apparently meaningless facade of the dream: each dream was caused by the attempted fulfilment of a wish. Approximately two years later again Freud came to believe symptoms had a similar origin. The same forces that created the dream also transformed wishes into symptoms. Freud's theorising also appeared to undergo a change, seeming to be cast in purely psychological terms. However, the earlier pseudo-physiological mode of thought was still present.

STRUCTURE AND MOTIVE IN DREAMS

Well before becoming interested in the psychoneuroses Freud had attempted to understand dreams. As a medical student he kept note-books of his own dreams and his letters to his fiancee refer to some of them and to his attempts at interpretation. By 1882 he had anticipated a basic feature of his later view on the structure of dreams:

> I never dream about matters that have occupied me during the day, only of such themes as were touched on once in the course of the day and then broken off. (Letter of 30.6.1882 to Martha Bernays cited in E. Jones, 1953-1957, *I*, p.351. Cf. Sulloway, 1979, pp.321-329)

When he translated Charcot's *Leçons du Mardi* Freud noted a similar thing about hysterical deliria:

> There emerges in hysterical deliria material in the shape of ideas and impulsions to action which the subject in his healthy state has rejected and inhibited .... Something similar holds good of a number of dreams, which spin out further associations which have been rejected or broken off. (Freud, 1892-1894, p.138. Note to Charcot's p.137)

This footnote appears to be Freud's earliest published remark on the dream and shows his interest to have been in the fact the dream developed from the trains of associations broken off rather than in the *content* of the rejected impulses. That is, the interest is a *structural* one. Other remarks of this period clearly have the same focus:

> For several weeks I found myself obliged to exchange my usual bed for a harder one, in which I had more numerous or more vivid dreams, or in which, it may be, I was unable to reach the normal depth of sleep. In the first quarter of an hour after waking I remembered all the dreams I had had during the night, and I took the trouble to write them down and try to solve them. I succeeded in tracing all these dreams back to two factors (1) to the necessity for working out any ideas which I had only dwelt upon cursorily during the day - which had only been touched upon and not finally dealt with; and (2) to the compulsion to link together any ideas that might be present in the same state of consciousness. The senseless and contradictory character of the dreams could be traced back to the uncontrolled ascendancy of this latter factor. (Breuer and Freud, 1895, p.67, n.1)

This comment may be dated as belonging to the first half of 1894 (Editorial Note, *Standard Edition*, *4*, p.xv) and makes it quite clear Freud's interest was in the cursory day ideation, which provided the starting point, and the compulsion to link ideas together, which gave the dream its structure. He had already recognised symbolism in dreams but placed no special emphasis on it. For example, in the *Preliminary Communication*, he and Breuer noted that a symbolic relation "such as healthy people form in dreams" sometimes connected the causal trauma and the symptom (Breuer and Freud, 1893, p.5). Nevertheless, almost two further years elapsed before Freud really paid attention to the content of dreams.

On 4 March, 1895 Freud recounted to Fliess what he called "a small analogy" to a "dream psychosis" (about which we unfortunately know nothing) of Emma Eckstein's:

> Rudi Kaufmann, a very intelligent nephew of Breuer's and also a medical man, is a late riser. He has his maidservant wake him, and then is very reluctant to obey her. One morning she woke him again and, since he did not want to listen to her, called him by his name, 'Mr. Rudi'. Thereupon the sleeper hallucinated a hospital chart [compare the Rudolfinerhaus] with the name 'Rudolf Kaufmann' on it and said to himself, 'So R.K. is already in the hospital; then I do not need to go there,' and went on sleeping! (Masson, 1985, Letter of 4.3.95)

While Freud did not comment that the dream "allowed" Rudi to stay asleep while "being" at the hospital I think we can agree with Schur

(1966b) that the wish-fulfilment hypothesis had already been formulated. The single image of Rudi-in-a-hospital-bed could be construed as expressing simultaneously the conflicting wishes to wake and go to the hospital and to continue sleeping.

The earliest unambiguous contemporary reference to the wishful character of dreams is a single sentence in a letter to Fliess of 23 September, 1895. Quite abruptly and without further elaboration Freud wrote: "A dream the day before yesterday yielded the funniest confirmation of the conception that dreams are motivated by wish fulfillment" (Masson, 1985). It is quite difficult to determine when Freud formulated this hypothesis. During the night of 23-24 July, 1895 we know he had a dream which he claimed in 1914 to be the first he ever submitted to detailed interpretation (Freud, 1900, p.106, n.1 of 1914) and through which he grasped "the general principle" (Masson, 1985, Letter of 18.6.1900). However, no contemporary reference to the motives or impulses expressed in this dream, known as the dream of Irma's injection, seems to have survived. Neither Schur (1966b, p.48) nor Grinstein (1968, p.46) accept Freud's claim that it was the source of the wish-fulfilment hypothesis even though at the end of the first part of the *Project* some elements of it were discussed in a context where "the purpose and meaning of dreams" was explicitly said to be wish-fulfilment. Because the first part of the *Project* was completed between 4 September and 8 October, 1895 (Editorial Note, *Standard Edition*, *1*, pp.284-285), about the same time as the abrupt announcement of the hypothesis to Fliess, it seems more likely Freud did not place wish-fulfilment central to his method of interpretation until about September, 1895 rather than July.

Actually, the earliest contemporaneous record of a dream explicitly interpreted as a wish-fulfilment is the following report headed "Another Wish-Dream" (which implies there had been at least one earlier similar interpretation) that Freud recorded about eighteen months later:

> "I suppose that this is a wishful dream," said E. "I dreamed that, just as I arrived at my house with a lady, I was arrested by a policeman, who requested me to get into a carriage. I demanded more time to put my affairs in order, and so on. It was in the morning, after I had spent the night with this lady." - "Were you horrified?" - "No." - "Do you know what you were charged with?" - "Yes. With having killed a child." - "Has that any connection with reality?" - "I was once responsible for the abortion of a child resulting from an affair. I dislike thinking about it." - "Well, had nothing happened on the morning before the dream?" - "Yes, I woke up and had intercourse." - "But you took precautions?" - "Yes. By withdrawing." - "Then you were afraid you might have made a child, and the dream shows you the fulfillment of your wish that nothing should happen, that you nipped the child in the bud. You made use of the feeling of anxiety that arises after a coitus of that kind as material for your dream." (Masson, 1985, Draft L of 2.5.1897)

Freud seemed to have shown the meaningless elements of the dream were associated with a second set of meaningful ideas. Anxiety about pregnancy had been so elaborated that the dreamer need not worry.

Freud had moved from being interested in the determinants of the structure of dreams to a concern with their content. But if dreams were to be interpreted as fulfilling wishes, how was their typically senseless appearance to be explained?

THE CONSTRUCTION OF THE DREAM

In explaining the devious expression of wishes in dreams Freud distinguished three aspects of the dream: its manifest content, its latent content, and the dream work. Put over-simply, the latent content provided the wish, the dream work disguised it, and the manifest content reflected its altered form. The disguise was brought about by the censorship, a function that endeavoured to keep the wish out of consciousness altogether.

The manifest content

The manifest content of the dream is the dream as remembered by the dreamer. It includes the visual images, contradictory impressions, lack of apparent structure, and so on. While it is unimportant in the practical sense, serving as a mere starting point for interpretation, it is of the greatest importance theoretically. The dream theory had to account for the construction of the manifest content out of the latent content.

The latent content

The latent content of the dream includes all the components from which the dream is built: the repressed infantile wishes, mostly of a sexual nature; the partly conscious current preoccupations, arising either from conscious experiences during the previous day or from unconscious wishes themselves; and sensory excitations from somatic sources, for example, those of hunger, thirst, or sex. Of these components, Freud regarded unconscious infantile sexual wishes as making the greater contribution

The latent content could not be experienced directly: its presence was inferred from analysis of the associations to the elements of the manifest content. In reaching the latent content the dreamer was required to adopt a particular attitude of mind. The process of association had to be attended to without any judgement being passed on the content of the associations. Every thought, no matter how unimportant or objectionable it might otherwise have seemed, had to be allowed into consciousness. When the dreamer put his own critical attitude to one side Freud believed the trains of thought that began with the manifest element necessarily ended at the unconscious ideas from which they had sprung.

The dream work

Four main processes transformed the meaningful latent content into the less comprehensible manifest content: condensation, displacement, representability, and secondary revision.

Condensation referred to the difference between the meagre manifest content and the vastness of the latent content as well as to the process by which several distinct waking-life images or ideas fused to form a single image or idea. Condensation is well illustrated by part of an important dream of Freud's - that of Irma's injection:

> A large hall - numerous guests, whom we were receiving. - Among them was Irma. I at once took her on one side, as though to answer her letter and to reproach her for not having accepted my 'solution' yet. I said to her: 'If you still get pains, it's really only your fault.' She replied: 'If you only knew what pains I've got now in my throat and stomach and abdomen - it's choking me' - I was alarmed and looked at her. She looked pale and puffy. I thought to myself that after all I must be missing some organic trouble. I took her to the window and looked down her throat, and she showed signs of recalcitrance, like women with artificial dentures. (Freud, 1900, p.107)

Associations to three elements of the complaints of stomach and throat pains, the examination by the window, and the pale, puffy appearance brought to mind three different people. One was another patient who had the same symptoms as Irma displayed in the dream; the second was another patient who had actually been examined alongside a window; the third was Freud's wife who had looked puffy and pale during one of her pregnancies. The single dream image condensed the features of these different people who had in common their recalcitrance to one or another form of treatment.

Displacement was either the replacement of one latent idea by another remote from it or a shift of emphasis from an important to an unimportant idea. Freud's dream of a botanical monograph illustrates the second type:

> I had written a monograph on a certain plant. The book lay before me and I was at the moment turning over a folded coloured plate. Bound up in each copy there was a dried specimen of the plant, as though it had been taken from a herbarium. (op. cit., p.169)

Freud thought the important ideas in the latent content concerned complications and conflicts in his relations with his colleagues as well as the charge that he was not serious enough and pursued his 'hobbies' too energetically. The botanical thoughts connecting these two important trains of thought with one another were themselves relatively unimportant. The central manifest image of the monograph was due to a displacement of emphasis from the two important trains to a less important single visual image.

Representation of the latent ideas produced a further and quite characteristic distortion. Ideas were typically represented visually, a change

necessitating the concrete representation of abstract ideas, the use of symbols, and peculiar portrayals of logical relations. For example, the logical relation of simultaneity was pictured by simultaneously present visual images of each thought; causal connections were temporal sequences of images or successive dreams on the one night; logical alternatives like "either-or" appeared together, without contradiction; negations were disregarded, and so on. The "logic" of the dream was completely at variance with that of waking life.

Freud also believed that many dream symbols represented sexual ideas and impulses directly, as in the following dream, related by what he described as an "innocent lady dreamer":

> She was putting a candle into a candlestick; but the candle broke so that it wouldn't stand up properly. The girls at her school said she was clumsy; but the mistress said it was not her fault. (op. cit., p.186)

A ribald student song about female masturbation was what the patient associated to the element of the candle. Partly because of this, Freud inferred the candle was a symbol of the penis.

Secondary revision, the fourth component of the dream work, ordered and structured all the elements, arranging the otherwise loosely and irrationally connected dream thoughts so they seemed more intelligible. Secondary revision was different from the other three components in that it usually added no elements of its own. Rather, it selected, rearranged, and emphasised what was already there. Secondary revision is, of course, a descendant of the compulsion to link ideas together Freud had remarked on much earlier. In some translations of Freud's works on dreams the concept has been rendered as "secondary elaboration", a phrase conveying more accurately the capacity of a revision to produce its own distortions.

Censorship

Censorship attempted to block the direct representation in consciousness of the repressed unconscious wishes. Although in *The Interpretation of Dreams*, Freud did not explicitly relate the mechanism of repression to censorship, his later writings make it clear that dream censorship was an aspect of repression.

Freud introduced the concept of censorship in explaining a dream manifestly expressing an exaggerated affection toward his friend, R. At first Freud said he had been resistant to interpreting the dream because it contained a thought he did not want to accept:

> When I had completed the interpretation I learnt what it was that I had been struggling against - namely, the assertion that R. was a simpleton. The affection that I felt for R. could not be derived from the latent dream-thoughts; but no doubt it originated from this struggle of mine. If my dream was distorted in this respect from its latent content - and distorted into its

> opposite, - then the affection that was manifest in the dream served the purpose of this distortion. In other words, distortion was shown in this case to be deliberate and to be a means of *dissimulation*. (op. cit., p.141)

The basis for Freud's conceptualisation of the dream censorship was an analogy with the social dissimulation and the verbal disguises that critics of the political and social order had to adopt. Indeed, Freud thought that dream-distortion and political censorship corresponded in such detail that the presumption of a similar causation was justified:

> dreams are given their shape ... by the operation of two psychical forces (or we may describe them as currents or systems); and ... one of these forces constructs the wish which is expressed by the dream, while the other exercises a censorship upon this dream-wish. (op. cit., p.144)

The peculiarities of the manifest content - the dissimulation and disguise - were due to the opposition of the censorship to the unconscious wish.

SYMPTOMS AS COMPROMISES

In the *Studies on Hysteria* and in other early work, Freud derived the content of the symptom from sensory and muscular innervations present at the time the symptom formed, either directly or by way of symbolisation. He otherwise advanced no particular reasons for a patient developing one symptom rather than another, or for the particular symbols that formed and for a whole class of hysterical symptoms - the so-called stigmata - no reasons at all were given. From about the middle of 1896 Freud seems to have begun viewing symptoms as representing wishes but it was nearly two and a half years before this idea was to be expressed as a theory.

At the beginning of 1896, in the draft of a paper as well as in the paper itself, Freud analysed the processes by which he believed a number of symptoms had formed but did not imply that any of them represented either *wishes* or compromises between different *wishes* (Masson, 1985, Draft K, of 1.1.96; Freud, 1896b). Obsessional ideas and affects were said to be only "a compromise between the repressed *ideas* and the repressing ones" (Freud, 1896b, p.170. Emphasis altered, MBM). Not until a letter of 30 May, 1896 do we find the germ of Freud's later view: there "almost all" symptoms were said to be compromise formations reflecting an opposition between *uninhibited* mental processes and the *inhibitory* force of thought (Masson, 1985). At the end of that year, in a letter of 17 December, 1896 Freud reported his first example of a symptom as a compromise: according to his analysis a fear of falling out of a window represented a compromise between the unconscious impulse of going to the window to beckon to a man "as prostitutes do", that is, with sexual intent, and its rejection because of the anxiety it caused (op. cit.). By the middle of the following year, Freud said a symptom was the result of a libidinal *impulse* summating with a later *wish* for the impulse to be punished: "symptoms, like dreams,

are *the fulfillment of a wish*'' (op. cit., Draft N of 31.5.97). When, only about two months later, the childhood seduction theory was banished it seems to have taken this new explanation of symptoms and the parallel with dreams with it. Not until the beginning of 1899 was the wish-compromise view of symptoms again referred to, even though there are many references in Freud's correspondence with Fliess over the same period to wishes in *dreams* (for example, op. cit., Letters of 31.10.97 and 3.12.97).

Then, at the beginning of 1899, Freud quite suddenly announced the solution to the origin of the seduction fantasies of the neurotic to which, of course, the symptoms were related. The most recent traumatic experiences were linked by associations, or as he put it ''projected back'', to the germs of a sexual impulse already existing in childhood. Freud discerned some connection with dream formation although its precise nature escaped him:

> I want to reveal to you only that the dream schema is capable of the most general application, that the key to hysteria as well really lies in dreams .... If I wait a little longer, I shall be able to present the psychic process in dreams in such a way that it also includes the process in the formation of hysterical symptoms. So let us wait. (op. cit., 3&4.1.99.)

The end of the period of waiting, and the time at which Freud finally came to equate symptom formation with dream production, can be dated exactly. Seven weeks later he wrote:

> My last generalization has held good and seems inclined to grow ... Not only dreams are wish fulfillments, so are hysterical attacks. This is true of hysterical symptoms, but probably applies to every product of neurosis ... A symptom arises where the repressed and the repressing thought can come together in the fulfillment of a wish. The symptom is the wish fulfillment of the repressing thought, for example, in the form of a punishment ....
>
> This key opens many doors. Do you know, for instance, why X.Y. suffers from hysterical vomiting? Because in fantasy she is pregnant, because she is so insatiable that she cannot bear being deprived of having a baby by her last fantasy lover as well. But she also allows herself to vomit, because then she will be starved and emaciated, will lose her beauty and no longer be attractive to anyone. Thus the meaning of the symptom is a contradictory pair of wish fulfillments. (op. cit., Letter of 19.2.99)

This formulation of the mechanism of symptom formation is virtually the same as that put forward in *The Interpretation of Dreams*:

> A symptom is not merely the expression of a realized unconscious wish; a wish from the preconscious which is fulfilled by the same symptom must also be present. So that the symptom will have *at least* two determinants, one arising from each of the systems involved in the conflict .... The determinant which does not arise from the *Ucs.* [the Unconscious] is invariably, so far as I know, a train of thought reacting against the unconscious wish - a self-punishment, for instance. I can therefore make the quite general assertion that *a hysterical symptom develops only where the*

> *fulfilments of two opposing wishes, arising each from a different psychical system, are able to converge in a single expression.* (Freud, 1900, p.569)

Sexual impulses originally arising in infancy but later repressed provided the first wish (op. cit., pp.605-606). Much later again when this unconscious wish was revived, it became linked with an opposing preconscious wish. A successful modification by the preconscious system might allow the compromise structure so formed to enter consciousness (op. cit., pp.562-563). Just how successful Freud thought these compromises could be is illustrated in the following case note:

> I was called in to a consultation last year to examine an intelligent and unembarrassed-looking girl. She was most surprisingly dressed. For though as a rule a woman's clothes are carefully considered down to the last detail, she was wearing one of her stockings hanging down and two of the buttons on her blouse were undone. She complained of having pains in her leg and, without being asked, exposed her calf. But what she principally complained of was, to use her own words, that she had a feeling in her body as though there was something 'stuck into it' which was 'moving backwards and forwards' and was 'shaking' her through and through: sometimes it made her whole body feel 'stiff'. My medical colleague, who was present at the examination, looked at me; he found no difficulty in understanding the meaning of her complaint .... The girl herself had no notion of the bearing of her remarks; for if she had, she would never have given voice to them. In this case it had been possible to hoodwink the censorship into allowing a phantasy which would normally have been kept in the preconscious to emerge into consciousness under the innocent disguise of making a complaint. (op. cit., p.618)

The most cursory consideration of the mechanisms illustrated in this case note shows how closely Freud had come to model symptom formation on dream production. In *The Interpretation of Dreams* he tried to provide a theory explaining both.

A THEORY OF THE MIND

Freud proposed thinking of the mental apparatus as a number of different systems, or groups of psychological processes, related to each other by the fixed temporal sequence in which they normally operated. While there was no necessary implication that the systems had 'locations' in specific 'parts' of the brain or mind, Freud did speak of them as being spatially extended and later referred to them as comprising a topographic model of the mind.

The systems of the mental apparatus

The systems were explicitly conceived of as components of a reflex mechanism:

> the psychical apparatus must be constructed like a reflex apparatus. Reflex

> processes remain the model of every psychical function. (Freud, 1900, p.538)

Although Brücke and Meynert were continuing to influence Freud's thinking, there was the characteristic teleological addition:

> at first the apparatus's efforts were directed towards keeping itself so far as possible free from stimuli; consequently its first structure followed the plan of a reflex apparatus, so that any sensory excitation impinging on it could be promptly discharged along a motor path. (op. cit., p.565)

Psychical processes were said to begin with a perception at the sensory end of the apparatus and to terminate typically in a motor response. Figure 9.1, which is based on Freud's own diagrams, shows how excitation created by

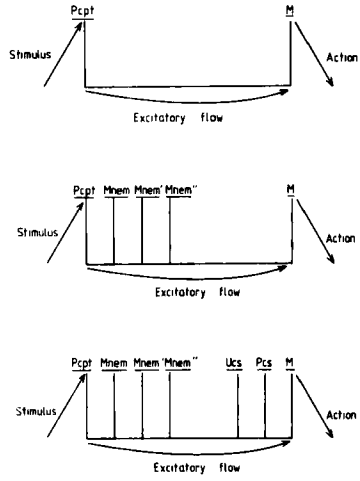

Figure 9.1. Freud's reflex-based mental apparatus (Freud, 1900, pp.537-541).

stimulation of the perceptual system, symbolised by the abbreviation *Pcpt.*, flowed through the apparatus to be discharged by the motor system, or *M.* At this point, it should be noted the model is very much a physiological one: a reflex apparatus with sensory and motor components activated by a "flow" of excitation. Indeed, over many years it has been apparent to philosophers, psychiatrists, and psychologists alike that the model simply extends that set out in the *Project* (e.g. McIntyre, 1958, pp.22-23; Wollheim, 1971, p.63; McCarley and Hobson, 1977; Hobson and McCarley, 1977).

Repeated stimulation of the system *Pcpt.* led to the unco-ordinated motor responses of the infant being replaced by the purposive behaviour of

the adult. This obvious effect of experience seemed to Freud to require assuming that distinct memory traces of each experience had been laid down and that adult behaviour was effective to the extent it drew upon them. Following a suggestion of Breuer's, Freud had argued in the *Studies on Hysteria* that the memory traces were not produced and retained in *Pcpt.*, and he now repeated the point:

> there are obvious difficulties involved in supposing that one and the same system can accurately retain modifications of its elements and yet remain perpetually open to the reception of fresh occasions for modification. (Freud, 1900, p.538)

Freud concluded that the memories were stored separately from *Pcpt.* He supposed, as is also illustrated in Fig. 9.1, there to be several systems of memory traces immediately following *Pcpt.*. These he symbolised by *Mnem.*, *Mnem.'* *Mnem.''*, and so on. The first *Mnem.* system contained the memory traces of perceptions that had occurred simultaneously, while *Mnem.'* had the same traces organised with respect to similarity, while *Mnem.''* and the other systems had the material organised around other kinds of logical relation. Since an association was a linkage of simultaneous perceptions, its basis was to be found in the *Mnem.* systems:

> Association would thus consist in the fact that, as a result of a diminution in resistances and of the laying down of facilitating paths, an excitation is transmitted from a given *Mnem.* element more readily to one *Mnem.* element than to another. (op. cit., p.539)

Again, even though no physiological processes were supposedly being referred to, Freud clearly retained Meynert's and Exner's notions about associations and facilitation. However, in one very important respect, the memory store is conceived of differently: instead of one system of memories there were several, containing, as they moved away from *Pcpt.*, progressively more abstract organisations of traces. Different levels of conceptual thought drew upon different stores of traces or, perhaps, the same traces organised in different ways.

The unconscious and preconscious systems, symbolised as *Ucs.* and *Pcs.* respectively, were held to be the two psychical agencies mainly responsible for dreams and symptoms. *Pcs.* was characterised as having the critical function of determining the admissibility of thoughts to consciousness. Mental processes occurring within *Pcs.* would become conscious provided they were of sufficient intensity and were attended to. *Pcs.* was also the agency that directed waking life, and since this was mainly through voluntary, conscious action, Freud located *Pcs.* adjacent to *M.* Freud placed *Ucs.* on the sensory side of *Pcs.* because whatever the intensity of processes in *Ucs.* they could become conscious only after modification by *Pcs.*, as Figure 9.1 indicates. Censorship, which is clearly a *Pcs.* function, was said to be located between *Pcs.* and *Ucs.* although it is

not so pictured on Freud's diagram. With this addition of *Ucs.* and *Pcs.* the model of the mental apparatus was complete.

Dream production began with excitation generated by the wishes stored in *Ucs.* but dreams were actually fashioned out of the modifications to that excitation as it passed through the various systems. At night, the wish caused a preliminary movement of excitation along the normal path leading toward *Pcs.* On the border of *Ucs.* and *Pcs.* the censorship forced the flow of excitation away from *M.* In any case, *M.* could not be activated because of sleep. The excitation now commenced a regressive journey toward the sensory end of the apparatus where, in cathecting *Pcpt.*, it produced the hallucinatory sensory images typical of the dream. To reach *Pcpt.* the excitation had to traverse the various systems of *Mnem.* in an order the reverse of that in which they had been laid down. Normal conceptual thought based on the logical relations stored in the more recent *Mnem.* systems therefore disappeared and was replaced with the logically more primitive thinking based on the relations stored in the older systems. Primitive relations of simultaneity, similarity, and contrast, for example, would be the last to be recathected before the visual metamorphoses of the dream thought. As Freud emphasised:

> *In regression the fabric of the dream-thoughts is resolved into its raw material.* (op. cit., p.543)

Given the mental apparatus to be structured as Freud supposed, and given the various systems successively modified the train of thought cathected by the excitatory process, the regressive movement of excitation through the apparatus seems to account for the peculiar logic and imagery of the dream. What then accounts for regression?

Infantile wishes and infantile thought

Freud believed that the regressive tendency of the dream was largely accounted for by supposing the basic wishes to be infantile and that the dream revived their infantile mode of satisfaction. He proposed that infantile mental life was governed by the primary process, that is, a tendency toward the free discharge of the quantities of excitation accumulated within the apparatus. According to the principle of constancy, excitation from outside impinging upon the sensory end of the reflex apparatus was discharged along motor pathways. While even the uncoordinated movements of the infant could remove a source of external stimulation, it was otherwise with excitation arising within the apparatus. Major somatic needs could not be satisfied by reflex motor discharge, for all that caused was screaming and helpless kicking. Only an experience of satisfaction - nourishment in the case of the hungry baby - could discharge the increased excitation. Since Freud also assumed that accumulations of excitation were experienced as unpleasure and discharge as pleasure, the experience of satisfaction generated pleasure. Consequently, future unpleasurable accumulations of excitation produced by the particular need

would set the apparatus in motion in order to repeat the experience of satisfaction, diminish the level of excitation, and so produce pleasure.

A wish was a movement of excitation that started with unpleasure and aimed at pleasure but, because of the infant's inability to control its circumstances, the attempted fulfilment of the wish was not through the re-creation of the actual experience of satisfaction; rather the infant hallucinated the percept of the satisfying object. Freud supposed the mnemic image of the perception concomitant with the experience of satisfaction had become associated with the memory trace of the excitation produced by the need and that the association was strengthened with each subsequent experience of satisfaction. Later, when the need again arose, its excitation would recathect the image of the satisfying object thereby producing an hallucination in the perceptual system. Infantile wishes directed excitation denied access to *Pcs.* into *Pcpt.*, because it was in that system the 'objects' satisfying the wishes had been found in the past. The free discharge of energy of the primary process created a mode of thought necessarily dominated by inner need and hallucinated objects. In the adult, therefore:

> *Dreaming is a piece of infantile mental life that has been superseded.* (op. cit., p.567)

The questions now arising are: what is it that supersedes primary process thinking, and why is it not available during dreaming?

Secondary process thought

During development Freud supposed a more realistic secondary process gradually replaced the earlier, primitive primary process. By itself the primary process could not satisfy internal needs. Hallucinated breasts do not feed hungry babies. If a real object were to be found, the structure of the apparatus demanded the excitation had to reach *M.* where it could initiate action. Realistic thinking had also to be based upon the availability of a large number of mnemic images of the various needs and their modes of satisfaction. Freud envisaged secondary process thought as the sending out of small amounts of excitation to those memory traces likely to prove relevant, the successive scrutiny of those traces, the successive withdrawal of cathexes from those unlikely to lead to a realistic solution, and the eventual finding of a path of action likely to satisfy the particular need. Not all the mnemic images needed to be cathected at any one time but free access to the whole of them was necessary.

Because realistic thinking required only small numbers of traces to be cathected at any one time, Freud supposed only small amounts of excitation would be needed to be expended. If a strong need was present during this tentative thought activity, the secondary process could draw on the rest of its energy to prevent or inhibit the free discharge of the excitation of the need. Secondary process realistic thinking was therefore quite different from infantile thinking because its:

> excitation is governed by quite different mechanical conditions from those in force under the dominion of the first system. When once the second system has concluded its exploratory thought-activity, it releases the inhibition and damming-up of the excitations [of the first system] and allows them to discharge themselves in movement. (op. cit., pp.599-600)

Freud attributed secondary process thinking to *Pcs.* and primary process thinking to *Ucs.* Sleep reduced the availability of the *Pcs.* mechanism with the consequence that primary process thinking could be used to fulfil unconscious, infantile wishes. Infantile mental life, usually superseded in day time, was reinstated at night.

Dreams gained part of their singularity from the impress of the primary process upon the trains of thought occurring in them. Mobilised for free discharge, the quantities of excitation cathecting each component of the train distorted the thoughts. Freud thought that mobilisation was effected in four different ways:

1. The excitation of the individual ideas could be accumulated in a single element of the train to produce an *en bloc* discharge. This was the process of condensation.
2. Excitation could be transferred to ideas of intermediate significance rather than to important ideas, that is, displacement of emphasis from the central thoughts would occur and the formation of compromise structures would result.
3. Excitation could be transferred along normally little used associational pathways to link ideas by homonyms and verbal similarities, thus giving rise to the strangeness of dream associations.
4. Excitation of one idea would not be accompanied by the inhibition of its opposite, as happened in normal thought. Mutually contradictory ideas could exist side by side or be expressed in the form of a single idea, as if there were no contradiction between them.

The peculiarities of the dream work, its complete contrast with secondary process thinking, derived from the greater mobility to which the excitation was subjected once the dream thoughts entered *Ucs.*

Dream wishes

Freud supposed the major role of the residues of daytime thinking, which he invariably found in dreams, was to provide a point of entry for the admission of *Ucs.* wishes into *Pcs.* The unconscious wish transferred its excitation onto the day residue and created or reinforced a preconscious dream wish. When the day residue was a worry or, more frequently, an indifferent or affectless idea, the transfer formed a new *Pcs.* dream wish. If the residue was a thought that had been suppressed during the day, the

transfer of the unconscious excitation reinforced it. In both instances the residue gained a new impetus to press toward consciousness. However, neither the new *Pcs.* dream wish nor the old *Ucs.* wish reinforcing it were admissible to consciousness. Both came up against the censorship of *Pcs.* that then initiated the dream work. Trains of thought subject to the dream work thus had their beginnings but not their motives in *Pcs.*

Some distortion of the *Ucs.* wish took place with this first censorship. The distortion was made the more readily because the unconscious wish was already generally attached to an indifferent or non-wishful day residue. Were it not for *Pcs.* the initial distortion would have moved further toward consciousness. Denied that passage, the combined train of thought, consisting of the *Pcs.* residues and the *Ucs.* wish, now moved back to *Ucs.* where they were subject to the inevitable distortion of the primary process. Infantile wishful experiences or the fantasies based upon them might then pull the combined *Ucs.* and *Pcs.* thoughts further along the regressive pathway. Chronologically earlier than the unconscious wish instigating the dream, these infantile wishes had their memory traces closer to *Pcpt.* than those of the instigating wish. The train of thought therefore tended to be drawn towards the sensorily strong images of the infantile wishes or fantasies. Regression was thus a two-sided process. It was:

> in all probability ... an effect of a resistance opposing the progress of a thought into consciousness along the normal path, and of a simultaneous attraction exercised upon the thought by the presence of memories possessing great sensory force. (op. cit., p.547)

The regressive movement of the train of thought towards *Pcpt.* via *Ucs.* or, more correctly, the movement of the quantities of excitation cathecting the elements of the train, was thus doubly determined by the push or resistance of the *Pcs.* censorship coupled with the pull or attraction of the infantile wishful experience or fantasy.

To complete the theory, Freud had to account for the indestructibility of infantile wishes and the function of the censorship. He based his explanation of these two central features of the theory on the development of the secondary process out of the primary and on the role of unpleasure in causing repression.

Repression and dreams

Freud proposed that a prototype of repression occurred in the primitive psychical apparatus even before the secondary process developed. Any perceptual stimulus producing painful excitation would create a trace associating the memory of the stimulus with the pain. Realistic or hallucinatory attempts to re-cathect the memory trace of the stimulus would necessarily tend to revive the memory of pain. But, since the primary process operated according to the pleasure principle, the attempted re-cathexis would fail. What Freud described as an "effortless and regular

avoidance'' of a distressing memory was a necessary part of primary process functioning and the prototype and first example of repression.

One consequence of this concept of prototypal repression is that memories of unpleasant experiences can never be available to secondary process thinking even if those memories are necessary to arrive at a realistic need satisfaction. Freud therefore gave the second system the power to inhibit the unpleasurable discharge at the very instant the painful memory was cathected. He emphasised this supposition as ''the key to the whole theory of repression'':

> *the second system can only cathect an idea if it is in a position to inhibit any development of unpleasure that may proceed from it.* (op. cit., p.601)

How did this power not do away with prototypal repression itself? During development the pleasurable affect of some of the infantile wishes was transformed into unpleasure. As Freud emphasised, attempts to cathect the mnemic image would now only generate unpleasure so:

> *it is precisely this transformation of affect which constitutes the essence of what we term 'repression'.* (op. cit., p.604)

Preconscious thoughts on to which a repressed unconscious wish had transferred its excitation shared the unpleasurable affect. By withdrawing its cathexis from the transference thoughts *Pcs.* would now avoid the *Ucs.* train of thoughts, and repress them.

Freud considered repression had one of three outcomes: success, symptom formation, or the production of a dream. Complete repression would mean that little trace of the unconscious wish would be found in waking life. However, if the unconscious wish received what Freud called ''an organic reinforcement'' that was passed on in turn to the transference thoughts, those vehicles of the unconscious wish would seek admission to consciousness without the usual cathexis from *Pcs.* But the *Pcs.* trend originally withdrawing the cathexes would also have its opposition reinforced. A symptom would form as a compromise between the repressed unconscious wish and the preconscious trend opposing it. The third outcome, which presumably occurred when organic reinforcement was not provided, was the dream:

> from the moment at which the repressed thoughts are strongly cathected by the unconscious wishful impulse and, on the other hand, abandoned by the preconscious cathexis, they become subject to the primary psychical process and their one aim is motor discharge or, if the path is open, hallucinatory revival of the desired perceptual identity. (op. cit., p.605)

Because the unpleasure the train of thoughts now conveyed could not be inhibited, the withdrawal of preconscious cathexis from the transference thoughts acted as a ''push''. Although Freud does not say so directly, it is clear censorship and repression were being equated, an equation already

implicit in his earlier discussion of the representation and suppression of affects in dreams. There, in the dreams of the death of a loved relative:

> We can thus plainly see the purpose for which the censorship exercises its office and brings about the distortion of dreams: it does so *in order to prevent the generation of anxiety or other forms of distressing affect.* (op. cit., p.267. Cf. pp.246, 460-462, 467, 488-489)

The mechanism of censorship/repression was central to the explanation of the dream. Censorship demanded distortion, as it were, and set the distorting mechanisms into action by the regression it compelled.

The immutability of unconscious wishes

Freud derived the indestructibility of infantile wishes from the later development of the secondary process:

> In consequence of the belated appearance of the secondary processes, the core of our being, consisting of unconscious wishful impulses, remains inaccessible to the understanding and inhibition of the preconscious; the part played by the latter is restricted once and for all to directing along the most expedient paths the wishful impulses that arise from the unconscious. (op. cit., p.603)

These early wishes were developmentally inaccessible; they could be neither inhibited nor understood; at most their energy could be directed or diverted toward higher aims. The wishes themselves could not be destroyed. Those wishes generating unpleasure if now revived were as inaccessible as the others and the affect attaching to them just as uninhibitable. But because of the affective transformation, they would now be experienced at the preconscious level as unpleasurable. Consequently, *Pcs.* would withdraw its cathexis from them, no transference of their energy would be possible, and they would not be diverted. Lacking the preconscious cathexis they fell back unaltered into *Ucs.* Once embedded in memory, infantile wishes would, to turn Freud's metaphor, prance each night upon the dream stage. Only death called a halt to their noctambulations.

My comparison has shown how closely dreams and symptoms resembled each other (Cf. also Ellenberger, 1970, p.491 and Sulloway, 1979, pp.345-346). By affixing their cathexes on to preconscious day residues, unconscious infantile sexual wishes initiated a progressive movement of excitation toward *Pcs.* Enough unpleasure was generated by this initial foray for preconscious cathexis to be withdrawn and a consequent reversal of the movement of excitation initiated. Earlier modes of need satisfaction stored in *Ucs.* now attracted the excitation and, in the regressive movement so maintained, the train of thought was subjected to the distortions of the primary process. The more ready discharge of energy in the unconscious produced condensation, displacement, and the disregard of normal logical relations. If organic reinforcement strengthened the train of thought, the

production of dream and symptom diverged. Without the reinforcement dream impulses began a third movement in the opposite direction: toward the perceptual system and hallucinatory expression. Only the wish to sleep then opposed that form of admission to consciousness. With the reinforcement, a third progressive movement toward the preconscious commenced, and if this new foray was successful, the wishful trend combined with an opposing preconscious impulse to gain admission to consciousness as a compromise. Dynamically and energetically dreams and symptoms were otherwise equivalent structures. At only two points was the resemblance between dream production and symptom formation not quite complete: the final point of entry to consciousness and the final opposition to that entry were different.

THE THEORY APPLIED TO THE CASE OF DORA

Within a year of the appearance of the first copies of *The Interpretation of Dreams*, on 4 November, 1899 (Masson, 1985, Letter of 5.11.1899), Freud began the treatment of an hysterical patient known as Dora (Freud, 1905a; Masson, 1985, Letter of 14.10.1900). His account of the case emphasised the importance of the interpretation of two of her dreams, and his explanation of her symptoms was based upon the theory of mental functioning just outlined. Dora's case accordingly provides an eminently suitable starting point for a critical evaluation of Freud's theory.

"Only her father's authority" had forced Dora to come to Freud; she herself was quite resistant to the idea of treatment (Freud, 1905a). "A slight passage of words" between her and her father had been followed by a loss of consciousness, possibly with convulsions and deliria, for which she had been amnesic. Other problems were soon revealed. From the age of eight years she had had laborious, difficult breathing (chronic dyspnoea) that was occasionally exacerbated. At about twelve years of age, she had developed unilateral, migrainous headaches that were invariably followed by severe coughing attacks. As time went on, these two symptoms separated, with the migraine ceasing at sixteen years of age, and the coughing becoming worse. Typically the coughing attacks lasted from between three to five weeks - although one has lasted several months - during which she would frequently lose her voice (aphonia). In addition she was low in spirits, dissatisfied with herself and her family, plagued by fatigue and lack of concentration, and unfriendly toward her mother - especially toward her mother's attempts to involve her in housework. Dora had contemplated suicide, perhaps not very seriously. She was then not quite eighteen.

Dora was "most tenderly" attached to her father, an affection increased by his many illnesses: when Dora was six years old he had had tuberculosis, requiring considerable nursing; when she was ten he had had a detached retina; and when she was twelve years of age he had developed a paralysis and mental disturbance following a confusional attack, all of

which were the late result of a syphilitic infection. Dora's mother was occupied all day with narrow domestic affairs, especially house cleaning, and she had no understanding of Dora's more active interests. Dora had withdrawn completely from her mother's influence, looking down on her and criticising her "mercilessly". [1] On the other hand, Dora was extravagantly fond of Frau K., a "young and beautiful woman" who, while nursing Dora's tubercular father, had begun an affair with him. A partial basis for the affair was probably provided by the fact, as Dora's father put it, "I get nothing out of my own wife". Dora, who had known of the affair for some time, nevertheless frequently visited the K.'s in company with her father. At such times she took "the greatest care" of the K.'s two children, being "almost a mother to them". Herr K., in his turn, was "most kind" to Dora; he frequently gave her "valuable presents" - once having flowers delivered daily - and often accompanied her on walks.

According to Dora, two years before treatment began, when she was sixteen, Herr K. had made some kind of advance or proposition to her. Whether it was of marriage or not was unclear, for Dora had interrupted him by slapping his face. After returning from the lakeside walk where the incident had occurred, Dora insisted on cutting her visit short and returning to her own home with her father. Two weeks later she told her mother of Herr K.'s advances, knowing the story would be passed on to her father, and insisted the family, especially her father, break off relations with the K.'s. Confronted by Dora's father, Herr K. denied Dora's story, even suggesting that the whole thing was a fantasy stimulated by too much reading on sexual topics. Dora's father accepted Herr K.'s explanation, opining that Dora's irritability, depression, and suicidal gesture, derived from his rejection of her pressure to stop seeing the K.'s.

Dora was full of reproaches against her father: she accused him of not wanting to consider Herr K.'s behaviour too closely, because it would disturb his own relation to Frau K., and she believed he used his ill-health as a pretext to maintain the relation with Frau K., that is, he was malingering. But, "None of her father's actions seemed to have embittered her so much" as his willingness to accept Herr K.'s story that the advances had never taken place. These reproachful thoughts, which had a basis in fact, were nevertheless of obsessional intensity: Dora admitted they were not fully justified but, even so, she could not put them out of mind, "'I can think of nothing else', she complained again and again" (Freud, 1905a, p.54). Freud regarded the intensity as pathological and 'reflected' that the reproaches served to cover a similar *self*-reproach:

> *this excessively intense train of thought must owe its reinforcement to the unconscious.* It cannot be resolved ... either because it ... reaches ... down

1. From Rogow's (1979) expansion of Freud's description of Dora's mother, Dora's reaction to her is quite understandable. Along similar lines, some writers have used Dora's case as an instance of Freud's neglect of real events as contributors to psychopathology after he gave up the seduction theory (M. I. Klein and Tribich, 1982a, 1982b).

> into unconscious, repressed material, or because another unconscious thought lies concealed behind it ... Contrary thoughts are always closely connected ... and are often paired off ... such ... that *the one thought is excessively intensely conscious while its counterpart is repressed and unconscious.* (Freud, 1905a, pp.54-55)

This newly formulated principle allowed Freud to guess at the content of Dora's self-reproaches:

> Her behaviour obviously went far beyond what would have been appropriate to filial concern. She felt and acted more like a jealous wife ... She was ... identifying herself both with the woman her father had once loved and with the woman he loved now. The inference is obvious ... she was in love with him. (op. cit., p.56)

If an unconscious love for her father provided the reinforcement for the reproaches she now directed at him, Freud now asked why the affection had been so recently revived. After all, until Herr K. had made his proposition, Dora had been on the most friendly of terms with Frau K., virtually acting as an accomplice in her affair with her father. Freud supposed Dora was in also love with Herr K., but that something about his advances to her had so roused her opposition that her feeling had become distressing. She had then been obliged:

> to summon up her infantile affection for her father and to exaggerate it, in order to protect herself against the feelings of love which were constantly pressing forward into consciousness. (op. cit., p.58)

Dora's emphatic denial of this interpretation of her behaviour was taken by Freud to signify the correctness of his explanation (ibid.), which in his view was further confirmed by such things as her dejected reaction to not having received a birthday present from Herr K., and the observation of a relative that even seeing Herr K. accidentally had caused Dora to go "as white as a sheet".

Dora's reproaches against her father served more than the purpose of suppressing her love for Herr K. - the previous intimacy of her relations with Frau K., her references to Frau K.'s "adorable white body", and her anger that Frau K. had betrayed her by supporting her husband's story that the scene at the lakeside had never taken place, suggested to Freud that those reproaches also suppressed her more deeply unconscious love for Frau K.:

> She told herself incessantly that her father had sacrificed her to this woman, and made noisy demonstrations to show that she grudged her the possession of her father .... The jealous emotions of a woman were linked in the unconscious with a jealousy such as might have been felt by a man. (op. cit., p.63)

It was against this pattern of feelings that Freud thought he had detected in the analysis of the pathological reproaches that he commenced his

interpretation of Dora's symptoms and two of her important dreams.

The first of Dora's dreams recurred periodically in exactly the same way. It had returned some nights before she told Freud of it:

> A house was on fire. My father was standing beside my bed and woke me up. I dressed quickly. Mother wanted to stop and save her jewel-case; but Father said: 'I refuse to let myself and my two children be burnt for the sake of your jewel-case.' We hurried downstairs, and as soon as I was outside I woke up. (op. cit., p.64. Emphasis removed, MBM)

She had first had the dream on the night of the day after Herr K.'s advances, it had recurred on each of the next three nights, and ceased on her returning home.

On arrival at the house where the K.'s were holidaying, Dora's father had expressed concern about the possibility of fire; that concern was part of the day residue incorporated into the dream's formation. Recent discussions at home had revived the topic of fire danger just before the dream's latest reappearance. The dream element of her father standing by the bed led to the recollection that, as she had woken from her afternoon sleep, following Herr K.'s proposition, she had found Herr K. standing alongside her. A key she had then obtained to lock the room was missing when she went to use it the next afternoon:

> It was then that I made up my mind not to stop on with the K.'s without Father. On the subsequent mornings I could not help feeling afraid that Herr K. would surprise me while I was dressing: *so I always dressed very quickly*. (op. cit., p.67)

Freud believed the dream corresponded to Dora's conscious intention to flee Herr K.'s attentions. He thought it was as if she had said to herself she needed to leave the house. In the dream the intention had been represented by its opposite: Dora had actually woken up after dreaming she had been able to get out of the house. The dream had been repeated until the intent had been realised through returning home with her father.

The theory required more than a conscious intent to form a dream. Questions to Dora about the element of the jewel-case retrieved two memories that led to another motivating source, this time an unconscious one. One memory was of a dispute between her mother and her father. Her mother had wanted a present of pearl drops but her father had given his wife a bracelet. The second memory, also from the period before the first dream, was of an expensive jewel-case Herr K. had given her. Freud interpreted the element "jewel-case" to stand for the female genitals, and supposed Dora had thought to herself that Herr K. was persecuting her, that he wanted to force his way into her room, that her "jewel-case" was in danger, and that, if anything happened, it would be her father's fault. The dream had expressed the opposite of the latter thought and represented Dora's father in the role of saviour. The combination of that presumed

inversion with Dora's inability to recall whether she would have liked the pearl drops herself (which Freud believed indicated Dora's repression of the thought), suggested to Freud that the rest of the train of thought also ought to be inverted. Dora was prepared to *give* to her father the sexual favours her mother had *withheld* from him. Herr K.'s present of the jewel-case provided the starting point of a parallel line of thought: Dora could also give to Herr K. that which *his* wife had also *withheld*. This latter thought was the repressed wish motivating the dream. As Freud explained:

> The dream confirms once more what I had already told you before you dreamt it - that you are summoning up your old love for your father in order to protect yourself against your love for Herr K. .... In short, these efforts prove once more how deeply you loved him. (op. cit., p.70)

When Dora did not accept this interpretation Freud tried another line of argument which he hoped would convince her.

Freud's dream theory required an infantile wish to be represented in the dream. Where was that wish? Freud proposed two trains of thought led off from the element "fire". One went to the idea of love via the symbolic meaning of fire. The second also led there, but via water as the contrary of fire. Because the genitals were wetted by drops of semen during intercourse this second train built on the recollection of the pearl drops Dora's mother had wanted from her husband. It then led on further, to thoughts and memories of childhood enuresis Dora recalled as having started in her seventh or eighth year. The dream had fulfilled an infantile wish about bed wetting. Freud seems to have assumed that because children were often woken at night to prevent them from wetting the bed, Dora's father had also woken her for the same reason. He further supposed Dora must have woken to find her father standing by her bed and that he had "perhaps" woken her with a kiss (op. cit., pp.72, 86). The "essence" of the dream (oddly tucked away in a footnote) could be represented as Dora's thinking:

> The temptation is so strong. Dear Father, protect me again as you used to in my childhood, and prevent my bed from being wetted! (op. cit., p.73, n.2)

The image of her father could appear in the dream as a substitute and cover for the image of Herr K. who had the day before the dream first occurred similarly stood by her as she woke, perhaps also with the intention of kissing her. Since Freud believed bedwetting having a late onset, as did Dora's, was caused by masturbation, the appearance of her father in the dream also rescued her from sexual temptation. Reinforcement of the adolescent appeal for her father's aid against Herr. K.'s sexual temptation came from the memory of her father's actions in guarding her against similar infantile temptations. This was the infantile wish reinforcing the unconscious source revealed by the associations to 'jewel-case'.

Even in this very condensed summary of the interpretation of Dora's dream, the features typical of Freud's description of dream formation may

be discerned. Freud would have assumed that the day residues about fire had become linked with a repressed unconscious wish to yield to Herr K. This wish had not been represented directly because the movement of excitation toward *Pcs.* had been turned back until, gaining reinforcement from the infantile wishes about bedwetting and masturbation, it was represented as fulfilled through the revival of the memory image of the saviour father. Condensation and displacement are shown by the multifarious connections of the elements of "fire" and "jewel-case" that also divert attention from the immediate and past concerns. The revival of primitive modes of thought may be seen in the representation of the thought of giving through its contrary of withholding.

I shall use Freud's analysis of some of Dora's minor symptoms, which entail a shorter examination than do the more complex ones, to reveal the same kinds of mechanisms at work there. Four years before Freud saw her, when she was fourteen, Dora had been surprised by Herr K. unexpectedly embracing and kissing her. Her reaction had been one of disgust and after freeing herself she ran away. Apart from a slight loss of appetite, the disgust had not seemed to produce any permanent symptom. However "a sensory hallucination" of the pressure of Herr K.'s embrace on the upper part of her body had recurred from time to time, and she was unwilling, four years later, to walk past "any man whom she saw engaged in eager or affectionate conversation with a lady" (op. cit., p.29).

Freud began by asserting that Herr K.'s embrace:

> was surely just the situation to call up a distinct feeling of sexual excitement in a girl of fourteen who had never before been approached. (op. cit., p.28)

He then said Dora's reaction of disgust was typical of the hysteric:

> I should without question consider a person hysterical in whom an occasion for sexual excitement elicited feelings that were preponderantly or exclusively unpleasurable; and I should do so whether or no the person were capable of producing somatic symptoms. (ibid.)

According to theory, the reversal of affect was a sign that repressed sexual ideas were already in existence and it was from these that the disgust partly derived. Displacement also played a part:

> Instead of the genital sensation which would certainly have been felt by a healthy girl in such circumstances, Dora was overcome by the unpleasurable feeling which is proper to the tract of mucous membrane at the entrance to the alimentary canal - that is by disgust. (op. cit., p.29)

Freud then applied certain of what he called "rules of symptom formation" (which he did not spell out) to the sensory hallucination and the compulsive avoidance behaviour. He supposed that, during the kiss, Dora had felt Herr K.'s erect penis pressing against her, and further supposed

there had been excitation of her clitoris. The sequence of events was:

> This perception was revolting to her; it was dismissed from her memory, repressed, and replaced by the innocent sensation of pressure upon her thorax, which in turn derived an excessive intensity from its repressed source. (op. cit., p.30)

Displaced clitoral excitation maintained the innervation of the sensation of pressure on the thorax. Freud assumed Dora thought men talking affectionately to ladies were likely to be sexually excited. Her avoidance thus prevented her from seeing the "somatic sign" of the erect penis.

Freud speculated that Dora's reaction of disgust to sexual excitement partly had a developmental basis. He believed disgust first arose in the child as a response to the smell and sight of excrement. Because of the excretory functions performed through or near to the genitals, the disgust was then transferred to sexuality itself. Another and more personal determinant of the reaction of disgust came from Dora's leucorrhoea, a vaginal discharge she had had from some uncertain but presumably early period. In Freud's view, the leucorrhoea was caused by masturbation, and we have already seen he also assumed masturbation had produced her bed wetting. Dora's mother had suffered from a similar discharge, or catarrh, which Dora believed to have been transmitted to her mother by her father. As early as the age of ten years Dora had associated her father's detached retina with "improper subjects" and two years later had heard syphilis mentioned as a cause of his confusional attack. Dora's last governess, who had been in love with Dora's father, had warned her that all men were frivolous and untrustworthy. Freud supposed:

> To Dora that must mean that all men were like her father. But she thought her father suffered from venereal disease .... She might therefore have imagined to herself that all men suffered from venereal disease, and naturally her conception of venereal disease was modelled on her one experience of it - a personal one at that. To suffer from venereal disease, therefore, meant for her to be afflicted with a disgusting discharge. (op. cit., p.84)

Her masturbation, the earliest sexual enjoyment she could have experienced, led to the disgusting leucorrhoea. Herr K.'s revival of her sexual feeling had not only revived the developmentally caused disgust, but also this more personal association with disgust.

Dora's infantile sexual wishes were repressed because they had come to be associated with disgust. The kiss, the embrace, and the pressure revived the wish with disgust rather than pleasure being experienced. *Pcs.* cathexis were withdrawn from this unpleasant intruder from *Ucs.* and excitation flowed back into *Ucs.*, where it had presumably been reinforced. On return to *Pcs.*, the compromise of the sensory hallucination had been

formed and allowed into consciousness. By maintaining part of the pattern of sensory innervation present during the kiss, the compromise satisfied both the old and new sexual wishes; by suppressing the sexual excitement producing the disgust, the demands of the censorship were met.

CRITIQUE OF THE CLINICAL APPLICATION

Freud's explanations of Dora's symptoms and dreams were supposed to be based upon the associations provided by *her* to the details of the symptoms and the elements of the dreams. His central methodological proposition, now familiar to us, was that it was:

> demonstrably untrue that we are being carried along a purposeless stream of ideas when, in the process of interpreting a dream, we abandon reflection and allow involuntary ideas to emerge. It can be shown that all that we can ever get rid of are purposive ideas that are *known* to us; as soon as we have done this, *unknown* - or, as we inaccurately say, 'unconscious' - purposive ideas take charge and thereafter determine the course of the involuntary ideas. (Freud, 1900, p.528)

Again:

> when conscious purposive ideas are abandoned, concealed purposive ideas assume control of the current of ideas. (op. cit., p.531)

However the details of the analyses of Dora's dream and symptoms shows only a very small proportion of the explanatory concepts derive from anything that can reasonably be described as *her* concealed purposive ideas. Freud's background assumptions, his *ad hoc* explanatory principles, and *his* personal associations contribute considerably more than Dora's.

The most important of Freud's assumptions is that Dora ought to have been sexually excited by Herr K.'s kiss. Her disgust is Freud's essential evidence for a once strong but now repressed sexuality. No association of hers points in that direction. It is also an assumption that masturbation in childhood caused Dora's leucorrhoea as well as her late-onset enuresis. The mere presence of those two disorders is Freud's *only* evidence of childhood sexual feelings. Dora herself had no recollection of having masturbated. Moreover, while she recalled she had been enuretic in her seventh and eighth years, she could not place the onset of her leucorrhoea. That failure has two consequences: it weakens the already purely circumstantial case of her ever having masturbated (Freud, 1905a, p.78), and it makes it impossible to corroborate Freud's surmise that the 'repression' of her sexuality, which the 'ending' of her 'masturbation' was supposed to signify, had actually preceded the attack of dyspnoea (op. cit., p.79).

Dora's most direct contribution to the evidence for her childhood masturbation was a so-called symptomatic act that Freud *interpreted* as a symbol of masturbation. After analysing part of Dora's self-reproaches,

Freud concluded that she had once been a masturbator. She rejected that suggestion when he put it to her. But, a few days later, when Freud observed Dora opening a small reticule she was wearing, putting her finger into it, shutting it, and so on, he took the action to stand for genital manipulation. Dora's reaction to his interpretation is not reported and it is possible Freud may not have even put it to her. If that is so, Freud must have regarded the mere possibility of the interpretation as confirming his supposition that she had masturbated.

A clitoral sensation produced by the pressure of the erect penis is necessary to explain the sensory hallucination and the avoidance behaviour. But the only associations to the kiss contributing to that explanation are Freud's and not Dora's. To Freud's question whether she knew arousal in the male was accompanied by penile erection, Dora gave a "prompt and frank" reply that she did not. Freud seems to have believed her. But, without that knowledge, the mechanism proposed for the phobia could not be verified. It also makes it unlikely there had been a clitoral sensation to reinforce the sensation of pressure on the thorax.

Sand is the only psycho-analyst to have provided an evaluation of Freud's treatment of Dora which matches mine. Her conclusions are as damming. She sets out ten claims which Freud makes in his report of the case and, after examining the evidence for eight of them, notes that:

> In each instance but one, the evidence provided for the contention was ... either flawed or scanty. (Sand, 1983)

She concludes:

> the case cannot serve as a demonstration of Freud's general thesis, that the aetiology of hysteria is psychosexual, nor of his several related specific theses regarding the role of trauma and repressed affect in the production of symptoms and dreams (op. cit.)

Among the claims she investigated were that the symptoms were caused by and represented a repressed affect, that childhood experiences of thumbsucking, bed-wetting, and masturbation contributed to the symptoms, and did so via a 'somatic compliance', and that the dreams, especially the first, were related to the symptoms. As Flowerman (1954) had done before her, Sand impugns Freud's use of negative instances as confirmatory, criticises Freud's use of his associations rather than Dora's, and his extraordinary reliance on fitting together the jigsaw puzzle pieces to form a plausible logical and associative structure - a concept which, naturally enough, Sand does not call by that name. She does this *without* assessing the value of Freud's evidence that there had been a recent sexual experience that led to repression. Freud gives absolutely no evidence that the embrace gave rise to a genital sensation and an orally based disgust which, after repressing the affect, created the symptom through an oral somatic compliance.

Rieff (1959, p.81), a non-analyst, thought "Dora could have turned down Herr K. for several good reasons .... Possibly she did not find him attractive". Erikson (1962) went so far as to question his master by wondering "how many of us can follow today without protest Freud's assertion that a healthy girl of fourteen would, under such circumstances, have considered Mr. K.'s advances 'neither tactless nor offensive'". Scharfman (1980, p.53) thought it "rather questionable that one would expect such a response in a girl that age when approached by a man who is her father's friend", a view shared by M. I. Klein and Tribich (1982a).

Much more important to the assessment of Freud's explanation is the reasonableness of Freud's assumption that Herr K.'s embrace and kiss had succeeded in arousing Dora genitally. For, without the pressure of Herr K.'s erect penis inducing genital excitation, the whole explanation of the symptoms fails: no repressed sexuality could have been revived, no feeling of disgust could be produced, no excitation could have been available for displacement on to the thorax, and no eyes need be averted from gallants in conversation. The same conclusion holds, rather more strongly perhaps, if one accepts Lewin's (1973-1974) fundamental re-interpretation of Dora's sexuality: "her sexual objects were women, and only women". If, as he concludes, "men did not turn Dora on", Herr K.'s kiss could have had no sexually exciting effect at all.

Ad hoc principles are most evident in the two bases proposed by Freud for his analysis of Dora's reproaches. That a string of reproaches might cover a similar string of self-reproaches was said by Freud to be something that "soon becomes evident" (Freud, 1905a, p.35). Freud said the idea that excessively intense reproaches might be reinforced by an unconscious trend of thought was suggested to him by "reflection" (op. cit., pp.54-55). No other justification is provided for these principles. Neither appears to have been discussed elsewhere in Freud's writings, although they do bear some resemblance to his views on 'counter-will'. From his assumption the reproaches simply cloaked the self-reproaches, Freud deduced Dora was in love with Herr K., and the unconscious reinforcement of the reproach was identified as her infantile love for her father. These two *ad hoc* principles, almost by themselves and without too much confirmatory evidence from Dora, seem to have provided Freud with the basis for characterising what he took to be two of the three most important emotional currents in her life. For, within what was probably two weeks of first seeing Dora, Freud was able to write to Fliess that her case "has smoothly opened to the existing collection of picklocks" (Masson, 1985, Letter of 14.10.1900) and it is certainly true he had made his characterisation before Dora had the first dream (Freud, 1905a, p.70).

Whatever the truth of these principles and their application it is evident they have a degree of arbitrariness. Lewin (1973-1974), in the first extensive reconsideration of the case, concluded, as we have seen, that Freud's picture of Dora's sexuality was basically incorrect. According to his

analysis of the reproaches, Dora was really in love with her own mother. What appeared to Freud to be a love for her father was really Dora's masculine identification that enabled her to retain her mother as her sexual object. Her love for Herr K. was a similarly motivated expression of feeling for Frau K. as a mother surrogate. Among other things this homosexual current explained her disgust with Herr K.'s kiss. On the other hand, Krohn and Krohn (1982) have it that Dora turned to Herr K. as a compromise for her repressed infantile and incestuous love for her father. Glenn (1980, p.29), however, has Dora struggling against K. because he was an unacceptable substitute for her father. Lewin (1973-1974), as one might expect, has her attracted to Herr K. as a secondary consequence of her unhappy love affair with her mother. That any of these re-analyses appears as plausible as Freud's is due largely to the slimly evidenced *ad hoc* principles and personal associations on which they all rest.

The influence of Freud's personal associations is just as marked in his interpretation of Dora's dream. That the dream element of being saved by her father represented the latent thought that it was her father who had actually exposed her to the danger of seduction was a speculation of Freud's, not an association of Dora's. Once having inverted this piece of the manifest content, Freud then, again on his own account, reversed the meanings of the dream thought concerning the present of the bracelet. In turn, that inversion generated his association of water as the opposite of fire and provided the links to thoughts of bedwetting and childhood sexuality. Each of the reversed-meaning interpretations came from Freud's associations - not one was provided by Dora. Freud actually presented Dora with those interpretations of the dream based on the reversed meaning of the elements before establishing whether or not she shared those associations with him (Freud, 1905a, pp.68-73). His conviction that his associations reflected the structure of her thoughts took precedence over her denials, uncertainties, and evasions. All such responses by her to his questions and interpretations were regarded as the fruits of repression and, therefore, as confirming the applicability of his associations. For example, Dora denied knowing that children were warned not to play with fire because they might wet the bed. Nor had she noticed a large match-stand freshly placed on Freud's table. Because of both these denials Freud assumed she was aware of the fire-water-bed wetting linkage he had discerned in the dream (op. cit., pp.71-72). Similarly her inability to recall whether she would have been pleased to receive the bracelet her mother had rejected was taken to mean she was confessing to a repressed thought (op. cit., p.69). Freud took the same line over the sexual allusion he recognised in the words 'jewel-case' - Dora's evasiveness as to whether she knew of its slang meaning confirmed she really had known, but had repressed the knowledge (ibid., n.4). Finally, her uncertainty about the time of the first occurrence of the dream was accepted as positive evidence it actually followed Herr K.'s proposal (op. cit., pp.65-66). Consequently the preconscious intent of the dream, the bed wetting allusions that led to

the latent thought of masturbation, the sexual association of the jewel-case, and the theme of a sexual love represented by the dream thought of the bracelet were all supplied by Freud in opposition to Dora's contrary associations or lack of them.

Much of Freud's analysis of the symptoms shows the same characteristic preference for his associations over hers. Apart from those mentioned earlier, one might note Dora's inability to recall whether she had masturbated as a child was taken in conjunction with symptomatic play with the reticule "as a further step towards the confession" Freud was seeking. Although, in the end, Dora remembered nothing, her failure to deny Freud's supposition was taken by him as confirming that she had masturbated (op. cit., pp.76, 79). Some modern analytic re-interpretations lead me to question the masturbatory symbolism. Glenn (1980, p.30) interprets the play with the reticule as a seductive gesture directed at Freud but Krohn and Krohn (1982) see it as the sexual penetration of another woman. Either interpretation severs the link with the masturbation Freud's reconstruction demanded.

It ought also to be noted that associations were obtained to only about one half of the elements of the dream's manifest content. None of the following elements were enquired into with sufficient persistence to trace their supposed unconscious roots: "My mother wanted to stop", "Father said: 'I refuse to let my children be burnt for the sake of your jewel-case'", and "We hurried downstairs". The element "A house was on fire" gave rise to some associations but was not itself traced to Dora's experiences or feelings. Some selection from the dream elements was clearly made, a fact also pointed out by Langs (1980, p.65) about Freud's failure to examine a number of specific day residues and by Kanzer (1980, p.76) who notes Freud's definite and positive reaction to the residue relating to the brother's being locked in at night.

Although not based on so detailed an analysis, there is a considerable body of opinion, mainly psycho-analytic, which supports my conclusions, or at the very least, is consistent with them. Rieff seems to have been the first to advert to Freud's domination of the treatment:

> Freud applauds his own persistence; he speaks of using facts against the patient and he reports how he overwhelmed Dora with interpretations, pounding away at her argument, until 'Dora disputed the facts no longer'. (Rieff, 1959, p.82)

Kanzer noted that Dora:

> was constantly pressured to confirm the analyst's interpretations and had little opportunity to freely bring forward her own associations, fantasies and ideas. (Kanzer, 1980, p.75. Cf. p.79)

Perhaps this was because, as I. Bernstein (1980, p.86) observed:

> Freud knew too much about Dora before the analysis started. It led Freud to make a number of formulations and to anticipate matters to an extent where he was relying less on material as it came from Dora than on the information he had already obtained outside the analysis.

Whether this was so or not, there is no doubt about the:

> uncharacteristic tenacity with which he [Freud] defends his interpretations to Dora and to his readers. (Krohn and Krohn, 1982. Cf. S. Fisher and Greenberg, 1977, pp.366-367)

And, with equally characteristic Lacanian ambiguity, Kohon describes Freud engaged in:

> a persistent search for truth - although we do not know whether it is Dora's truth or Freud's. (Kohon, 1984)

Begel (1982) even raises the possibility that Freud's interpretation of Dora's smelling smoke as she woke from the dream as a transference on to him of Dora's desire for a kiss from Herr K. was based as much upon Freud's wish as Dora's. Although expressed more politely than Rieff, these psycho-analytic opinions are just as damning of Freud's objectivity as his.

Various theoretical problems that plagued Freud at the time have been proposed as accounting for his behaviour. Maddi (1974) appears to have been the first to point out Freud was:

> ecstatic about Dora because he regarded her case as the clearest and most decisive evidence for his formulations.

Many psycho-analysts have subsequently accepted this point. Krohn and Krohn (1982) spoke of Freud's heavy narcissistic involvement "in looking for a case that would demonstrate and justify his emerging theories". Much the same thing was said by Langs (1980, p.63) and Scharfman (1980, pp.54-55) went so far as to say that in view of these "larger aims" the treatment was really of "secondary importance". But, of course, none of these psycho-analytic critics do what their criticisms really demand. Not one of them fundamentally rejects Freud's basis for interpreting the case. Each is wedded to what they think of as basic and reliable 'facts' about Dora. Despite their criticisms of Freud's unrelenting influence, most of them nevertheless seem to think that the resulting associations, even if they were, at the very least, obviously contaminated by Freud, provide valid information about Dora's symptoms and dreams.

Psycho-analysts most frequently use these criticisms to explain why Dora broke off the treatment. What is then most often stressed is Freud's sexualisation of it. Freud himself would undoubtedly have acknowledged the sexuality of his imagery about the case: it opened to his collection of picklocks, Dora's dream stood on two legs, etc. (Maddi, 1974; Scharfman, 1980, pp.49-50; Kanzer, 1980, pp.78-79; Kohon, 1984; Glenn, 1986).

Whether one accepts this Damoclean flight of psycho-analytic fancy or not, there can be no doubt that what have been called Freud's "repetitive sexual interpretations" eroticised Dora's relation to Freud (Scharfman, 1980, p.50. Cf. Langs, 1980, pp.63-65; Kanzer, 1980, p.79; Gill, in Reppen, 1982). Blos (1973), speaking specifically of Dora's adolescence, said:

> the consolidation of her neurotic condition had been short-circuited by the fact that her analysis was being conducted as if an adult neurosis already existed. As a consequence, the adolescent ego became overwhelmed by interpretations it was unable to integrate, and it simply took to flight. If there is one thing adolescent analysis has taught us, it is that ill-timed [sexual] interpretations are unconsciously experienced by the adolescent as a parental - that is, incestuous, seduction.

Glenn (1980, 1986) and Kohon (1984) have endorsed this conclusion, the former pointing to an important further consequence:

> Freud's premature interpretations of Dora's symptomatic acts and their masturbatory significance must have convinced her that he was a dangerous adult trying to seduce her. (Glenn, 1980, p.36)

In line with this, Glenn emends Freud's interpretation of the dream to include Dora's fantasy "that she was once more in danger sexually in her analysis with Freud as she had previously been with Herr K." (op. cit., p.65). Glenn has it that Freud's interest in her sexuality caused *Freud* to appear in Dora's dream as seducer as well as protector. One cannot go this far, of course. Dora had had the dream *before* the treatment began!

Muslin and Gill (1978) believe part of the difficulties Freud had with Dora were due to a very strong *positive* countertransference he had to her (Cf. Begel, 1982). Showing a little more insight than most psycho-analysts, they say of their interpretations of other of Dora's behaviour, "Of course, we recognize the speculative element in our suggestions and that, with ingenuity, anyone could make many more". Just as well. As they themselves report, but do not comment on, Marcus (1984) had already argued that Freud's handling of the case was impeded by his strong *negative* countertransference to Dora, an interpretation later also preferred by Possick (1984. Cf. Decker, 1982; Jennings, 1986).

Whether or not there was 'sexualisation' of the relation, there can be no doubt that Freud's interest in a sexual etiology contributed materially to the sexual content of Dora's so-called free associations.

We should also note that subsequent reinterpretations of Dora's dream have the same arbitrariness as Freud's original. For slightly different reasons, Lewin (1973-1974) and Krohn and Krohn (1982) claimed the fire represented destructiveness and aggression. On the other hand, Kanzer (1980, p.73) said the rescue from the fire by the father was a birth fantasy, an interpretation he took to be confirmed by the 'fire-water' dichotomy (which was, of course, Freud's!). Opposed to all of these is Erikson's

characteristically equivocal addendum:

> in Dora's first dream the *house* and the *jewel case*, besides being symbols of the female body and its contents, represent the adolescent quandary: if there is a fire in 'our house' (that is, in our family), then what 'valuables' (that is, values) shall be saved first? And indeed, Freud's interpretation, although psychosexual and oedipal in emphasis, assigns to the father standing by the girl's bed not the role of a wished-for seducer, but that of a hoped-for protector of his daughter's inviolacy. (Erikson, 1962)

Slipp (1977) and Decker (1981), admittedly in somewhat different ways, have placed so much stress on the social and/or interpersonal factors operating in Dora's family that intra-personal psychodynamic interpretations and formulations hardly seem needed to explain the case (Cf. Decker, 1982)

Whether or not the somewhat contradictory interpretations of the Dora case can be reconciled, the most important point to emerge from our analysis is that, whatever might have been said in the abstract about the subject's unconscious purposive ideas generating the associations, most of them actually came from Freud's quite conscious and purposive ideas.

CRITIQUE OF THE THEORY OF THE MIND

Two things distinguish Freud's theory of the mind from those of the philosophers, psychologists, and psychiatrists who had before him given credence to unconscious mentation. In his theory, mental contents are unconscious, preconscious, or conscious because they belong to the systems *Ucs.*, *Pcs.* or *Cs.* Most other theorists had proposed, like Fechner (cited in Meynert, 1884/1885, p.214), only that conscious ideas were distinguished from unconscious ideas by being more intense (Whyte, 1960). For Freud, no matter how intense an idea is, it cannot become conscious unless it makes itself admissible to the system *Cs.* Freud's adaptation of the reflex model of mental functioning is also different from that of his immediate predecessors - Brücke, Meynert and Exner - in that it can be activated by stored *psychological* needs. None of Freud's colleagues or teachers had been much concerned to explain psychological wishes, especially those in dreams, so that it had not been necessary to think of them being stored in a permanent repository. Meynert's model, for example, allowed that the infant might suck in its dreams, but it is clear that he thought this was the effect of the revival of cortical motor images of sucking by an actually present hunger need (Meynert, 1884/1885, p.170).

Once it is granted that a regressive flow of excitation can occur within the apparatus, and that that flow activates the functions and contents of each of the systems within it, Freud's model of the mind allows the immediate deduction of many of the important features of the dream. The placement of the systems *Ucs.* and *Pcs.* toward the motor end of the

apparatus defines the point at which the regressive flow of excitation is initiated, while the primary process of *Ucs.* modifies the flow such that the wish is disguised before appearing at *Pcpt.* The instigation by unconscious wishes, the incorporation of day residues from *Pcs.*, the effects of censorship, the consequences of the primary process in condensation and displacement, the revival of primitive modes of thought, and the hallucinatory visual imagery of the end product are the necessary consequences of the order in which the systems are successively energised. Closer examination of the model shows, however, significant deficiencies in the explanations of certain of the properties of dreams, of one of the important groups of symptoms, and of a number of aspects of normal mental function. Freud's account of the relations between the primary and secondary process is found to be similarly unsound. Repression and organic reinforcement, the two concepts most crucial to an understanding of dreams and symptoms, are uncharacterised theoretical terms and non-explanatory.

Most of the defects of the model itself arise either from the very assumption of a fixed temporal sequence in its working or from ambiguities in the way in which consciousness was thought to arise. Before considering these criticisms it is worth stressing that the model is no theoretical fiction, assembled for mere convenience of thinking about the dream, as has been positivistically implied by Gedo and Goldberg (1973, pp.3-5, 48-59). Freud thought his theory was an approximation to the real state of affairs and that if it were to be replaced it should be only by a theory that corresponded better to reality (Freud, 1900, pp.610-611). Freud's basic assumption was that a given psychological process resulted from the particular order in which the systems operated. He thought the minimum premise entailing that outcome was the supposition that excitation appropriate to a given process passed through the systems in a particular temporal sequence (op. cit., p.537). This premise, said Freud:

> does no more than fulfil a requirement with which we have long been familiar, namely that the psychical apparatus *must* be constructed like a reflex apparatus. Reflex processes *remain* the model of every psychical function. (op. cit., p.538. My emphasis, MBM)

No mere analogy with a reflex was meant, as Arlow and Brenner (1964, p.46) have argued. Nor was it a "crude analogy" that originated with Breuer, as Wollheim (1971, p.63) seems to imply. The reflex conception of the mental apparatus, and the fixed order with which the systems within it functioned, were the two central givens of Freud's theory.

Normally the movement of excitation is from the sensory to the motor end of the apparatus but, under special circumstances, the direction of the flow can be reversed. Two points should be made. First, Garma (1969) has noted "it is rather difficult to see how the reverse journey along the nervous reflex arc could be accomplished." This part of the theory seems to be based on inconsistent assumptions, whether or not its reflex basis is

meant to be taken seriously. Second, apart from a somewhat ambiguous reference to "a simultaneous exploring of one path and another, a swinging of the excitation now this way and now that" (Freud, 1900, p.576), Freud does not seem to have allowed for the simultaneous flow of excitation in opposite directions. Yet certain classes of hysterical symptoms and children's reactions to the frustration of their needs conflict with the assumption of a unidirectional excitatory flow. When the infant is most hungry, and presumably hallucinating the object that would satisfy its need, it is also most generally restless, often making sucking movements and crying. The hallucination requires a flow of excitation from *Ucs.* to *Pcpt.*, but the motor activity and the crying requires a simultaneous flow to *M.* Similarly, hysterical symptoms in which a motor response coincides with an hallucination, like some of those of Anna O. or Emmy von N., also require a simultaneous activation of both *Pcpt.* and *M.* While the hallucinations can be readily conceptualised as regressive products similar to the visual images of dreams, situating the motor system toward the output end of the apparatus makes it impossible to account for motor symptoms to be present at the same time. The path to *M.* is progressive while the path to *Pcpt.* is regressive.

Consciousness, Freud thought, was a kind of double-sided sense organ. One side of *Cs.* is directed to the system *Pcpt.* where it senses the existence of perceptual qualities. Attention cathexes disposed upon excitations within *Pcpt.* make those qualities conscious as the perceptions of particular sensory modalities. The other side of the sense organ is directed to "the interior of the apparatus itself" (op. cit., p.616), where in *Pcs.* it senses the qualities of pleasure and unpleasure resulting from variation in the quantities of excitation. Kanzer (1981) finds the work of Lipps, the Munich psychologist, to be the source of this particular idea about consciousness. When outlining the systems in *The Interpretation of Dreams* Freud did not at first indicate the relation of consciousness to them. Some thirty pages later he introduced the double-sided sense organ notion, expanding on this only in the very last section of the book, by which time it was clear that *Cs.* was directed to *Pcpt.* and to *Pcs.* A footnote of 1919 to the outline of the systems located consciousness in "the system next beyond the *Pcs.*" and followed this with the *non sequitur* "in other words, that *Pcpt.* = *Cs.*" Freud had made his intention clear even if the argument was faulty (Cf. Freud, 1900, pp.541, n.1, 574-575, 615-616).

What this dual sided concept entails is that there can be only two places in the apparatus at which consciousness arises: *Pcpt.* and *Pcs.* Combined with the assumption of a fixed temporal sequence or "direction" to the flow of excitation, this limited access to *Cs.* makes for problems in explaining thinking in dreams and in normal life.

According to the regressive plan, the products of the dream work can only become conscious through *Pcpt.* However much a dream thought may have been modified on its journey from *Pcs.* to *Mnem.*, it has to be

further transformed into sensory images before being represented in the manifest content. Thus, in Chapter 7 of *The Interpretation of Dreams*, where the theory of the mental apparatus is outlined, the discussion of the transformation by the primary process of a train of thought originating in *Pcs.* is entirely in terms of thoughts being prepared for perceptual representation (Freud, 1900, pp.592-597). Because direct access to *Cs.* from *Mnem.* is not provided for, there is no way in which the dream thoughts can appear in the dream as thoughts *per se*. Consequently, the presence of dream thoughts in the dream as other than perceptual images contradicts a most fundamental feature of Freud's theory. Arlow and Brenner have drawn much the same conclusion, although the force of their point is weakened by their incorrect assumption that Freud's theory requires the regression undergone by the dream thoughts to be both uniform and complete (Arlow and Brenner, 1964, pp.118-119). If by "complete" they meant all thoughts had to be in sensory form, their criticism may be accepted. But in insisting on the uniformity of regression, Arlow and Brenner seem to have overlooked some quite compelling reasons Freud advanced for supposing the primary process affects dream thought differentially: the associative closeness of a thought to the repressed idea, the number of associations a dream thought had with others, the intensity of the thought, and its capacity for visual representation. Each of these factors determine the degree of regression possible or necessary (Freud, 1900, pp.284, 295, 306-307, 340, 344, 544).

Freud is sometimes able to explain how thoughts *per se* can be incorporated into the manifest content of the dream without contradicting the requirement of sensory representation. For example, if the censorship was unable to prevent the manifest content from causing anxiety, Freud assumed the censorship itself might add the thought, "This is only a dream", to reduce the importance of the manifest content. Although the judgement is a genuine intellectual act its origins are in a still active portion of consciousness and is not produced by the dream work. Thoughts such as these, added to the dream after the manifest content has been nearly completed, need not appear in sensory form. However, not all thoughts occurring in the manifest content are like these *Pcs.* judgements. From Freud's own descriptions it is obvious some thoughts are part of the very fabric of the latent content. For example, in his dream of Irma's injection, Freud had dreamed "I thought to myself that after all I must be missing some organic trouble" and "I thought to myself that there was really no need for her to do that" (op. cit., p.107). When Freud's analysis of these thoughts is examined, they are obviously an integral part of a latent content otherwise represented visually. Although the thoughts may have originated in *Pcs.* while Freud was awake, and while they might not have been much distorted, it is impossible to regard them as simple *Pcs.* additions to an almost completed manifest content. The closeness of their links to other latent thoughts requires they should have sensory representation.

Freud does recognise that intellectual activity in the dream also poses the problem of how such an apparently *rational product* can result from the *irrational dream work*. This is, of course, a different problem from the representation of thought. It is a problem of where the rational ideas come from. Freud's solution is again that the thinking is only apparently produced by the primary process: the dream thoughts are once more actually in existence before the dream begins, mostly as *Pcs.* thoughts left incomplete during the day, and are simply fitted into the dream with greater or lesser distortion (op. cit., pp.445-459).

Had the model of the mental apparatus provided for direct access from *Mnem.* to *Cs.* without them going through *Pcs.*, dream thoughts other than sensory images could appear directly in the manifest content. Such a pathway would also allow for a more adequate explanation of normal recollective thought. In Freud's view, normal recollective thought required access to the raw material of the memory traces without perceptual hallucinations being produced. His "explanation" is that such thinking is initiated by a conscious process producing a regression only as far as *Mnem.* but which stops short of reactivating *Pcpt.* (op. cit., pp.542-543). Even were it not just a description, there are two other reasons why it would be a total failure as an explanation: first, since access to memories associated with non-repressed ideas is also possible only via *Ucs.*, the distortion the primary process should produce in them is avoided by some quite unspecified means and, second, the method by which the regressive process is halted at *Mnem.* is left unexplained. If mechanisms located in *Cs.* or *Pcs.* are supposed to be responsible, it is difficult to see how they have direct access to either *Ucs.* or *Mnem.* contents.

Arlow and Brenner (1964), Holt (1967), and Gill (1963) are among the psycho-analytic theorists who have drawn attention to the many inconsistencies with which Freud characterises the systems and functions of the mental apparatus. Although a number of the arguments advanced by these workers is accepted in the evaluation that follows, it is necessary to point out that some of Gill's critique is based on fairly gross misunderstandings of Freud's theory. Thus Gill (1963, pp.33-34) virtually identifies the primary process, which is *the mode of functioning of Ucs.* and cannot become conscious, with the dream itself, which is conscious. The dream is formed by the primary process at the behest of the censorship, and must meet its requirements before becoming conscious. It is precisely because some aspects of the dream do derive from the censorship that they are admissible to consciousness. Gill's confusion is made worse by his belief that Freud attributes the function of censorship to the primary process (op. cit., pp.98-101). Reading Gill's quotations from Freud carefully, and in context, shows Gill to be incorrect.

Censorship also poses a problem in that there are inconsistencies in its location, in its mode of operation, and in its relation to the dream work. Freud, variously places the censorship in *Pcs.* or on the border between

Pcs. and *Ucs.*, while also proposing a further censorship between *Pcpt.* and *Cs.* But, wherever the function is located, it is conceived of as exerting some continuous control over the distortion being brought about by the dream work. It is almost as if the distortion is being continuously tested to see if it has gone far enough. At a number of places in *The Interpretation of Dreams* the different locations of the censorships are discussed (Freud, 1900, pp.235-236, 553, 567-568, 615-618), and Arlow and Brenner (1964, Chapter 2 and pp.133-135), Abrams (1971a), and Gill (1963, Chapters 1 and 5) set out most of the problems caused by them. In essence, the difficulties reduce to the fact that, wherever it is located, the censorship has to exercise functions inconsistent with that location. For example, if the censorship is thought of as being a *Pcs.* function, it has to be regarded as a part of the secondary process; how then does it gain access to and exercise continuous control over primary processes going on in *Ucs.* Sandler and Sandler (1983) explore this particular "inconsistency" of Freud's in some detail. Their conclusion is that it is not possible to restore the second censorship either to this or to Freud's ' later theory of the mental agencies. McIntosh (1986) bravely resolves the problem of the second censor by declaring it to be purely terminological. He rejects the criteria of the relation to consciousness and the primary-secondary process for dividing the psyche. For him the basic division is between linguistic and non-linguistic activity and it is not then difficult for him to say "it is simply a misnomer to call the two systems the conscious-preconscious and the unconscious".

An additional complication, to which less attention seems to have been paid, is the matter of whether or not censorship is required after the dream thoughts have been transformed into sensory images. Freud's most direct remark was that consciousness would follow:

> when the content of the dream-process has become perceptual, by that fact it has, as it were, found a way of evading the obstacle put in its way by the censorship and the state of sleep in the *Pcs.* It succeeds in drawing attention to itself and in being noticed by consciousness. (Freud, 1900, p.574)

Censorship between *Pcpt.* and *Cs.* here seems specifically excluded. On the other hand Freud describes an attentional cathexis from *Pcs.* being directed on to the dream "after it has become perceptual" (op. cit., p.578). One imagines this attentional function as involving censorship. And, then again, in describing the excitation of *Pcpt.* produced by normal stimulation of sense organs, Freud remarks the image is "probably submitted to a fresh revision before it becomes a conscious sensation" (op. cit., p.616). Now, if external stimulation of *Pcpt.* provokes the censorship, the same ought to be true when the stimulation comes from within. Dream images are vivid enough to be mistaken for real images and there can be no basis therefore for the censorship differentiating between the internal and the external excitation of *Pcpt.* If the externally produced image is subject to censorship, the same would have to be true of the internally produced image.

A final confusion over the placement of the censorship is to be seen in an implication of the 1919 footnote equating *Pcpt.* with *Cs.* (Freud, 1900, p.541, n.1). If those two systems are identical there can hardly be a censorship between them. And, according to the footnote, the identity of *Pcpt.* with *Cs.* is implicit in the original theory. Of course, in the case of normal stimulation, Freud might have meant only that, because all such stimulation begins at *Pcpt.*, the excitation to which it gives rise must pass through *Mnem.* and *Ucs.*, before meeting the censorship at the border of *Pcs.* and *Ucs.* or in *Pcs.* But that proposal has its own difficulties: it is unlikely Freud could have consistently maintained that all mental contents had to pass through the system *Ucs.* before becoming conscious.

Censorship is also related to condensation and displacement in an inconsistent way. Both processes form part of the dream-work, seemingly contributing equally to dream distortion (op. cit., pp.307-308), both reflect primary-process thinking, having as their more intimate mechanism the free discharge of energy (op. cit., pp.595-596), a basis said to enable displacements of energy to facilitate condensation (op. cit., p.339), yet only displacement was said to be a function of the censorship (op. cit., pp.308, 507, 533), [although Freud did at least once attribute both processes to the same motive (op. cit., p.482)]. Some years after *The Interpretation of Dreams* had appeared, Freud remarked:

> although condensation makes dreams obscure, it does not give one the impression of being an effect of the dream-censorship. It seems traceable rather *to some mechanical or economic factor*, but in any case the censorship profits by it. (Freud, 1916-1917, p.173. My emphasis, MBM)

He once again added that displacement was "entirely the work of the dream-censorship" (op. cit., p.174). What the inconsistency reflects is Freud's difficulty in reconciling an explanation of dreams in terms of a regressive flow of excitation, where distortions are produced automatically, with an explanation in terms of wishes, psychological forces, and counter-forces. I return briefly to this conflict in the next section.

The concept of secondary revision suffers from the same kinds of obscurities as censorship. Freud's description of secondary revision as forming part of the dream work has the consequence of it creating coherence despite its being part of a disorganising primary process. Although Gill (1967) has noted that the difficulty of attributing judgement in dreams to secondary revision was removed by Freud's later supposition that it was not part of the dream work (Freud, 1923a, p.241) to then think of it as a *Pcs.* function entails the further inconsistency of allowing *Pcs.* direct access to the primary process. Breznitz (1971) has drawn attention to the fact that Freud describes secondary revision in *The Interpretation of Dreams* as taking place at three distinctly different times: during the dream work, during sleeping but after the dream work has been completed, and during the attempted recollection of the dream. No function belonging to any one system can operate at the different levels these different times

imply. Heynick (1981) has brought out very clearly how Freud's attempt to make it do so in allowing for speech to be incorporated in dreams created "a theoretical problem less amenable to a satisfying solution". As we have noted, secondary revision is obviously the direct descendant of "the compulsion to link together any ideas that might be present in the same state of consciousness". Here Freud tries to put it to work across three different "states". But, in his model, no one function can so operate.

The characteristics of the systems *Ucs.* and *Pcs.* and those of the associated primary and secondary processes are inconsistent with one another in a number of important respects. The inconsistencies may be conveniently considered developmentally and in relation to the functions exercised by each system. Holt (1967) provided a number of weighty reasons for supposing Freud's characterisation of primary process functioning to be such that secondary process thought, and *Pcs.* as a system, can never have developed from it. On the basis of experimental findings and direct observations, he argued it is most unlikely the infant's memories are sufficiently veridical for it to make a discrimination between real and hallucinatory images. On general grounds he also argued that repeated frustration following "barren hallucinatory attempts at immediate gratification" is unlikely to lead to adaptive, secondary process methods of need satisfaction. What is known from child development studies and the psychology of learning is, broadly speaking, consistent with this argument. Holt's most telling argument (curiously relegated to a footnote) is that the infant would be unable to differentiate the real and hallucinated objects: both would be present simultaneously as the need is satisfied. No dynamic capable of urging the infant toward the real world can then be created. Thus, while it remains true that an hallucinated breast cannot feed a hungry baby, the infant's inability to differentiate the synchronously proferred, perceptually identical, real breast that can, would simply not allow the secondary process to develop. Steele and Jacobsen (1977), who believe that Freud's proposition that the primary process precedes the secondary is mere assumption, and a problematical one at that, also argue that the problem of the emergence of the secondary function is not *how* the change takes place but that there is *any* change at all. They further point out that, before an hallucinatory wish-fulfilling object can be conjured up, it must have been first experienced. Lastly, they point out that it is difficult to see how there could ever be any observational proof of an hallucinatory stage. On the grounds that the degree of cognitive differentiation required contradicts "all the available evidence on cognitive development", Wolff (in Schafer, 1965) has challenged the assumption that the first instance of ideation could be hallucinatory wish-fulfilment. Of Provence's view that a theory of infant development within the framework of psycho-analysis is possible, he also said it was "more our hope than our achievement".

Of the inconsistent characterisation of the properties and functions attributed to *Ucs.* the most important is that the unconscious is supposed to be able to store highly structured repressed fantasies. According to the

description of the primary process, the free discharge of energy prevents the development or storage of structured thoughts. Yet analysis of dreams and symptoms supposedly reveals the existence of well organised but unconscious fantasies. Fifteen years after completing *The Interpretation of Dreams* Freud himself acknowledged this logical difficulty, which has also been commented upon in more recent times by a number of psychoanalytic theorists (Freud, 1915c. Cf. Gill, 1963, 1967; Arlow and Brenner, 1964; Arlow, 1975). Although it is well recognised that Freud eventually made profound alterations to his theory in attempting to overcome this inconsistency, one important implication of the initial formulation seems never to have been drawn. It is that Freud's explanations of the formation of symptoms and of some types of dreams require the existence of a class of fantasies the theory of the mind says can not exist. Repressed fantasies can not exist in *Ucs.* and can not therefore be incorporated into dreams. Dreams incorporating such fantasies disprove the theory. Further, because fantasies well enough structured to resemble real memories of childhood seduction cannot exist in *Ucs.* they cannot explain hysterical symptoms.

Finally, three important deficiencies are discernible in Freud's explanation of the indestructibility of the infantile unconscious wishes. First, the mere lateness of the development of the secondary process does not entail the inability of that process to gain control over the earlier wishes. Freud's account of the development of the secondary process out of the primary is a description rather than an explanation. Second, there is an important inconsistency in Freud's description of the transformation of affect in repression that is crucial to the creation of the reservoir of repressed infantile wishes. Freud argued the transformation could be inferred to have taken place when children were observed to react with disgust to activities they had previously found pleasurable. He then hinted that the alteration was "related to the activity of the secondary system" (Freud, 1900, p.604). If he is thus ascribing the transformation to the secondary system, Freud has assumed the secondary process has enough access to the affect of the unconscious memories to turn it into its opposite. As we saw, Freud elsewhere proposed that the second system was unable to alter those memories or to do more than direct their excitation into realistic channels. Consequently, this part of the theory makes inconsistent assumptions. Third, organic reinforcement, the force enabling the repressed wish to gain access to *Pcs.*, is not only uncharacterised - it is undefined. In *The Interpretation of Dreams* it is mentioned only once, and then only in the context of the revival of the permanently repressed wish (op. cit., pp.604-605). In line with this is Brenner's (1979) observation that drives themselves are not explicitly included in the theory of the mind set out in *The Interpretation of Dreams.* Even in Dora's history, where the notion is mentioned frequently, it is quite inadequately described. Freud's account of the immutability of infantile wishes is thus a mixture of description, assumptions, and uncharacterised theoretical terms masquerading as an explanation. He advanced no compelling reasons for supposing repressed

unconscious wishes from infancy remain unchanged over time and continue to influence the production of adult dreams and symptoms.

Perhaps there were no such reasons. According to Hartmann, Freud can be read as arguing for *two* outcomes of repression, "mere repression and the true disappearance of an old desire or impulse" (H. Hartmann, 1952). That is, the repressed impulse may be so changed as to disappear. Loewenstein has also said Freud once actually did acknowledge that psychoanalytic treatment caused repressed unconscious wishes to *lose* their effects after they had been raised to consciousness and correlated with reality (Arlow, 1958). I take it that Freud does hold that treatment enables previously repressed unconscious mental contents to become accessible to consciousness or, to anticipate his later famous slogan: Where *Ucs.* was, there *Cs.* shall be. However, E. Kris (1956), M. H. Stein (1965), Malcolm (1982, pp.161-162), and A. L. Rosenbaum (1983) all cite instances of patients who retained no knowledge at all of the dynamic processes allegedly revealed to them in their successful treatment.

Two not unimportant general consequences may be deduced from the preceding criticisms: explanations using the concept of repression or censorship - its coyly disguised relative - will be limited because of the uncharacterised nature of those terms. A similar restriction applies to the use of the concept of organic reinforcement. At a basic level then, Freud's explanations of many phenomena, and not just of symptom formation or dream production, are rather less than adequate.

A 'THEORY' OF WISH-FULFILMENT?

Two rather different kinds of theory may be discerned within *The Interpretation of Dreams.* The first is cast in terms of mental structures and the regressive flow of excitation from one structure to another, while the other draws on such concepts as drives and wishes, and mental forces that, like the censorship, oppose them and force them into disguise. According to the first theory, the peculiarities of the dream arise because the unpleasure, which the arrival of a repressed dream thought from *Ucs.* would cause if it entered *Pcs.*, forces the thought away from *M.* Passing through the primitive *Mnem.* systems it is further distorted by being prepared for representation as a visual image in *Pcpt.* This theory is a structural and economic (energy) one. By itself it accounts, as Freud noted, for the characteristically distorted manifest content (Freud, 1900, p.543). On the second theory, referred to by some psycho-analysts as the clinical theory, an unacceptable infantile wish, forced out of consciousness and kept out by a repressive force, seeks re-admission but can only do so if its disguise bypasses or hoodwinks the repressive censorship. Dream distortion is forced on the latent dream thought by the demands of the censorship.

Because Freud regarded the theory of the mental apparatus as so much disposable conceptual scaffolding (op. cit., p.536), it might be argued that at least some of the criticisms I have set out in this Chapter can be met by noting they are largely irrelevant to the second theory. Many psychoanalysts have done so, most recently G. S. Klein (1975 and 1976, Ch. 3), Schafer (1976), and Stolorow and Atwood (1982). Even without a reflex theory of the mental apparatus it would still be possible to think of repressed wishes as the source of dreams, to regard their peculiarities as a result of condensation and displacement carried out at the behest of the censorship, and so on. All that would seem to be lost, if it may be so put, is the general theory explaining dream production at other than this fairly immediate psychological level.

Three summary points from among many may be made about this argument. First, it concedes Freud was no theorist. Second, in what sense does a set of statements based on the proposition that dreams are wish-fulfilments constitute a *theory* of dreaming? Third, if such a theory is possible, how might it be tested?

Let us leave the first point for later elaboration and begin with the second. Whether the wish principle was arrived at by Freud analysing his own dreams, as the contemporary evidence suggests, or whether it was based primarily on the dreams of his patients, as Freud's later accounts imply (Freud, 1900, pp.100-101; 1916-1917, p.83; 1932, pp.219-220), an impression is conveyed, as Fancher (1971) comments, of the principle resulting from strictly empirical and inductive enquiry. Freud also claims:

> In most dreams it is possible to detect a central point which is marked by peculiar sensory intensity ... [which] is as a rule *the direct representation of the wish-fulfilment* (Freud, 1900, p.561. My emphasis, MBM)

If this quality can be detected so readily - although Freud neither defines it nor gives examples - the wish-fulfilment proposition must be empirical. Now, many of the facts about dreams are explained with similar empirical propositions. Suppose, for example, it were true that direct wish-fulfilling dreams occur less frequently among adults than among children. [2] Explaining that fact requires the central wish proposition and two ancillary propositions, one of which assumes that realistic thinking replaces wishful childish thinking during development and another that assumes that during adult dreaming some realistic thought is still present. Both of these propositions are empirical. That being the case, it is possible that the theory is not a theory at all but a collection of empirical statements. I

2. As far as I can determine it is very doubtful that children's dreams are all that transparent. On the basis of his many studies, Foulkes (1978, p.44) concludes that children's dreams "seldom" have an obvious wish-fulfilling character. All that Ablon and Mack (1980) could be sure of was that children's dreams were based on "what matters most deeply to the child". They also issued a caution about the value of such concepts as 'the working through of traumatic experiences' or 'mastery' in relation to frightening children's dreams.

believe this point holds even if the principle of wish-fulfilment does emerge "rather suddenly as a logical deduction from the assumptions [of the *Project*]", which is the central thesis of Fancher's (1971) paper, that emergence is not at all inconsistent with the principle having been arrived at empirically before the *Project* was written.

Third, whether the wish-fulfilment theory can be tested at all seems to have been doubted by Freud himself. About a month after *The Interpretation of Dreams* appeared, while investigating his theory with a willing philosopher-subject, he wrote to Fliess, "There is too much that is new and unbelievable, and too little strict proof. I did not even succeed in convincing my philosopher, though he was providing me with the most brilliant confirmatory material." He added, *ad hominem*, "it is easy for a philosopher to transform inner resistance into logical refutation" (Masson, 1985, Letter of 9/14.12.1899). Freud also said the proposition that dreams were wish-fulfilments was an 'assertion' or 'assumption' that could neither be proved nor disproved:

> we have constructed our theory of dreams on the assumption that the dream-wish which provides the motive power invariably originates from the unconscious - an assumption which, as I myself am ready to admit, cannot be proved to hold generally, though neither can it be disproved. (Freud, 1900, p.598)

And, in a slightly different way:

> this assertion cannot be proved to hold universally; but it can be proved to hold frequently, even in unsuspected cases, and it cannot be contradicted as a general proposition. (op. cit., p.554. Emphasis removed, MBM)

Freud's reference to the impossibility of establishing the universality of the proposition is very likely an allusion to the problem of trying to establish the truth of such propositions inductively. Even were every dream ever dreamt to be analysed and found to confirm the wish principle, that demonstration would not guarantee it would hold for the next one.

In any case, a successful demonstration might not carry much weight. According to R. M. Jones (1970/1978, pp.120-122), who is generally quite sympathetic to Freud's dream theory, wish fulfilment is a "consequence" of dreaming and not its "cause". Unconscious wishes, in his view, can only be incorporated into dreams and not instigate them. Making a related but somewhat stronger point, Foulkes (1978, p.45 and n.4) argued that the wish-fulfilment hypothesis is not empirical but "the only possible outcome" of using free-association within the framework set by Freud's other theoretical ideas.

Freud's claim that the wish-fulfilment proposition cannot be "disproved" or "contradicted" may involve an even more serious objection. If it means anything other than that the theory is completely impervious to empirical test, it must refer to some difficulty in the method of

analysis itself, perhaps the indeterminacy of the result:

> Dreams frequently seem to have more than one meaning. Not only ... may they include several wish-fulfilments one alongside the other; but a succession of meanings or wish-fulfilments may be superimposed on one another, the bottom one being the fulfilment of a wish dating from earliest childhood. (op. cit., p.219)

Elsewhere Freud said "it is in fact never possible to be sure that a dream has been completely interpreted" (op. cit., p.279). Sulloway described how virtually none of the early reviewers of *Interpretation of Dreams*:

> even the most friendly and respectful, failed to point out the questionable and unprovable nature of many of Freud's dream analyses. (Sulloway, 1979, p.347. Cf. Kiell, 1988)

Sulloway also drew attention to the role played by what he terms "the methodological difficulties entailed in the objective application of [Freud's] technique" (ibid.). In *The Interpretation of Dreams*, Freud did not set out any rules by which dreams were to be interpreted. His failure was criticised at the time and he did concede the fact (Decker, 1977, p.173). Writing as a psycho-analyst, Spanjaard was explicitly critical. He could find:

> nowhere in Freud's writings ... an exact formulation of [the] procedure for conducting interpretations. (Spanjaard, 1969)

And there were many other analytic complainants whom Spanjaard cites. Over eighty years after Freud's main work on dreams, Grinstein (1983) eventually came to the rescue with his "systematic presentation of basic rules" of interpretation.

Even where there are rules they are readily denied. Thus Palombo (1984) denied the neutrality of the day residue: it "is always the point of access to an associative network of emotionally significant recent events". Unlike some of the recent commentators, however, Palombo does not deny the role of the repressed childhood wish, largely because he wants to use characteristics of the manifest content to identify the associational pathway leading to it. Consequently, there is uncertainty about how to interpret, as well as questions about the complexity of the task. Both provide plausible reasons for Freud's conclusion that the wish-fulfilment proposition could be neither proved nor disproved.

There is another difficulty about wishes. The basic wish represented in a dream had to be "an infantile one" (Freud, 1900, p.553). But it is noticeable this proposition was not established for important dreams like those of Irma's injection or of the botanical monograph. Eleven years later Freud added to the short essay *On dreams* the proposition that "repressed infantile *sexual* wishes provide the most frequent and strongest motive-forces for the construction of dreams" (Freud, 1901a, p.682. My emphasis,

MBM). But Freud hardly ever demonstrated that there was any infantile wish let alone that it was sexual. Jones, who has also noted the absence of evidence for infantile repressed wishes, says in fact that "there almost never is" a way of testing for their presence (R. M. Jones, 1970/1978, pp.12-13, 123. Cf. Foulkes, 1978, pp.37, 57-58). Freud asserts but does not prove. In any case, Steele and Jacobsen (1977) argue that the match between the interpreted manifest dream, which is supposed to be the latent dream, and the latent dream itself is always "uncertain" and conclude that "In no way do associations show the childhood scene as a cause of the manifest dream". They declare Freud's contrary conclusion to be a simple *post hoc* explanation. Freud, they also observe, relied explicitly on the "copious and intertwined associative links" provided by the dreamer, that is, on the dream equivalent of the logical and associative structure, but in so far as he says he has suppressed the relevant material from his own dreams, he passes "beyond the bounds of acceptable evidence". After noting that the wish in the Irma dream was a preconscious one, Erikson (1954) attempted to find the infantile sexual wish behind it. That he actually used his own associations rather than Freud's is sufficient evidence that the rules for interpretation are too generously framed for the real role of infantile sexual wishes to be established.

This basic indeterminance in the interpretative process has generated the many attempts to revise the wish-fulfilment theory so as to do away with unconscious wishes, censors, and disguises altogether. As early as 1937, Lorand stressed the clinical utility of direct interpretation of the manifest content, an approach that although criticised then, seems always to have appealed (Babcock, 1966), being re-endorsed recently by Eckardt (1982) and Pulver (1987). Partly this is due to Freud's own example. Spanjaard (1969) pointed out how Freud incorporated the manifest reproaches he directed against Irma "as a very essential element" in his interpretation and that, in the Dora dream, the manifest content "is identical" with the interpretation. Again, Greenberg and Pearlman (1978) used Schur's (1966b) material concerning Emma Eckstein's operation to re-interpret the Irma dream. According to them, the manifest content portrays, in a way but little disguised, Freud's then current waking concerns. Although Emma was not Irma, as Schur and they suppose (Masson, 1984, pp.205-206), Fliess had, at Freud's request, examined Irma to rule out nasal pathology (Freud, 1900, p.117) and it is reasonable to suppose Freud's anxieties about Irma and Emma were linked. Greenberg and Pearlman accept Schur's contention that Freud had repressed the Emma episode. As a consequence he came to emphasise the meaning of the dream as "a mere effort to disguise an unacceptable wish pressing for discharge". They do not see the concepts of drive-discharge and dream-censorship as any longer "necessary to our understanding of dream formation", a conclusion apparently shared by Eckardt (1982).

Although Greenberg's and Pearlman's argument about the Dora dream is plausible it is not without difficulties. First, how does the wish that

Freud seemed to uncover fit the drive discharge model. Second, neither they nor Schur consider a much more likely interpretation than Freud's 'repressing' the Emma incident. Freud, unwilling to have Fliess' negligence paraded in public again, prepared an interpretation for the book in which reproaches about his own carelessness, rather than Fliess', were laid to rest. The central point of Greenberg's and Pearlman's argument is plausibly convincing nevertheless: Freud's conscious worry about Emma is directly reflected in the manifest content. In fact, they go further and cite the analyses by Schorske (1975) of some of Freud's other dreams which show the manifest content to have derived from important waking experiences rather than from the indifferent ones his theory postulated. Palombo also points out that although Freud thought the relations between the elements of the manifest content, particularly the coherent ones, should be disregarded as illusory, he also routinely broke his own rule:

> The meaning of the dream as a whole is often both plainly visible and clearly related to the meanings that emerge from associations to its individual elements. To see this we have to look no further than the most interpreted of all of Freud's dreams, *the dream of Irma's injection.* (Palombo, 1984. My emphasis, MBM)

This reliance on the manifest content has led to such absurdities as Hartman's (1983) outline of eight very different "themes" discerned by psycho-analysts in Freud's Irma dream - to which he then adds his own and those of Elms (1980) and Blum (1981). Since then, Langs (1984) counted nine equally different *interpretations*, also made by other psycho-analysts, before he too added his own (Cf. McLaughlin, 1981).

Non-analytic approaches seem equally as valid as the analytic. Using a structuralist approach developed for the study of myth, Kuper and Stone (1982) arrive at an interpretation similar to that of Greenberg and Pearlman (1978), but which is in some respects more comprehensive and seemingly correct interpretation. Yet their method is so fundamentally different from the psycho-analytic that, as they say, free-association and "the entire distinction between the manifest and the latent dream is put into question". Much the same thing can be said about Spence's (1981) use of Freud's waking concerns to interpret the botanical monograph dream. Using his own quite specific and non-psycho-analytic rules, Spence thinks independent judges could *generate* a dream similar to Freud's merely from "a description of the dream day and its three key events". Alongside prediction like this, post-diction interpretation would be child's play. The wish-fulfilment 'theory' would require neither confirmation nor disproof because it would simply not be needed.

As we have seen, shortly after *The Interpretation of Dreams* appeared, a 'victim' offered himself to Freud as a subject through whose dreams the dream theory might be tested. He was the Viennese philosopher, Heinrich Gomperz. After six months the pair gave up. As Gomperz wrote:

> The experiment proved a complete failure. All the 'dreadful' things which he suggested I might have concealed from myself and 'suppressed' I could honestly assure him had always been clearly and consciously present in my mind. (Cited in Masson, 1985, p.388)

After nearly ninety years, psycho-analytic and critical thinking about dream interpretation seems to have confirmed Gomperz.

THE SOURCES OF THE DIFFICULTIES

Decker (1977, pp.288-289) has shown that among the contemporary responses to Freud's *The Interpretation of Dreams* there was a well defined trend in which the ingenuity and plausibility of Freud's explanations were acknowledged at the same time as obvious but unclearly formulated doubts about their validity were expressed. For example, in what Decker described as "one of the most appreciative discussions", Sokal wrote:

> In the analysis of ... particular examples ... Freud shows himself to be a true master of psychological observation, though just this virtuosity of his interpretive artistry may arouse in some a doubt as to the scientific worth of the theory. (op. cit., p.289)

Decker took Sokal's use of the phrase 'interpretive artistry' to be a "damning indictment" of Freud. More recently Laplanche was even more condemnatory:

> The arguments are off the mark, and often the logic which is used is the famous logic of the 'cauldron' [3] or indeed, to take another image, that of the famous bent rifle, which enables one to shoot round corners (Laplanche, 1981)

He illustrates his point by saying that Freud's arguments in support of the wish-fulfilment thesis show:

> inadequate examples, inappropriate reasoning, recourse to the pseudo-certainties of the manifest content ...[and] the contradictory revisions in the successive editions of the book. (ibid.)

Yet Laplanche is insistent about the theory: "it works"! For him, as for most psycho-analysts, Freud's theses win out over the logic or the facts.

My analysis has identified the explanatory flaws that provide the bases for these misgivings. Partly they follow from the very assumptions of the theory itself and partly they come from Freud's not specifying the rules for interpreting dreams and symptoms (and we will see in Chapter 15 that it will never be possible to specify them). Even were we prepared to apply the theory to Freud's associations rather than to Dora's, such explanatory

3. Laplanche so alludes to the irrational, primary process mode of thought of the unconscious as Freud conceived it.

gaps are revealed that Freud's interpretations and speculative reconstructions cannot be taken seriously.

Over and above those issues is the matter of the control which Freud's own ideas exerted over the associations and interpretations generated during the application of the theory. We have found no reason to suppose, as ought to be the case with a scientific theory, that the personal beliefs of the observer are likely to be irrelevant to tests of the theory. Freud believed any one observer ought to be able to uncover the same unconscious wish as any other. As early as 1909, the American psychologist Morton Prince, whose attitude to Freud's theory was then reserved but not unsympathetic, confirmed some aspects of the dream theory but not, despite reasonably intensive inquiry, the roles of wishes, processes like censorship or repression, or compromise formations (Prince, 1910a. Cf. E. Jones, 1910; Prince, 1910b). Prince's findings are virtually at one with those contemporary psycho-analysts who have found it unnecessary to draw on either Freud's drive-discharge theory or the concepts of wish-fulfilment, censorship/repression, and the disguise of dream thoughts.

It is, of course, a commonplace that dream interpretations are similar within those schools of psychotherapy using it but different between them. Here the six discrepant interpretations of a set of dreams gathered together by Fosshage and Loew (1978) from a single patient are of interest. The interpreters had no direct knowledge of the patient or of her associations, so that the data provide a less than complete test of the commonplace. But hardly anything is shared in the interpretations. Of especial importance is the grossness of the discrepancies between the two psycho-analysts, one (Garma) more or less adopting a classical approach, and the other (Padel) the revisionist "object-relations" standpoint. Even Freud's hard-and-fast symbols were given quite different meanings.

A well documented instance throws similar doubt on the objectivity with which symptoms are analysed. During the 1920's Otto Rank, a close colleague of Freud's, announced he had been able to trace symptoms back to recollections of the trauma of birth. In discussing the role of suggestion in therapy, Edward Glover, a leading British psycho-analyst, remarked on:

> the rapidity with which some analysts were able to discover "birth traumas" in all their patients for some time after Rank first published his book on the *Trauma of Birth*, and *before it was officially exploded.* (Glover, 1931b. My emphasis, MBM)

Glover here implicitly acknowledges that at least some of the clinically discovered 'facts' of psycho-analysis depend upon the preconceptions of the analyst. It is more than curious that a *science* should have something that can be described, even flippantly, as an 'official' view of its facts. His description raises the possibility that the official view is based on the particular set of preconceptions held by the dominant members of a given "school" of analysis.

CONCLUSIONS

Many features of the dream may be deduced if the mental apparatus has the structure Freud supposed it to have and if the flow of excitation within it during dreaming is predominately regressive. In relation to symptoms, the main weakness is that the flow of excitation at any one time is assumed to be unidirectional so that the theory is unable to account for the simultaneous occurrence of hallucinations and motor activity. Inconsistencies in the placement of the censorship make it hard to see how that function is exercised and the limited access of the systems to consciousness makes it impossible to explain how preconscious thoughts can be incorporated into the fabric of the dream as thoughts or how normal recollective thinking takes place.

The characterisation of the systems of the apparatus, particularly the unconscious and preconscious systems and their related primary and secondary processes, is inadequate and inconsistent. Consequently the theory cannot explain how infantile wishes and other mental contents come to be unconscious, repressed, or indestructible. Neither can the theory account for the formation of the structured and unconscious fantasies basic to the explanation of hysteria and the other psychoneuroses.

There is, of course, the further problem that what is called the 'theory' of dream formation may not be a theory at all. If it is a theory, it is either a theory that does not involve processes different from those of waking thought, and so does not require special testing, or it is quite untestable in any ordinary way.

Leaving these problems aside and looking at the theory from Freud's point of view, there is a most noticeable gap in that the repressed infantile sexual wishes supposed to be basic to dream formation are treated as givens. Freud makes no attempt to characterise them or to probe their origins. In a sense the theory is too mentalistic. It is a theory of a mental apparatus without a bodily or organic core. The theory of childhood sexuality to be examined in Chapter 10 fills the gap by providing what purports to be a biological basis for infantile sexual wishes.

10 A THEORY OF SEXUALITY

> Holmes: The individual represents in his development the whole procession of his ancestors, and ... a sudden turn to good or evil stands for some strong influence which came into the line of his pedigree.
>
> Conan Doyle: *The Return of Sherlock Holmes.*

Prior to 1900 Freud had emphasised the role of sexual motives in normal and abnormal behaviour but it was not until the *Three Essays on the Theory of Sexuality* of 1905 that the systematic attention he had given to the nature of sexual behaviour *per se* resulted in a formal theory of sexuality. The central problem was that Freud's interpretations and reconstructions of the sexuality of the psychoneurotic, especially the hysteric, seemed to show that it had been dominated from early childhood by perverse impulses. It was not surprising, then, that Freud's consideration of sexuality was based equally on an examination of the sexual practices of the sexually deviant and the manifestations of sexuality in childhood.

I begin Chapter 10 with an attempt to determine the origins of Freud's concept of an instinctual sexual drive. I argue that its characteristics owe more to the picture of human sexuality he drew in the actual neuroses and to the explanatory problems created by the collapse of the childhood seduction theory than to the biological and psychiatric literature of Freud's day. Then, after summarising his basic arguments and concepts concerning perverse and infantile sexuality, I make two critical evaluations. One is of the concept of the sexual instinctual drive itself, where I argue that Freud's earlier work provided him with an inappropriate model for the new concept. The other is of the theory of sexual development, where the argument is that, rather than giving an explanation of how adult sexual life emerged, Freud gave only a description - and an inaccurate one at that.

THE SEDUCTION THEORY AND THE SEXUAL DRIVE

After the seduction theory collapsed, Freud gradually came to the view that the patient had recalled a fantasy. As we shall see, various theoretical and logical requirements virtually forced him to postulate some internal cause for the perverse sexual content of the fantasies, for the ways in which the fantasies were connected to the symptoms, and for the ways in which what were undoubtedly memories of other real events became linked with both.

Already in the four to six month period before the collapse of the seduction theory Freud had given a reasonable amount of consideration to neurotic fantasies (Masson, 1985, Letters of 6.4.97, 2.5.97 and Draft L, Letter of 16.5.97, Draft M of 25.5.97, and Draft N of 31.5.97). The content of these fantasies was not of childhood seduction, of course, and Freud thought of them as having a different place among the determinants of symptoms. Freud's first, and rather rapid conclusion, was that, although these other fantasies were based on early memories, they actually formed in later childhood and were "psychic facades produced in order to bar access to these memories" (Draft L of 2.5.97). The kinds of fantasies Freud mentioned in this connection were those of worthlessness in female patients, which he regarded as based upon an identification with the servant girls who had been involved in liaisons with the patients' fathers or brothers (Draft L), fantasies of being illegitimate (Draft M), and (possibly) death wish fantasies directed toward the parent of the same sex (Draft N).

Memories of real events, often the 'primal scene' of the seduction, gave rise to fantasies and impulses which in turn determined the symptoms (Draft M). The memories bifurcated, one part being "put aside and replaced by fantasies", while "another accessible part" seemed to lead directly to the impulses. The fantasies were primary; Freud even wondered if later on the impulses themselves derived from the fantasies (Draft N). Now, although Freud mentioned only the hostile death wish toward the parent of the opposite sex as an example of such an impulse, he described the impulses as perverse, usually in contexts implying they were always so. However, impulses deriving from specifically sexual experiences were first mentioned in the Letter of 2.5.97, where they were described as appearing in a distorted form as the symptoms of obsessional neuroses. Then, only two weeks before the collapse of the seduction theory, Freud linked the symptoms, the memories, the fantasies, and the perverse impulses together by stating that, as well as causing symptoms, memories gave rise to the fantasies and to the impulses. When both of these were repressed "the higher determinations of the symptoms already following from the memories make their appearance" (op. cit., Letter of 7.7.97).

An alternative source

When the seduction theory collapsed it was not only necessary to find another source for the symptoms - the interconnection of perverse impulses and fantasies with the symptoms had also to be explained. The more specific questions with which Freud was faced were: Why was the false memory one of a perverse sexual experience?, What was the nature of perverse sexual activity in general?, and, What was the nature of perverse sexual activity in childhood? The thesis Freud eventually arrived at was that perverse impulses existing in childhood gave rise to both the fantasies and the symptoms. Thus, instead of pathways from "the primal scenes" of seduction bifurcating and leading to the impulses and fantasies which then

re-united at the symptoms, the impulses were now the original forces and they led by way of the fantasies to the symptoms.

But, well before he abandoned the seduction theory, Freud had proposed that most of the basic hysterical symptoms, as well as some "exceedingly common" others, reproduced the sensations produced in the victim's mouth, anus, and genitals during the seduction experience (Freud, 1896c, pp.214-215). Only those sensations had the right determining quality for the resultant symptoms. Similarly, in the search for sexual satisfaction perverse adults repeated the most minute details of the act into which they had been seduced, once again experiencing the same sensations (Masson, 1985, Letters of 17.12.96, 3.1.97, 11.1.97, 12.1.97, 24.1.97). Clearly an explanatory substitute for the 'memory' of a 'real' seduction experience had also to account for these particular details.

When Freud commented to Fliess on the consequences of the collapse of the seduction theory, he noted "It seems once again arguable that only later experiences give the impetus to fantasies, which [then] hark back to childhood" (op. cit., Letter of 21.9.97). One of the difficulties with this view of the origin of fantasies was its failure to explain how *all* patients incorporated into their fantasies "horrible perverse details which often are as remote from their experience as from their knowledge" (op. cit., Letter of 3/4.10.97). This explanatory problem, a result of the assumption of uniform causation, seems to have been why Freud continued to believe that there must have been some real sexual experience in childhood, a belief he maintained for some considerable time, if inconsistently, after informing Fliess of the collapse of the childhood seduction theory (op. cit., Letters 3/4.10.97, 15.10.97, 31.10.97, 14.11.97, 12.12.97, 22.12.97, 16.1.98, 30.1.98, 10.3.98, 27.4.98, 20.6.98, 27.9.98, 21.12.99, and 8.1.1900 and Editorial Notes to *Standard Edition*, *1*, pp.260-261).

Although Sulloway (1979, pp.207-210) expertly demolishes the myth that Freud's self-analysis was the reason for his abandoning the seduction theory and postulating the theory of infantile sexuality, it should not be overlooked that it was through his self-analysis that Freud came to regard the fantasies as normal psychological products rather than as pathognomic of neurosis. Only about eight to ten weeks after his analysis began he seems to have noticed in himself the same feelings and fantasies he had previously seen only in his patients (Masson, 1985, Letters of 14.8.97, 3/4.10.97, and 15.10.97). His immediate conclusion was that the parentage fantasies he had seen previously in paranoiacs and the combination of sexual feelings toward the mother and jealousy of the father of the hysteric were "universal" events of early childhood. Once everyone had been "a budding Oedipus in fantasy" (op. cit., Letter of 15.10.97).

Perversion and repression

That the germ of this newly discovered universal sexual feeling was perverse in its object was plain enough, but rather more was required to

explain the sensory content of the horrible perverse details that came to be incorporated into neurotic fantasies, and presumably into normal ones as well. A solution was provided by the line of thinking about repression Freud had been pursuing prior to the collapse of the seduction theory and which was more or less independent of the enquiry into the origins of fantasies. As has been seen, at the beginning of 1896 Freud proposed repression came about because the quantity of unpleasure released by premature sexual stimulation in childhood was vastly increased if an attempt was made to revive the earlier memory after puberty. The difficulty with the theory was "the origin of the unpleasure" (op. cit., Draft K of 1.1.96). Had Freud been able to identify the source, the tendencies of the mental apparatus to reduce quantities of excitation and avoid unpleasure, tendencies governed by the principle of constancy and the pleasure principle, would have brought about repression automatically.

Already by the beginning of 1897, Freud had concluded that perverse sexuality in the adult came about because of some kind of failure of repression. In discussing the nature of the defence against memories, Freud asked how analogous seductions sometimes gave rise to perversions instead of hysteria, or obsessions, or one form of paranoia, or acute hallucinatory amentia (op. cit., Draft K of 1.1.96). His eventual answer was that for perversion to result there was either no defence at all or the defence took place before the (unspecified) psychic apparatus had been completed (op. cit., Letter of 6.12.96). From this point of view, hysteria was *repudiated perversion* and not merely *repudiated sexuality* (ibid); or, as he was to put it a few weeks later, hysteria was the *negative* of the perversions (op. cit., Letter of 24.1.97).

While the formula focussed attention on repression, it was not until Freud postulated an automatic basis for it that he was able to solve some of the problems of introducing a childhood sexual impulse into his theory. Significantly enough, the first hint was in that same context of explaining how the same childhood sexual experience sometimes resulted in neurosis and sometimes perversion. Freud supposed pleasurable "sexual release" was obtained during childhood from the stimulation of a number of different parts of the body, or erogenous zones, but at a later time their stimulation released a substance that generated anxiety and so brought repression about automatically (op. cit., Letter of 6.12.96).

The date of Freud's letter rules out Moll's (1897, 1933) extension of the notion of erogenous zones as the main source of Freud's idea, as Sulloway (1979, p.518) seems to imply. Chambard (1881), a pupil of Charcot, had observed that orgasm could be produced in some somnambulistic hysterics by simply touching the inner side of the thigh, the groin, nipples, neck, and palms of the hand. He called these especially sensitive parts of the body "centres érogènes." By analogy with Charcot's hysterogenic zones, for the areas did not overlap, Féré renamed them "zones érogènes" (Féré, cited in Binet and Féré, 1887/1887a, p.152).

Krafft-Ebing (1898, 1965) then adopted and popularised the concept but it was Moll who first publicly extended it to include many other areas of the body and proposed that their stimulation produced sexual excitation physiologically, "without any primary and direct psychical activity" (Moll, 1897, p.93, 1933, p.126).

Freud's conceptualisation of repression as an automatic process seems not to have been developed further until about a month after he had concluded that his own Oedipal feelings were universal. Suddenly, he then attempted to use what he saw as an important difference between human and animal sexuality to explain how the zones changed their function and released only unpleasure. For animals, the sight of the zones was always sexually exciting and internal sensations arising from their stimulation contributed to the animals' own libido "the way the sexual organs proper do". In adult humans, on the other hand, the appearance and stimulation of the zones, even the idea of it, produced repellent sensations and unpleasure.

Freud supposed the difference had arisen during phylogenetic development when man's adoption of an upright carriage changed the role of smell now causing, as he put it, "formerly interesting [nasal] sensations attached to the earth" to arouse disgust (Masson, 1985, Letter of 14.11.97. Cf. Letter of 11.1.97). Because of his general adherence to the biogenetic law of recapitulation (Sulloway, 1979), as well as the influence of Moll (1897, 1933; Masson, 1985, Letter of 14.11.97), and possibly also precipitated by the influence of James Mark Baldwin's views on phylogeny and ontogeny, about which he had read only a few days before (Masson, 1985, Letter of 5.11.97. Cf. Broughton, 1981), Freud supposed that a similar change had taken place during the ontogenetic development of the child. Stimulation of the erotogenic zones in the child produced "something that is analogous to the later release of sexuality". During development the zones lost this capacity and the revival of earlier memories involving their stimulation caused "an internal sensation analogous to disgust" (op. cit., Letter of 14.11.97). From this post-pubertal release of unpleasure, repression followed automatically. Freud found the organic origin of the unpleasure necessary for automatic repression in an ontogenetic recapitulation of a phylogenetically changed function.

There was, if I can so put it, a bonus: the areas of the body once able to release sexuality - the anus and the mouth and throat - were precisely the areas that seemed to have been involved in the childhood seduction 'experiences'. Stimulation of those zones in childhood was now a necessary condition for later neurotic repression and symptom formation, and it was that particular form of stimulation which provided the "horrible perverse" details. Memories of childhood genital stimulation also released more sexuality on revival than originally but a non-neurotic compulsion was the result. Normality, in the sense of freedom from repression or compulsion, was thus dependent upon all the erotogenic zones having escaped stimulation in childhood. Perversions were due to failures to

abandon the zones of the mouth and the anus, failures inherent, it was implied, in the developmental process itself. Here we might note Compton's (1981a) observation that Freud had to formulate "some concept of sexuality which could encompass not only the normal sexual behaviour of adults - genital union in the service of reproduction - but also aberrant sexual behaviour, childhood sexual manifestations, and the warded-off sexual impulses of neurotics". Even with it, the automatic organic mechanism of repression still required some real experience to have occurred, and we have noted Freud continued to search for an external source for some time. Then, quite late (and again quite suddenly), his self-analysis seemed to provide a solution:

> fantasies are products of later periods and are projected back from what was then the present into earliest childhood; the manner in which this occurs also emerged - once again by a verbal link. (Masson, 1985, Letter of 3.1.99)

Freud's answer to the rhetorical question he asked next was to be of the utmost significance to the development of his theory generally and to his concept of instinctual drive in particular:

> To the question "What happened in earliest childhood?" the answer is, "Nothing, but the germ of a sexual impulse existed". (ibid)

Because the sensory content of the symptom had to reproduce that of the cause, there had to have been sexual sensations and they had to have arisen in the mouth and anus. But, if by "nothing" Freud meant that those sensations had not been aroused by external stimulation it followed that the sensations had to be the result of the child's *own* activities and, if the tendency of the organism to avoid unpleasure was not to be violated, those activities had to generate pleasure in the zones (Masson, 1985, Draft K of 1.1.96; Freud, 1950/1954, *Project*, Part III, Section 11). Further, if the sexual content of symptoms and perversions was to be explained, infantile self-stimulation had to produce, not mere pleasurable sensations but sexual sensations, if not actual sexual satisfactions. Consequently the 'germ' of the sexual impulse had to be auto-erotic, had to use the child's own body as its object, and had to be capable of generating sexual sensations in the anus and in the mouth and throat. Once Freud had concluded that "nothing" had happened, his expectation about the relation between the sensory content of symptom and cause, his assumptions about the ubiquity of the pleasure principle, and possibly, as Amacher (1974) has suggested, the logical demands of the reflex model itself, forced the further conclusion that the child had a sexual life consisting of auto-erotic activities, some of which were a non-genital kind.

Freud's self-analysis is usually cited as the source of his concept of an infantile sexual drive but it seems to me very unlikely that he could have recalled any sexual sensations from his early childhood. Nor does it seem likely that he could have made any observations during his self-analysis

that led in any direct way to the conclusion that fantasies were projections back to childhood. What I see as a more likely source of Freud's new view was his sudden realisation that his different expectations and theoretical requirements could be met by postulating auto-erotic impulses in the early period of life. As soon as that view had been adopted, it became possible to consider 'nothing' need have happened in childhood.

Child v. adult sexuality

Having decided that a sexual impulse existed in childhood, Freud then had to consider whether it resembled the adult drive or not. If he adopted the view that the immature and mature forms of the drive were essentially the same, he had to explain how the childhood behaviour came to be replaced. Why would there be any change? On the other hand, if he took the view that adult and infantile sexuality were different, he had two other problems: in what sense was childhood sexuality actually sexual and how did the two forms come to be related as they did in perverse adult sexuality.

There is some evidence Freud hesitated between these two conceptualisations. Certainly he found the formulation of what he came to call 'a sexual theory' very difficult. Almost a year after announcing to Fliess that he was considering developing a theory of sexuality as an immediate successor to his *The Interpretation of Dreams* (Masson, 1985, Letter of 11.10.99), Freud's letters are full of such complaints as that he was "waiting for a spark to set the accumulated material on fire" he had been collecting on sexuality (op. cit., Letter of 26.2.1900. Cf. Letters of 27.10.99, 5.11.99, 7.11.99, 9.11.99, 19.11.99, 26.11.99, 9.12.99, 24.12.99, and 10.7.1900). This prolonged gestation is of considerable interest because the solution Freud eventually adopted was already present in his correspondence with Fliess almost two years earlier. This is especially true of the Letter of 14.11.97 containing the idea of an initial release of sexuality being later replaced by unpleasure.

Examination of some aspects of the case of Dora throws further light on Freud's hesitation in placing the functions of the erotogenic zones in a conceptualisation of a childhood sexuality that was essentially the same as adult sexuality. Freud hesitated about the publishing itself. Although his analysis of the case was accepted for publication in January, 1901, he withdrew it for some 4-5 months before re-submitting it only to withdraw it again, finally allowing its publication only after about a further four years (Editorial Note, *Standard Edition*,7, pp.3-4, 322). It seems to me to be very probable that Rogow (1978) is correct in pointing to Freud's break with Fliess as contributing substantially to his hesitation. The beginning of the differences between Freud and Fliess cannot be dated with certainty but they had become overt by August of 1900. Dora was initially seen in the following month and Freud completed the first draft of his paper by the end of January 1901. When describing to Fliess the framework he intended to use, Freud drew on ideas that were either Fliess' in their entirety, like the

conflict between female and male tendencies and the notion of bisexuality, or, like the erotogenic zones, notions to which Fliess had made a major contribution. It is precisely these concepts which are attenuated or missing altogether in the published account of 1905.

Hence it is of the greatest interest that while Freud told Fliess that Dora's symptoms would be "traced back to the character of the child's sucking" (op. cit., Letter of 30.1.01), his tracing did not involve the notion that stimulation of the oral erotogenic zone produced sexual excitation. Despite his conviction that the adult Dora entertained an oral sexual fantasy, Freud described her intense childhood oral stimulation as creating only a "somatic compliance", by which he meant a mere channel through which displaced adult sexual excitation might be expressed. However, as his remarks on somatic compliance reveal (and as his earlier use of the as-then-unnamed concept confirms) the process by which compliance was produced did not have to be sexual. For example, such a decidedly non-sexual event as rheumatic disease could, as with Elisabeth von R., leave behind a channel of expression that could be used for the realisation of an unconscious mental conflict (Breuer and Freud, 1895, p.147; Freud, 1905a, pp.29-32, 40-42, 51-52).

I find it curious that Freud's explanation of Dora's symptoms makes no use of the most characteristic propositions of his later theory: the auto-erotic nature of childhood sexuality and its expression through a number of independent instinctual drives, each with its own aim and object. Indeed, even though Dora's sucking was central, Freud did not even describe it as auto-erotic. There is a similarly curious quality in Freud's treatment of the feeling of disgust he thought to index and repress Dora's oral sexual activity. Prior to the Dora case, Freud had already proposed that disgust originated in the altered role of the sense of smell. But, when actually discussing Dora's disgust, he emphasised the anatomical association of the organs of excretion with the genitals. However, later, in the *Three Essays*, he was inclined, as he had been some nine years earlier, to assess that association as of minor importance (Cf. Masson, 1985, Draft K of 1.1.96; Freud, 1905a, pp.31-32; 1905b, p.152). Despite a number of similarities, the sexual theory implicit in the Dora analysis is not the one found in the *Three Essays*. What is more important, it appears to be a theory based on a *qualitative* difference between childhood and adult sexuality.

In the *Three Essays* Freud adopted the alternative view: childhood and adult sexuality were essentially the same. Each erotogenic zone was the organ of a component sexual drive and stimulation of it produced most of the drive's excitation. Other component drives, like looking, were not located in the same kinds of erotogenic zones but they also generated sexual excitation. Consequently, Freud's descriptions of the effects of stimulation stressed the frankly sexual outcomes. For example, sensual sucking, the exemplar of childhood sexual activity, had obvious sexual consequences in an orgasm-like reaction. A good deal of the evaluation of

Freud's concept of an infantile sexual drive revolves around the problem of the sense in which the childhood behaviour was sexual.

THE SEXUAL IMPULSE

Freud began the first of the *Three Essays on the Theory of Sexuality* with a conceptual analysis of the sexual instinctual drive. He distinguished between the libido, sexual objects, and sexual aims. 'Libido' referred to the sexual instinctual drive in much the same way the term 'hunger' was ordinarily used to refer to the nutritional instinctual drive. The concept of libido was not characterised explicitly, Freud used only an hydraulic analogy: libido was described as being diverted from one activity to another (Freud, 1905b, pp.156-157), as flowing like a blocked stream into collateral channels (op. cit., pp.170, 232), and as accumulating as a store of energy that could be used for other than sexual purposes (op. cit., p.232). Nine years later, when libido was defined formally as the psychical energy of the sexual instinctual drive (Freud, 1914b), Freud was only making explicit the energetic conception of libido already implicit in the *Three Essays* (Freud, 1905b, p.163). By 'sexual object' Freud meant that from which sexual attraction proceeded, normally an adult person of the opposite sex. 'Sexual aim' referred to the act toward which the instinctual drive tended and through which it was satisfied. Normally the aim was:

> the union of the genitals in the act known as copulation, which leads to a release of the sexual tension and a temporary extinction of the sexual instinct - a satisfaction analogous to the sating of hunger. (op. cit., p.149)

For both aim and object the standard of normality was biological.

Essay I - Perverse sexuality

Perversions were deviations of either sexual aim or object. Inversion, as Freud called homosexuality, showed the libido could be directed toward persons of the same sex. Some of the facts about homosexuality suggested to Freud that it resulted from a developmental disturbance and that "a bisexual disposition [was] somehow concerned" (op. cit., p.143). Both male and female inverts had, he thought, a tendency to choose "feminine" sexual objects. In contrast their sexual aims were likely to be much more variable and to range from simple, emotionally intense friendships to sexual contacts of many different kinds. At the least, inversion showed that the connection between the drive and the object was less intimate than might usually be thought. In fact, drive and object were "merely soldered together" (op. cit., p.148). The origins of the sexual instinctual drive itself seemed to be independent of the object's attractions. Freud thought these tentative conclusions were consistent with those deviations in which the sexually immature or animals were chosen as objects.

Ordinarily more than just the genitals of the sexual object were, as Freud put it, 'valued' or judged positively. Indeed, there was a tendency to *overvalue* the whole of the body of the object, a tendency that helped turn activities connected with parts of the body other than the genitals into sexual aims. From the fact that feelings of disgust were so frequently aroused by oral-genital and anal-genital contacts Freud concluded that disgust was a "mental force" that restricted the sexual aim to genital activity. However, in some 'normal' sexual acts as well as in perverse ones the drive was strong enough to override disgust. Overvaluation of the object could also be strong enough to extend to any thing associated with the object - such as a part of the body like the hair, or an article of clothing - however inappropriate that thing, or fetish, might otherwise be. In the perversion of fetishism the longing for the fetish took the place of the object itself and the normal aim was abandoned completely.

The preliminaries to genital union could become perversions if fixation upon them produced aims that replaced the normal. For example, looking at the sexual object was normally sexually exciting, but it became perverse when only the genitals were looked at or when it overcame disgust and excretion was looked at (voyeurism) and supplanted sexual union altogether. Freud added shame as a second mental force which had to be overridden for perversions of looking to come into being.

Sadism and masochism were brought about by the relative weakness of a third mental force. Freud took these two perversions to be the active and passive forms of a single condition. Sadism was based on the aggressive element of male sexuality sometimes used in overcoming the resistance of the sexual object. Masochism had its roots in the sexual overvaluation of the object that gave rise to the need for humiliation and subjection. The capacity for pity at the painful suffering of another had to be overcome for sadism to be possible and pain itself had to be overridden if masochism were to develop. Pain, the third 'mental force', ordinarily prevented both.

From his analysis of the perversions Freud concluded that mental forces like shame, disgust, and pain acted as resistances to libidinal expression. In normal development the sexual instinctual drive had to struggle against them. He also supposed these forces gave direction to the libido's development. Freud also claimed to have shown that perversions were based upon "the convergence of several motive forces" (op. cit., p.162), and that they were of a composite nature. For him, the sexual instinctual drive was not simple but made up from various components.

Turning now to the sexual life of neurotics, Freud claimed his analyses had shown that the energy of the sexual instinctual drive was the only constant source of energy in the psychoneuroses, that the sexual life of neurotics was expressed in their symptoms, and that the symptoms actually constituted their sexual activity. This last claim was based specifically on the analysis of Dora's symptoms (op.cit., p.163). Because of the

intensification of the mental forces of shame, disgust, and morality, the hysteric's sexual life was said to combine an excessive aversion to sexuality with an excessive craving for it. Drawing on the ideas expressed much earlier to Fliess, Freud claimed that psycho-analyses had revealed that the sexual drives of psychoneurotics were always perverse:

> symptoms are formed in part at the cost of *abnormal* sexuality; *neuroses are, so to say, the negative of perversions.* (op. cit., p.165)

"Without exception" the unconscious mental life of neurotics showed inverted impulses and tendencies toward every kind of perversion, especially those assigning a genital role to the mouth and the anus. The aims involved in looking, in exhibiting, in sadism, and in masochism were said to be motivated by "component" instinctual drives that were claimed to play "an especially prominent part" in the formation of symptoms (op. cit., p.166). Masochistic impulses were said to be essential to understanding why neurotic symptoms involved suffering, while a claimed connection between libido and cruelty was held to account for the transformation of love into hate. Freud stressed that whenever an impulse having two forms was found in the unconscious of the neurotic, it would always be accompanied by the other: in the unconscious the exhibitionist was at the same time a voyeur. Freud also claimed isolated perverse impulses were hardly ever found in the neurotic; usually there would be "a considerable number and as a rule traces of them all" (op. cit., p.167).

Freud then proposed that perversions as well as neuroses could be traced back to a number of component instinctual drives. The organs capable of receiving stimuli that contributed to the component impulses were called "erotogenic zones" and in perversions involving the mouth and anus those zones clearly functioned as substitutes for the genitals. The same was held to be true of less obvious cases: in perversions of looking the eye could be an erotogenic zone, while in sadism and masochism the whole body surface had that role.

While the perverse sexuality of the psychoneurotic could be due to a constitutional tendency, another factor might also be present. When neuroses were precipitated by repression or a failure of normal sexual satisfaction, the libido behaved like:

> a stream whose main bed has become blocked. It proceeds to fill up collateral channels which may hitherto have been empty. Thus ... what appears to be the strong tendency (though, it is true, a negative one) of psychoneurotics to perversion may be collaterally determined, and must, in any case, be collaterally intensified. (op. cit., p.170)

Neurotic patients drew differentially on their perverse dispositions and on the various internal and external factors that had blocked libidinal expression.

Freud concluded the first of the *Three Essays* by arguing that because perverse impulses were to be found in the psychoneuroses, and because "an unbroken chain bridges the gap" between the neurotic and the normal, the disposition to perversion "must form a part of what passes as the normal constitution" (op. cit., p.171). A constitutional disposition of this sort ought to be observable in children. By foreshadowing that it might also be the case that the sexuality of the neurotic remained in or returned to an infantile state Freud also foreshadowed the main subject matter of the second of the *Three Essays*. Attention should be directed, he said, to the sexual life of children, seeking there the influences governing the development of "perversion, neurosis or normal sexual life" (op. cit., p.172).

Essay II - Childhood sexuality

Freud began the second of the *Three Essays* by observing that the study of sexual activity in early childhood had been neglected. He supposed this was due partly to infantile amnesia, that is, to the general tendency for people to be unable to recall other than fragmentary impressions of that part of their life before the age of six to seven years. But, because psychological examination showed experiences from that period had left "the deepest traces" (op. cit., p.175), it followed those early impressions had not been abolished. In fact, infantile amnesia resembled the amnesia of the neurotic for traumatic events. After asserting that infantile amnesia was due to repression, Freud then noted its existence provided a further resemblance between the mental states of children and neurotics - the first being the infantile nature of neurotic sexuality. He then asked:

> Can it be, after all, that infantile amnesia, too, is to be brought into relation with the sexual impulses of childhood? (op. cit., p.175)

Freud then argued that later hysterical repression was made possible by a store of repressed early childhood memories.

From the frequent reports of sexual impulses in early childhood, and from the nature of the childhood memories of the neurotic, it seemed to Freud that germs of sexual impulses were present at birth. They developed for a time, were then overcome by a process of suppression, but reappeared at puberty. From what Freud said elsewhere in the *Three Essays*, the efflorescence of childhood sexual activity was between the third and fifth years. A period of latency then set and lasted until the beginning of adolescence. During the latency period the "dams" of disgust, shame, and aesthetic and moral ideas built up to restrict the flow of the sexual instinctual drive. While it might seem that the dams were products of education:

> in reality this development is organically determined and fixed by heredity, and it can occasionally occur without any help at all from education. (op. cit., pp.177-178)

Because of this organic determination education should limit itself:

> to following the lines which have already been laid down organically and to impressing them somewhat more clearly and deeply. (op. cit., p.178)

The dams were actually constructed from the sexual impulses themselves. At the beginning of the latency period the sexual drive was diverted from sexual aims into new, non-sexual ones, that is, it was sublimated. Freud averred that all historians of civilization had observed sublimation in the development of culture itself - "accordingly" the same thing happened in the development of the individual. Sublimation was based partly on the non-reproductive character of the childhood impulses and partly their perversity, "which, in view of the direction of the subject's development, can only arouse unpleasurable feelings" (op. cit., p.178). The unpleasure evoked opposing mental forces that built up the mental dams of shame, disgust, and morality in order to suppress the unpleasure. Freud first called these forces 'reacting impulses' but two years later he gave them their final name of 'reaction formations' (Freud, 1907b, p.124).

Freud then took as an exemplar of childhood sexual activity what he called "sensual sucking" and made a detailed examination of it. By this term Freud meant the evidently pleasurable, non-nutritive sucking by the infant, usually of the thumb, but sometimes of other parts of the body. Basing himself largely on the observations of Lindner, an Hungarian paediatrician who had reported one of the few systematic studies of sucking, Freud claimed sensual sucking seemed to absorb the attention completely and to lead either to sleep or to "a motor reaction in the nature of an orgasm" (op. cit., p.180). According to Freud "no observer had felt any doubt as to the sexual nature of this activity" (ibid.). Because it was directed toward the infant's own body rather than toward other people, the aim of sensual sucking was auto-erotic, and the lips were an erotogenic zone. To Freud it seemed clear this sensual sucking was sexual and that it was determined by a search for the pleasure first experienced when sucking at the mother's breast:

> No one who has seen a baby sinking back satiated from the breast and falling asleep with flushed cheeks and a blissful smile can escape the reflection that this picture persists as a prototype of the expression of sexual satisfaction in later life. (op. cit., p.182)

The pleasure provided by sensual sucking was not secondary to the satisfaction of hunger. Nutritive sucking expressed the instinct of self-preservation and sensual sucking the sexual impulse. Freud put it later that the sexual drive at first attached itself to or leaned upon the nutritive activity, only afterwards becoming independent of it.

Sensual sucking had the two essential characteristics of infantile sexual activity: it was auto-erotic and its aim was dominated by an erotogenic zone. Although any part of the body could serve as an erotogenic zone it

was those parts most richly endowed with sensitive nerve endings and in which rhythmic stimuli evoked "a feeling of pleasure possessing a particular quality" that were often adopted (op. cit., p.183). In all erotogenic zones the aim was the same: obtaining satisfaction through stimulation. Freud had supposed that during sucking a pleasurable sensation, due "no doubt [to] stimulation by the warm flow of milk", had been first experienced during feeding (op. cit., p.181). Later remembrance of that pleasure created a need said to reveal itself by:

> by a peculiar feeling of tension, possessing, rather, the character of unpleasure, and by a sensation of itching or stimulation which is centrally conditioned and projected on to the peripheral erotogenic zone. (op. cit., p.184)

The aim attempted to replace:

> the projected sensation of stimulation in the erotogenic zone by an external stimulus which removes that sensation by producing a feeling of satisfaction. This external stimulus will usually consist in some kind of manipulation that is analogous to the sucking. (ibid.)

Sucking upon the thumb or with the lips brought about a feeling of satisfaction by reproducing the pattern of stimulation caused originally by the warm flow of milk. The same mechanism held for the other zones: pleasurable sensations first experienced in the anus or the genitals later gave rise to a feeling of tension and a centrally conditioned sensation of itching that, when disposed of by "appropriate stimulation", brought about satisfaction (op. cit., pp.186-188).

Freud noted how intense excitation of the anal zone was virtually ensured by the various intestinal disorders of early life. Children who held back their faeces to produce "violent muscular contractions" and "powerful stimulation" of the anal mucosa were using the faecal mass as a masturbatory stimulus. Holding back the bowel contents "must no doubt cause not only painful but also highly pleasurable sensations" (op. cit., p.186). The child's refusal to defaecate on request was said to be "one of the roots of the constipation which is so common among neuropaths" (op. cit., p.187). Freud also claimed that most neurotics had special and secret scatological practices and ceremonies. The anus was therefore an erotogenic zone and its stimulation gave sexual pleasure.

The anatomical position of the genitals, their natural secretions, and their cleansing all combined to ensure their potentiality for producing pleasurable feeling was realised early. Genital stimulation in infancy laid the foundation for the eventual primacy of the genital zone over the other erotogenic zones as well as for infantile masturbation itself. In Freud's opinion, infantile masturbation began and disappeared early. Revived prior to the fourth year, it persisted for a time until it was once more suppressed. The revival was either through a centrally determined tickling sensation

leading to masturbation proper or through nocturnal enuresis which imitated nocturnal emission. Although external influences were not necessary to bring about the reappearance of sexual activity, seduction into sexual activity by an adult or another child was of "great and lasting importance" (op. cit., p.190).

Freud believed that many different forms of perverse sexual activity could be developed in the child who had been prematurely seduced, that is, seduction could make the child polymorphous perverse. From this he concluded that children had, "innately present in their disposition", an aptitude for all kinds of sexual irregularities. He then claimed that children were like the "average uncultivated" sexually inexperienced woman who could be led on by a clever seducer to adopt every sort of perversion. Freud, as Gilman (1981) has pointed out, now echoed some of Weininger's ideas about prostitution and constitutional tendencies:

> considering the immense number of women who are prostitutes or who must be supposed to have an aptitude for prostitution without becoming engaged in it, it becomes impossible not to recognise that this same disposition to perversions of every kind is a general and fundamental human characteristic. (Freud, 1905b, p.191. Cf. Weininger, 1903/1906; Masson, 1985, Letters of 20.7.1904, 23.7.04, 26.7.04, and 27.7.04)

The disposition to perverse sexuality was not only realised through the erotogenic zones. The component instinctual drives of looking (scopophilia), exhibitionism, and cruelty also contributed. Unlike the auto-erotic activity of the erotogenic zones, the activities induced by these component drives were initially relatively independent of sexual satisfaction and "from the very first involve other people as sexual objects" (op. cit., p.192). Early seduction could draw attention to the genitals and create an interest in looking at the genitals of others. Because that interest was likely to be satisfied only when the child was watching someone else micturate or defaecate, adult voyeurism would result. If the scopophilic impulse was repressed, the individual would become a compulsive viewer of the genitals of others. Children who were especially cruel were, Freud believed, justly suspected of precocious sexuality and, in them, no barrier of pity for the other halted the impulse of cruelty. The absence of that barrier might also lead to unbreakable connections being established between painful stimulation of the subject's own body and erotogenic sensations with a resultant masochism.

At this stage of his argument Freud thought he had shown sexual excitation arose in three different ways: from the reproduction of organic need satisfactions, by peripheral stimulation of erotogenic zones, and through the component instincts. In common with other writers of the period (e.g. Moll, 1897, 1933; Krafft-Ebing, 1898, 1965), Freud recognised other sources, in his case five, all of which involved stimulation of a rather general type: thermal stimulation from warm baths, mechanical excitation from such passive movements such as rocking and swinging, muscular

activity of the kind involved in wrestling and romping, intense emotional excitement (pleasurable or unpleasurable), and intellectual strain. Freud's evidence that these kinds of stimulation produced pleasurable sensations consisted of such facts as that children insisted on the incessant repetition of games of swinging and being thrown into the air and that rocking was used habitually to induce sleep in restless children. Calling these pleasurable sensations 'sexual' required an identification of 'satisfaction' with 'sexual excitation', and Freud therefore proposed those terms could be used "without distinction" (Freud, 1905b, p.201). He also noted that frankly sexual excitation sometimes followed such general stimulation.

From what he now referred to as his "tentative suggestions" about the sources of sexual excitation, Freud concluded with "more or less certainty" (op. cit., p.204) that sexual excitation was initiated in a more or less direct fashion by stimulation of the sensory surfaces, especially of the erotogenic zones. A great number of internal processes also produced excitation as a concomitant effect once their intensity had passed beyond certain limits. The component instinctual drives were:

> either derived directly from these internal sources or are composed of elements both from those sources and from the erotogenic zones. (op. cit., p.205)

It also seemed to Freud to be likely that variations in the contribution to sexual excitation from these indirect sources contributed to the "multiplicity of innate sexual constitutions" (ibid). Finally, Freud voiced the suspicion that all the pathways leading from other bodily functions to the sexual were also traversable in the reverse direction. For him the existence of these reverse pathways explained how disorders of the sexual function caused non-sexual disorders. For example, if sexual satisfaction arose during feeding, a reverse connection explained how nutritional disorders came about when sexuality was disturbed. Reverse pathways along which the sexual disturbances 'trenched' upon other somatic functions could also serve as channels for sublimation in the healthy.

Essay III - Sexual development

Freud began the last of the *Three Essays* by describing the developmental changes that had to take place if the infantile sexuality he had postulated were to be transformed into normal adult sexuality. First, the activities of the component drives had to combine to achieve pleasure in the genital zone, and the other erotogenic zones had similarly to become subordinate to the genitals. Second, the predominantly auto-erotic sexual instinctual drive had to be directed toward an object. Third, what Freud termed an "affectionate current" had to converge with the sexual current upon the sexual object and the sexual aim.

What Freud described as the "primacy of the genital zone" (op. cit., p.207) came about because of changes in the effects of stimulation of the

other zones. Stimulating them at puberty produced sexual excitement in the genitals as well as their own particular kind of infantile pleasure. Genital sexual excitation was marked by physical changes in the genitals and a psychological state of "a peculiar feeling of tension of an extremely compelling character" (op. cit., p.208). Primarily because the tension was accompanied by an impulse to change, Freud insisted that the tension had to be experienced as unpleasure. Were pleasure felt there would have been no tendency to change. Nevertheless there was a contradiction:

> If, however, the tension of sexual excitement is counted as an unpleasurable feeling, we are at once brought up against the fact that it is also undoubtedly felt as pleasurable. In every case in which tension is produced by sexual processes it is accompanied by pleasure; even in the preparatory changes in the genitals. (op. cit., p.209)

Freud attempted to solve this problem by reanalysing the contribution of the erotogenic zones to sexual excitation. Erotogenic zone stimulation in the adult clearly caused both pleasure and an increase in genital sexual excitement. With continued stimulation, the excitement built up to the point where sexual substances were discharged in orgasm, an act accompanied by pleasure of "the highest" intensity (op. cit., p.210). Freud distinguished the pleasure due to stimulation of the zones from the tension of sexual excitation proper and from the pleasure due to discharge. He called the former "fore-pleasure" and the latter "end-pleasure". Fore-pleasure was "the same pleasure" already produced on a smaller scale in infancy. Its new function was to contribute to the end-pleasure by increasing the level of sexual tension and so eventually producing " the greater pleasure of satisfaction" (op. cit., p.211).

Freud felt he had solved the problem of how pleasure from stimulation of the erotogenic zones gave rise to the need for the greater pleasure of orgasm and also thought he had explained deviations of sexual aim. In them there was an imbalance between the slight amount of sexual tension produced as compared with the considerable amount of fore-pleasure:

> The motive for proceeding further with the sexual process then disappears, the whole path is cut short, and the preparatory act in question takes the place of the normal sexual aim. (op. cit., p.211)

If during childhood the erotogenic zone or the component instinct contributed "an unusual amount of pleasure", and if "further factors" then brought about "a fixation", it would become difficult for the old fore-pleasure to become subordinate to the normal adult aim. Many of the perversions were based upon this mechanism of "lingering over the preparatory acts of the sexual process" (ibid.).

Sexual excitation itself could not be treated "even hypothetically, in the present state of our knowledge" (op. cit., p.215). Because of his insistence that tension could not be felt as pleasurable, Freud had to have

pleasure and tension "connected [only] in an indirect manner" (op. cit., p.212). The internal organs and the production of chemical substances were involved but, apart from outlining a chemical basis for the theory of sexuality which we have already considered in discussing the revisions to the theory of anxiety, Freud had to leave the basis of sexual excitation unspecified.

Sharp distinctions were also established at puberty between the masculine and the feminine character. Before puberty there were differences in the development of inhibitions, which took place earlier in girls than boys; in the expression of the component instincts, which tended to take the passive form in females; and in repression, which tended to be greater in females. Otherwise the differences were minimal. Indeed the auto-erotic activity was so similar in males and females that Freud believed libido was "invariably and necessarily of a masculine nature" and that the sexuality of little girls was "wholly masculine" (op. cit., p.219). Puberty induced two quite different processes in the sexes. In boys there was a great accession of libido but in girls "a fresh wave of repression" (op. cit., p.220). The repression was precisely of the girl's clitoral, that is, masculine sexuality. Stimulation of the clitoris had now to produce excitation of the vagina if the vagina was to supplant the clitoris as the predominant zone. Repression of the girl's masculine sexuality thus prepared the way for the full development of female sexuality. Because females had to change their leading erotogenic zone while males did not, females were more prone to neuroses, especially to hysteria.

Having explained how the genital zone had achieved primacy and how the new sexual aim had been adopted Freud now turned to the process of explaining how the adult sexual object was found at puberty. Freud described the period of earliest infancy in which the sexual instinctual drive was linked to nourishment as one in which the mother's breast had been a sexual object. Auto-erotism began with the loss of the breast and the subsequent redirection of the sexual impulse on to the subject's own body. When auto-erotism ceased at puberty the direction taken by the sexual impulse was once again toward the breast:

> There are thus good reasons why a child sucking at his mother's breast has become the prototype of every relation of love. The finding of an object is in fact a refinding of it. (op. cit., p.222)

Throughout the whole time of the child's dependence upon others, "even after sexual activity has become detached from the taking of nourishment", children learned to feel a love for those who helped them and who satisfied their needs. Freud believed this love to be modelled on the suckling's relation. The sexual character of the child's dependence upon others was to be seen in the anxiety of children, "originally nothing other" than a feeling they had lost the person whom they loved:

> In this respect a child, by turning his libido into anxiety when he cannot satisfy it, behaves like an adult. On the other hand an adult who has become neurotic owing to his libido being unsatisfied behaves in his anxiety like a child: he begins to be frightened when he is alone. (op. cit., p.224)

Why, then, did male and female adults not both choose the mothers - after all, both had had her breast as their first object?

Adult object choice was, he said, first of all guided by the exclusion of blood relatives from consideration. In the human child the postponement of sexual maturation meant an incest barrier could be erected. Freud believed that pubertal fantasies, based as they were upon infantile tendencies now strengthened by somatic pressure, showed the direction of the child's sexual impulse to be toward the parent of the opposite sex. When the fantasies were repudiated, a process of detachment from parental authority could take place and a choice of an object other than the parent made. Failures of detachment occurred most often in girls who, if they retained their fathers as objects would be sexually anaesthetic and cold toward their husbands Because the psychoneurotic had repudiated sexuality generally, the activity of finding an object remained unconscious. A characteristic combination of an exaggerated need for affection with an equally exaggerated horror of sexuality developed as a consequence. Freud claimed psycho-analyses had shown that in neurotic females this characteristic resulted from incestuous object choices. But, even when no such abnormal consequence ensued, Freud believed the incestuous choices of infancy had long-lasting effects:

> There can be no doubt that every object-choice whatever is based, though less closely, on these prototypes. A man, especially, looks for someone who can represent his picture of his mother, as it has dominated his mind from his earliest childhood. (op. cit., p.228)

Although he said that "other starting-points" from infancy might affect adult object-choice, he did not specify them.

Guidance of the adult choice had also to prevent inversion. An adult of the opposite sex had to be chosen. Freud thought the strongest factor was the attraction "opposing sexual characters exercise upon one another" (op. cit., p.229), the same factor responsible for sexual differentiation in the fantasies of puberty (op. cit., p.227). Freud was unable to indicate the basis of this attraction and supposed further that by itself it was insufficiently strong to determine an opposite-sex choice. Reinforcement by social prohibitions against inversion was necessary because:

> Where inversion is not regarded as a crime it will be found that it answers fully to the sexual inclinations of no small number of people. (op. cit., p.229)

Freud presumed that a further powerful contribution to the choice of female objects by men came from the male's recollection of the affection shown

him in childhood by his mother and other females who had cared for him. In women the development of impulses of rivalry toward other females were thought to "play a part" as well as the sexual repression at puberty in discouraging them from choosing among their own sex (ibid. n.3).

Although much condensed, the best summary of Freud's theory is the one he made only a year after the *Three Essays* had been published:

> *normality* is a result of the repression of certain component instincts and constituents of the infantile disposition and of the subordination of the remaining constituents under the primacy of the genital zones in the service of the reproductive function ... *perversions* correspond to disturbances of this coalescence owing to the overpowering and compulsive development of certain of the component instincts, ... *neuroses* can be traced back to an excessive repression of the libidinal trends. (Freud, 1906a, p.277)

I now turn to the evaluation of this developmental theory and the basic concepts of which it is constituted.

THE GENESIS OF THE CONCEPT OF INSTINCTUAL DRIVE

It is quite easy to show that Freud's 1905 concept of instinctual drive is quite different from the conceptualisations of instinct or drive current around the turn of the century. No one else used the term libido in quite the same way, distinguished so clearly between the aims and objects of the instinctual drive or, of especial importance, stressed its internal sources. 'Libido', although not well defined at first, encompassed the sexual drive in the same way 'hunger' took in the nutritional drive. 'Aim' referred to the act toward which the drive tended and through which it was satisfied. Normally this was the union of the genitals in intercourse. 'Object' meant that from which sexual attraction emanated - ordinarily an adult of the opposite sex. 'Source' was also not well defined at first but clearly referred to the internal processes that determined the drive.

Instinct, drive, and source The literature of the late 1900's on the concept of instinct and the judgements of historians shows the German term *Instinkt* (and its English equivalent 'instinct') was ordinarily used to describe simple reflex-like behaviours that were relatively invariant in form, unmodifiable by experience, and elicited by external stimuli (Preyer, 1882, 1893a, 1893b; James, 1890b; Sully, 1892; Morgan, 1896; Groos, 1899, 1901; McDougall, 1905; Jennings, 1906; Drever, 1917; Bernard, 1924; Wilm, 1925). Although some kinds of instinctive behaviours, like mating or nestbuilding, were recognised as dependent upon internal conditions those conditions were not emphasised. As Fletcher put it about Darwin, Morgan, James, McDougall:

> Whilst these writers were well aware of the existence and importance of these features, the state of knowledge at their time precluded any detailed positive statement as to their nature. (Fletcher, 1957, p.69. Cf. Morgan, 1896, pp.8, 207)

Hence, the existing emphasis placed on external causes in discussions of the factors that elicited behaviour could only be reinforced by this ignorance. It is not surprising, then, that McDougall noted:

> Some writers have given ... organic conditions an undue prominence while neglecting the essential part played by sense-impressions. (McDougall, 1908, p.23, n.1)

On the other hand, the internal condition signified by the term *Trieb*, then most commonly rendered as impulse rather than as drive (as it is nowadays), had a connotation stressing the motor element to the detriment of the goal of the behaviour, so giving it the meaning of a blind impulse (Morgan, 1896, p.140). Sully, who recognised that certain kinds of motor activity were prompted to some degree by *Trieb*, noted that that term was:

> commonly confined to those innate promptings of activity in which there is no clear representation of a pleasure, and consequently no distinct desire. Here the active element is greatly in excess of the intellectual. (Sully, 1892, p.580)

When discussing why he had chosen to use the term *Trieb* in his work on play, Groos summed up the dilemma as follows:

> We lack a comprehensive and yet specific term for those unacquired tendencies which are grounded in our psycho-physical organism as such. The word instinct does not cover the ground with its commonly accepted definition as inherited association between stimuli and particular bodily reactions. Even the imitative impulse which is responsible for the important group of imitative plays is not easily included in this idea, because no specific reaction characterises it. It is safer, therefore, to speak of such play as the product of "natural or hereditary impulse" although even that is not entirely satisfactory, since many psychologists connect the idea of impulse [*Trieb*] with a tendency to movement. (Groos, 1899, p.1, 1901, p.2)

In a footnote, Groos admitted he had modified his former view that the traditional concept of instinct was adequate to encompass variations in imitative play, and elaborated the point later (Groos, 1899, pp.364-372, 1901, pp.284-290). The importance Groos' point is twofold: his discussion of instinct and drive is one of the few attempted in the nineteenth century and his work was well known to Freud, who read it some time before completing the *Three Essays* (Freud, 1905b, p.173, n.2).

If the discussions of Sully (1892), McDougall (1905, 1908), and Jennings (1906) reflect contemporary thought accurately, it was not usual for the internal drive tendencies alluded to by Groos (those "grounded in our psycho-physical organism") to be formally incorporated into concepts of instinct. Fletcher (1957, p.6) has asserted the contrary. According to him, most of the early writers believed instinctive behaviour to be elicited only when the appropriate internal state was also present (Fletcher, 1957, p.6). However, Wilm's description of the situation seems to me to be more

accurate. He observed that all that existed toward the end of the nineteenth century was only a tendency, although a growing one, to think of instincts as dependent upon internal states. When neurophysiological or mental antecedents of the behaviour were referred to it was as:

> a vague restlessness or craving, as in hunger, migration, or sex, due to a condition of unstable equilibrium ... usually evoked by *an external stimulus*, but often occurring spontaneously, leading to an action which results in the relief of the restlessness by the attainment of the object of the conation, and to temporary satiation. (Wilm, 1925, pp.145-146. My emphasis, MBM)

Wilm's very general descriptions of the relation between instinct and drive and of the instinct being elicited by external factors are well removed from the quite specific assertion of Fletcher. Indeed, so rarely was it proposed that instincts were *motivated* by drives, that Lorenz (1937/1957) has been able to insist, apparently without challenge, on Craig's (1918) priority in formulating such a connection.

Lorenz is almost certainly wrong in his claim. Drever (1917. Cf. Fairbairn, 1939-1941) had already examined a number of similar formulations before Craig's relatively little known paper appeared. Dugald Stewart's concept of 'appetite' as it related to instinct, a concept well known in the early part of the nineteenth century, is a clear precursor of later motivational concepts of drive. Appetites originated in bodily states, were periodic rather than constant, and were accompanied by a feeling of unease. Stewart recognised three appetites of hunger, thirst, and sex and noted the impulses to which they gave rise were directed toward their respective objects, such as food, etc. Of some interest to the later criticisms of Freud's concept (and to the collapse of Lorenz's) was Drever's identification of the fundamental inadequacy of these motivational formulations: whole classes of instinctive behaviours are quite unrelated to drive states. Drever (op. cit., pp.247-249) mentioned fear and anger as examples of instinctive reactions to which the internal state merely predisposed the organism to react; he might well have added avoidance behaviours generally are hardly ever so motivated.

Aim and object The characteristics of aim and object clearly differentiate Freud's concept of instinctual drive from the notion of a *Trieb*-like impulse lacking direction. The characteristic of source (which he came to define as a continuously active physiological process) distinguishes Freud's concept just as clearly from stimulus elicited concepts of instinct. Freud's thinking was in line with the tendency described by Wilm but it has a number of novel features. While Burnham (1974) noted many of the apparently new terms used by Freud in the-turn-of-the-century literature on sexuality, the essentials of his concept are not to be discerned there either. For example, while Burnham is correct in claiming Krafft-Ebing used the term drive (*Trieb*) and recognised self-preservation and sexual drives, Krafft-Ebing's

definition of the drive construct is far removed from Freud's: he made no distinction between aim and object (indeed, his use of the latter term seems to be entirely descriptive) and he spoke of a drive as having "organic sensations" as its instigator rather than as having a source. Even though Krafft-Ebing recognised the distension of the seminal vesicles he did not propose it as the source of a sexual drive. Krafft-Ebing's concept of drive was "psychophysiologically" composed of ideas, which could be awakened either centrally or peripherally, and the pleasurable feelings associated with them (Krafft-Ebing, 1898, 1965). Consistent with this difference between Krafft-Ebing and Freud is Krafft-Ebing's speaking of homosexuality as involving an opposite sexual *feeling*, rather than an opposite object and an altered aim, and of the perversions without specific reference to either aim or object. In these respects, there is little correspondence between Freud's concept and those of the other writers mentioned by Burnham, namely Bloch (1907, 1908), Havelock Ellis (1900-1901/1897-1899, 1903), Eulenberg (1902, 1902/1934), and Kraepelin (1889, 1899, 1904, 1903/1907). With the exception of Moll, this is also true as far as I can determine of the many other writers on sexuality mentioned by Sulloway (1979, Ch. 8). Moll's (1897, pp.88-93, 1933, pp.121-126) picture of sexuality resembled Freud's mainly in the emphasis placed on the impelling role of physical pressure in the seminal vesicles. Freud's threefold characterisation of the sexual instinctual drive is quite original.

Component drives The other main characteristic of Freud's concept, that of being composed of a number of separate component instinctual drives is definitely not to be found in the treatments by other writers of the period even though, as Sulloway has argued, it is foreshadowed by some of Moll's notions. Moll had it that the sexual drive consisted of two component drives: one to approach, touch, fondle, and kiss a person of the opposite sex and the other to bring about orgasm. These two impulses could be "sundered" [Auseinanderfallen], and Moll concluded that their tie might not necessarily be a close one (Moll, 1897, pp.8-11, 24, 1933, pp.28-30, 48). Moll also made rather more of use than did most other writers of the notion that stimulation of various parts of the body, for which he used the French term *zones érogènes*, could generate sexual excitation. And Moll also based a good deal of what he had to say on comparisons between the activity of the child and that of the perverse adult.

Freud's concept Several of the crucial elements of Freud's first concept of instinctual drive, some in a reasonably developed form, are however to be found in his correspondence with Fliess, especially in Draft D, possibly of 21.5.1894, Draft E, possibly of 6.6.94, and Draft G, possibly of 7.1.1895 (Masson, 1985), in the *Project* of September-October 1895 (*Standard Edition, 1*) and in the first paper on the actual neuroses (Freud, 1895a). Generally speaking, Compton confirms my opinion in saying that "sexuality ... was first acknowledged to be important at a theoretical level by Freud in his concept of the actual neuroses" (Compton, 1981a). From

the sources we have mentioned, the elements of what Freud called his 'schematic picture of sexuality' are:

1. a *terminal organ* in which a recurrent physiological process gave rise to mechanical pressure on the nerve endings situated there (e.g., in the seminal vesicles in the male).
2. *physical sexual excitation*, sometimes called somatic sexual excitation, arising in the nervous system as a result of the pressure in the terminal organ.
3. a *psychosexual group of ideas*, that is, a group of ideas concerned with sexuality, to which the physical sexual excitation could become linked after it had exceeded some threshold value.
4. *psychical tension*, later to be called *libido*, that resulted from the linkage of the physical excitation with the psychosexual group of ideas.
5. a *specific action* and *specific reaction*, which obtained an object capable of discharging the excitation and placed it in a favourable position for the transmission of sensations to a spinal centre. [1]
6. a *reflex action*, controlled by a spinal centre, which discharged the substances in the terminal organ, eliminated the pressure there, and reduced the physical excitation and the associated libido.
7. *voluptuous feelings* [? or sensations] proportional to the quantity of excitation discharged, conducted from the terminal organ after discharge to the psychosexual group for the mode of discharge to be repeated when the tension recurred.

While certain aspects of the picture obviously applied more to males than to females, Freud believed it could be extended in principle to them and, as we have seen, he used it to explain actual neuroses in women.

In Drafts D and E, apparently for the first time, Freud distinguished between two kinds of excitation affecting the psyche: external excitation, which had transient effects, and internal excitation, which had constant effects. In Draft E, sources of excitation external to the individual (Erregungsquelle) were said to give rise to a quantity of exogenous excitation that could be disposed of by any reaction reducing the excitation by the same amount. Hunger, thirst, and the sexual instinctual drive (Sexualtrieb) were mentioned as three types of internal source (Quelle) giving rise to endogenous tension that could be dealt with only by a specific reaction (Spezifische Reaktionen) that prevented the further production of excitation. The term specific reaction approximates that of aim in that such reactions are activities that eventually prevent the further

1. These concepts of Freud's have no relation to the similarly designated ethological concepts.

production of excitation and satisfy the drive by lowering the level of tension. Further, elsewhere in Draft E Freud appears to equate the term aim (ziel) with the "working over" of the *physical* tension generated by the sexual drive, and he may even have equated the aim with what he called the physico-sexual act (physisch-sexualem Akt), by which term he seems to have meant copulation. Freud's later remarks in the *Project* definitely equate specific action with aim, for he there distinguishes more clearly between the specific actions as the means by which the sexual objects (Sexualobjektes) are brought to the subject, and the "reflex contrivances", or reflex mechanisms, that remove the endogenous stimulation. So regarded, the specific action (or reaction) is thus the activity through which the drive is satisfied and to that extent is equivalent to aim (Cf. the discussion of "Normal Processes" in Part III of the *Project*). The term for sexual object, the last of the elements comprising the concept of sexual drive, was first used in the schematic picture of sexuality in Draft G (Compton, 1986c), where it was placed externally to the psychic. In the *Project* the usage was entirely consistent; there it was an exact equivalent of the food that satisfied the hunger drive.

Consequently, Freud's use of the terms source and object, and possibly aim, in his early correspondence with Fliess seems reasonably similar to that in the *Three Essays*. As we shall see, it is much the same as in the later *Instincts and their Vicissitudes*. Three differences should be noted, however. First, the sexual instinctual drive of the *Three Essays* actually consists of a number of different drives, the early concept does not even hint at such separation. As Sulloway (1979, pp.299-305, 310-311) has suggested, this difference may well be due to the influence of Moll's (1897, 1933) notion of component drives. Second, in the *Three Essays*, both aim and object are described as variable with respect to the drive, the object more so than the aim, but this variability is not mentioned in Freud's earlier discussion. Again, Moll's notion that the components might be sundered may be an influence. Third, in *Instincts and their Vicissitudes* a drive is described as *having* a source, whereas earlier it was said *to be* a source. These three differences are also those which most clearly distinguish Freud's concept from those of all of his contemporaries, even from those of Moll's I have just mentioned and those of Krafft-Ebing (1879-1880, 1904, 1898, 1965) and, as we have just seen, the origins of the most characteristic features of Freud's concept are to be found in the explanatory tasks set by the collapse of the childhood seduction theory.

EVALUATION OF THE CONCEPT OF SEXUAL DRIVE

At first sight the changes Freud made to his picture of human sexuality in the *Three Essays* are considerable. However, each of the new drives is modelled almost exactly on the earlier conception of the adult drive. Most of the difficulties with Freud's concept of infantile sexuality result directly from the adoption of this model.

Freud's conceptualisation was based on an especially intimate relation between erotogenic zones and component drives, on the one hand, and sexual excitation on the other. Stimulating a zone or eliciting a component produced two kinds of excitation: sexual excitation and the excitation appropriate to the zone or drive itself. For example, when visual sensations were produced by the eye being stimulated, a "specifically sexual" excitation was produced in addition to the purely visual excitation. It was this which lent the component drive, here scopophilia, "a sexual character" (Freud, 1905b, p.168 and n.1.). Erotogenic zones like the mouth and the anus could become:

> the seat of new sensations and of changes in innervation ... in just the same way as do the actual genitalia under the excitations of the normal sexual processes. (op. cit., p.169)

Consequently the erotogenic zones and the organs associated with the component drives did not simply produce sexual excitation, they were also, as Freud put it, terminal organs (op. cit., p.168).

Somatic sexual excitation or something analogous to it was produced at each of the erotogenic zones and by each of the component drives as well as in the genitals. Each zone and organ of a component drive was also a terminal organ, capable of generating sexual excitation, which by energising ideas, initiated a specific action that in turn brought about a state of satisfaction. Of course, in Freud's new picture the linkage had to be with a group of ideas different from those making up the adult's psychosexual group, but that change was only minor. Similarly, rather than the object of the sexual drive being pictured as something external to the individual, the childhood object of each drive had to be named as the individual. The specific actions had to be appropriate to the newly designated terminal organs and objects but, like the aim of the adult drive, had also to bring about a reduction of physical and psychical excitation.

Sexual excitation?

The first problem with Freud's concept of sexual instinctual drive is the inadequacy of the explanation it generates of the production of sexual excitation. In the *Three Essays* this problem is that of how the need for repeating the stimulation of an erotogenic zone is created. Superficial, every day experience seems consonant with Freud's premise that the remembered pleasure of an act may lead to a tendency to its repetition. However, any such need must have other and more important determinants. In Freud's early picture of human sexuality, a connection between the voluptuous feelings of satisfaction and the psychosexual group of ideas contributed to the tendency to repeat the act, and a similar mechanism had been included in the model of need satisfaction set out in Chapter 7 of *The Interpretation of Dreams* (Cf. Masson, 1985, Draft G, possibly of 7.1.95, and Freud, 1900, pp.564-567, 598-603). But, in these discussions of satisfaction, the pleasurable memory is a mere creature of a need

independent of and anterior to it. Freud's account of the primary process postulated that a change in the continuous impact of a need like hunger came about only through satisfaction ending the internal stimulus. Once that happened, associations were formed between the memory image of the nourishment bringing the satisfaction and the memory trace of the excitation produced by the need. Then:

> As a result of the link that has thus been established, *next time* this need arises a psychical impulse will at once emerge which will seek to re-cathect the mnemic image of the perception and to re-evoke the perception itself, that is to say, to *re-establish the situation of the original satisfaction*. (Freud, 1900, pp.565-566. My emphasis, MBM)

Feelings of satisfaction are remembered only when a need revives them. Both in the hallucinatory context and when there is real satisfaction it is the need which re-animates the memory of the pleasure of satisfaction and not the reverse. The same conclusion follows, perhaps more evidently, from Freud's discussion of the experience of satisfaction in the *Project* (Part I, Section 11). And because of the way in which Freud conceptualised memories, that must be so: Freud sees memory traces as inert records requiring to be suffused with energy (cathexis) before they can be recollected as memories. Like the filaments of an electric light globe they are not allowed the luxury of a spontaneous glow. Remembered pleasures must be determined by something other than the association between the memories of the satisfactions and the acts that brought them about.

At two places in Freud's discussion in the *Three Essays* one can sense the presence of an undefined, probably physiological process lying behind the need for repetition of the stimulation. First, Freud says of the projected sensation of itching that it is "centrally conditioned", an allusion that I believe applies only to a central drive state. Second, Freud counted a "feeling of tension" as one of the indicators of the need for repeating the stimulation, a feeling much more consistent with the presence of an unsatisfied, organically based need than with the memory of a pleasurable satisfaction. In fact, because Freud equated unpleasure with tension, and pleasure with tension *reduction*, it is quite impossible for the recollection of a *pleasurable* satisfaction to reveal itself as a tension. Continuously active physiological processes, like those underlying hunger or the adult sexual drive, would produce tension, revive the memory of the mode of previous satisfaction, and so initiate action. Only a conceptualisation of this type is fully consistent with the discussion of needs and satisfaction in *The Interpretation of Dreams* and which had been outlined earlier in both the *Project* and the works on the actual neuroses. Needs conceived of in the way described in the *Three Essays* can motivate nothing. For a satisfying act to be initiated, the simple association proposed by Freud had, as it were, to be 'pushed' by a central need state behind it.

In *Instincts and Their Vicissitudes*, Freud's first substantial discussion of instinctual drive theory after the *Three Essays*, the process we have

discerned behind the need for repetition of stimulation was made quite explicit. *All* instinctual drives were described as having a source (Quelle) in a physiological activity exerting a constant force in the mind (Freud, 1915a, pp.118-119). Of the sexual instinctual drives Freud said:

> They are numerous, emanate from a great variety of organic sources, act in the first instance independently of one another and only achieve a more or less complete synthesis at a late [? later] stage. (op. cit., p.125)

The need for repetition of erotogenic zone stimulation could thenceforth be referred to the organic source of its associated component drive.

Freud found it easy to incorporate this addendum into the framework of the *Three Essays*. A 1915 amendment to that work described an instinctual drive as "the psychical representative of an endosomatic, continuously flowing [? welling up] source of stimulation" and Freud went on to say:

> What distinguishes the instincts from one another and endows them with specific qualities is their relation to their somatic sources and to their aims. The source of an instinct is a process of excitation occurring in an organ and the immediate aim of the instinct lies in the removal [? cessation] of this organic stimulus. (Freud, 1905b, p.168 and n.1)

Each of the drives originally mentioned in the *Three Essays* was encompassed by the new definition. Each now had its own organic source as well as an aim and an object. In addition, whether they arose in a zone or, like looking, were not so located, they were now all called components of the sexual instinctual drive (Freud, 1915a, pp.125-126).

The new sources were entirely hypothetical, of course, and it was this lack of reference to even an imaginary physiological process that marked no real advance over the simple association originally postulated for explaining sexual excitation. Consider, for example, the eye as the organ of the sexual component drive of scopophilia or sexual curiosity. What recurrent physiological process within the visual system of the child could produce sexual excitation as a precursor to psychical tension or libido? Or, once non-nutritive sucking has become independent of feeding, what are the continuous chemical and biological processes determining that oral activity? In no way can these and similar questions about the other component drives be answered. Freud's modelling of the component drives more fully on his older picture of adult sexuality had not explained sexual excitation in childhood. The physiological processes upon which the drives depended were nothing more than mere physiological figments, or, more correctly, pseudo-physiological characterisations.

Sexuality in childhood?

The second problem with the concept of the infantile sexual drive is the description of its aims and excitation as sexual. Studied objectively, most childhood activity of the kind Freud took to be motivated by component

drives altogether lacks the distinguishing marks of sexuality and, as absurd as it may sound, much the same can be said about perverse adult activity. Considered subjectively, the data provided by clinical-therapeutic investigation is not only too much influenced by expectations, but it is too fragile to bear the weight of the sexual interpretation Freud gave it.

First of all, although Moll (1909/1912, pp.172-173) had raised doubts about Freud's sexual interpretation of the observations on sucking made by Lindner, only recently has it become apparent how grossly Freud misrepresented them in order to support his view that sucking had a sexual aim. Bieber, who arranged a partial (and inadequate) translation of Lindner's (1879-1880, 1975/1879-1880) paper, pointed out some of Freud's 'inaccuracies' in his comments on it (Bieber, 1975). My comments on the complete translation are not constrained by any kind of identification with psycho-analysis and are rather stronger (Macmillan, 1980; Lindner, 1980/1879-1880). Of the 500 children studied only sixty nine exhibited what Lindner called pleasure-sucking [Wonnesaugen]. Lindner nowhere "clearly recognized" its sexual nature, as Freud (1905b, p.180) had claimed. Rubbing of the genitals or breast in conjunction with sucking took place in only five of the pleasure suckers and then every time with only two of these. Sucking was only associated with masturbation proper in four cases, although Lindner conceded there may have been more: he "did not consider it socially proper to inquire or investigate more deeply." Freud was also quite incorrect in claiming a resemblance between orgasm and the motor reaction Lindner had observed in four of his cases. Lindner called these four children "exultant" [exaltirt] suckers and described how their sucking movements became more forceful, how the sucked object was pushed further into the mouth, and how any associated rubbing movements became more intense. The exultant sucker would then:

> reach a state of rapture [Verzuckung] by shaking his head up and down, his spine as in an emprosthotonous writhing forwards, his feet stamping up and down, or if he is lying, jerking. This is the stage at which the exultants draw their own blood, pull out their hair, or stop up their organs of smell and hearing .... Sometimes he is so completely engrossed ... that he will pay no attention to threats and will be deaf to kind words. (Lindner, 1980/1879-1880)

No post-sucking relaxation was described. It was certainly not true, as Freud (1905b, p.180) implied, that Lindner depicted orgasm or even sleep as the outcome of 'exultant' or any other form of sucking. Lindner's observations were that all but occasional suckers sucked at any time, wherever they might be. He actually added, "they prefer this pleasure most of all shortly before going to sleep, soon after waking up, and after a bath". This lack of connection between the 'orgasmic' reaction and sleep, especially the failure of sleep to follow, detracts considerably from Freud's interpretation of pleasure sucking as sexual.

Further, signs like the flushed cheeks and blissful smile of the infant who falls asleep after feeding are equivocal indicators of sexual satisfaction. Freud's definite inference was warranted only if such expressive and bodily responses were found in what are incontestably states of sexual satisfaction and only in them. Ordinary observation contradicts: similar reactions result from the satisfaction of many different needs. Freud's observation was of doubtful relevance for another reason: during feeding both the hunger and the sucking impulses were satisfied at the same time, all he could have concluded therefore was that the satisfactions of one or the other, or both, of those needs produced similar expressions of contentment. Freud later portrayed the blissful state as if it occurred independently of nourishment, after "sensual sucking has in itself brought ... satisfaction" (Freud, 1916-1917, p.313). Even this cavalier re-description does not count as evidence if it is true that non-sexual satisfactions may be just as blissful.

Freud provided much less evidence for interpreting the other activities as sexual. Anal stimulation gave sexual enjoyment because the infant's sensations during delayed defaecation were "no doubt" highly pleasurable. Childish exhibitionism was sexual because children "show unmistakable satisfaction in exposing their bodies, with especial emphasis on the sexual parts". Children who are especially cruel "usually gave rise to a just suspicion of an intense and precocious sexual activity" (Freud, 1905b, pp.186, 192, 193). A similar lack of relevance marks most of Freud's observations on the effects of mechanical excitation, muscular activity, extreme emotion, and intellectual effort. Particular difficulties stand in the way of accepting the undoubted pleasure resulting from such generalised forms of stimulation as rocking and swinging as sexual. At first Freud simply asserted "sexual excitation" was so produced, equating it with both sexual satisfaction and pleasurable sensations. He then said these concepts "can to a great extent be used without distinction" (op. cit., p.201). But an explicit reservation about the nature of the "extraordinary pleasure" children appeared to obtain from satisfying their need for muscular activity shows Freud had some doubts:

> Whether this pleasure has any connection with sexuality, whether it itself comprises sexual satisfaction or whether it can become the occasion of sexual excitation - all of this is open to critical questioning, which may indeed also be directed against the view ... that the pleasure derived from sensations of *passive* movement is of a sexual nature or may produce sexual excitation. (op. cit., p.202)

Freud attempted to justify his equating of 'sexual' with 'pleasurable' on the grounds that, from late childhood onward, non-genital stimulation produced sexual excitation in the genitals in addition to the "pleasure of satisfaction" appropriate to the non-genital stimulation itself (op. cit., p.212). What this argument reduces to is the ascription of the almost mature form of the relation between genital and non-genital satisfaction to

the earlier, immature period. Once again Freud assumed what had to be proved, namely, that these childhood pleasures are sexual and that they give sexual satisfaction. Freud's final attempt at resolving the difficulty is in a later discussion of the relation between non-genital fore-pleasure and genital end-pleasure and is considered in the next section.

Not only is it difficult to accept Freud's interpretation of component drive pleasure as sexual - none of the activities typically follows a pattern of building up to a climax followed by a relaxation. Lindner's description of "exultant" sucking does not include a definite ending of the activity, the same is true of Levy's (1927) observations of less intense forms of sucking, and Wolff (cited in Dahl, 1968) has remarked "non-nutritive sucking seems to be inexhaustible". Ozturk and Ozturk (1977) have shown that children who have been left to fall asleep alone after feeding tend to become thumb-suckers. They suggest that if thumb-sucking is followed by sleeping it may be through a simple reversal of this causal connection. Except as it may be associated with defaecation, anal stimulation seems on its own not to follow the typical pattern. Indeed, if the observations of Spitz and Wolf (1949) represent the real state of affairs, external stimulation of the anus is hardly ever to be seen in childhood: they saw it in only 16 of 384 infants. Faecal play is the main mode of 'anal' activity. Some doubt is thrown even upon this observation by Heimann's (1962) finding that infants *dislike* their faeces. With genital stimulation the matter is less clear. While it has long been known that direct manipulation may induce orgasm or orgasm-like reactions in female children, and to a lesser extent in males, it does not seem to be a frequent consequence (Moll, 1909/1912, pp.57-59). Spitz and Wolf did not often see even self-absorption and withdrawal in children manipulating their genitals. In the absence of some type of external intervention, what appears to happen is that genital play is "terminated" by the child simply passing on to another activity. Kleeman's conclusion from one mother's detailed observations of her young son's reaction to discovering his penis is consistent with these points. After describing the child's penile manipulation as deliberate and accompanied by erections and expressions of pleasure she drew attention to two of the qualities characterising erotic activity as an "absorption of the attention by it and a mounting excitation" and concluded:

> Mild pleasure rather than the prominence of these other two qualities marked William's tactile stimulation of his genitals in the first year. (Kleeman, 1965)

Kleeman cites eight other psycho-analysts who have made the same observations. As Moll had long ago noted, despite the frequency of genital manipulation in children, orgasm or orgasm-like reactions are rare.

Part of the reason that activity supposedly motivated by component drives does not follow the pattern of mounting excitement and relaxation is that it lacks a consumatory segment which brings the activity to a close.

For the organic drives, Freud thought a "feeling of satisfaction" was brought about when a specific or consumatory reaction discharged the tension of the drive. Thus, the ingestion of food satisfied the hunger drive and the voiding of sexual products relieved adult sexual tension. But, as Moll (1909/1912, p.58) observed, it was precisely that "voluptuous acme" that was "wanting" in the child. More generally, no such discharge mechanism was or could be described for either infantile or adult component drives. Pleasurable sensations from sucking, anal stimulation, or genital manipulation would have to be produced continuously, without coming to a definite end, which is exactly what is found. And, in relation to Freud's model of need satisfaction, that consequence is a quite untoward one - it makes his component drive thesis very doubtful. A consumatory reaction capable of terminating component activity with a degree of definiteness ought to characterise every instinctual drive.

Sexuality in the perverse?

At the other end of the developmental spectrum the characterisation of the behaviour of the perverse adult was similarly problematical. In perversions based on fixations of the sexual preliminaries, Freud had concluded that there had been a transformation "into new sexual aims that ... take the place of the normal one", that is, they provided sexual gratification (Freud, 1905b, p.156). Longing for the fetish, exhibiting one's self, and satisfying sexual curiosity were also said to have become perversions once they had taken the place of the normal aim (op. cit., pp.154, 157). Further, in those perversions based upon anatomical extensions, the mouth and anus were said to "behave in every respect like a portion of the sexual apparatus" (op. cit., p.169). In hysteria these same zones were also said to undergo changes in innervation and become capable of producing genital-like sensations. Perverse adult activity thus had a definite sexual aim and led directly to orgasmic sexual satisfaction.

However, in the literature on perversion studied by Freud prior to writing the *Three Essays*, and especially in the case histories reported by Bloch, Havelock Ellis, Eulenberg, Kraepelin, and Krafft-Ebing, the perverse act was hardly ever described as bringing about orgasm and tension relief by itself. Among psycho-analytic writers on perversion there is also general agreement that preparatory acts do not supplant the normal sexual aim. What they do do is to give rise to sexual excitement that must then be satisfied genitally. Balint (1936/1938), who referred to similar and much earlier remarks *en passant* by Ferenczi and Sadger, observed the perverse activity produced only excitement, relief being obtained only from subsequent coitus or masturbation. He added that this observation "applies equally to active and passive scopophilia, fetishism, sadism, masochism or whatever form the perversion may take". He repeated this view unchanged some twenty years later (Balint, 1956), during which time it was endorsed implicitly by Bemporad (1975) and explicitly by no less an authority than Fenichel (1945b, p.325). A similarly authoritative

contradiction of Freud comes from the 1974 study group on perversion established by the American Psychoanalytical Association:

> The cases considered by the group revealed that perverse behaviour is usually terminated with genital orgasm no matter where the stimulus was applied. Though the major source of gratification lay in the extragenital stimulation genital orgasm seemed to constitute the 'final common path' of sexual discharge. (Ostow, 1974, p.4)

Prudently the group qualified this generalisation by saying that is was "of course, uncertain whether the sexual excitement *always* finds a genital outlet" (ibid. My emphasis, MBM). If genital satisfaction is so intimate a part of the perverse act, if the perversion is only an "indirect way to genital end-pleasure" as Balint (1936/1938) put it, it cannot be the case in perversion that the normal aim has been replaced.

If adult perverse activity neither brings orgasm about directly nor provides a substitute for genital satisfaction, and especially if, by itself, it does not terminate with obvious signs of tension discharge, the curious and seemingly nonsensical question poses itself of the way in which perversions are sexual. Should perversions turn out not to be sexual, what kinds of behaviour ever could be? This question is undoubtedly the same question Compton (1981a) calls the "larger problem" in Freud's theory:

> How does one tell reliably what are sexual manifestations? What concepts are needed to state criteria for sexual manifestations in general? And for sexual manifestations in children in particular?

Perversions clearly are sexual, but just as obviously their sexuality must be rather different from Freud's picture of them. And, if they are sexual and Freud's theory is to hold, the childhood behaviours must be sexual in the same way.

Nor is Freud's slogan of neurosis as the negative of perversion now thought to have ever been true. Some of those who characterise it as "elegantly simple and powerful" simultaneously criticise it severely (Compton, 1986a). To the extent that the slogan has been modified, the changes are not due to the accumulation of new clinical data, as Compton claims. They are alternative perspectives about what is already supposed to be known. Gillespie (1952), for example, sees *similarities* between perverse activity and the neuroses at the unconscious level. As he himself says, it is no wonder that psycho-analysts find it difficult to place the perversions in a developmental sequence. Does it not follow that this must also be true of childhood sexual activity generally?

Body as sexual object?

The third problem with Freud's concept of sexual instinctual drive lies with the claim that it is the subject's own body which is the sexual object. As has been seen, well before the *Three Essays* were written, Freud had

come to view the essence of adult perversions as childhood behaviours that had escaped repression. Consequently, if he were to maintain this view after characterising the childhood activity as auto-erotic, adult perversions had also to be without object. And that is how he represented them. But there were contradictions in the characterisation. After having first characterised sensual sucking as objectless (Freud, 1905b, pp.181-183), Freud then nominated the mother's breast as the child's first object (op. cit., p.222). He did not draw his readers' attention to the fact that if the sucking was to be regarded as a substitute for sucking at the breast it was nevertheless directed toward an object ('a refinding of it'). Further, if the loss of the mother's breast really caused the drive to become auto-erotic and if it remained so until puberty, the child's sexually based love for others during latency could hardly be regarded as an object-love based on the suckling's relation (op. cit., pp.222-223). And, although Freud granted such component instinctual drives as scopophilia, exhibitionism, and cruelty involved other people "from the very first" (op. cit., pp.191-192), he did not remark on the ensuing contradiction of his conceptualisation of the other erotogenic zones - which did not, of course, involve objects - giving a sexual colouration to the component drives (op. cit., pp.167-168).

In the second edition of the *Three Essays*, published in 1910, Freud insisted that the separation in time between the stages of auto-erotism and object-love was an artefact resulting from his deliberate use of mode of exposition that heightened the conceptual distinction between them (op. cit., p.193, n.2). Because this lamentable excuse does nothing to resolve the conceptual confusion between the two modes of satisfaction, it appears to have been (justifiably) ignored by most psycho-analytic writers on libido theory. Even were it acceptable, it would not affect the basic criticisms.

One has to agree with Compton (1981a) that Freud seems to imply:

> that the sexual drives proceed *simultaneously* along auto-erotic and object-directed developmental pathways. (My emphasis, MBM)

And also agree that "It would be difficult not to acknowledge that there is some confusion here." Freud's claims about objectless childhood sexual drives have also been judged incorrect by a number of other psycho-analytic writers - although it ought to be pointed out that, like Compton's, their verdicts tend only to notice the contradictory descriptions of infantile sexuality in the *Three Essays*.

Freud's own modifications to the theory of psychosexual development emphasised the relation of the drives to objects at the expense of their auto-erotic proclivities. Except as a synonym for genital masturbation, Freud ceased to use the term auto-erotism. In *Instincts and their vicissitudes* auto-erotism was dropped as a developmental phase (Spruiell, 1979) and, in Freud's last works infantile sexuality is not even described as auto-erotic (Cf. Kanzer, 1964).

Many quite different psycho-analytic writers have asserted that the infant is dependent upon sexual objects from the beginning and that auto-erotism is a secondary development. According to Zetzel (1955b) among them are Ferenczi, Balint, Melanie Klein, and Fairbairn. Fairbairn (1941, 1952, 1956) especially criticised Freud's view that the libido sought only pleasure at the erotogenic zones:

> The libido theory is based ... upon the conception of erotogenic zones. It must be recognized, however, that in the first instance erotogenic zones are simply channels through which the libido flows, and that a zone only becomes erotogenic when libido flows through it. The ultimate goal of the libido is the object. (Fairbairn, 1941)

He later qualified this formulation by observing it was the individual in his libidinal capacity, rather than the libido itself, who was object-seeking but continued to stress:

> it is implicit in the libido-theory that the object only becomes significant in so far as it is found to provide a means of forwarding the pleasure-seeking aim. (Fairbairn, 1956)

Fairbairn's revision places object seeking propensities more centrally than Freud did in the *Three Essays* and his basic criticism has been accepted by many other psycho-analysts even though his particular theory has not. Some, ego-psychologists, like the Blancks, even take their reforming zeal seriously enough to suggest "disposing of the psychosexual stages of libidinal development" altogether (M. I. Klein, 1983).

Many analysts, among them Fenichel (1945b, pp.324-366) and Gillespie (1956b/1965), have criticised Freud's view that perverse adult activity does not involve objects. Gillespie specifically rejected Freud's portrayal, adopting as a basis for his own approach the 1923 interpretation of perversions by Sachs, who had insisted that component drives generally appear in perversions only *after* they become attached to objects. According to Sachs, component drives underwent this transformation as they passed through the Oedipal situation. Sachs used as the starting point for his argument the modification Freud himself had made in *A child is being beaten* (Freud, 1919a), published some fourteen years after the *Three Essays*. Freud had not taken his argument to its logical conclusion. When Sachs did so, he totally demolished Freud's original thesis (Sachs, 1923/1986. Cf. Compton, 1986a, 1986b). Psycho-analyses of real perverse activities do not show them to be without objects, as Freud thought. Perversions cannot be more or less direct manifestations of objectless component drives that have evaded repression. In the course of discussing Limentani's views, Arlow (in Jaffe and Naiman, 1978) reiterated that not until 1919 had Freud put forward a more complex view of perversions. Arlow specifically criticised Limentani's concept of perversion as "essentially a restatement of the thesis that perversion ... represent[s] total discharge and very early instinctual representations". I take this

formulation to be identical with Freud's in the *Three Essays* (Cf. Balint, 1956; Gillespie, 1952, 1956a, 1956b/1965, 1964; Jaffe and Naiman, 1978).

Observation v. clinical analysis

It might be reasonably objected that the above points place too much weight on direct observations of behaviour, especially of childhood behaviour, and not enough on the clinical analysis and interpretation of symptoms. After all, had not Freud himself insisted repeatedly on the equal or even greater importance to his theory of data obtained in therapeutic investigations (Freud, 1905b, pp.163, 169, 192, 201), actually going so far as to say in the 1920 preface to the Fourth Edition of the *Three Essays*:

> If mankind had been able to learn from a direct observation of children, these three essays could have remained unwritten. (op. cit., p.133)

According to Freud, the difficult question of "the general characteristic which enables us to recognise the sexual manifestations of children" was answered by "the concatenation of phenomena into which we have been given an insight by psycho-analytic investigation" (op. cit., p.180). Or, as he said later, he had called:

> the dubious and indefinable pleasurable activities of earliest childhood sexual because, in the course of analysis, we arrive at them from the symptoms after passing through indisputably sexual material. (Freud, 1916-1917, p.324)

For example, clinical-therapeutic investigations of cases like Dora's provided the main evidence for characterising thumb-sucking as sexual. Dora's childhood thumb-sucking was thought of as creating a "somatic compliance" in the oral zone through which later sexual excitation found expression (Freud, 1905a, pp.51-52). Herr K.'s kiss aroused disgust because the sexual excitation supposedly produced in Dora by the pressure of his penis against her genitals had supposedly been displaced upwards on to a now abandoned erotogenic zone. It was because the sensory content of a symptom had to repeat that of its precursor that the earlier activity had to be sexual. Genital excitation, the one kind that was undoubtedly sexual, could find reflection in only those organs compliant enough with the demands of the sexual instinctual drive to have been sexually excited in the past. It is this purely theoretical requirement that Freud "arrives at" in the course of analysing the symptoms.

Freud also argued against the hypothesis that the childhood behaviours originally produced an indifferent organ pleasure that became sexual only during development. A biological analogy suggested itself to him. He asked his readers to consider the problem posed by the very different bean and apple-tree having originated from similar looking seedlings:

> Am I then to suppose that they are really alike, and that the specific

> difference between an apple-tree and a bean is only introduced into the plants later? Or is it biologically more correct to believe that this difference is already there in the seedling? (Freud, 1916-1917, p.325)

Freud then claimed it was:

> the same thing when we call the pleasure in the activities of an infant-in-arms a sexual one. (ibid)

Abnormal adult sexual pleasure, albeit in repressed form, seemed always to have been foreshadowed by a childhood pleasure in the same zone. From the analogy the earlier pleasure had also to be also sexual.

Three things may be said about the privileged status Freud accorded his clinical-therapeutic data. First, although much of my criticism has been based on data gathered by direct observation a preference for clinical-therapeutic data cannot be used to dismiss them. They are, in fact, precisely the kinds of direct observations used by Freud himself to support the sexual characterisation of the *Three Essays*. If they are unimportant to my arguments about sexual characterisation they must also be unimportant to Freud's. Why then did he cite them at all and apparently lay such stress upon them? Second, it is extremely doubtful if the sexuality of childhood activities can be established satisfactorily through the analysis and interpretation of symptoms. To the extent that Freud's reconstruction of the history of Dora's symptoms rested on the sensory content expectation, and it does seem to have been its fundamental basis, the sexuality of her thumb-sucking was pure supposition, incapable of empirical confirmation. All that the clinical data can reveal is whether or not one kind of adult symptom or activity is regularly anticipated by a particular type of activity in childhood. Clinical data cannot illuminate the psychological content of what the child does. This point leaves aside, of course, Freud's use of his associations rather than Dora's, his doubtful interpretations of her behaviour, and the inconsistencies in his piecing together the fragments of her developmental history, as well as the question of whether the method of free association is uninfluenced by expectations. Third, Freud's plausible garden analogy misses the point as much as does his concluding assertion. It is rather like arguing that because the ear is involved in both activities, the child's pleasure at hearing the bird calls of Leopold Mozart's *Toy Symphony* is the same as that of the adult listening to Bach's *Suites for Solo Cello*. And, simply because oral stimulation is involved in both, is it to be claimed seriously that the pleasure derived from non-nutritive sucking is the same as that of concluding an ample dinner with a good vintage port? Adult characteristics may well develop from an earlier stage without being prefigured in their childhood precursors.

In some measure the preceding arguments are consistent with one of the main points made by Wolff in summarising and endorsing Chodoff's (1966) criticism of Freud's theory of infantile sexuality:

> apparent similarities between the sexual perversions of adults ... and childhood pre-occupations with specific body processes and anatomical orifices, are in no sense evidence for the psychosexual life of infants. What appears (sic) to be similarities on the surface, are nothing more than analogies of form from which we can infer nothing about meaning or erotic content of the young infant's psychological reality. (Wolff, 1967)

To turn the rather old epigram revived by Chodoff: Freud seems to have been in considerable error in supposing that infants enjoy themselves in infancy in the same way as do adults in their adultery. I have gone further than either Chodoff or Wolff, of course. The similarities between what Freud took to be sexual activity in childhood and adult sexuality in its perverse and normal variants are not even analogies of form. Only by what amounts to a systematic misrepresentation can the childhood activities be pictured as sexual in any way at all.

EVALUATION OF THE THEORY OF SEXUAL DEVELOPMENT

In the last section it was evident that Freud had described correctly neither the perverse sexual activities of the adult nor their supposed infantile counterparts. Even were it the case that the infantile drives and the predisposition had been described with perfect accuracy, Freud's theory of sexual development was still unable to explain how these elements could be assembled into the pattern of normal and abnormal adult functioning. This failure of the theory is best approached through successive analyses of the accounts of object choice, of the establishment of genital primacy, and of the scope of the mechanism of repression.

Object choice

One of the main deficiencies of Freud's account of object choice is that it does not explain how the female comes to choose an object of the opposite sex. The basis of the deficiency lies in the assumptions that the object of importance to the child of either sex is the mother's breast, that the breast is the first object, and that the suckling's relation with the mother is the prototype of all other pre-pubertal relations. On these assumptions the male child was provided with a female object from the beginning. At puberty he had only to erect the barrier against incest by taking "up into himself the moral precepts" expressly excluding the choice of his mother or someone in the circle of his immediate relatives. Something more than this mechanism was required to ensure an opposite sex choice in the female. For were she simply to undergo the same development as the male, she would still be left with female objects. To overcome this problem Freud proposed that at puberty repression transformed the sexuality of the female from its masculine, infantile form into its feminine, adult form. Once the basic sexuality of the female had been so changed, it appeared to be simply a matter of the attraction of opposites coming into operation, a

mechanism Freud described as "the strongest force working against a permanent inversion of the sexual object" (Freud, 1905b, p.229). Both steps in this process had been made necessary by the assumption that the important object had been the mother's breast. But neither the first and crucial step, the replacement of masculine sexuality by feminine sexuality, nor the second, choosing a male adult, was explained adequately.

Because Freud assumed that the little girl's sexuality was masculine, he had also to explain how it changed into feminine sexuality. Changes at puberty that supposedly increased libido in the male were supposed to produce a fresh wave of repression in the female. Nothing else in the theory presupposed such an outcome, except perhaps the circularly based and equally *ad hoc* assumption that in females "the tendency to sexual repression seems in general to be greater" (op. cit., p.219). What this repression had to produce was the exchange of the excitability of the clitoris for that of the vagina. What Freud described was a selective inhibitory process that put an end to the capacity of the clitoris to respond to manipulation in the old way, but which somehow allowed it to become excited enough during the normal sexual act for clitoral excitability to be transferred to the vagina (Eissler, 1977, n.15).

It was the *male's* sexual drive, aroused by the very repression of libido in the female, which actually created the new female response:

> The intensification of the brake upon sexuality brought about by pubertal repression in women serves as a stimulus to the libido *in men* and causes an increase of its activity. Along with this heightening of [male] libido there is also an increase of sexual over-valuation [by the male] which only emerges ... in relation to a woman who holds herself back and who denies her sexuality. *When at last the sexual act is permitted and the clitoris itself becomes excited, it still retains a function: the task, namely, of transmitting the excitation to the adjacent female sexual parts.* (op. cit., p.221. My emphasis, MBM)

Consequently, although pubertal repression was supposed to inhibit clitoral excitability, it did so with a very strange selectivity: the inhibition lasted only until heightened male sexuality incited the normal sexual act. The re-excitation of the previously restrained but now disinhibited clitoris then sparked off vaginal responsiveness rather than its own orgasm. No real explanation of the change in the sexuality of the female was being offered. Freud had merely described what he needed to explain.

With respect to the choice of an adult male as the female object, Freud's explanation broke down completely. First, even if repression accounted for the change in the leading erotogenic zone, that change together with the attraction of opposite sexual characters did not account for the repudiation of the mother. In any case, Freud described the repudiation only for the male. The female was described as repudiating the *father* (op. cit., pp.225-227), although the theory had not only not provided

her with such an object, but her relation to the father as object had not even been mentioned. Nor was male homosexual object choice touched on.

We must note that the *Three Essays* at least contains and may even be built upon a paradox: although it was the unconscious perverse tendencies of the psychoneurotic *female* that posed the original problem, Freud's theory was written almost completely from the point of view of *male* sexuality (Montgrain, 1983). By this I do not refer to the quite trivial point of the masculine linguistic forms with which his ideas are expressed, but to such things as the male model implicit in Freud's accounts of the suckling's relation to the mother, the mother's role in teaching the child how to love, the role of the male in awakening normal female sexual responsiveness, and the discharge of sexual substances in relieving sexual tension, to name just a few. In Chapter 14, the inability of the theory to portray female psychosexual development with any consistency, especially female object choice, is shown to stem partly from this male orientation.

Genital primacy

Of genital primacy it can be said that the deficiencies of Freud's account of its establishment undoubtedly reflect inherent contradictions in his characterisation of infantile and adult sexuality. On the one hand, certain aspects of the theory required both forms to give rise to the same kinds of sexual satisfactions. On the other, the very idea that genital primacy existed in the adult implied infantile sexuality to be different in some way. Freud's descriptions of how genital primacy was attained reveals two different and equally unsatisfactory explanatory mechanisms. I term Freud's first explanation "the changed role" solution. In it he proposed that the non-genital zones gradually lost their capacity to give specifically sexual satisfaction over the same period that the genitals acquired it. I call the second explanation "the augmentation" solution. According to it, the non-genital erotogenic zones had *always* been able to contribute to sexual excitement in the genitals at the same time as they produced their own particular pleasurable satisfaction. As the child grew older, all that happened was that this effect became more marked, or was augmented.

In the changed role explanation a sharp distinction was made between the end-pleasure of sexual satisfaction and the fore-pleasure preceding it. In childhood, all zones had been equal in the kind of pleasure they produced. On maturation, stimulation of the non-genital zones produced genital sexual tension as well as their own particular pleasure. Because Freud gave no reasons for this change in the role of the zones, the most that can be said is he had described the change rather than explained it.

The augmentation solution proposed that stimulation of the non-genital zones in childhood produced sexual excitement and slight amounts of sexual tension in the genitals themselves. During development this effect

stabilised, the quantity of genital excitation became greater, and finally the adult form of the relation between fore-pleasure and end-pleasure emerged. Freud described the embryonic form of the relation only in the second half of childhood, from about eight years of age until puberty, but it is clear he presumed it to be present also in the first half. As was mentioned earlier, his equating of sexual satisfaction, sexual excitement, and pleasurable sensations, when he discussed the effects of passive and active movements, is based on that presumption (Freud, 1905b, pp.208-233). The augmentation solution has the advantage over the changed role solution in having to account only for the growth or development of an already existing function rather than having to explain how the more radical change is effected. While that growth would itself have to be explained, the solution does have the potential for co-ordination with general development processes. Pubertal sexual development makes sexual excitation and sexual satisfaction possible for the first time. If it were better accounted for, that fact alone would remove some of the mystery of the relation between pleasure, excitation, and satisfaction.

However, were the augmentation solution to be adopted, it would then become impossible to regard adult perversions as infantile activities simply carried over into adult life and as independent of genital sensibility. Freud's suggestion that perversions developed when genital excitation was weak as compared with the non-genital pleasure was consistent with the augmentation solution, of course. But he could specify the conditions of that developmental anomaly only very vaguely. All he could say was that the zone or component involved in the adult perversion had "contributed an unusual amount of pleasure" in childhood and that further factors, which he did not name, had produced a fixation (op. cit., p.211).

Consequently, if pleasurable satisfaction and sexual excitement were independent to begin with, the perversions were comprehensible but the mechanism of genital primacy was not. Alternatively, if satisfaction and excitement were linked from the beginning, genital primacy was explicable but not the perversions. Even were Feldman's (1956) claim true that genitality is strong enough in the pre-genital phase to give "the erotogenic zones an orgiastic potentiality", the problem is not thereby solved. In brief, the adoption of the augmentation solution would require such marked changes in the conceptualisation of infantile and perverse adult sexuality, and such equally radical alterations in the explanation of psychosexual development, it is doubtful if Freud's sexual theory could survive. Freud, of course adopted neither solution. Nor did he resolve the difficulties it might have overcome. In the concluding Summary of the *Three Essays* Freud conceded the recalcitrance of the problems associated with genital primacy, end-pleasure, and fore-pleasure:

> We were reluctantly obliged to admit that we could not satisfactorily explain the relation between sexual satisfaction and sexual excitation, or

> that between the activity of the genital zone and the activity of the other sources of sexuality. (op. cit., p.233. Cf. Compton, 1981a)

This concession, which seems to have been overlooked by most of those interested in psycho-analytic theory, trenches so deeply into the foundations of Freud's infantile sexual theory as to undermine it altogether.

Repression

Lastly we turn to repression. We begin by observing that Freud does not define repression in any way in the *Three Essays*. We discern he thought of it as producing a reversal of affect rather than as stripping the affect from an idea. Thus, if repression of the oral zone took place, the individual would "feel disgust at food and will produce hysterical vomiting" (Freud, 1905b, p.182). Again, since the pleasurable sensations derived from passive movement could create a compulsive linkage with sexuality, repression, "which turns so many childish preferences into their opposite", would create feelings of nausea if the adult or adolescent was swung or rocked (op. cit., p.202). Finally, the psychoneuroses were said to result from infantile perverse activity when "a reversal due to repression" took place (op. cit., p.238). So conceptualising repression was a necessary complement to the characterisation of infantile sexuality as pleasurably auto-erotic. Unless reversal took place, infantile modes of satisfaction would never be given up. But what was the source of the unpleasure that supplanted the original pleasure? When he still believed in the seduction theory, Freud supposed that sexual 'experiences' were unpleasant in themselves (hysteria) or were linked with a later unpleasantness (obsessions). The stimulation of the supposed memories of these unpleasant experiences simply released fresh unpleasure (Freud, 1896b, p.166, n.2; Masson, 1985, Draft K of 1.1.96). Obviously this thesis could not be maintained after the collapse of the seduction theory.

The organic mechanism of automatic repression eventually replaced the original stripping of affect. Now, one of the difficulties with the organic explanation was that, in not knowing how the evolutionary change from 'interesting' to 'disgusting' sensations had come about, the uncharacterised process had been simply transferred from the individual to the species. There was the further difficulty that the implicit recapitulation assumption made a completely uncharacterised biological process responsible for sexual development. This biological process is implicitly present, of course, in the *Three Essays* and may be detected in at least three places. First, Freud said the reversal of affect brought about by repression was due to "internal causes" (Freud, 1905b, p.238). Second, the order in which the component instinctual impulses appeared, as well as the length of time before they succumbed "to the effects of some freshly emerging instinctual impulse or to some typical repression", was said to be "phylogenetically determined" (op. cit., p.241). Third, the building of the dams of disgust, shame, and morality was said to be organically determined

and fixed by heredity (op. cit., pp.177-178). And although Freud's phrasing of the relation between repression, reaction formation, and the dams is markedly inconsistent, there is little doubt that organic repression was the builder of the dams (op. cit., pp.178, 232). In a 1915 footnote to the *Three Essays* he made the point explicitly:

> these forces which act like dams upon sexual development.- disgust, shame and morality.- must also be regarded as historical precipitates of the external inhibitions to which the sexual instinct has been subjected during the psychogenesis of the human race. We can observe the way in which, in the development of individuals, they arise at the appropriate moment, as though spontaneously, when upbringing and external influence give the signal. (op. cit., p.162, n.2)

The Editor's Introduction to the *Three Essays* formally identifies disgust, shame, and morality as "the repressive forces" (Freud, 1905b, p.127). The formulation is inaccurate: in Freud's view repression *created* the forces.

All that could ever be observed were facts about the appearance of shame, disgust, and morality, observations by themselves having little relevance to either recapitulation or to the presumed hereditary nature of the process. Perhaps this is why Freud's conceptualisation of these "mental forces" as part of a phylogenetically determined process are never paid much attention in the psycho-analytic literature. If their instinctual aspects are mentioned at all, it is to reject their instinctual determination (Cf. Kinston, 1983; Spero, 1984; Miller, 1986).

In the theory of psycho-sexual development, repression was no more than an uncharacterised component of a mysterious biological unfolding. And, to the extent the assumptions on which it rested were inconsistent with the evidence on the inheritance of acquired characteristics, it was most unlikely that it had any explanatory power at all.

Not only was repression uncharacterised. Neither its results nor the conditions under which it operated could be described with any definiteness. Thus, Freud assumed the abandoned activity could be resumed once again if the mental forces were overridden by sufficiently strong libido without seeming to see this qualification contradicted almost everything else he had then recently written about it. In Chapter 7 of *The Interpretation of Dreams*, for example, activities similar to those involved in restimulating old erotogenic zones had been supposed to produce an unpleasure incapable of being inhibited. Yet Freud also explained the occurrence of "perverse" sexual activity in normal people, and the readiness with which the "average uncultivated woman" could be led toward a perverse sexual life, by strong libidinal demands overriding the unpleasure. If repression were responsible for the permanent renunciation of the zones, not even the amatorial skills of a Casanova should have been able to coax pleasure from them again.

One resolution of this contradiction would have been to differentiate

"normal" from "pathological" repression and to suppose that libidinal urgings could overcome the former but not the latter. At several places Freud implied that such a distinction could be made, but nowhere indicated what the difference between the two kinds of repression might be, nor the conditions giving rise to each (Masson, 1985, Letters of 30.5.96, 6.12.96, and 14.11.97). However, something like "normal" repression was needed to account for "normal" sexuality. Perversions were due to failures of repression and the repression that caused hysteria resulted in excessive sexual cravings and aversions. Normal repression had to cause the erotogenic zones to be abandoned but sometimes allow libidinal urgings to overcome the repressive forces as well as attenuating cravings and aversions. Repression of a quite different type or magnitude from that causing neuroses must have been involved, but Freud neither defined the nature of the difference nor specified the conditions under which the outcome was normal or pathological. In this respect also his developmental theory is without explanatory power.

Finally, the mechanism of repression seems too limited to explain object choice. The main modifications to the infantile disposition were the replacement of auto-erotism by object-love and the creation of a genital aim. While Freud's summary statement explicitly attributed the new aim to repression (Freud, 1905b, pp.237-238), it was not at all evident what brought about the change in object. A very strong case can be made for repression being thought to be responsible. The prohibition against an incestuous object-choice was described as a barrier (op. cit., p.225), a term otherwise used only for the repressive mental force opposing cruelty (op. cit., p.193). That usage matched the connotation of the obstacles and dams opposing auto-erotic libidinal aims. Incestuous pubertal fantasies given up before the final choice was made were described as being repudiated (op. cit., p.227), and Dora's heterosexual libido was similarly said to have been energetically suppressed (Freud, 1905a, p.60). All these terms are consistent with an appeal to repression as the determinant of object-choice. Certainly, if repression was not responsible that whole process was left without a directing influence. Now, if repression was the reversal of the affective quality of *sensations* produced in abandoned erotogenic zones, it was hard to see how that could affect objects. Only if objects somehow shared in the after-glow of the sexual sensations produced in the zones could they or the tendencies toward choosing them have been repressed. Not only did Freud not propose such a connection, erotogenic zone activity was auto-erotic, lacking objects with which the sensual pleasure could be shared. By linking repression so closely to the erotogenic zones, Freud had effectively prevented himself from explaining object choice.

BIOLOGICAL DETERMINISM AND SENSORY CONTENT

When Freud set out for Fliess his reasons for rejecting the childhood seduction theory, he confessed he had been so greatly influenced by them

he had been "ready to give up two things: the complete resolution of a neurosis and the certain knowledge of its etiology in childhood" (Masson, 1985, Letter of 21.9.97). The theory set out in the *Three Essays* saves these two basic features. Proposing that germs of a sexual impulse were to be found in infancy and that they consisted of component drives seeking auto-erotic satisfactions allowed Freud to continue to link the sensory content of the symptom to an earlier experience, albeit an auto-erotic one, with similar content. Because of this retention of the deterministic ideas embedded in the concept of the logical and associative structure of the neurosis, a "complete resolution" was still possible. And with that proposal, as Freud had foreseen, "the factor of a hereditary disposition regains a sphere of influence from which I had made it my task to dislodge it" (ibid). This resurgence of hereditary influence involved more than a simple predisposition: the order in which the component drives appeared, the length of time for which they were manifest, the construction of the dams restricting the sexual impulse, and the very mechanism of repression itself all had an hereditary basis. As Laplanche and Pontalis (1968) put it:

> The sexual development of the child ... is defined as endogenous and determined by sexual constitution.

Whether thought of as influences of an organic-constitutional kind or as characteristics acquired through the action of external influences during mankind's evolution, these instinctual determinants are simply a necessary consequence of the deterministic views to which Freud continued to adhere after the collapse of the seduction theory. If the neuroses were not the consequences of seduction experiences, and if expectation and unconscious suggestion were to be rejected as influencing patients' recollections, only uniform hereditary influences could have been responsible for the clinical uniformities Freud needed to explain.

Freud's pseudo-biology provides so good a screen for his inadequate logic that it is rarely pierced even when the 'biology' is criticised as, for example, by Jacobsen and Steele (1979). When Freud went about "constructing an infantile past", as Jacobsen and Steele put it, it was not merely to provide a basis from which *a* causal explanation of abberrant adult sexuality could be derived, as they argue. The resulting construction had to meet his *particular* deterministic ideas.

CONCLUSION

Freud's concept of a sexual instinctual drive filled the gap left by the collapse of the seduction theory and provided his mentalistic theory with an organic base. The drive was the most important and constant source of energy of the wishes that had to be repressed and, therefore, for the creation of fantasies and dreams as well as for the maintenance of neurotic symptoms. It was clearly of equal importance in motivating the

perversions, infantile sexuality, normal character formation and, through sublimation, the development of civilization itself.

For Freud the most telling evidence for an infantile sexuality having the characteristics he ascribed to it was not that obtained from direct observations of childhood but from his reconstructions of the histories of adult symptoms. Perhaps he was wise to give his clinical analyses such privileged status because not only did his own and Lindner's observations fail to substantiate the characterisation but none have been found since.

Putting these difficulties aside, and granting the novelty of Freud's concept of a sexual instinctual drive, its field of action was one-sided. It had no role other than as the main component of the new biologically oriented theory of sexuality. In fact, it was not until 1915, in the third edition of *The Three Essays*, that Freud offered a reasonably complete definition of the drive and describe something of the wider significance of its energy (Freud, 1905b, pp.168, 217-219. Cf. Compton, 1981b). Even then it was next to impossible to see how aggression arose other than through some kind of frustration of the libidinal drive. As characterised by Freud, the pleasure-seeking drive could no better explain the supposed intimate connection between libido and cruelty than other theories. Further, the mental forces of shame, disgust, and morality were pictured as constructed from the very libidinal material they were supposed to control.

These explanatory discontinuities result from the theory being based upon a single instinctual drive. They would be removed by opposing sexuality with an ego-instinctual drive having mental and biological attributes resembling those of the sexual drive. Energy from such an ego drive could fuel aggressive behaviour and give strength to the repressive forces and other ego functions. An ego-instinctual drive of this kind did provide the basis for the final changes Freud made to his theory. It is set out and examined in Part III.

PART III

THE FINAL SYNTHESIS

Chapters 11 to 13

11 Ego and Ego-Instincts

12 Instinct Theory

13 Mental Structures

11 THE EGO AND THE EGO-INSTINCTUAL DRIVE

Shotover: A man's interest in the world is only an overflow from his interest in himself.

Shaw: *Heartbreak House*, Act II.

In Part IV, which consists of Chapters 11 to 13, I examine the origins and validity of some of the central components of the final version of Freud's personality theory: the instinctual drives and the structures of the mind, what Freud called "the mental apparatus", which regulated them. My analysis reveals that the important factor which led Freud to alter his conception of the mental apparatus from one consisting of conscious, pre-conscious, and unconscious systems to one made up of an ego, a super-ego, and an id, was the concept of instinctual drive.

Were I mounting an argument, rather than describing and evaluating what happened, I would say that its general line would be that Freud's introduction of the concept of instinctual drives in the *Three Essays* actually *necessitated* the final version of his personality theory. We shall see in Chapter 11 that once Freud had postulated an infantile sexual drive which was not only regulated via an automatic, organically based repression, he had to propose another instinctual drive as a counter to it and that the re-introduction of the concept of the ego is part of that counter. Freud then attempted to resolve the several problems associated with this conceptualisation with the concept of a death instinct and I examine that concept in Chapter 12. Freud's introduction of the ego, super-ego, id model of the mind, which I consider in Chapter 13, was in turn made necessary by the need to find a home for the death instinct.

What I describe and evaluate in this Chapter is Freud's conceptualisation of mental life as a conflict between sexuality and the ego. Particular attention is paid to the means by which Freud thought the ego-instinctual drive gave its energy to the standards of the ego because the real conflict took place between them and the demands of sexuality.

CONFLICT BETWEEN INSTINCTUAL DRIVES

Freud expanded the role given instinctual motive forces five years after the *Three Essays* when he briefly but formally described the concepts of ego and ego-instinctual drives for the first time. The paper in which Freud did

this, *The psycho-analytic view of psychogenic disturbance of vision* (Freud, 1910e), began with a kind of return to Janet: hysterical blindness was due to certain ideas connected with the act of seeing being cut off from consciousness. Freud now asserted that to understand this dissociation:

> we must ... assume that these ideas have come into opposition to other, more powerful ones, for which we use the collective concept of the 'ego' ... and have for that reason come under repression. (op. cit., p.213)

He then asked:

> But what can be the origin of this opposition, which makes for repression, between the ego and various groups of ideas? .... Our attention has been drawn to the importance of the instincts in ideational life. We have discovered that every instinct tries to make itself effective by activating ideas that are in keeping with its aims. These instincts are not always compatible with one another; their interests often come into conflict. *Opposition between ideas is only an expression of struggles between the various instincts.* (op. cit., pp.213-214. My emphasis, MBM)

The notion that instincts activated ideas in keeping with their aims was, of course, a lineal descendant of the notion that somatic excitation linked up with the group of psychosexual ideas and invested or cathected them with energy. But, if the sexual instinctual drive cathected sexual ideas, what was the drive that invested the set of ideas constituting the ego with energy and that opposed sexuality? An answer to that question was provided by the concept of ego-instinctual drives:

> From the point of view of our attempted explanation, a quite specially important part is played by the undeniable opposition between the instincts which subserve sexuality, the attainment of sexual pleasure, and those other instincts, which have as their aim the self-preservation of the individual - the ego-instincts. (op. cit., p.214)

Mental life was henceforth to be seen as expressing a fundamental conflict between two biologically based drives. Freud persisted with the ego vs. sexual drive conceptualisation until 1923, when he replaced it with the variant biological thesis that a death instinct, which he named Thanatos, provided the opposition to a poetically named Eros, or life instinct.

ORIGINS OF EGO AND EGO-INSTINCT CONCEPTS

We have seen that as early as the *Studies on Hysteria* Freud thought of the repression of incompatible ideas as due to a force located within the ego:

> The patient's ego had been approached by an idea which proved to be incompatible, which provoked on the part of the ego a *repelling force* of which the purpose was defence against this incompatible idea. (Breuer and Freud, 1895, p.269. My emphasis, MBM)

The psychical repelling force continued to be active. Precisely at the moment when the patient tried to recall the incompatible idea Freud had become aware:

> of *resistance*, of the same force as had shown itself in the form of *repulsion* when the symptom was generated. (ibid.)

He concluded:

> Thus a psychical force, aversion on the part of the ego, had originally driven the pathogenic idea out of association and was now opposing its return to memory. (ibid.)

Although both the ego and the force that it deployed were extraordinarily central concepts, neither was characterised further. In the *Studies on Hysteria*, the term 'ego' was given some kind of meaning in that it was treated as a synonym for 'self', 'person', or 'consciousness' (Rapaport, 1959; Laplanche and Pontalis, 1967/1973, pp.132-135). The ego of *The Interpretation of Dreams* was, as Pumpian-Mindlin (1958-1959) put it, "a poorly delineated shadow represented chiefly in the form of the censorship". Neither work contained any characterisation of the force that drove it. Some fifteen years were to pass before that indeterminacy changed.

Why did Freud not develop the concepts of ego and ego-force for such a long time? The first reason which suggests itself is that those notions were not at all necessary to the organic hypothesis of automatic repression which was in the forefront of his thought until about 1905. Freud's hypothesis was that automatic repression came about when the role of smell changed as man adopted the upright carriage. Children went through a similar developmental sequence with the result that in adult life the revival of a memory of the stimulation of those zones also produced disgust:

> To put it crudely, the memory actually stinks just as in the present the object stinks; and in the same manner as we turn away our sense organ (the head and nose) in disgust, the preconscious and the sense of consciousness turn away from the memory. (Masson, 1985, Letter of 14.11.97)

Erotogenic zones were not mentioned in *The Interpretation of Dreams*, but what Freud there described as "the essence" of repression was exactly the transformation of affect supposedly produced by organic repression (Freud, 1900, pp.600-605). The mechanism of repression mentioned in the *Three Essays*, published five years later, was also based on a similar transformation of affect. As Hoenig (1976) puts it about this period of Freud's theorising, "the repressive forces like disgust and morality are conceived as largely constitutionally given." An organically based automatic avoidance of unpleasurable memories rendered a force to push unpleasant memories out of consciousness and deny them re-entry unnecessary. Neither a structure like an ego nor a force to motivate it was required.

I believe it to be significant that whenever Freud spoke of repression subsequent to the *Three Essays* he almost always described it as resulting from a psychological force rather than from an automatic process. For example, in his analysis of the German writer Jensen's story of *Gradiva*, the force repressing the instinctual eroticism was mentioned frequently and always in contexts that were psychological (Freud, 1907a, pp.68-69, 90-93). With one possible exception, this purely psychological characterisation seems to be true of every one of the descriptions and discussions of repression which Freud set out over the period 1906-1909. For example, in trying to assess the effects of socially enforced standards of sexual behaviour (what he termed "'civilized' sexual morality") on mental illness, Freud spoke repeatedly of the repressive and suppressive effects that the "demands of civilization" made upon the sexual instinctual drive (Freud, 1908c, pp.188-190, 200). Similarly, in the lectures on psycho-analysis delivered at Clark University in 1909, Freud declared the repressive forces to be "the subject's ethical and other standards", while he said that the unpleasure that preceded repression resulted from "acceptance of the incompatible wishful impulse or a prolongation of the conflict" over it (Freud, 1910a, p.24). Instead of education limping along in the tracks of some organically determined processes that had created the dams of shame, disgust, and morality, it was "under the influence of education" that those mental forces were now said to be brought into being (op. cit., p.45).

If by 1910 Freud was picturing repression as due to forces that denied consciousness to ideas incompatible with the individual's ethical and moral standards what are we to make of his earlier portraying of repression as due to an automatic tendency to avoid the unpleasure of re-stimulated, abandoned, erotogenic zones? Is it simply another inconsistency of formulation? Or, does it mean that between 1905 and 1910 Freud abandoned a biological conceptualisation and replaced it with a psychological? Neither. The two kinds of repression were brought into relation with each other by making one the precursor of the other: organic, automatic repression became 'primal repression' and the ego force caused 'repression proper' or 'after-pressure'. The first operated in childhood and provided a nucleus or point of fixation around which later memories pushed out of consciousness by repression proper could adhere.

Both the ego-instinctual drive responsible for repression proper and the two stage process of repression were introduced into the theory within a twelve month period (Freud, 1910e, 1911a). With them Freud arrived at a more plausible theoretical synthesis. First, a conception of mental life as resulting from opposed forces demands that the second force be of 'equal dignity' to the first, in this case in having a similar biological basis. As Compton (1981c) has said:

> The construct of ego drives ... represented an effort to give 'the ego' some theoretical status comparable to that of the sexual drives: conflict cannot

occur between forces which cannot meet on the same plane.

Even a developing ego able to draw upon the energy of a self-preservative instinct might be considered as possessing enough strength to match that of the sexual instinctual drive. An independent but related point is that once repression proper was conceived of as due to an ego-instinctual drive, the inconsistency was overcome of proposing that forces deriving from the subject's ethical and moral standards could repress the sexual drive when such standards were weak. Something like an inconsistency of this sort was present, of course, in the supposition of *The Interpretation of Dreams* that repression could be carried out by the embryonic developing secondary process of *Pcs.* (Freud, 1900, pp.605-606). The same point applies to the proposal in *On Dreams* (Freud, 1901a, pp.678-679) that the ego was responsible for repression - how could a weak structure initiate repression? If emergent ethical and moral standards were to counter an adult type of sexual drive, the ego in which they were located had to be supplied with a considerable source of energy from the very beginning of its formation. Only an inherent or instinctual source met that requirement. Third, while it is possible to see how disgust, shame, and pity might be related to an erotogenic zone being abandoned, it is very much more difficult to accept that kind of explanation for the appearance and function of ethical and moral standards themselves ('morality'). Although he did not acknowledge this difficulty, Freud eventually turned to a developmental process in which the ego-instinctual drives were central to account for the emergence of a set of standards in the ego.

Now, it should be noted that nothing like these reasons were put forward by Freud when he introduced the concept of ego-instincts. However, it is of interest that in the several works that preceded the paper on visual disturbances, Freud did draw attention to the very large amounts of energy that the ego needed to exercise its repressive function (e.g., Freud, 1910a, pp.53-54; 1910c, p.146).

I now consider the one possible exception to Freud's characterisation of repression as a psychological force in his 1905-1910 publications. In a paper on hysterical fantasies Freud stated that:

> Hysterical symptoms arise as a compromise between two opposite affective and instinctual impulses, of which one is attempting to bring to expression a component instinct or a constituent of the sexual constitution, and the other is attempting to suppress it. (Freud, 1908a, p.164)

Some aspects of the passage are very obscure - one instinctual impulse is described as bringing "a component instinct" or a "constituent" to expression - and it seems to hark back to the *Three Essays*. But it seems to me better construed as looking forward to the paper on vision, foreshadowing the argument that opposed one kind of instinctual energy to another. For this reason I would quarrel with H. Hartmann (1952) who says the early neglect of the ego was due to the "impact of the theory of

instincts''. On my reading, it was not until instinctual forces were extended into the ego that the ego itself became important.

The main reason put forward by psycho-analysts for Freud's introduction of the concepts of ego and ego-instinctual drives is that it was not until the period 1905-1910 that the concepts of self and ego became important for psycho-analytic explanations of homosexuality, schizophrenia, and paranoia. On this view, the concepts followed from clinical observations. However, that is only part of the story.

Freud first emphasised self and ego in his interpretation of a sexual perversion in which the individual gained sexual satisfaction from fondling his own body. In these cases and in homosexuality, or sexual inversion as Freud called it, Freud supposed that the self had been chosen as a love-object - an interpretation that naturally drew attention to the self as a concept. To refer to the tendency to self-love, Freud adopted the term 'narcissism' from Nacke and Havelock Ellis - Narcissus being the Greek youth of legend who fell in love with his reflection. Freud's first published use of the new term was in a 1910 footnote to the second edition of the *Three Essays* where he offered the following explanation of inversion:

> In all the cases we have examined we have established the fact that the future inverts, in the earliest years of their childhood, pass through a phase of very intense but short-lived fixation to a woman (usually their mother), and that, after leaving this behind, they identify themselves with a woman and take *themselves* as their sexual object. That is to say, they proceed from a narcissistic basis, and look for a young man who resembles themselves and whom *they* may love as their mother loved *them*. (Freud, 1905b, p.144, n.1)

At about the same time as this footnote was written, Freud undertook a biographical study of Leonardo da Vinci. In it he argued Leonardo had marked sublimated homosexual tendencies having their basis in a similar family constellation (Freud, 1910b, pp.99-100). After repeating the conclusion of the footnote, but qualifying it by saying that it was based on only ''a small number'' of patients, Freud added that some of the women who cared for the children were ''masculine ... able to push the father out of his proper place'', but that in other cases the father had actually been absent during the boy's childhood (op. cit., p.99). Both situations led to a ''very intense erotic attachment'' that could not continue consciously:

> The boy represses his love for his mother: he puts himself in her place, identifies himself with her, and takes his own person as a model in whose likeness he chooses the new objects of his love. In this way he has become a homosexual .... He finds the objects of his love along the path of *narcissism*. (op. cit., p.100)

By so drawing attention to the self as a sexual object the concept of narcissism virtually forced the ego back into Freud's theory. Later, when

narcissism was seen as a phase through which *all* individuals passed, the status of the ego within the theory was confirmed.

Freud's concepts of ego and ego-instinctual drives are best seen as drawing together three threads: one required him to postulate an instinctual source of energy responsible for the force causing repression proper, the second required an agency or set of ideas through which the instinct could operate, and the last required the recognition of the self as an object that could be loved.

NARCISSISM AS A DEVELOPMENTAL STAGE

Through a complex analysis of the connection between narcissism and homosexuality that he believed he had discerned in schizophrenia and paranoia Freud attempted to characterise the ego rather more fully than in the paper on disturbances of vision. Redefinitions of narcissism and fixation were central to this analysis. Narcissism was now to be thought of as a developmental stage, inherently connected with homosexuality and one in which erotically tinged social drives were created out of sublimations of the sexual drive. Fixation was now a failure to pass through a stage completely and, in the narcissistic stage, the failure produced a disposition toward a later frank resexualisation of these social drives. Paranoia was marked, Freud thought, by the patient's struggle to prevent these sublimations from being undone.

The subject of Freud's analysis was Daniel Paul Schreber, a Prosecuting Judge of a higher court district in Dresden who had developed a paranoid illness shortly after being promoted to that position. Schreber had come to believe that God had directly inspired him to redeem the world and restore mankind to a lost state of bliss. God had first to destroy the world, then transform Schreber into a woman and create a new race by breeding with him. Schreber seems to have believed that it was his transformation into a woman, or its possibility, that made him the object of persecution and sexual abuse by the staff of the hospital in which he was confined. After some years of hospitalisation, Schreber made a considerable recovery, becoming, apart from his delusions, normal in almost every respect, able to participate fully in everyday social and intellectual life and able to manage his own affairs. He then wrote an account of his illness and commenced what proved to be a completely successful legal campaign, in which he represented himself, for his discharge. After release from the hospital he found a publisher for his story. It was because of the minute detail with which Schreber had recorded his particularly florid combination of megalomania and paranoid persecutory delusions that Freud was able to use Schreber's *Memoirs of My Nervous Illness* (1903/1955) for his analysis.

Almost at the very beginning of his analysis of the Schreber *Memoirs* Freud focused on the relation between narcissism and homosexuality. Citing his own study of Leonardo and some clinically based conclusions of Sadger he argued that narcissism was a *stage* of development that began when the individual unified his separate:

> sexual instincts (which have hitherto been engaged in auto-erotic activities) in order to obtain a love-object; and he begins by taking himself, his own body, as his love-object, and only subsequently proceeds from this to the choice of some person other than himself as his object. (Freud, 1911a, pp.60-61)

Speculating that this newly described stage "may perhaps be indispensable normally", Freud claimed that many people lingered in it long enough for adult homosexuality to arise:

> What is of chief importance in the subject's self thus chosen ... may already be the genitals. The line of development then leads on to the choice of an external object with similar genitals - that is, to homosexual object-choice. (op. cit., p.61)

So placed between auto-erotism and object love, narcissism was the immediate developmental precursor of homosexual object-choice.

Social drives arose at this stage, Freud asserted, because, even after an heterosexual choice, homosexual tendencies were not done away with. In sublimated form they combined with the ego-instincts to form social drives like friendship, comradeship, *esprit de corps*, and a love of mankind in general. All these drives had a strong erotic component. Freud now drew on what he called the "opinion" he had expressed in the *Three Essays* that infantile fixations provided a basis for a decomposition of the sexual instinctual drive (Freud, 1905b, p.235), and asserted that those fixations created dispositional points around which later illnesses formed. A backward or regressive flow of libido to one of these points expressed itself in a form appropriate to the earlier fixation. Freud implicitly assumed that the social humiliations and slights about which paranoics complained were attempts to prevent a regressive resexualisation of their social drives:

> People who have not freed themselves completely from the stage of narcissism - who, that is to say, have at that point a fixation which may operate as a disposition to a later illness - are exposed to the danger that some unusually intense wave of libido, finding no other outlet, may lead to a sexualization of their social instincts and so undo the sublimations which they had achieved in the course of their development. (Freud, 1911a, p.62)

Here Freud drew on Abraham's interpretation of the related condition of schizophrenia, or dementia praecox as it was then called. K. Abraham (1908/1927a) had argued that when the schizophrenic withdrew from the world there was a loss of those feelings that arose from sublimations of the libido, including the "finer social sublimations" and the sublimations (*sic*)

producing "shame, disgust, moral feelings, pity, etc."

Having so used the fixation-disposition-regression notion to explain the paranoic's complaints, Freud drew a more general conclusion:

> Since our analyses show that paranoics endeavour to protect themselves against any such sexualization of their social instinctual cathexes, *we are driven to suppose* that the weak spot in their development is to be looked for somewhere between the stages of auto-erotism, narcissism and homosexuality, and that their disposition to illness ... must be located in that region. (op. cit., p.62. Original emphasis altered, MBM)

Here the connection between narcissism and homosexuality is attributed to their joint fixation in a normal development stage rather than, as in the case of Leonardo, because of the repression of a strong incestuous object choice.

Freud also used the hypothesis of a regression to a fixation-produced disposition to explain the magnitude and frankly sexual content of Schreber's delusions of persecution. Schreber's illness had been preceded by dreams that an earlier mental illness had returned. One morning, while partly awake, he had also had the feeling "that it really must be rather pleasant to be a woman succumbing to intercourse", an idea he said he would have rejected had he been fully awake (Schreber, 1903/1955, p.63). Freud described this idea or feeling as a dream and interpreted it as the revival of a homosexual wishful fantasy directed toward the psychiatrist who had successfully treated Schreber for the earlier, less serious illness (Freud, 1911a, pp.33, 59). Shortly after the idea or feeling had occurred to him, Schreber's wife went away for four days. During that time he had had "a quite unusual number of pollutions (perhaps half a dozen)" in the one night. These he blamed as the cause of the immediately ensuing breakdown (Schreber, 1903/1955, p.68). Freud interpreted this sequence of events to mean that "the basis of Schreber's illness was the outburst of a homosexual impulse" directed toward the psychiatrist (Freud, 1911a, p.45). Repression of the wish had been achieved through the withdrawal or detachment of libido from the psychiatrist-as-object. However, the wish had returned in the form of a delusion in which the wish had been projected on to the psychiatrist - it was not Schreber who had homosexual longings for the psychiatrist but the psychiatrist who desired Schreber.

Freud also drew upon Abraham's ideas to explain Schreber's megalomania or delusions of grandeur. Abraham had proposed that the schizophrenic's well-known lack of interest in people and the external world was due to a destruction of the capacity for object-love, to the patient's libido having turned away from animate and inanimate things:

> The mental patient transfers on to himself alone as his only sexual object the whole of the libido which the healthy person turns upon all living and inanimate objects in his environment, and accordingly his sexual over-estimation is directed to himself alone and assumes enormous dimensions. For he is his whole world. (K. Abraham, 1908/1927a. Cf. H. C. Abraham

and Freud, 1965, especially Letters of 26.7.07, Freud to K. Abraham, and of 9.8.07, K. Abraham to Freud).

Freud endorsed this idea completely: following Schreber's withdrawal of his libido from the psychiatrist, he withdrew it more generally, redirecting it on to his ego. Schreber was great because he was his whole world, Schreber was persecuted because others had homosexual desires for him.

Freud had been able to derive the main features of Schreber's illness - his megalomania, the sexualisation of his social drives, and the homosexual persecutory delusions - from the supposition that there had been a regression to a narcissistic stage located immediately prior to the stage of homosexual object-choice.

NARCISSISM, OMNIPOTENT THOUGHT, AND REALITY

More was involved in narcissism than just the choice of self as the object of the sexual instinctual drive. Once chosen, the functions of the self were decisively altered by their being sexualised. Freud believed the change could be shown through comparisons of the beliefs and practices of so-called primitive peoples with those of children and obsessional neurotics. From his comparison, in *Totem and Taboo* (1912-1913), Freud concluded that all three overvalued thought, that is, they all put more value on thinking than on reality. Freud paid particular attention to the magical practices of those who believed in animism - the view that all things in the world were alive, most often being animated by the spirits of departed relatives. Believers in animism used magic as a technique for gaining control over the spirits inhabiting their worlds (Freud, 1912-1913, p.78). Some magic seemed to be based on an association of ideas by similarity, as when peasants had intercourse in the fields to encourage their crops to grow. Others were based upon a real or imagined association by contiguity, as when the hair or nails of an intended victim were treated in a hostile manner. Magical acts like these showed "the domination of the association of ideas", that is, of thinking (op. cit., p.83), but Freud believed associations by themselves were insufficient to explain magic - magical acts had to be motivated by wishes and there had to be an "immense belief" in their power (ibid.). According to Freud, when children satisfied their wishes in an hallucinatory way, they shared this "belief" and he concluded that childish play and animistic magic were based on valuing thinking too highly, what he called an "excessive valuation" of thinking:

> an attitude towards the world ... which, in view of our knowledge of the relation between reality and thought, cannot fail to strike *us* as an overvaluation of the latter. Things become less important than ideas of things: whatever is done to the latter will inevitably also occur to the former. (op. cit., p.85)

Well before writing *Totem and Taboo*, Freud had observed a similar

overestimation of the power of thinking in one of his obsessional patients. On one occasion, not long after the patient had been irritated by something done to him by a complete stranger, the stranger had died. The patient described this power as showing the omnipotence of his wishes (Freud, 1909b, pp.226, 233). In *Totem and Taboo* itself Freud described this omnipotence as showing an "overvaluation of mental processes as compared with reality" (Freud, 1912-1913, p.87). To Freud the belief seemed peculiar to neither that patient nor to obsessional neurotics generally. All neurotics displayed omnipotence in that they granted a greater reality to thought than to experience.

Freud linked omnipotence to the modes of thought of the child and the animist via libido theory and narcissism. Narcissism was now described as a *sub-stage of auto-erotism* in which the unified sexual impulses cathected the subject's own ego that was itself now said to be "constituted at about this same time" (op. cit., pp.88-89). Omnipotence was due to the thought processes of the ego becoming sexualised:

> Primitive men and neurotics, as we have seen, attach a high valuation - in our eyes an *over*-valuation - to psychical acts. This attitude may plausibly be brought into relation with narcissism and regarded as an essential component of it. It may be said that in primitive men the process of thinking is still to a great extent sexualized. (op. cit., p.89)

A considerable part of this primitive attitude was said to have survived in the neurotic, where it was enhanced by repression and presumably involved some degree of regression. In both the neurotic and the so-called primitive:

> The psychological results must be the same ... whether the libidinal hypercathexis of thinking is an original one or has been produced by regression: intellectual narcissism and the omnipotence of thoughts. (op. cit., pp.89-90)

Freud then used his recapitulation notion to relate the development of man's view of the world to the libidinal development of the individual. Historically speaking, animism had given way to the religious view that had in turn been superseded by the scientific world view. In the child, narcissism corresponded to animism "chronologically and in its content", religion to the choice of parents as objects, and science to the stage:

> at which an individual has reached maturity, has renounced the pleasure principle, adjusted himself to reality and turned to the external world for the object of his desires. (op. cit., p.90)

The child's view of reality developed through the same stages as mankind's own world view.

Notice how Freud had subtly extended the conception of narcissism from that of a simple cathection of the ego by the libido, which merely turned the whole ego into a sexual object, to a more complex form where

the *functions* of the ego themselves were modified by being sexualised.

EGO-INSTINCTS AND REALITY

Freud regarded one of Schreber's symptoms as very characteristic of paranoia. His interpretation of it seemed to throw further light on the way in which the ego built up a picture of reality. The symptom was Schreber's belief that he was the sole survivor of catastrophic events which had brought the world to an end. He subsequently came to feel that people in the world about him were unreal - they were fleetingly improvised beings (or "miracled up, cursorily improvised" beings to use the translation of the *Standard Edition*). Again following Abraham, Freud explained:

> The patient has withdrawn from the people in his environment and from the external world generally the libidinal cathexis which he has hitherto directed on to them. Thus everything has become indifferent and irrelevant to him, and has to be explained by means of a secondary rationalization as being 'miracled up, cursorily improvised'. The end of the world is the projection of this internal catastrophe; his subjective world has come to an end since his withdrawal of his love from it. (Freud, 1911a, p.70)

Freud answered the question of how any contact with the objects of the paranoic's world was now possible by attributing that contact to the ego-instincts. Instead of libidinal cathexes maintaining the paranoic's picture of the external world, 'cathexes' of the ego-instinctual energy, what Freud termed 'interest', now had that role:

> It cannot be asserted that a paranoic ... withdraws his interest from the external world completely .... The paranoic perceives the external world and takes into account any alterations that may happen in it, and the effect it makes upon him stimulates him to invent explanatory theories (such as Schreber's 'cursorily improvised men'). (op. cit., p.75)

By so attributing to the paranoid ego the functions of maintaining relations with reality and building up some sort of picture of it, Freud seems also to have assumed that these functions were exercised by the normal ego.

Freud's correspondence with Jung shows that at the time he was completing the Schreber paper he was also working on another describing how the normal ego came to acquire its functions, especially those which enabled it to construct a realistic picture of the world. Eventually entitled *Formulations on two principles of mental functioning* (Freud, 1911b), this paper actually seems to have been originally conceived of as a part of the Schreber study. Freud wrote to Jung:

> My Schreber is finished, a short supplement or rather preface formulating the two principles, is being put in final shape today. (W. McGuire, 1974, Letter 225F of 18.12.10)

No such preface appeared. Whether the physical connection between the

Schreber analysis and the *Formulations* was as close as the letter suggests, there can be no doubt about the close conceptual relation between them.

The main proposition of the *Formulations* was that knowledge of reality was the function of a *reality-ego*. Freud began his argument by reviving the notions of the primary process and the model of need satisfaction originally outlined in the *The Interpretation of Dreams*. Unconscious mental processes were said to be "the older, primary processes, the residues of a phase of development in which they were the only kind of mental process" (Freud, 1911b, p.219). Governed by the pleasure principle, unconscious processes strove for pleasure and withdrew from unpleasure. [1] Internal needs interrupted the primal state of psychical rest but, eventually, there was disappointment with the hallucinated object of need satisfaction and the hallucinatory mode of thought was abandoned:

> Instead of it, the psychical apparatus had to decide to form a conception of the real circumstances in the external world and to endeavour to make a real alteration in them. (ibid)

What was then introduced into the mind was what was real, even if it happened to be disagreeable. No longer was the mind confined to pleasurable activities. What Freud termed the "reality principle" had come to govern mental life.

Only the ego-instinctual drives sought realistic modes of satisfaction from objects in the external world and it was only through them that consciousness, attention, memory, judgement, action, and thinking were produced (op. cit., pp.220-221). Consciousness, which came from the increased importance of external reality, was based on an appreciation of the differences between various kinds of sensory input or 'sensory qualities' and not merely on the distinction between unpleasure and pleasure. The function of attention developed from the search for realistic modes of need satisfaction. Memory resulted from laying down a record of the results of the periodic searches. Judgement replaced repression when decisions were made if a given idea was true or not, that is, whether it was in accord with the memory traces of reality. Mere motor discharge was converted in to action directed toward altering external reality. Thinking also delayed motor discharge until the memory of an appropriate action was found. Freud thought of this as a type of experimental action in which small amounts of cathexis were successively displaced onto the memory traces until the right ones were found. For this purpose the conversion of freely displaceable cathexes into 'bound' cathexes was necessary, and this was brought about by "the level of the whole cathectic process" being raised (op. cit., p.221). Freud also supposed that all thinking had originally

1. Freud introduced this term for the first time in the *Formulations*. Previously the principle had been known as the unpleasure principle. I have used the later term, pleasure principle, throughout.

been unconscious. Only when thinking involved the cathexis of word-representations was it perceptible to consciousness.

Sexual instinctual drives played no part in producing these ego-functions. Being auto-erotic and incapable of frustration they could not initiate development. Indeed, were there only sexual impulses, their auto-erotic aims would have prevented development altogether, or at least have retarded it until some other process, an internal change for example, forced the first steps toward object-choice.

Just as ego-functions resulted from the ego-instincts and could be attributed to a reality-ego, so the functions involved in maintaining the auto-erotic satisfactions of the sexual instincts were attributed to an earlier *pleasure-ego*. If the former was governed by the reality principle, the latter was under the sway of the pleasure principle. Hence the contrast:

> the pleasure-ego can do nothing but *wish*, work for a yield of pleasure, and avoid unpleasure, so the reality-ego need do nothing but strive for what is *useful*. (op. cit., p.223)

In the *Formulations*, Freud made a wholesale transfer to the two new 'egos' of the principles of mental functioning he had first described in *The Interpretation of Dreams*. The primary process of the system *Ucs.* now belonged to the pleasure-ego, while the secondary - from *Pcs.* and *Cs.* - were given to the reality ego. The new functions were executants of a process motivated by the ego-instinctual drives which resulted in a realistic world view so long as the drives maintained their independence of the sexual drives originally attached to them. Sexual influence was an ever present danger. At any time a 'weak spot' might give way and allow the libido access to the functions of the reality-ego. As it had done in Schreber's case, this influence of the re-emergent sexual drive could result in a fantastic world view dominated by the pleasure principle.

While it is possible to see the above extensions of Freud's theory as the result of clinical observations on narcissism, homosexuality, and the psychoses, they are better regarded as the not too remote consequences of his introducing the concept of a sexual instinctual drive and supplementing the organic theory of automatic repression. I have already argued that the concepts of ego and ego-instinctual drive were the first fruits of these two alterations. As soon as narcissism and the symptoms of psychoses were interpreted within a fixation-repression framework it was fairly easy to make the ego responsible for the sense of reality. Once the energy of the sexual drive had been withdrawn from the world and the subject's ego re-cathected, there was only the energy of the ego-instinct left with which contact could be made. Abnormalities resulting from regression to a previous fixation point implied that the world of the normal had also been constructed by an ego motivated by the energy of an ego-instinctual drive. The *Cs.*, *Pcs.*, and *Ucs.* components of the model of the mental apparatus previously described for normals and neurotics could be rearranged to form

the two new structures of the pleasure and reality-egos, and their primary and secondary processes redescribed as falling under the governance of the reality and pleasure principles.

REPRESSION, STANDARDS, AND CONSCIENCE

So far Freud's analysis provided only an outline of how the ego developed. He had said nothing about where the standards necessary for repression proper came from. It was also in *Totem and Taboo* (1912-1913) that Freud tried to account for them. His explanation was based on an analysis of the irrational taboos of so-called primitive peoples and the similarly baffling rituals of the obsessional neurotic.

Freud thought that taboos were based on a particular combination of love and hate, what he called "emotional ambivalence". In three of the major classes of taboo objects that he considered - those of one's dead relatives, one's dead enemies, or one's rulers - unconscious hostility was projected outwards:

> The hostility, of which the survivors know nothing ... is ejected from internal perception into the external world, and thus detached from them and pushed on to someone else. It is no longer true that they are rejoicing to be rid of the dead man; on the contrary, they are mourning for him; but, strange to say, *he* has turned into a wicked demon ready to gloat over their misfortunes and eager to kill them. It then becomes necessary for them, the survivors, to defend themselves against this evil enemy; they are relieved of pressure from within, but have only exchanged it for oppression from without. (Freud, 1912-1913, pp.62-63)

Ambivalence was essential to this projection-based belief that the souls of the dead were transformed into persecutory demons (op. cit., pp.64-65) and to the animistic view that the world was inhabited by "innumerable spiritual beings both benevolent and malignant" (op. cit., pp.75-76).

From the resemblances between taboos and conscience, Freud argued that ambivalence was also involved in conscience. Conscience kept one of two opposing feelings unconscious, repressing it by a compulsive dominance. From that standpoint:

> it seems probable that conscience too arose, on a basis of emotional ambivalence, from quite specific human relations to which this ambivalence was attached. (op. cit., p.68)

Freud now had to show how conscience was linked to the standards of behaviour involved in repression. He tried to do this some five years later, when some of his notions about the ego had crystallised. He then argued there was a direct relation between the standards and repression:

> Repression, we have said, proceeds from the ego; we might say with greater precision that it proceeds from the self-respect of the ego. The

> same impressions, experiences, impulses and desires that one man indulges ... will be rejected with the utmost indignation by another .... The difference ... can easily be expressed in terms which enable it to be explained by the libido theory. We can say that the one man has set up an *ideal* in himself by which he measures his actual ego, while the other has formed no such ideal. For the ego the formation of an ideal would be the conditioning factor of repression. (Freud, 1914b, pp.93-94)

By adulthood it was the ego-ideal rather than the ego that acquired the cathexes of the ego-libido, and it was this sexualised or cathected ego-ideal, rather than the ego itself, which provided the standards for initiating repression.

As Freud pictured it, the concept of the ego-ideal brought with it a new ego-function, one which scrutinised the actual ego, measuring it against the standards of the ego-ideal, and by that means ensuring that narcissistic satisfaction was maintained (op. cit., p.95). Freud believed that it was through the conscience that this new ego-function was exercised. Through it the paranoic's delusions of being watched or noticed could also be understood. Freud said that many paranoid patients complained that:

> all their thoughts are known and their actions watched and supervised; they are informed of the functioning of this agency by voices which characteristically speak to them in the third person ('Now she's thinking of that again', 'now he's going out'). This complaint is justified; it describes the truth. A power of this kind, watching, discovering and criticizing all our intentions, does really exist. Indeed, it exists in every one of us in normal life. (ibid.)

Interpreting the voices as regressive manifestations allowed Freud to attribute the genesis of the ego-ideal to "the critical influence" of parents and other authority figures (op. cit., p.96). Conscience, the watchman of the ego-ideal, was "at bottom" also an embodiment of parental and social criticism.

Freud had previously said that the emotional ambivalence at the basis of the individual's view of reality was generated by criticism from parents and others whom the individual loved. Now he was holding ambivalence responsible for the formation and scrutiny of the social standards that governed the relations between individuals and their social world.

NARCISSISM, LOVE, AND HATE

Notwithstanding the detail with which Freud had explored Schreber's world, or the potentially universal sphere of action that the two principles of mental functioning opened up, and the role given the ego-instinctual drive, he had not provided much more than an outline of the way in which the development from narcissism occurred. Only in *Instincts and their vicissitudes* (Freud, 1915a) was Freud was able to integrate his ideas into a

more or less comprehensive developmental theory.

Freud began his developmental account by arguing that the organism's capacity for distinguishing between a stimulus and an instinctual drive provided it with a means of differentiating the external world from the internal. Stimuli arose from the external world and tended to have only a momentary impact on the organism. On the other hand, instinctual drives impinged on the organism from within and tended to act as constantly applied forces. As a consequence, the organism could respond to or dispose of stimuli by a single motor response - typically one of flight, or at least one that removed the organism from their presence. Instinctual drives had to be responded to quite differently: removal or flight was impossible, only the provision of a satisfaction that altered the internal source of the drive could end its operation. Even a helpless living organism was therefore able to distinguish external from internal: 'external' could be ascribed to those stimuli that motor responses would bring to an end and 'internal' to those where such action was of no avail (Freud, 1915a, pp.118-119). This "sound objective criterion" enabled what Freud now rather confusingly called "the original 'reality-ego'" to distinguish between its internal and external environments (op. cit., p.136). The birth of the ego and its reality sensing function derived from the differences in the responses to instinctual drives and to stimuli.

After so setting out the differences between stimuli and instincts, Freud drastically modified his concepts of narcissism and auto-erotism:

> Originally, at the very beginning of mental life, the ego is cathected with instincts and is to some extent capable of satisfying them on itself. We call this condition 'narcissism' and this way of obtaining satisfaction 'auto-erotic'. (op. cit., p.134)

Narcissism was no longer a type of object-choice taking place *after* auto-erotism nor was it a subdivision of the auto-erotic stage. Narcissism was now a primal state present from the beginning of mental life and auto-erotism was the mode of instinctual satisfaction practiced in it.

The new definition of narcissism heightened the problem of how development from such a primal state of bliss could occur. For this purpose Freud made use of "the most important" of his postulates - the principle of constancy - now re-formulated as follows:

> the nervous system is an apparatus which has the function of getting rid of the stimuli that reach it, or of reducing them to the lowest possible level; or which, if it were feasible, would maintain itself in an altogether unstimulated condition. (op. cit., p.120)

In a further return to the basic ideas of the *Project*, Freud argued that it was consistent with the principle that simple movements "mastered" stimuli by withdrawing the organism from them. Instinctual pressure, on the other hand, forced the nervous system into "involved and interconnected" activ-

ities that so changed the external world that objects of satisfaction could be wrested from it. Instincts were the "true motive forces" behind the development of the nervous system and the picture of reality that its activities built up. The pleasure principle was also involved:

> the activity of even the most highly developed mental apparatus is subject to the pleasure principle, i.e. is automatically regulated by feelings belonging to the pleasure-unpleasure series, [so that] we can hardly reject the further hypothesis that these feelings reflect the manner in which the process of mastering stimuli takes place. (ibid.)

Adding to the two principles the "working hypothesis" that the ego and sexual instincts were the only kinds of drive allowed Freud to generate an explanation for narcissism being abandoned. Ego-instinctual drives were necessarily directed to the outer world of objects. Needs like those of hunger and thirst had to be satisfied by external objects if the ego was to continue to glow with sexual libidinal light. Self-preservation drives forced the bliss of primal narcissism to be given up.

The end of narcissism was not the only thing brought about by the differences in the modes of satisfaction of the two classes of instinctual drive: from it Freud derived the development of the ego and the opposition that he needed between the ego and sexual instincts if repression was to take place. During the narcissistic phase Freud supposed the external world to be indifferent for the purposes of satisfaction and not to be cathected with the interest of the ego-instinctual drives. The ego itself, the ego-subject, corresponded to what was indifferent or even unpleasurable. The ego necessarily acquired objects from the external world and, in so far as those objects were sources of pleasure, they were incorporated into it, or to use Ferenczi's (1909/1952a) term "introjected". On the other hand, anything within the ego that produced unpleasure, such as an unsatisfied instinctual drive, was expelled and projected on to the external world (Freud, 1915a, pp.135-136). Through introjection and projection the original reality-ego was purified and transmuted into a pleasure-ego that divided the external world and itself into two parts:

> a part that is pleasurable, which it has incorporated into itself, and a remainder which is extraneous to it. It has separated off a part of its own self, which it projects into the external world and feels as hostile. (op. cit., p.136)

After this transmutation the ego as subject corresponded with pleasure and the external world with unpleasure.

A further consequence of the appearance of the object was the development of the emotion of hate. According to Freud:

> hating, too, originally characterized the relation of the ego to the alien external world with the stimuli it introduces .... At the very beginning ... the external world, objects, and what is hated are identical. (ibid.)

Objects that satisfied needs were loved and incorporated into the ego. After incorporation, any remaining objects "coincide with what is extraneous and hated" (ibid.). Hatred for that which caused unpleasure came to be associated with the ego-instinctual drives and love of that which gave pleasure became linked with the sexual drive. As the ego developed out of the narcissistic stage, pleasure and unpleasure began to be reflected in the ego's relations to its objects. An object that was a source of pleasurable feelings would generate a motor urge to bring it close to the ego and incorporate it. Such an object was loved. On the other hand, if the object caused unpleasure, the motor response would repeat the original attempt at avoidance by increasing the distance between the ego and the object. Such an object was hated. The hatred might become intense enough to initiate an aggressive action designed to destroy the object. Love and hate thus reflected the relations of the whole ego to its objects.

Freud noted that in ordinary language it was not usual to speak of a love for the objects of the ego or self-preservative instinctual drives; rather one spoke of 'needing' them. The word 'love' tended to be restricted to sexual objects in the narrower sense, being used properly only after the component drives had been unified and directed toward reproductive ends. The use of the word 'hate' showed:

> no such intimate connection with sexual pleasure and the sexual function .... The relation of *unpleasure* seems to be the sole decisive one. The ego hates, abhors and pursues with intent to destroy all objects which are a source of unpleasurable feeling for it, without taking into account whether they mean a frustration of sexual satisfaction or [a frustration] of the satisfaction of self-preservative needs. (op. cit., p.138)

Freud had thus derived hate from the ego's struggle to maintain and preserve itself independent of the external world. Because of its origins, hatred was always to be found in most intimate relation with the self-preservation drives; love, of course, remained in intimate connection with sexual pleasure. Freud therefore proposed that the antithesis between love and hate repeated itself in the opposition of the ego to sexuality. Ego came into conflict with sexuality, eventually to repudiate sexuality completely.

To summarise: the object orientation of the self-preservation drives (1) was behind the movement away from primal narcissism, (2) forced the ego to make finer differentiations than those between internal and external, thereby creating a reality oriented ego, (3) developed the latent opposition between ego and sexuality necessary for repression, and (4) created the hatred necessary for the feeling of ambivalence that contributed to both the animistic world view and the function of conscience.

THE CONFLICT BETWEEN THE EGO AND SEXUALITY

The proposition of Freud's most central to the theorising I have considered in this chapter is that there is a basic conflict between the ego-instinctual and sexual instinctual drives. Evaluating this proposition is complex because detours must first be made to evaluate four independent but related matters. The first detour relates to the concept of ego-instinctual drive itself: how adequate is Freud's concept and of what value is his picture of the relation between the drive, the ego, and the various functions assigned it? Second is the serviceability of Freud's explanation of homosexuality. The relevance, of course, is because it is partly with respect to inversion that the concept of narcissism was first introduced into the theory of personality development. The third of the preliminaries needing scrutiny is the thesis that dispositional points are caused by infantile fixations and that subsequent regression to them is the source of apparently new psychological characteristics. Lastly there is the problem of how primal narcissism is abandoned, for it was from that process that the conflict between ego and sexuality was supposed to have developed.

Detour 1

Ego-instinctual drives share the four characteristics distinguished by Freud in *Instincts and their vicissitudes* (1915a): pressure (Drang), source (Quelle), aim (Ziel), and object (Objekt). That these attributes are possessed by only a small number of the component sexual drives was evident in the last chapter. They apply to an even smaller number of the ego-instinctual drives. For example, consider pressure, which Freud took to mean the amount of force or the measure of the demand for work expressed through action that the drive represented. "The characteristic of exercising pressure is common to all instincts; it is in fact their very essence" (op. cit., p.122). But, how meaningful is it to speak of the pressure of the ego-instinctual drives for mastery, for example? Similarly, Freud took the source of an instinctual drive to be "the somatic process" whose stimuli were represented in the mind as a drive. Now, while it may just possible to comprehend the sources of the nutritional drives in this way, it is much less easy to see what meaning attaches to the notion of a somatic process as the source of non-biological ego-instinctual drives. Loewenstein (1940) long ago remarked that Freud's:

> arrangement of the characteristics of instinct fits in very well with the sexual instincts and with those of hunger and thirst. But as far as I know no one has ever tried to apply it to the other manifestations of instinctual life, such as those, for instance, which spring from the instinct of self-preservation.

Neither has the almost fifty year period since Loewenstein wrote seen any such application. In one of the classical statements of psycho-analytic instinct theory, E. Bibring inadvertently drew attention to the reason:

> The criterion of source which had been used for the classification of the sexual instincts was carried over to the ego instincts, of which the nutritional instincts served as the typical example. They too could be linked with organs of origin and termination, once more with the help of hypothetical chemico-physiological processes. (E. Bibring, 1936/1941)

But, for almost all the ego drives other than hunger or thirst, it is as inappropriate to speak of sources as it is of organs of origin and termination, and quite bizarre to think of them as having a chemico-physiological basis like that of the sexual drive.

Freud's conceptualisation of instinctual drives is so tied to the hunger-sexual model that extension to other drives is impossible. Although other reasons are usually given, it is not difficult to see that it is precisely this 'organic' conceptualisation of all instinctual drives which is responsible for the well-known difficulty that psycho-analytic theory has in understanding the ego-instinctual drives. Freud himself admitted that little could be said positively or definitely about the ego-instinctual drives, but attributed this to the relative ease with which sexual drives were observable in the psychoneuroses as compared with the difficulty of observing ego drives in the paranoid and schizophrenic disorders (Freud, 1915a, pp.124-125). However, if the causes of what we might call 'ego-behaviour' are different in their very nature from those satisfying sexual and hunger needs, no amount of psycho-analytic observation of neurotics or psychotics could ever throw any light on the ego-drives.

Freud did argue that a knowledge of instinctual sources was "not invariably necessary" in psychology because sources could sometimes be *inferred from aims* (op. cit., p.123). His argument must be regarded with considerable caution if not rejected completely. By definition, behaviour always involves a motor factor, or a demand for work, and the individual often seems to feel a quality of pressure in behaviour. Further, much behaviour can be described as directed toward a goal, terminating once that goal is achieved. Almost all behaviour therefore possesses three of the four attributes that Freud used to characterise behaviour motivated by instinctual drives. The *only* feature distinguishing the latter from behaviour in general lies in it having a source in an internal somatic process. Inferring sources from aims therefore effectively reduces the number of defining characteristics to three and blurs the distinction between instinctual and non-instinctual behaviour. Indeed, if it is true, as E. Bibring (1936/1941) has claimed, that "the idea of source was the most important" of the criteria for classifying instinctual drives, it is improper, logically speaking, to infer the source from the aim.

Freud also seems to have believed that the behaviours were motivated by instinctual drives because they led to the same pattern of satisfaction. For example, in the *Three Essays*, childhood exhibitionism was regarded as motivated by a sexual drive, partly because small children showed "an

unmistakable satisfaction'' in exhibiting themselves naked. Naturally, only if this satisfaction is identical with that of the satisfaction of an instinctual drive, and no other, can the similarity be taken as unequivocal evidence for instinctual drive motivation. Self-observation readily reveals many obviously non-organically based needs associated with increased levels of tension from which pleasurable satisfaction in tension relief is obtained. Ranging from the drive to catch a bus to the gambler's compulsive placement of his bet, these needs also possess attributes which Freud regarded as instinctual. They therefore reduce the evidential value of the resemblance.

Several commentators have remarked on a peculiar vagueness in Freud's use of the concept of ego-instinctual drive. E. Bibring (1936/1941) drew attention to Freud's virtual equating of the constructs of ego and ego-instincts: ''the ego instincts stood for an ego.'' H. Hartmann (1948) noted that important components of the ego-instinctual drive, such as the tendency to master the environment ''which show a definite relation to self-preservation had a rather indefinite position in the system''. Novey (1957) went so far as to claim that Freud was ''plagued by the need to assign some substantial rôle to the ego instincts'' and that he failed to do so. Sjoback (1973, p.14) observed that Freud's discussions of ego-instinctual drives ''are not very detailed and hardly give us a more tangible picture of the dynamics of conflict than the one to be found in his earlier writings''. More recently, Compton (1981b) drew attention to a set of problems created by Freud's failure to define either ego or ego-instinctual drive. An ambiguity inherent in the very conceptualisation of instinctual drive is responsible for this incorporeality. It leads in turn to an inability to specify the relation between ego-drives and ego-functions. All of these critical considerations apply, at least in part, to the recent attempts to revive the concept of an ego-instinctual drive (Khantzian and Mack, 1983), including even those attempts, like Plaut's (1984), based on the assertion that the drive is characterised by a source, an aim, and an object.

The ambiguity of the concept of instinctual drive is discussed in the Editorial Note to *Instincts and their vicissitudes* (Freud, 1915a, pp.111-116), where it is pointed out that the inconsistencies in Freud's usage make it difficult to arrive at a single definition. The Editors differentiate between two contradictory meanings of the term, meanings that I shall term the *representational* and the *cathectional*. The representational meaning is found in most of the pre-1915 definitions and takes an instinctual drive to be ''the psychical representative of somatic forces'' (op. cit., p.112). Here an instinctual drive is equated with a mental representation, which presumably is an idea of a physiological process. The second meaning, the one that predominates after 1915, distinguishes sharply between the drive and the representative. The drive is a non-psychical something, presumably a bodily process, that supplies energy to or cathects an instinctual representative (op. cit., p.113). Our first difficulty is not knowing, even vaguely, what Freud might have meant by an idea being the mental representative of a drive. It cannot be the case that the ideas are as distinct as those of, say,

the objects of the drive or that they are the conscious sensations Applegarth (1971) takes them to be. It seems to me that the hunger drive, for example, is probably to be thought of as represented by those ideas and sensations that cause us to say we are hungry, rather than by an idea as definite as the food that would satisfy it. But one cannot be sure.

Our second problem is that neither meaning is consistent with the *use* to which the concept is put in the works considered in this Chapter, where an instinctual drive is said to provide energy to, or activate those ideas "in keeping with its aims" (Freud, 1910a). An instinctual drive that *is* a psychical representative cannot cathect anything for, plainly, one idea cannot cathect another. Cathection for Freud is always *of* an idea by energy, or more correctly of the neural traces of the idea. Activation of ego-functions and the cathection of the ego requires a supply of energy rather than a mere connection with the inert traces of other ideas. On the other hand, the cathectional definition clearly requires the instinctual representative to be *permanently* cathected (e.g. Freud, 1915b, p.148), a requirement that makes it impossible to see how the drive can remain the same drive while it cathects other ideas. For example, if the sexual instinctual drive is the process that supplies energy to the ideas representing it, how can other, non-sexual ideas be cathected by it and the drive be still considered sexual? Although not without their own difficulties, the concepts of libido in the second of Freud's explanations of anxiety neurosis and of the quota of affect in the *Studies on Hysteria*, posit a psychological energy having an endogenous source which is linked to or associated with a particular idea or groups of ideas that may also be diverted to other ideas. This implicit definition is consistent with the ways in which Freud *uses* the concepts of cathexes and instinctual drives before and after 1915, although it contradicts both of the formal definitions.

It is this very ambiguity which is reflected in Freud's failure to specify the relation between ego-drives and ego-functions. From his view of drive it is, at best, possible to imagine only how functions that are groups of ideas, such as the ego-ideal or the conscience, might be enlivened by an instinctual drive. Functions like attention or the laying down of memory traces are not exercised by groups of ideas and, however well they might be described in terms of energy, it is impossible to see how instinctual energy cathects the executive part of such functions. For example, fluctuations in attention can be plausibly described as reflecting fluctuations in the quantities of energy bestowed on the ideas being attended to, but it is not at all easy to see how the executive or controlling mechanism acquires libidinal energy or applies cathexes to the ideas. Even more acute is the problem of functions like consciousness or judgement: Freud simply does not provide enough information about them to begin imagining how they are related to energy. Although some of Freud's descriptions of ego-functions in terms of cathexes have a certain plausibility, there is a decided vagueness in his specification of the relation between the ideas constituting the ego-functions and the energy said to cathect them.

Because the ego-instinctual drive was first evoked to explain repression, it is ironical that the hiatus between ego-functions and ego-drive is to be seen so clearly in that mechanism. Two aspects of the point may be distinguished: first, the source of the standards from which repression proceeds and, second, the nature of the force responsible in the three stage process of repression, originally described in the Schreber analysis. Let us accept that there is some kind of opposition between the sexual and ego-drives and that the former is held in check by the latter. How do activities motivated by ego-instinctual drives lead to standards of behaviour being established? Freud is silent on the matter. Parental and other criticisms are involved, but what have they to do with ego-drives?

As to the second point, the newly described process of repression virtually makes the ego unnecessary. Freud had proposed that repression be divided into the three phases of fixation, repression proper or after-pressure, and the return of the repressed (Freud, 1911a, pp.67-68). Fixation first created a "libidinal current" that behaved like one belonging to *Ucs.*, that is, like one that had been repressed and which attracted other trends to it. In the second phase ('repression proper') "the more highly developed systems of the ego" forced the psychical derivatives of the fixated libidinal impulse and other trends that came into conflict with the ego out of consciousness. The third phase, 'the return of the repressed', occurred when impulses deriving from fixation and repression proper manifested themselves in consciousness as symptoms. Now, Freud does not describe the mechanism of fixation or primal repression at all. One is left to assume that it either results from the heightened pleasure of some component activity, because only that mechanism, mentioned in passing in the *Three Essays*, had ever been suggested, or that it is the automatic organic mechanism in another guise. Either way, no reference need be made to ego or ego-drive. Further, ideas fixated under primal repression were kept out of consciousness by a *Pcs.* counter-force or anticathexis. Freud speculated:

> It is very possible that it is precisely the cathexis which is withdrawn from the idea that is used for anti-cathexis. (Freud, 1915c, p.181)

Nunberg (1932/1955, pp.243-245, 251, 281), H. Hartmann (1950), Parkin (1983), and Plaut (1984) *seem* to accept that Freud means the source of *Pcs.* anticathexes is the libidinal cathexis of the repressed idea itself. My doubt is whether some of these judgements are not based on a retrospective view through spectacles provided by Freud's later theory in which ego-instinctual drives had a different place (Cf.Applegarth, 1971). However, if anticathexes are formed from the libidinal energy detached from the repressed idea there is no need to postulate a repressive force based on another form of energy. The conclusion follows that the ego-instinctual drive does not explain how standards form and that, even if it did, neither primal repression nor repression proper need draw upon its energy.

The first detour is now complete. Summarising: the concept of an

ego-instinctual drive is inadequate in itself and the relation between the drive and the structures and functions that it services is too vaguely stated to have genuine explanatory value.

Detour 2

Turning to the second of the preliminaries - homosexuality and narcissism - it must be noted that even before the 1910 footnote to the *Three Essays*, Freud had advanced an explanation of homosexuality having very little to do with narcissism. Set out in *On the sexual theories of children* (Freud, 1908d) and in the *Analysis of a phobia in a five year old boy* (Freud, 1909a), this pre-narcissism theory proposed that in childhood the future male homosexual chose a woman as object and held to that choice:

> so long as he assumes that they too possess what in his eyes is an indispensable part of the body; when he becomes convinced that women have deceived him in this particular, they cease to be acceptable to him as a sexual object. He cannot forgo a penis in any one who is to attract him to sexual intercourse; and if circumstances are favourable he will fix his libido upon the 'woman with a penis', a youth of feminine appearance. Homosexuals, then, are persons who, owing to the erotogenic importance of their own genitals, cannot do without a similar feature in their sexual object. (Freud, 1909a, p.109. Cf. Freud, 1908d, p.216)

Freud added that the homosexual remained fixated at a point between auto-erotism and object-love but, unlike the equivocality of the reference to the overvaluation of the genitals, that proposition had nothing to do with the later concept of narcissism. 'Between auto-erotism and object-love' meant a stage before *normal* object-love, not that there had been no object-choice at all. I shall call this explanation of homosexuality the disappointment-substitution hypothesis. Freud summarily indicated the evidence for it:

> As my expectations led me to suppose, and as Sadger's observations have shown, all such people pass through an amphigenic phase in childhood. (op. cit., p.109, n.1)

If by 'amphigenic' is meant 'amphigonic', all that Sadger could have shown is that the later invert passed through a phase of bi-sexuality - a rather more general point than the particularity of the disappointment-substitution hypothesis - and but little advance on the vagueness of Freud's conclusion in the *Three Essays* that a "a bisexual disposition is somehow concerned in inversion" (Freud, 1905b, pp.143-144).

Freud's second explanation of male homosexuality was the first to involve some concept of narcissism and was set out in the *Three Essays* and the *Leonardo* study. According to it, homosexuality was based on the repression of an overly strong incestuous desire for the mother, a quite different basis from disappointment with her. In fact, according to this second explanation, the child was so little disappointed that he identified with her after the repression, then selecting as his objects those young men

whom he could love as his mother had once loved him. It is at this point that Freud brought narcissism into the explanation. Homosexuals were said to "identify themselves with a woman and take *themselves* as their sexual object" and their new object choices to "proceed from a narcissistic basis" (Freud, 1905b, p.144, n.1. Cf. 1910b, pp.99-100). As in the perversion that Nacke had described, Freud here used the term narcissism as a synonym for self-loving behaviour and hardly as a concept at all. This second explanation I shall term the repression-identification hypothesis, has only in common with the disappointment-substitution hypothesis the choice of object being made well past the auto-erotic stage. However, it was again the analyses of "a small number" of homosexuals by Sadger supplemented, it is true, by some conducted by Freud himself (1910b, p.99 and n.1. Cf. 1905b, p.144, n.1 of 1910) which provided the evidence.

Not until the analysis of the Schreber *Memoirs*, when a third and very different explanation of homosexuality was put forward, did Freud transform narcissism from a method by which an object was chosen to a stage in a developmental process. According to the Schreber analysis, homosexuality resulted from the individual's failure to abandon sufficiently quickly his own body, especially his genitals, as a libidinal object. Later in life his choice was therefore of a person with genitals similar to his own. The fixation was supposed to be at a stage prior to the choice of *any* external object, when the auto-erotic components, groping toward unified expression, selected the individual himself as a love object. I call this third explanation the narcissistic-fixation hypothesis (Freud, 1911a, pp.60-61).

Now, although the differences between this third explanation and the two others are obvious the main evidence for it is exactly the same - Freud's and Sadger's analyses (Cf. Freud, 1905b, p.135, n.1; 1910b, p.99, n.1; 1911a, p.60, n.1). No new clinical observations could have led to the changes in the hypotheses. Freud's use of the same evidence to support three quite different explanations shows his theorising to be based on a rather more flexible conception of the relation between observations and conclusions than is usually the case in scientific enquiry. For this reason alone, judgement must be suspended on the adequacy of Freud's derivation of inversion from a narcissistic fixation and also, as a consequence, on his proposition that development begins from an original narcissistic bliss.

Detour 3

As we depart on the third of the detours, we note that it was in the Schreber analysis that Freud made the first application of the supposition that fixations caused dispositions to illness which manifested themselves in later regressions. The *Three Essays* contains the earliest extensive use of the notion of fixation and, while two fairly distinct meanings of that term may be discerned, *neither* is related to the concept of regression. One meaning, broadly speaking, relates to the aim of the sexual impulse and the other to the effect of the early choice of object. In both instances, 'fixation'

is used to explain how aspects of childhood mental life come to be carried over into adulthood: aims fixated in childhood appeared in the adult as perverse aims and objects fixated in childhood appeared in the neurotic as repressed incestuous object choices. In contrast, the term regression is not only not used at all in the *Three Essays* there is only one rather special circumstances in which it is even implied:

> In cases in which someone who has previously been healthy falls ill *after an unhappy experience in love* it is also possible to show with certainty that the mechanism of his illness consists in a turning-back of his libido on to those whom he preferred in his infancy. (Freud, 1905b, p.228. My emphasis, MBM)

Freud did not posit a closer relation between fixation and regression until the *Five Lectures on Psycho-Analysis* he delivered in 1909 (Freud, 1910a). It is there that Freud outlines the thesis that fixations create dispositions to later illness in the form of points to which the libido can later regress.

There is no doubt that the consideration given by Freud and others to dementia praecox and paranoia was the source of the new view of the relation between fixations, dispositional points, and regression. Among the ideas that Freud passed on to Abraham in mid-1907 was the possibility that some paranoics had "only inadequately completed the path from auto-erotism to object love", a developmental failure that created a "predisposition" and a point to which regression later took place. Despite Freud's contrary assertion, Abraham did not develop the suggestion. It seems to have been Freud himself who developed it in his study of the Schreber *Memoirs* (H. C. Abraham and Freud, 1965, Letter of 9.8.07, K. Abraham to Freud. Cf. K. Abraham, 1908/1927a, Freud 1911a, p.41, n.1). At the end of September 1910, three months before the Schreber study was finished, Freud mentioned to Jung that in that work he had been able to take "quite a step forward in explaining the mechanism of the choice of neurosis" (W. McGuire, 1974, Letter 212F of 24.9.1910). As we known from Freud's later work, that choice is determined by precisely by fixation, disposition, and regression.

In the Schreber analysis Freud developed the fixation-disposition-regression hypothesis by first citing his and Sadger's work in support of a proposed narcissistic stage between auto-erotism and object love (Freud, 1911a, pp.59-62). He then asserted that fixation during the narcissistic stage caused the sublimated homosexual feelings that contributed to the social drives. He then advanced his "opinion" from the *Three Essays* that fixations produced dispositional points. Fixation in the narcissistic stage was then said to create a disposition that led to a re-sexualisation of the social drives should regression occur. Freud then claimed that psycho-analyses showed that paranoics struggled against this re-sexualisation. He then said he was "driven to suppose" (op. cit., p.62) that there had been a fixation somewhere between the stages of auto-erotism, narcissism, and homosexuality. Freud next asserted that his evidence and his proposition

had "shown" that fixations caused dispositions to subsequent illness (op. cit., p.67), a conclusion then used to explain the delusions, the megalomania, and the difference between paranoia and dementia praecox. For example, the attachment of libido on to the ego in megalomania was made possible by a previous fixation of the same kind in the earlier stage of narcissism (op. cit., p.72). Freud said that dementia praecox showed a more marked withdrawal from the external world than did paranoia, a difference now attributable to a more complete regression of libido. Freud therefore located the fixation point in dementia praecox in the stage of auto-erotism, further back than in paranoia (op.cit., pp.76-77).

Neither the observational nor logical bases of Freud's arguments stand scrutiny. The psycho-analyses by Sadger and himself were originally cited to support quite different views of narcissism and nowhere in the *Three Essays* was fixation described as producing a disposition around which symptoms might later form. Although Freud had argued that persecutory delusions were based on a repressed homosexual wish, he put forward no such argument for the sexual interpretation of the slights and humiliations about which the paranoic complained.

Freud's estimation of the role of homosexual impulses has also been disputed. Struck by the similarity between Schreber's delusional symptoms and the very real peculiarities of his upbringing, a number of psycho-analysts seem almost to suggest that there was no disorder at all (Niederland, 1951, 1959a, 1959b, 1960, 1974; Schatzman, 1971, 1973; Breger, 1978; Klein and Tribich, 1982a, 1982b; Lyons, 1982). Many others, according to Frosch (1981), have put constructions on unconscious homosexuality so significantly different from Freud's that there has almost been a "denigration" of his thesis. Of those Frosch cites, most deny or ignore the role of homosexual tendencies altogether except to refer to them as "pseudo manifestations" or "pseudo phenomena". An author not discussed by Frosch goes even further by having homosexuality as a defence against paranoia! (Juni, 1979). Frosch (1981) also notes a number of attempts to place the fixation point elsewhere. Of the more radical, the first to pay any attention to Schreber's relation to his mother, is R. B. White's (1961) interpretation of the disorder as an oral one in which the "unmanning" is a regressive attachment to the mother rather than castration.

Strong objections are made to the even more basic notions of regression and detachment of libido. Pao (1977) does away with fixation and regression altogether, asserting that "it does not explain the data" (Cf. Ogden, 1980). Freud's very basic propositions about the schizophrenic's withdrawal of libido have also been abandoned. Freeman, T. (1977) began his paper by saying "for many years now there has been a continuing and sustained criticism" of Freud's hypothesis that the basic disorder in schizophrenia is a decathexis of object representations. He says that there are patients with delusional ideas who maintain their object cathexes and others, the persecutory group, in whom there is a decathexis followed by a

return to the love object. Katan (1979) seems to agree. Here modern psycho-analysts prefer Jung over Freud. Within a few months of the publication of Freud's study of Schreber, Jung wrote to Freud that the loss of the reality function "cannot be reduced to repression [i.e. detachment, MBM] of libido". He said he had had difficulties "throughout the years" in attempting to apply this aspect of libido theory to schizophrenia (W. McGuire, 1974, Letter 287J of 11.12.1911). (As became evident later, this theoretical difference was one of the main reasons for the break between Freud and Jung).

Within the set of Freud's propositions it is as difficult to distinguish premises from conclusions as it is facts from assertions. What it was that 'drove' Freud to suppose that there had been a fixation between auto-erotism, narcissism, and homosexuality was not the force of a logical argument. Rather it was that that supposition made it possible to relate the mental content and psychological processes evident in Schreber's symptoms to a supposed earlier period in which the same processes and content could be supposed to have been present. Only that supposition explained to Freud the narcissistic processes and the content of the symptoms. And, being a supposition, Freud's claim that by being "driven" to it, he had "shown" that fixations created a disposition to a subsequent illness is hardly justified. Nor had he established that conclusion in such a way that it could be used for explaining megalomania or the difference between dementia praecox and paranoia. Freud's supposition is best thought of as an extension to the psychoses of the expectation that at the core of the neuroses there had to be a sensory content similar to that in the symptom.

Detour 4

Our fourth detour is made necessary by a certain obscurity in the way in which the relations between auto-erotism, narcissism, and the ego are conceived. The question at issue is whether all three co-exist from the first or whether there is some kind of developmental sequence linking them. When first discussing auto-erotism and narcissism Freud said:

> we are bound to suppose that a unity comparable to the ego cannot exist in the individual from the start; the ego has to be developed. The auto-erotic instincts ... are there from the very first; so there must be something added to auto-erotism - a new psychical action - in order to bring about narcissism. (Freud, 1914b, pp.76-77)

The new psychical action, or 'operation in the mind', as it has been elsewhere translated, is the development of the ego (Novey, 1957; Laplanche and Pontalis, 1967/1973, pp.255-257). Consequently, Freud's first view was that auto-erotism existed before either the ego or narcissism. However, according to a slightly later description:

> Originally, at the very beginning of mental life, the ego is cathected with instincts and is to some extent capable of satisfying them on itself. We call

> this condition 'narcissism' and this way of obtaining satisfaction 'auto-erotic'. (Freud, 1915a, pp.134)

Here Freud seems to say that ego and narcissism are present from the start.

The question of whether or not the ego is present from the beginning of mental life depends on the meanings of the terms 'ego' and 'mental life'. If the instinctual drives that motivate auto-erotic activities are 'mental' representations of somatic forces, it follows that mental life of some kind must commence as soon as the somatic sources begin the production of endogenous excitation. And, because the sexual instinctual drive so brought into being is without object, auto-erotic activity must also start then. Thus, only if the mental life provided by the mere presence of mental representatives is equated with ego-activity can there be auto-erotic cathection of an ego and also, therefore, the presence of a narcissistic state "from the very first". On the other hand, if narcissism involves some concept of self, however primitive, mental life must take time to develop and cannot be present "at the very beginning".

After examining Freud's differing formulations of the concepts of auto-erotism and narcissism, Kanzer (1964) concluded that Freud really meant auto-erotism to be "a mode of pleasure to be obtained from one's own body, more specifically the erogenous zones, at any time of life" and that narcissism was an "ego-organization that functions from the beginning of mental life". He endorsed the view that auto-erotism, ego, and narcissism were born together, but seems not to have noticed that 'mental life' had then to begin immediately. Neither did he see that once the distinction between auto-erotism and narcissism is so abolished "it is difficult to see just *what* is supposed to be cathected" as Laplanche and Pontalis (1967/1973, pp.337-338) have observed. It is as difficult not to conclude that the proposition that ego, narcissism, and mental life are all present from the start overlooks the mental nature of instinctual drives and the relatively long time the ego takes to develop.

The last detour may be completed by considering how the description of auto-erotism as the mode of instinctual satisfaction in the narcissistic state undermined the thesis that dementia praecox and paranoia had separate dispositional bases. Freud's original argument was that however frequently paranoia seemed to be complicated by symptoms of dementia praecox, it was essential to maintain it as an independent clinical entity (Freud, 1911a, p.76). Schizophrenia had a different dispositional fixation:

> The regression extends not merely to narcissism ... but to a complete abandonment of object-love and a return to infantile auto-erotism. The dispositional fixation must therefore be situated further back than in paranoia, and must lie somewhere at the beginning of the course of development from auto-erotism to object-love. (op. cit., p.77)

Freud was certain enough of this difference to propose that the term 'paraphrenia' be used instead as a substitute for 'dementia praecox' and

'schizophrenia' then both in somewhat confusing use. His term suggested some relation with paranoia but distinguished what had been called schizophrenia or dementia praecox from it.

Clearly Freud's distinction and the new name made sense only as long as auto-erotism and narcissism were thought of as separate stages in a developmental sequence. As soon as they were thought of as components of a single primitive state, there could no longer be two dispositional points and two disorders corresponding to them. Freud made this change. In *On narcissism* he still maintained some difference between paranoia and schizophrenia but, because in that work auto-erotism and narcissism were linked more closely than previously, he *extended* the term paraphrenia to cover *both* schizophrenia and paranoia. Hedging his bets, he continued to use the term 'paraphrenia proper' to refer to schizophrenia (Freud, 1914b, pp.82, 86-87). Following *Instincts and their vicissitudes*, in which the developmental sequence was abolished altogether, the effective distinction between the two disorders also disappeared. Eventually Freud gave up the term paraphrenia altogether (Cf. Editor's Note to Freud, 1911a, p.76, n.1). Despite Freud's advocacy, it is quite understandable why the term paraphrenia never achieved wide circulation; it had become a single name for two disorders that most psychiatrists (and psycho-analysts) saw as reasonably distinct. Of no great moment in itself, what the change does reflect is Freud's altered conception of the relation between auto-erotism and narcissism. To that extent the change is consistent with the conceptualisation that both were present at the beginning of life.

The conflict

Having now completed the four detours we now turn to the problem of the opposition between the ego and sexuality. Two related issues must be considered: first, what motivates the development from primary narcissism and, second, how does the opposition itself grow.

Three paths from narcissism Freud offers three contradictory explanations for narcissism being abandoned. In *On narcissism* Freud posed a question:

> Here we may even venture to touch on the question of what makes it necessary at all for our mental life to pass beyond the limits of narcissism and to attach the libido to objects. The answer ... would ... be that this necessity arises when the cathexis of the ego with libido exceeds a certain amount. (Freud, 1914b, p.85)

Note that this overcathection of the libido, curiously thought of as a damming *within* the ego rather than *by* it, is not pictured as a consequence of a failure of the libido to find satisfying objects. What is proposed involves no objects at all. It is, as it were, a completely internal motive, drawing in no way upon the external world. However, Freud's second reason for the movement away from narcissism makes a veiled reference to

the external world:

> The development of the ego consists in a departure from primary narcissism and gives rise to a vigorous attempt to recover that state. This departure is brought about by means of the displacement of libido on to an ego ideal imposed from without. (op. cit., p.100)

Objects in the external world provide the basis for the ego ideal and, to that extent, the second motive drew on some of the effects of that world. The motive is quite different from the first and is inconsistent with a chronology of ego development that places the formation of the ego ideal at all late in childhood. The third motive was the one that found a permanent place in the developmental theory:

> Those sexual instincts which from the outset require an object, and the needs of the ego-instincts, which are never capable of auto-erotic satisfaction, naturally disturb this state ... and so pave the way for an advance from it. (Freud, 1915a, p.134, n.2)

This is the same basis that had been suggested in the *Formulations* (Freud, 1911b, pp.222-223), and is similar to that implied in *On narcissism* (Freud, 1914b, pp.87-88). Although Freud's description is straightforward enough, the co-operation here pictured between the ego and sexual drives poses problems for the opposition which he later wanted to establish. Three so very different starting points for the movement away from narcissism may also seem an explanatory extravagance. However, they do make handsome provision for saving hypotheses.

Ego v. sexuality As to the opposition between sexuality and the ego, the essence of Freud's derivation is that love and hate become intimately associated with the sexual and ego-instinctual drives respectively, repeating the fundamental antithesis of those feelings in their own relation. The plausibility of the derivation rests on ambiguities in the concepts of object and object-incorporation and on Freud's overlooking significant positive connections between the two kinds of drive.

Restricting consideration to *Instincts and their vicissitudes*, the main thrust of Freud's argument is easy enough to follow: the impact of a stimulating, object-providing external world on a primitive organism concerned only to maintain its internal constancy causes the organism to hate that world. Opposition between ego and sexuality is a consequence of the ego drives, which in seeking out objects, disturbs the auto-erotically self-sufficient state of primal narcissism. Although Freud's starting point is fully consistent with the end-point to which he was striving, there are two problems with his thesis that the external world is the source of unpleasure: the inexorable demands of internal instinctual drive states are far from pleasant, and it is actually objects coming from outside that bring pleasurable satisfactions. He deals with these hindrances peremptorily - need satisfying objects are incorporated in the ego, so ceasing to be a part

of the external world, while instinctual demands are transferred from the internal to the external world by projection. The arbitrariness of these solutions becomes apparent when the postulated mechanisms are analysed. Although modelled on the analogy of eating, object incorporation does not mean that the thing in the external world becomes a physical part of the individual. Incorporation is rather the establishment of functional associations between the mnemic traces of the object and the network of associations constituting the ego. The ego seeks pleasure, the object gives pleasure, *ergo* the two sets of associations become linked. But it would be just as consistent to say that these functional connections ought to modify the ego's elementary picture of the external world so that it interprets the world as a source of pleasure and as something that should be loved. "The dominance of the pleasure principle" is an equivocal basis for hatred being maintained after incorporation. With equal logic it allows *either* love *or* hate to result from the impress of external objects.

Of the two things that can be said about projection, the first virtually repeats the point about introjection. Unsatisfied needs are undoubtedly unpleasurable but the prolonged dependence of the infant means, as Freud made clear in other contexts, the need-satisfying objects from the outside world are provided especially readily. Does not Freud's model of need satisfaction predict, just as readily, that the unsatisfied need will be intimately linked with the image of the externally produced satisfying object? And would not those associations counter the tendency to project the unpleasure outward? The second point is that if it is true to say that unsatisfied need states provide the basis for discriminating the internal world from the external, it is surely inconsistent also to claim that unsatisfied needs fuse the two worlds by projecting the one upon the other. Introjection and projection are virtually the only functions of the pleasure-ego, and their abrupt introduction into the theory as the purifiers of the original reality-ego is as logically satisfying as the appearance of a *deus ex machina* in a morality play.

Freud's argument also overlooks the effect of the initial close, anaclitic attachment of the sexual instinctual drives to the ego-drives. When sucking and hunger needs are satisfied by a single activity, how can a closer association form between hatred and ego than between hatred and the sexual instinctual drive? And, ought not Freud's assumption that some of the sexual component drives require objects "from the outset", a point relegated to a footnote along with other crucial inconsistencies (Freud, 1915a, p.134, n.2), lead to the search for those objects associating sexuality and hatred at least as strongly?

A final peculiarity of Freud's explanation of the ego's relation to hated objects should be mentioned. The converse of the motor urge to bring pleasure-giving objects closer and to incorporate them was said to be a motor urge that:

> endeavours to increase the distance between the object and the ego and to repeat in relation to the object the original attempt at flight from the external world with its emission of stimuli. (op. cit., p.137)

Freud said that repulsion from the object was felt as hate, a feeling that could intensify to the point of creating an intention to destroy the object. With that intensification, the motor urge to *avoid* the object was conveniently transformed into an urge to *pursue* it (op. cit., p.138).

Freud here assumed what had to be demonstrated. On commonsense grounds, the feeling appropriate to the avoidance of an avoidable unpleasant object is annoyance, not hatred. In supposing that unpleasure can be intensified to the point of hatred and that, at the same time, the motor urge to avoid was turned into its opposite of pursuit, Freud filled the very gaps with which his argument had to deal. That we do learn to love and hate what may be termed objects and that we do sometimes avoid and sometimes pursue them is self-evident; what Freud cannot do consistently is to explain how these emotions and actions are related to each other.

Objects and the cathexes of objects Part of the difficulty in Freud's derivation of the love-hate opposition arises from the ambiguity of the concept of 'object'. Although not much discussed, it is evident that the way 'object' is used indicates there is little agreement about its meaning (Green, 1977; Compton, 1986c). Compton distinguishes five fundamental object concepts (Compton, 1986c) and three others less central (Compton, 1986d) in Freud's own writings. All these meanings are foreshadowed, as Boesky (1983) has shown, in Freud's use of the term "object presentation" in *On Aphasia* to refer to a complex of perceptual associations.

Compton (1986d) links Freud's different usages to the evolution of what he sees as Freud's two models of excitation reduction. In Freud's very early instinctual drive theory an object is the *sexual object* and, as such, both the source of excitation and a means to its relief. In the second model the object is the *perceptual object* that becomes incidentally associated with need-satisfaction. Compton argues that the discrepancies are not innocuous because "What is buried in the ambiguous usage is one of the most fundamental problems of psychology: How does anything become mental?" (Compton 1986d). Putting it more specifically, Compton (1985a) asks: "How does the likeness on which identification is based become mental and how does the mental alteration ... occur?"

In the belated discussion of the concept of object in the recent psychoanalytical literature, the essence of the object as the mental representation of a real object has frequently been remarked (Compton, 1985a, 1986d; Rangell, 1985; Abend, and Rangell, in Goldberg, 1985). To my knowledge, no one takes up the point, which I take to be the essence of the answer to the question of how anything becomes mental and how an alteration occurs, that the cathexis of an object or its introjection or projection requires connections to be formed between the "traces" or

"mnemic residues" constituting a structure like the ego and those making up the representation of the object itself. Here we have to be clear about what object-cathexis entails. Years ago, Bellak succintly put the point:

> it only appears that cathexes are really placed on external objects. Actually, the investment of libido is made in the (internal) object representations. (Bellak, 1959)

Although using the term "mental representation" rather than object, Arlow (1980) takes the same position.

My discussion of this basic notion of cathexis in relation to identification, projection, and incorporation indicates the magnitude of the problems entailed. Ornston (1978) observes that even Freud "did not try to describe projection meta-psychologically. Other analysts, who have tried, have had great difficulty". Is the immensity of this problem partly responsible for Boesky's (1983, and in Goldberg, 1985) proposal to do away with the concept of mental representation as a structure? If that were done, what would become of the various internalisation processes?. What is evident is how little attention is paid to the mechanism even in contexts where it might be expected. Thus, although Arlow (1980) discusses the mental representations of pleasurable and unpleasurable objects in the context of the pleasure and reality egos, he does not extend his consideration to what projection and introjection involve. Similarly, Rangell (1985), who endorses the mental representation notion of cathexis, discusses internalisation - but not projection - without mentioning how ego and object representations are brought together. Meissner (1980), it seems to me, comes closest to seeing that the problem is how "properties and characteristics of the external object ... are ... processed so that they become an inherent part of the subject's self". While he also discusses a number of internalisation processes, he also omits projection, a fact possibly related to a shyness in recognising that the basis of all these processes must lie in the association and dissociation of memory traces. Where mechanisms of internalisation are discussed without examining the means by which object representations are linked to or disconnected from the representations constituting the ego, as in Ornston's (1978) analysis, little or no difference will be found between projection and introjection.

As a very last point, we may observe that if Freud's whimsical fancy of the bad external world shattering the dream-like self-sufficiency of the child is to be maintained, auto-erotism *has* to be the mode of satisfaction in it. And maintained it was. Freud thought that only a basic difference between auto-erotism and object-seeking tendencies provided a suitable basis for deriving the opposition between ego and sexual drives. Broadly speaking, it can be seen that arbitrary notions are necessary if a fundamental antithesis between the two kinds of drive is to be arrived at: pleasure-giving objects must be incorporated into the ego without changing the

ego's feelings toward the external world and sexual objects have to have effects different from the objects of the ego-drive. It is difficult to escape the conclusion that much of Freud's arbitrariness and inconsistency was motivated by this theoretical requirement. Certainly the antithesis was not required by any facts he adduced.

THE BASIS OF THE CONCEPTUAL INADEQUACY

One of the minor puzzles about the concepts of ego and ego-instincts is the unexpectedness of their introduction into Freud's theorising. The Editor's Note to *Instincts and their vicissitudes* remarks that although the sexual instinctual drive had been introduced in the *Three Essays* and that the related concepts of sexuality and libido were present much earlier, "The other party to the conflict, 'the ego', remained undefined for much longer" (Freud, 1915a, p.114). Self-preservation drives "had scarcely ever been referred to" and there seemed no reason for relating them to the ego's repressive function (which itself had not been much mentioned either). Then, in the paper on visual disturbances:

> with apparent suddenness ... Freud introduced the term 'ego-instincts' and identified these on the one hand with the self-preservative instincts and on the other with the repressive function. (op. cit., p.115)

I pointed out in the second section of this Chapter how well these new notions fitted in with organic automatic repression being restricted to causing primal repression and how well they met the requirement for a counter to the sexual drive to be instinctual. The new concepts derived solely from the need to provide a basis for a concept of repression consistent with the rest of the theory rather than from new observations. Because that need had arisen suddenly their introduction into the theory had also to be sudden.

Freud's expediency in theorising about the ego and its drive may be measured by his rather offhand remark that what was left over from the sexual component of neurotic conflict could be "brought together under the rubric of ego instincts" (Nunberg and Federn, 1962-1975, *4*, Minute 168 of 27.3.1912). It is not at all remarkable therefore that 'ego' was not clearly differentiated from 'ego-instincts' (E. Bibring, 1936/1941) or that both concepts were so inadequately characterised. Even though the concept of ego was provided with functions in the *Formulations*, that provision is achieved by little more than a transfer of primary process functions from *Ucs.* to the pleasure-ego and the secondary from *Pcs./Cs.* to the reality-ego. Devised for other purposes, these older functions could not be brought into close relation with the energy provided by the newly proposed instinctual drive. Whatever the other virtues of the studies of Leonardo, of Schreber, and of *Totem and Taboo*, the discussions of the ego in them contribute little or nothing to the resolution of the obscurity of the

relation between ego-function and ego-drive. The primary role of those works was to bring into the theory the notion of the ego as sexual object and not to throw light on the ego-drive and its functions.

Much of the difficulty with the new instinctual drive theory results from deficiencies in Freud's use of sexual and hunger drives as models. Paradoxically, it is precisely because the sources of the sex and hunger drives fitted (or seemed to fit) Freud's persuasive psuedo-physiology that they are so inappropriate as models for the ego-instinctual drives and most of the component sexual drives. Although each instinctual drive was eventually described as having a source, for most of drives those sources were uncharacterised, lacking even pseudo-physiological referents. Even for the elementary scientific operation of classification, Freud's theory was not viable. By adopting sex and hunger as models, Freud trapped himself completely in Schiller's poetic fancy that the world might be turned by hunger and love.

As an aside, we might note that Freud's appeal to Schiller is marvellously ironical. The theme of *Die Weltweisen* is that until the *real* forces driving the world are understood, its movement *might as well* be attributed to hunger and love! Further, Schiller did not restrict instinctual forces to the two Freud considered. Freud transmuted Schiller's fancy into three theoretical dogmas: first, that "as the poet has said, all the organic instincts ... may be classified as 'hunger' or 'love'", second, that love and hunger are equivalent to sexual and self-preservation drives respectively, and last, that there was an "undeniable opposition" between the two kinds of drive (Freud, 1910b, pp.214-215). Nothing in *On Narcissism* or *Instincts and their vicissitudes* takes our understanding beyond this combination of the poetic and the dogmatic.

Provided one goes no further than the generalisation of an opposition between ego and sexual drives, Freud's thesis has a degree of plausibility. Because of the superficial similarity between repression proper and the conscious and deliberate attempt to thrust unpleasant or repugnant ideas out of mind, it does not seem unreasonable to describe repression as resulting from an incompatibility between the ego's standards and a sexual idea. What Freud does not show, or makes little attempt to show, is how the ego-instinct acts in repression. Ego-instinctual energy plays no role in repression caused by the transformation of affect from an abandoned erotogenic zone. Neither does it have a role if the fixation of primal repression is due to an excessive pleasure. As for the anticathexis provided by *Pcs.*, if it is responsible for maintaining primal repression and initiating repression proper (Freud, 1915c, pp.180-181), Freud nowhere describes how *Pcs.* acquires its energy, nor why it should draw on the ego-drives. Consequently, either the relation between *Pcs.*, anticathexes, and ego-instinctual drives is quite unspecified - in which case the proposed mechanism has little more explanatory power than Freud's interpretation of Schiller - or the anticathexes derive from the libido - in which case Freud has given up

the poetic dogma - and the ego-instinctual drives are simply not involved in repression at all.

Vagueness about the relation between the sexual and ego-instinctual drives is also reflected in certain peculiarities of the effects of the sexual drive upon the ego, in the inadequacies of the account of the movement away from primal narcissism, and in the tortuous explanation of the subsequent opposition between the two drives. Although it may be uncertain what it was that was cathected in narcissism, the cathexis resulted in sexual energy being located *within* the ego itself (E. Jones, 1935-36; E. Bibring, 1936/1941). Ernest Jones described *On Narcissism* as "a disturbing essay" because this store of libido in the ego tended to obliterate the distinction between the ego drives and the sexual drives. Freud nevertheless defended separate identity of the drives. Biologically they were different and opposed and the analyses of hysteria and obsessional neuroses had also "compelled" him to keep to the distinction: "I only know that all attempts to account for these phenomena by other means have been completely unsuccessful" (Freud, 1914b, pp.77-78). Neither argument is compelling. The point about the psychoneuroses is nothing other than that he used repression to explain them. The biological grounds are little more than an extension of Schiller's poesy.

One effect of the narcissistic ego being cathected was what Freud called the sexualisation of thought. Yet the notion that thought in narcissism was sexualised is not required by the argument nor is it justified by the evidence. Ferenczi showed that omnipotence of thought can be derived solely from a continuation of the hallucinatory wish-fulfilling thinking of the infant (Ferenczi, 1913/1952c). Any object, whether of the sexual instinctual drive or not, can be conjured up by wishing its presence. The evidential basis is just as shaky. Freud himself seems to have first applied the phrase 'sexualization of thought' to the tendency of obsessional patients to brood over their ideas. The brooding was seen as a regression from action to thought, a regression made possible in part by the intensity of the instinctual drive for sexual knowledge in the patient's childhood. Obsessional brooding was a regressive manifestation in which "the sexual pleasure which is normally attached to the *content* of thought becomes shifted on to the *act* of thinking itself" (Freud, 1909b, p.245. My emphasis, MBM). In *Totem and Taboo* sexualisation had a rather different effect: it produced omnipotence of thought through an overvaluation of the *consequences* of thinking, rather than the sexual pleasure of a mere *act* of thought (Freud, 1912-1913, pp.88-90). Freud's "evidence" cannot justify both of these meanings. Once again, the vagueness of what exactly happened when the ego was cathected made both equally plausible.

Theoretical consistency also required that during the development from the primal narcissistic state associations be generated between sexuality and love on the one hand and ego and hatred on the other. Otherwise the two drives could neither develop nor maintain an antithesis. The mechan-

ism Freud finally chose from the three he put forward - the ego drive's search for objects - was the easiest to adapt. But his adaptation was at the cost of having to make arbitrary assumptions about the effects of introjection and projection. Only if pleasure-giving sexual objects were introjected and unpleasant need states projected could Freud maintain that the ego drives were directed solely toward hated, external objects. Freud also had to overlook the effects of those sexual drives that seek objects from the external world and to assume that when both drives were satisfied on the same object there was a dissociation between the effects of satisfaction - otherwise pleasure would accrue to both drives equally.

The difficulties in explaining hatred may be put another way. Basic to Freud's theory is an opposition between two types of drives, both of which seek pleasurable satisfactions; that fact alone makes it difficult to imagine how, other than through drive-frustration, emotions other than love can arise. Indifference, or perhaps annoyance, might be thought of as resulting from the continued impact of unsought stimuli, but nothing else. It should now be obvious that Freud would have severe difficulty in explaining masochistic behaviour. And he does. If masochism and sadism are found together it is economical to derive them either from a common source or from one another. The pleasure principle rules out a primary masochism: one cannot assume that an organism would inflict pain upon itself. But, hatred and aggressiveness had to be devoid of sexual content; their status as ego-instinctual drives saw to that. So they provided no basis for the infliction of pain. These constraints virtually forced Freud into assuming a primary sadistic drive, having as its aim the mastery and humiliation of the object rather than the infliction of pain:

> Psycho-analysis would appear to show that the infliction of pain plays no part among the original purposive actions of the instinct [of mastery]. A sadistic child takes no account of whether or not he inflicts pains, nor does he intend to do so. (Freud, 1915a, p.128)

Freud went on to argue that once children experienced the effects of painful stimulation themselves, this originally active primary sadistic drive turned back upon them and changed into a passive form. Masochism proper developed when painful stimuli generated sexual excitation and the individual sought out others to inflict pain upon. Another turning around of the drive then took place. Directed to the individual's objects in an active way it thus generated sadism proper.

Now, psycho-analysis had not 'shown' that there was no primary masochism; indeed, within a very few years, a primary masochism was to be made one of Freud's most central propositions. The only thing that had been 'shown' was the logical inconsistency of assuming a primary masochistic drive in an organism devoted solely to the pursuit of pleasure and the avoidance of pain.

CONCLUSION

The most important general conclusion is that the deficiencies of Freud's new theory of the ego, its functions, and its drives result from constraints imposed by other parts of the theory. All the elements of the new theory were in existence well before the paper on vision and it could almost have been made up from them without any new facts. The *Studies on Hysteria* provided the notion of an ego and a repressing force located in it, *The Interpretation of Dreams* set out the primary ego functions and those that developed from them, and the *Three Essays* adumbrated the concept of instinctual drive. The concept of an ego that was the object of the sexual drive and the supposition that fixations produced dispositions to later regressions were the only really new elements. While observations of perversion and psychosis had contributed, they were not related in a consistent and convincing manner to the supporting evidence. There were also constraints on the ways in which the components could be fitted together, constraints imposed by the requirements that all instinctual life be governed by the principles of constancy and pleasure and for there to be two opposed sets of instinctual drives. A real theoretical synthesis of these elements escaped Freud. At almost every point he retreated into vagueness or made arbitrary assumptions that created further inconsistencies.

In Chapters 12 and 13 we shall see how Freud tried to escape these constraints by moving beyond the notion of a pleasure principle, by modifying much of his thinking about instinctual drives, and by making very considerable alterations to his model of the mind.

12 THE INSTINCT THEORY FINALIZED

Now, whom doth it not conceme to learn,
both the danger, and benefit of death?

Anonymous Introduction to Donne's *Deaths Duell.*

In 1920 Freud announced a major revision of his conceptualisation of instinctual drives and of the conflict between them. The battle was no longer between ego and sexuality. Those two old antagonists were fused together in a new life drive named Eros, and the force of death itself, surely the most weighty of all contenders, matched in struggle against it. Partly as a result, the model of the mind outlined in *The Interpretation of Dreams* had also to be revised. In this Chapter I examine the new conceptualisation of instinctual drives and consider Freud's new theory of the mental structures in Chapter 13.

Although the stages through which Freud's thinking passed as he revised the instinctual drive conceptualisation are almost impossible to reconstruct, two distinct lines of thought can be differentiated: on the one hand, he brought together the sexual and ego-drives under the aegis of Eros and, on the other, he selected death as opponent. We do not know which of these notions came first.

Several writers have traced the concept of the death instinct back to the impression that the massive cruelty, destructiveness, and devastation revealed in the First World War made upon Freud. There is no doubt about the depth of Freud's disillusionment, nor the extent to which he believed that primitive instinctual passions had been unleashed (Freud, 1915d). The war had, he said:

> destroyed not only the beauty of the countrysides through which it passed and the works of art which it met with on its path but it also shattered our pride in the achievements of our civilization, our admiration for many philosophers and artists and our hopes of a final triumph over the differences between nations and races. It tarnished the lofty impartiality of our science, *it revealed our instincts in all their nakedness and let loose the evil spirits within us* which we thought had been tamed for ever by centuries of continuous education by the noblest minds. (Freud, 1916a, p.307. My emphasis, MBM)

Similarly, in his lectures, he asked his students to:

> consider the Great War which is still laying Europe waste. Think of the vast amount of brutality, cruelty and lies which are able to spread over the

> civilized world. Do you really believe that a handful of ambitious and deluding men without conscience could have succeeded in unleashing all these evil spirits *if their millions of followers did not share their guilt*? Do you venture, in such circumstances, to break a lance on behalf of the exclusion of *evil from the mental constitution of mankind*? (Freud, 1916-17, p.146. My emphasis, MBM)

Freud's impeaching of instinctual forces is here quite definite. However, he made no immediate alteration to his views on the sources of aggression. Nor need he: destructiveness, brutality, and cruelty resulted from the aggressiveness and hatred he had derived from the ego-instinctual drives. Indeed, Freud had made that derivation in the very same weeks in which he so forcefully expressed his dismay at the results of the War (Editorial Introductions to Freud, 1915a, p.111 and 1915d, p.274). Consequently, it is not surprising that Freud used the ego-instincts to explain the instinctual basis of wartime destructiveness (Freud, 1915d, pp.280-283).

We may also note that when Freud first intimated that there were instincts other than the sexual and ego-drives, he did so in a context in which he failed to mention either aggressiveness in general or the War in particular (Freud, 1919d). The same point holds for *Beyond the Pleasure Principle*, in which the death instinct was formally introduced into psychoanalytic theory (Freud, 1920a). As the Editors of the *Standard Edition* (*19*, p.157, n.2) remark, it was only from 1930 on that Freud turned his attention more to the *outward* direction of the death instinct - to aggressiveness and destructiveness. The impact of the war seems neither to have forced Freud to reconsider the origins of hatred and aggression nor to have contributed directly to his concept of a death instinct.

On the other hand, some writers have traced the revised instinct theory, or at least that part of it that fused the sexual and ego-drives, to a theoretical difficulty created by the concept of narcissism. Narcissism required the retention of some of the original store of libido within the ego and that retention, it was argued, either masked the independent energy (''interest'') that the ego-drive possessed, or it did away with an independent ego energy altogether. On this view, Freud's revision of the instinct theory resulted from his bowing to the theoretical necessity for disposing of a superfluous concept. While there is a sense in which this is correct, it has to be pointed out that over quite a long period Freud vigorously defended his separation of the two classes of drive (Freud, 1914b, pp.76-81; 1915a, pp.123-125; 1916b, pp.316-318; 1916-1917, pp.350-357, 412-430; 1917b, pp.137-139; 1919c, pp.208-210). Usually he argued that the transference neuroses - hysteria and obsessional neuroses - were otherwise impossible to understand. The conflict between the demands of sexuality and the standards of the ego was absolutely critical to their explanation. Separation of the drives was also defended in terms of the very different sources from which they arose. Sometimes Freud made the additional point that to abolish the distinction would make concessions

to the then recent Jungian and Adlerian heresies. Jung had proposed that there was only one kind of mental energy and that it was non-sexual; by so doing, he posed an immediate challenge to the central position of sexuality in Freud's theory. Adler's challenge was as strong, for he wanted to make what was to Freud essentially an ego-drive the sole or main force motivating behaviour. What Jung and Adler both abandoned was sexuality and, with it, the notion of a conflict between ego and sexuality.

A limitation to the argument from narcissism stands out: only the bringing together of sexual and ego-instinctual drives is explained. There seems no necessity for the new drive to be a death instinct. Yet there is a sense in which it is precisely the reconciliation of ego with sexuality that determines the attributes of the new opponent. This sense is provided by Freud's characteristic mode of thinking that always limited conflict to two parties. Either Freud thought this way because he could not otherwise explain mental conflict to himself, as his own writings and the opinion of Fairbairn (1939-1941) suggest, or because he had a predilection for casting explanations in terms of two opposing processes - sometimes called, rather confusingly, his 'dualism' (E. Jones, 1935-1936; 1953-1957, *II*, p.320 and *III*, pp.266-267; Arlow, 1959; E. Bibring, 1936/1941; Loewenstein, 1940; Flugel, 1953). Certainly there was no logical requirement for there to be only two instincts. As Gillespie (1971) has noted, the essential bipolarity (as he correctly calls it) of Freud's theory was "not a necessary deduction from conflict". More recently, Holt (1975a) has convincingly pointed out that psycho-analytic facts about conflict do not necessitate the assumption that "all motives may be reduced to any two". Nor, as Satinover (1986) has shown, did the explanation of transference neuroses really depend on the conflict being between libidinal and ego energies.

However, once Freud decided to remain within the two motive framework, the rest was decided. First, the opponent had to be able to counter an instinctual drive - *ergo* it had itself to be instinctual. Second, the behavioural manifestations of the new drive had to be muted. Otherwise it would not have been so long overlooked. For the same reason, the new drive had to have its source in some not-readily-identifiable bodily process. Finally, since both the sexual and ego-drives were governed by the pleasure principle (the former in its 'pure' version, the latter in the modified form of the reality principle), the new antagonist had to be independent of the pleasure principle and beyond it.

Where then was the evidence of an instinctual force opposed to Eros and governed by something other than the pleasure principle? And where was its source? To Freud it seemed that a substantial part of the answer to the first question was provided by behaviours that showed a compulsive tendency to repeat unpleasant experiences. What Freud termed the 'daemonic character' of such repetitions directly suggested that the compulsion proceeded from:

> the instinctual impulses and probably inherent in the very nature of the instincts - a compulsion powerful enough to overrule the pleasure principle, lending to certain aspects of the mind their daemonic character. (Freud, 1919d, p.238)

In *Beyond the Pleasure Principle*, Freud expanded on this assertion in three ways: he attempted to demonstrate that the behaviours did in fact violate the pleasure principle, he argued that all instinctual drives were repetitive and had been acquired specific historical circumstances, and he interpreted the compulsion to repeat as a manifestation of an instinctual drive having death as its aim. Death was now the new opponent for Eros. Freud conceived of the drive as having both inwardly and outwardly directed forms, forms that allowed a ready derivation of masochism, sadism, and aggressiveness and so filled an important gap in psycho-analytical theory.

I begin this chapter with summaries of Freud's clinical and theoretical arguments for recognising a compulsion to repeat and for deriving a death instinct. An evaluation of the logical and evidential bases of his theses is then made. Answers are sought to a number of specific questions including the following: Were the behaviours beyond the pleasure principle? Was the compulsion to repeat instinctual? Did instinctual drives have the characteristics Freud attributed to them? Why did organisms die? What were the mechanisms by which the individual coped with trauma? How were the various principles and tendencies that regulated or governed mental processes related to one another? I claim that the proper answers to these questions contradict Freud's theses. Psycho-analytic alternatives to Freud's death-instinct based explanations of the compulsion to repeat, of sadism and masochism, and of aggressiveness are considered briefly before the final summary conclusion.

THE CLINICAL BASES OF REPETITION

Freud thought there were three behaviours governed by something other than the pleasure principle. They were the transference of some of the patient's feelings on to the analyst, the repetition of terrifying dreams in the traumatic neuroses, especially in the war neuroses, and a particular type of children's play. I begin with Freud's analyses of them before considering his attributing them to the compulsion to repeat. After that I consider the claim that the compulsion had its basis in an instinctual death drive.

Transference Freud began his discussion of repetition and transference by drawing attention to the problem of obliging the patient "to confirm the analyst's construction [of what had happened] from his own memory" (Freud, 1920a, p.18). Resistance often prevented unconscious ideas being brought to consciousness in this way. The patient could not remember the repressed material and acquired "no sense of conviction of the correctness of the construction that has been communicated to him" (ibid.). Freud

claimed that instead of accepting his interpretation, the patient repeated in action some version of the repressed ideas. What was re-enacted was always some part of the patient's infantile sexual life that brought with it:

> no possibility of pleasure, and which can never, even long ago, have brought satisfaction even to instinctual impulses which have since been repressed. (op. cit., p.20)

Of necessity the child's sexual activity had to end in failure and the loss of parental love. Childhood sexual drives could never be satisfied in actuality. Scorning the child's sexual love might result in the child developing some persistent but vain expectation of satisfaction from the parent. Or, unable to procreate or solve the mystery of where babies come from, the child might come to feel quite unable to accomplish anything. Neither remembered nor acknowledged in treatment:

> Patients repeat all of these unwanted situations and painful emotions .... They seek to bring about the interruption of the treatment while it is still incomplete; they contrive once more to feel themselves scorned, to oblige the physician to speak severely to them and to treat them coldly; they discover appropriate objects for their jealousy; instead of the passionately desired baby of their childhood, they produce a plan or a promise of some grand present - which turns out as a rule to be no less unreal. (op. cit., p.21)

Freud stressed that:

> None of these things can have produced pleasure in the past .... In spite of that, they are repeated under pressure of a compulsion. (ibid.)

A year later, in 1921, he added a sentence to emphasise the point even further. Freud noted that although the childhood activities were motivated by instinctual drives "intended to lead to satisfaction" no lesson seemed to have been learned from the fact that each past attempt at satisfaction had led "only to unpleasure" (ibid.).

Freud also thought the lives of some essentially normal people were directed by a similar compulsion to repeat. Among the many examples he cited were the lover whose successive affairs always passed through the same stages before reaching the same unsatisfactory termination, the individual whose friendships were successively betrayed by his friends, and the benefactor who was abandoned in turn by each of his proteges and who seemed forever doomed "to taste all the bitterness of ingratitude" (op. cit., p.22). All such people seemed pursued by a malignant fate that compelled them to repeat the same mistake over and over again.

Traumatic neuroses A similar tendency for a repetition of the unpleasurable manifested itself in the traumatic neuroses when, as Charcot's patient Le Log___ had done, the patient relived the traumatic event or some derivative of it in his dreams. Freud asserted that such repetitions astonished people far too little:

> They think the fact that the traumatic experience is constantly forcing itself upon the patient even in his sleep is a proof of the strength of that experience: the patient is, one might say, fixated to his trauma. (op. cit., p.13)

Fixations of this kind, Freud argued, could not explain why the memories intruded at night, but not at all during the day-time, and why the wish-fulfilling function of dreams did not prevent the reappearance of the memory. If the wish-fulfilment function were to be saved, it followed that in some way it had to have been upset or diverted from its purpose.

Play Freud had observed his grandson playing in a way that seemed to illustrate the compulsion to repeat (Freud, 1900, p.461, n.1. of 1919). At the age of one and a half years the child displayed few overt signs of distress when his mother left him even though he was much attached to her. However, he did have:

> an occasional disturbing habit of taking any small objects he could get hold of and throwing them away from him into a corner, under the bed, and so on, so that hunting for his toys and picking them up was often quite a business. As he did this, he gave vent to a loud, long-drawn-out 'o-o-o-o', accompanied by an expression of interest and satisfaction ... this was not a mere interjection but represented the German word '*fort*' ['gone']. (Freud, 1920a, pp.14-15)

Other observations confirmed the meaning of the vocalization: the child repeatedly threw a toy on a string out of sight while saying 'o-o-o-o', but hailed his retrieval of it "with a joyful '*da*' ['there']", and he used 'o-o-o-o' to accompany the crouching by which he made his image disappear in a mirror (op. cit., p.15 and n.1). Freud eventually realised that the toy-throwing was a game, that the only use the child made of any of his toys was to play 'gone' with them and that the throwing away, rather than the recovery, was its essential feature (op. cit., pp.14-15). He interpreted the game as compensation for the child's lack of reaction to the mother's going away. But it was the unpleasant part of that experience that was being re-enacted, not her return.

Alternative interpretations of the play were possible. Freud distinguished five. First, because the child was in a passive situation that had overwhelmed him, he was actively repeating the experience in play in an endeavour to master it. Instead of a compulsion to repeat, the behaviour might be motivated by "an instinct for mastery ... acting independently of whether the memory was in itself pleasurable or not" (op. cit., p.16). It was also possible that the child was satisfying a hostile impulse, revenging himself upon his mother for leaving him. Some of the child's later behaviour was consistent with this explanation (ibid.). Third, Freud noted that children often tended to repeat "everything that has made a great impression on them in real life". Another motive for the repetition might therefore be an attempt to master the situation by abreacting the

considerable affect associated with it (op. cit., pp.16-17). A fourth motive was suggested by the prevalence in childhood of "the wish to be grown up and to be able to do what grown-up people do" (ibid.). Freud remarked finally that in re-enacting an unpleasant experience a child frequently handed on "the disagreeable experience to one of his playmates and in this way revenges himself on a substitute" (ibid.). Because all five of these alternative motives yielded pleasure as their final outcome, the repetitive behaviour could still be subsumed under the pleasure principle. None required the supposition of "tendencies *beyond* the pleasure principle, that is, of tendencies more primitive than it and independent of it" (ibid.).

Some of the other repetitive behaviours were similarly ambiguous. Transference repetition might be due to the ego, "clinging as it does to the pleasure principle", calling upon the compulsion to repeat in order that repression be maintained (op. cit., p.23, n.1). Rational factors might explain why people repeated the same errors throughout their lives and there might be "no necessity to call in a new and mysterious motive force" (ibid.). Freud concluded that the least doubtful instance was the repetition of the traumatic dream.

Quite suddenly Freud interrupted these considerations to assert that:

> on maturer reflection we shall be forced to admit that even in the other instances the whole ground is not covered by the operation of the familiar motive forces. Enough is left unexplained to justify the hypothesis of a compulsion to repeat - something that seems more primitive, more elementary, more instinctual than the pleasure principle which it over-rides. (op. cit., p.23)

If this primitive instinctual force did exist one had to learn:

> what function it corresponds to, under what conditions it can emerge and what its relation is to the pleasure principle - to which, after all, we have hitherto ascribed dominance over the course of the processes of excitation in mental life. (ibid.)

Freud's concluding remarks thus tentatively characterise the force responsible for the compulsion to repeat. He now left his clinical evidence and turned to a theoretical analysis in which he tried to show that the compulsion had an instinctual basis.

THE THEORETICAL BASIS OF REPETITION

Even were it true that the compulsion to repeat overruled the pleasure principle that fact alone did not show it arose from an instinctual drive. Freud's theoretical analysis attempted to meet the problem in two ways: first he tried to show how a repetitive tendency for coping with trauma might have evolved and he then attempted to establish that the very essence of instinctual drives was that they were conservative, historically acquired,

and directed towards re-establishing or repeating an earlier state of affairs. Freud's arguments were conducted within the constraints of his conflict model: there would be two classes of drive in opposition, one of which would incorporate the sexual drive.

Trauma and repetition

It seemed to Freud that the very factors which had caused the evolution of the system *Pcpt.-Cs.* explained how a mechanism for dealing with trauma that overrode the pleasure principle had been acquired. Consciousness, which arose in *Pcpt.-Cs.*, seemed to consist of two things: perceptions of excitations originating in the outside world and feelings of pleasure and unpleasure arising from within. *Pcpt.-Cs.* had therefore to be thought of as lying between the inside and the outside of the organism. Basing himself on Breuer's proposition from the *Studies on Hysteria*, Freud then argued that if a system were permanently modified by successive excitations it would lose the capacity for responding to new stimulation. Responsiveness and modifiability were thus mutually exclusive.

So great was the intensity of stimuli impinging on the primitive organism from the external world that the transmission of excitation through its outer cortical layer was completely facilitated and memory traces could not be laid down there. Consciousness would arise instead. Further, precisely because it was adjacent to the powerful stimuli from the external world, Freud concluded that *Pcpt.-Cs.* could not be further modified. Using what he claimed was Breuer's distinction between two kinds of energy, Freud described its elements as being able to carry only energy capable of free discharge and not to be able to retain or bind it.

Energy arising from the external world had a second effect. Internally undifferentiated organisms having only the capacity to respond to stimuli developed a protective shield that absorbed most of the external energy before passing it on in less intense and non-destructive quantities to the deeper layers. Special modifications of the outer layer capable of taking in small quantities of this attenuated excitation and sensing its nature and direction were located immediately beneath the shield. These were the sense organs. Energy from the external world had thus created a protective shield having a primitive *Pcpt.-Cs.* system behind it. The protective function was paramount:

> *Protection against* stimuli is an almost more important function for the living organism than the *reception* of stimuli. (Freud, 1920a, p.27)

Protection was generally not required from internal drive stimuli - their magnitudes were "more commensurate with the system's method of working". Because they were not attenuated the feelings of pleasure and unpleasure they generated came to predominate. Therein lay the importance of the pleasure principle. However, should internal stimuli become too intense they too were treated as if they had originated in the

outside world and the mechanism of the protective shield used to reduce them. Such was the basis of the process of projection (op. cit., p.29).

How then could the pleasure principle be overruled? The traumatic situation itself suggested the answer. Traumatic excitations were those arising externally that were strong enough to break through the protective shield, flood in towards the interior of the organism and there cause a profound disturbance in the distribution of energy. The organism was faced with the task of:

> mastering the amounts of stimulus which have broken in and of binding them, in the psychical sense, so that they can then be disposed of. (op. cit., p.30)

Cathectic energy, mobilised from throughout the organism, was used to construct an anticathexis "on a grand scale" in order to effect the binding. Freud then inferred that:

> a system which is itself highly cathected is capable of taking up an additional stream of fresh inflowing energy and converting it into quiescent cathexis, that is of binding it psychically. The higher the system's own quiescent cathexis, the greater seems to be its binding force; conversely, *therefore*, the lower its cathexis, the less capacity will it have for taking up inflowing energy. (ibid. My emphasis, MBM)

The last part of Freud's conclusion harks back, of course, to the Meynert-Exner mechanism of facilitation, both as Breuer had used it in the theory of the *Studies on Hysteria* and as Freud had adapted it for his *Project.*

Traumatic neuroses came about when the protective shield was breached unexpectedly. Freud regarded the individual's unpreparedness for the danger as equivalent to a lack of cathexis in the first of the systems reached by the incoming stimuli and as a necessary condition for the development of the neurosis. When the danger was expected, the anticipatory cathexis (or 'hyper-cathexis') of these systems enabled binding to take place. Thereupon "a preparedness for anxiety" arose. Without the hyper-cathexis the alien stimuli could not be bound in the first systems and continued to flood in until the hastily prepared anticathexis was able to stop them. Repetitive dreams were not so much attempting to fulfil a wish as:

> helping to carry out another task, which must be accomplished before the domination of the pleasure principle can even begin. These dreams are endeavouring to master the stimulus retrospectively, by developing the anxiety whose omission was the cause of the traumatic neurosis. (op. cit., p.32)

Retrospective binding was attempted without reference to the pleasure principle. In view of the ultimate aim of his argument, it was perhaps surprising that, though Freud described the binding as independent of and more primitive than the pleasure principle, he also stressed that it was not in contradiction with it (ibid.).

Instinctual drives could sometimes produce changes in the distribution of energy matching those produced by external trauma. Instinctual impulses were not attenuated by the protective shield and their point of contact with the mental apparatus was the system *Ucs.* Consequently they were subject to the laws of the primary process and their freely mobile energy pressed strongly for discharge. Freud now claimed that it was easy to identify the primary process:

> with Breuer's freely mobile cathexis and the secondary process with changes in his bound or tonic cathexis. If so, it would be the task of the higher strata of the mental apparatus to bind the instinctual excitation reaching the primary process. (op. cit., pp.34-35)

A disturbance analogous to a traumatic neurosis was produced when these higher strata failed. And again, only after binding or mastery had taken place was the pleasure principle able to dominate mental life. Freud reiterated that this repetition was also independent of the pleasure principle rather than opposed to it (op. cit., p.35).

Repetition and instinctual drives

Freud now suddenly altered the thrust of his argument to assert that the manifestations of the compulsion to repeat in children's play and in transference were highly instinctual in character. Indeed, so striking was this characteristic that "some 'daemonic' force" seemed to be at work (op. cit., p.35). Freud here harked back to the paper, published a few months before, in which he had first described the compulsion and where he had argued that what required recognition was that the basis of the repetitions was "inherent in the very nature of the instincts" (Freud, 1919d, p.238). Now, in *Beyond the pleasure principle*, Freud said that it had been precisely this daemonic quality that had led him to this hitherto unrecognised or insufficiently stressed aspect of instinctual life. He now concluded:

> *It seems, then, that an instinct is an urge inherent in organic life to restore an earlier state of things* which the living entity has been obliged to abandon under the pressure of external disturbing forces; that is, it is a kind of organic elasticity, or, to put it another way, the expression of the inertia inherent in organic life. (Freud, 1920a, p.36)

Freud acknowledged that this interpretation of the daemonic instinctual force was the very opposite of the popular view. Most frequently, instinctual drives were thought of as vehicles of change. But, to support his interpretation, he cited the view of "many biologists" that instinctively governed migrations of fish and birds were due to those animals seeking out the localities in which their species had once resided, but from which they had become accidentally separated. However, "the most impressive proofs" came from heredity and embryology where it had been shown that the developing organism always recapitulated the structures and forms from which its own species had evolved (op. cit., p.37).

A deathly argument Having thus established the possibility that some instinctual drives might have the restoration of an earlier state as their aim, Freud then tried to prove that it was universally true. Because of the disorder of issues that he raised, it is not at all obvious that Freud's intention was to do this through a formal deductive argument. He said he wished "to pursue to its logical conclusion" the following hypothesis:

> all the organic instincts are conservative, are acquired historically and tend towards the restoration of an earlier state of things. (op. cit., pp.37-38)

But this "hypothesis" turns out to be Freud's first premise. To it he conjoined a second: everything that died naturally did so only for internal reasons. Coupling the premises compelled the conclusion:

> '*the aim of all life is death*' and, looking backwards, that '*inanimate things existed before living ones*'. (op. cit., p.38.)

For the conclusions to carry any weight it had to be shown that both premises were true. A ready proof that instincts were conservative seemed to be available for the first premise. If it were true, it followed, Freud argued, that organic development was due to changes in the external world being impressed on elementary living entities. A changeless world prevented the organism from changing because:

> if conditions remained the same, it would do no more than constantly repeat the same course of life. (ibid.)

Variations in external circumstance modified the organism and the changes were stored up to be repeated as acquired instinctual dispositions. Instincts were therefore conservative. Freud also pointed out that it was inconsistent to maintain that the goal of an instinctual striving could be a state not previously experienced.

As for the historical component of the first premise, Freud described inanimate matter as being converted into living substance by the action of an external force generating a tension in it. The tension necessarily sought to cancel itself out and in so doing brought the first instinctual drive into being. Its aim, of course, was to return the newly created living substance to its previous inanimate and tensionless state. Paradoxically enough, in the very historical circumstance from which life had evolved, Freud seemed to have found an instinctual drive aimed at the destruction of that life. Even present-day life could be viewed as a manifestation of the death drive. After life had been created, the repetition of successively acquired instinctual dispositions prevented the organism from dying as easily as it had first been able to. The phenomena of life were thus nothing more than manifestations of the circuitous paths to death that newly acquired and conservative instincts forced upon the living organism. Because their real function was to ensure that the organism died in its own way, self-

preservation drives were thus fundamentally conservative. Indeed, they were mere creatures of the death instinct.

The generality of conservation Freud had now to show that all the other instinctual drives were similarly conservative. He began by considering the sexual drives. Present-day elementary organisms were, he thought, probably much like their primaeval ancestors and the same could be said of the germ-cells of multicellular organisms. The latter had retained their original structure and their dispositions even though they had become separated from the organism as a whole. When, in the present, the germ-cells began their development, they repeated the act to which they owed their own existence. One portion developed fully and the other became a new germ-cell. Because sexual instinctual drives guided the destinies of the germ-cells, it followed that the sexual instincts were also conservative in bringing back "earlier states of living substance" (op. cit., p.40). They were additionally conservative in that they were resistant to external influences. They preserved life for long periods and Freud concluded they were "the true life instincts" (ibid.). Because they acted against the other, death-seeking instinctual drives it could be concluded that there was an opposition between the sexual and other drives - an opposition "long ago recognized by the theory of the neuroses" (ibid.).

Freud next considered whether there were any drives that did not seek to restore an earlier state. Were there any drives that aimed at goals never before achieved? (op. cit., p.41). It did not appear to him that there was a universal drive towards higher development in the animal world. Nor was there an untiring impulsion towards perfection in human life. To the extent that the latter seemed to be present, it was due to instinctual repression in a minority of leaders whose drives never ceased:

> to strive for complete satisfaction, which would consist in the repetition of a primary experience of satisfaction. (op. cit., p.42)

Taken together, the death drive, the sexual drives, and all of the other instinctual drives could be considered as conservative. Not only did none of them aim at the new and unknown; all were directed towards restoring the conditions that had existed immediately prior to their own coming into being. Freud thought that this established the truth of the first premise.

Sexuality and the first premise A problem remained with the sexual instinctual drive. Although Freud had been able to draw a "sharp distinction" between it and the death drive he felt unable to attribute to it a really "conservative or rather retrograde, character corresponding to a compulsion to repeat". While sexual drives did reproduce primitive states:

> what they are clearly aiming at by every possible means is the coalescence of two germ-cells .... If this union is not effected, the germ-cell dies along with all the other elements of the multicellular organism. It is only on this condition that the sexual function can prolong the cell's life and lend it the

> appearance of immortality. (op. cit., p.44)

Freud felt quite unable to say what:

> important event in the development of living substance [was] being repeated in sexual reproduction, or in its fore-runner, the conjugation of two protista [i.e. protozoa]. (ibid.)

The three parts of the initial premise did not, after all, seem to apply fully to the sexual instinctual drives.

The second premise Freud then went on to test the validity of the second premise, that of natural death being due solely to internal causes. There was, he noted, little agreement among biologists on the subject. However, he discerned an important analogy with Weismann's then influential biological theory: the germ-plasm, which was concerned with reproduction, was immortal, while the soma, virtually the rest of the body, was mortal. The distinction seemed to Freud to be a morphological corollary to his differentiation of the two classes of drive. True, in two significant respects the analogy was incomplete. Weismann's distinction applied only to multicellular organisms and not to the unicellular, in which the individual was both germ-plasm and soma. Death could therefore appear only with the development of multicellular organisms and not, as Freud's argument required, at the very beginning of life. Second, while Weismann did hold that the multicellular organism died for internal reasons, the types of cause he proposed, such as imperfect metabolism or defective differentiation, lacked the iron necessity that Freud sought. The death of multicellular organisms was therefore "of no interest".

It was the alleged immortality of unicellular organisms that seemed more relevant. Some workers had kept ciliate infusoria alive for over 3,000 generations but others had noticed signs of senescence well before that time. Two things seemed to postpone senescence and eventual death. If aging individuals were allowed to conjugate, both were rejuvenated and it was only if each successive generation was provided with fresh nutrient medium that the infusorian become immortal. Infusoria died if the nutrient was not changed. Experiments showed the infusorian's own metabolic products to be fatal. Freud concluded:

> An infusorian, therefore, if it is left to itself, dies a natural death *owing to its incomplete voidance of the products of its own metabolism.* (op. cit., p.48. My emphasis, MBM)

In at least some unicellular organisms it seemed to Freud that internal causes brought about a natural death. Even were that not so Freud argued that the validity of the second premise could be maintained by distinguishing between overt and covert processes. The very primitiveness of protozoan organisation might hide those internal processes that found visible morphological expression in more advanced organisms. The death drives might be so completely hidden "it may be very hard to find any

direct evidence of their presence''. From what Freud called his ''dynamic'' standpoint - as opposed to the morphological - it was ''a matter of complete indifference'' whether natural death occurred in protozoa or not. Further, while Freud had interpreted the generation experiments with protozoa as supporting the conclusion that they died for internal reasons, the qualification could still be made that even if protozoa:

> turned out to be immortal in Weismann's sense, his assertion that death is a late acquisition would apply only to its *manifest* phenomena and would not make impossible the assumption of processes *tending* towards it. (op. cit., p.49)

So interpreted, Freud did not think biology flatly contradicted his ''recognition of death instincts''. Consequently, he felt ''at liberty'' to continue to entertain their possibility, an attitude that was again reinforced by the ''striking similarity'' between the distinction between the life and death instincts and Weismann's separation of the germ-plasm from the soma. Freud also noted that his conceptualisation was in accord with Hering's thesis that two opposed processes of anabolism and catabolism were at work in living tissue. Perhaps they too were guided by instincts.

A death instinct manifest? Were there any instinctual drives that did not belong to either of the new classes? Now granting that the original instinctual polarity had ''proved to be inadequate'' (op. cit., p.52), Freud pointed out that the ego-drives could readily be fused with the sexual drives and brought under the rubric of a life instinct. After all, narcissism had required the recognition of a portion of the ego instincts as libidinal. But, Freud went on:

> The difficulty remains that psycho-analysis has not enabled us hitherto to point to any instincts other than the libidinal ones. That, however, is no reason for our falling in with the conclusion that no others in fact exist. (op. cit., p.53) [1]

Taking sadism as a possible representative of the death instinct, admittedly ''a displaced one'', Freud reconsidered the polarity of love and hate in object-love. Sadism could be thought of as the force motivating hatred and, therefore, as the sought-for manifestation of the death instinct. However, this rethinking required a modification of the thesis proposed in *Instincts and their vicissitudes* that sadism was primary. Sadism had had to be transformed into masochism by being turned around upon the ego before it could be deflected on to objects as secondary sadism. What Freud now proposed was a primary masochism. Once this masochistic drive

1. The Editors of the *Standard Edition* expand 'instincts' here to 'ego-instincts', so making the whole sentence refer to them. Apart from violating the direction of Freud's argument, which sought to demonstrate that *all* the instincts so far observed in psycho-analysis belonged to the class of life instincts, the interpolation makes nonsense of the next part of the argument in which Freud produced an example of an instinct belonging to the other class.

fused with the life instincts, it was turned away from the ego and on to objects as sadism, although it could return as a secondary masochism. If this re-analysis were accepted, a new polarity of life versus death could replace the older one of sexuality versus ego.

From within the framework provided by the new polarity, Freud re-interpreted the evidence about conjugation. Conjugation prevented senescence because the exchange introduced new substances into each member of the pair. Cell division in the eggs of the sea urchin was similar. The process normally took place after fertilisation but could be initiated by the introduction of chemicals into the animal's environment. Both changes were, Freud said, the result of the influx of fresh amounts of stimulus, a view that:

> tallies well with the hypothesis that the life process of the individual leads for internal reasons to an abolition of chemical tensions, that is to say, to death, whereas union with the living substance of a different individual increases those tensions, introducing what might be described as fresh 'vital differences' which must then be lived off. (op. cit., p.55)

Freud now put forward the most astonishing of his paradoxes:

> The dominating tendency of all mental life, and perhaps of nervous life in general, is the effort to reduce, to keep constant or to remove internal tension due to stimuli ... a tendency which finds expression in the pleasure principle; and *our recognition of that fact is one of our strongest reasons for believing in the existence of death instincts.* (op. cit., pp.55-56. My emphasis, MBM)

The pleasure principle turned out to be entirely in the service of the death instincts! Life instincts introduced unpleasurable tensions - pleasurable relief was caused as the tensions were eliminated by the death instincts. Freud did not seem to notice that the conclusion had two very untoward consequences: the *life* instincts were immediately removed from the domain of the pleasure principle and it had become unclear in what sense the various repetitions were *beyond* the pleasure principle.

Despite his advocacy, Freud had shown only the death instinct had the character of a compulsion to repeat. That attribute was not possessed by the sexual instinctual drive. True, the two germ-cells involved in sexual reproduction could be thought of as re-enacting the beginning of organic life, but the "essence" of sexual life was a coalescence of two cell bodies that did not seem to repeat an earlier event (op. cit., p.56). In whatever way the tendency to coalesce had come about (for example, along Darwinian lines following a first chance conjugation) the life instincts must have already been in existence by the time it happened. Freud therefore recognised that the retention of the death instinct hypothesis was at the cost of admitting the life instincts had been present from the beginning (op. cit., pp.56-57).

At best Freud had found equivocal support in biology for the complete applicability of his initial premise. He consequently turned to philosophy, there finding a legend that attributed sexual drives to the need to restore an earlier state. In Plato's *Symposium*, Aristophanes was made to tell a story of how homo- and hetero-sexual forms of attraction had arisen. Originally there had been three types of living creatures: men, women, and a union of the two. Each of these creatures was doubled, possessing two heads, two sets of limbs, and so on. Zeus had one day cut these primaeval beings in two, whereupon each half had come to desire reunion - one half-man with its other half, one half-woman with its counterpart, and each now single man or woman with its mate. Freud asked whether or not one should follow the lead given by Plato:

> and venture upon the hypothesis that living substance at the time of its coming to life was torn apart into small particles, which have ever since endeavoured to reunite through the sexual instincts? that these instincts, in which the chemical affinity of inanimate matter persisted, gradually succeeded, as they developed through the kingdom of the [protozoa] in overcoming the difficulties put in the way of that endeavour by an environment charged with dangerous stimuli - stimuli which compelled them to form a protective cortical layer? that these splintered fragments of living substance in this way attained a multicellular condition and finally transferred the instinct for re-uniting, in the most highly concentrated form, to the germ-cells? (op. cit., p.58)

At this point Freud broke off his speculations about what it was that the sexual drive sought to restore.

The grand thesis Although Freud's following Plato's hint might not be thought worth taking seriously, there are two things of considerable importance to which it leads. In the first place, it enables Freud to incorporate into a single thesis all of the developmental events he thought relevant: the historical acquisition of death and life instincts having conservative and restorative aims, the differentiation of the protective shield in elementary organisms, the formation of multicellular creatures, and the specialisation of germ-cells and soma. It also makes explicit another constraint: sexual instinctual drives could only appear simultaneously with the appearance of the first living organism and their aim had to be to restore an original unified state. Only a tension created by some catastrophic event that had wrenched them from a larger just-living whole could give them that aim. It was not enough that germ-cells "repeat the performance to which they owe their existence", or even that they soberly repeat a "chance conjunction". Neither of these actions could have generated the tension required by an instinctual drive; rather, they supposed such a tension to exist already.

Freud began his concluding summary of *Beyond the pleasure principle* by remarking that if the restoration of an earlier state was a universal characteristic of all instinctual drives, it was not a matter over which the pleasure principle at first had any control. Neither did it follow that

restoration and the pleasure principle were opposed. He conceded that he had "still to solve" the problem of the relation between instinctual repetition and the dominance of the pleasure principle. Adverting to the opening section of his theoretical analysis, Freud said that he had found that:

> one of the earliest and most important functions of the mental apparatus is to bind the instinctual impulses which impinge on it, to replace the primary process prevailing in them by the secondary process and convert their freely mobile cathectic energy into a mainly quiescent (tonic) cathexis. While this transformation is taking place no attention can be paid to the development of unpleasure; but this *does not* imply the suspension of the pleasure principle. On the contrary, the transformation occurs *on behalf of* the pleasure principle; *the binding in a preparatory act which introduces and assures the dominance of the pleasure principle.* (op. cit., p.62. Emphasis altered, MBM)

Here the pleasure principle appears to refer to tension reduction or discharge because, after describing how the subjective experience of the sexual act, "the greatest pleasure attainable by us", was "associated with a momentary extinction of a highly intensified excitation", Freud concluded:

> The binding of an instinctual impulse would be a preliminary function designed to prepare the excitation for its final elimination in the pleasure of discharge. (ibid.)

Freud had failed to clarify the relationships between the principles.

FREUD'S THESES EVALUATED

Evaluating Freud's attempt to establish that there was a death instinct capable of being the opponent of a fused sexual and ego-instinctual drive requires finding the answers to the following seven questions:

1. Were there classes of behaviour that lay in some sense beyond the pleasure principle and were they manifestations of a compulsion to repeat?
2. Had the compulsion to repeat an instinctual basis?
3. Were all instinctual drives historically acquired, conservative, and did they aim to restore an earlier state of affairs?
4. Did organisms die for internal reasons only?
5. What were binding and mastery? How were they related to instinctual processes?
6. What did it mean to say that the activities of the death instinct were *beyond* the pleasure principle?
7. How were the principles like those of pleasure and constancy related to each other?

I now take up these questions sequentially.

Beyond pleasure?

Freud's classifying repetitions in transference, traumatic neuroses, and children's play as violations of the pleasure principle was based on an entirely subjective clinical judgement. The subjectivity can be seen directly in his dismissal of the "familiar motive forces" yielding pleasure and indirectly through a comparison of his earlier and later explanations of traumatic and transference repetitions. We have seen that just at the point where the arguments about the alternative explanations in *Beyond the Pleasure Principle* seem most evenly balanced, if not actually against the need to postulate a new motive, Freud announced that his "maturer reflection" required the rejection of pleasure explanations and justified the hypothesis of a compulsion to repeat that overrode the pleasure principle (Freud, 1920a, p.23). No logical grounds are given for the choice and we can only conclude that it was determined by the subjective weighing of the attractiveness of the new against the plausibility of the old.

Transference repetition, in the sense with which we are concerned, was introduced in the discussion of Dora's case where it referred to her transference on to Freud of feelings that she had first felt towards significant people in her immediate past (Freud, 1905a, pp.116-120). Freud believed, for example, that during Dora's treatment her feelings of being sexually tempted by Herr K. had been revived and transferred on to him. Frightened that she might succumb in the same way that she had unconsciously wished to yield to Herr K., Dora had then broken off the treatment (op. cit., pp.70 and n.2, 74, 118-119). Because she had not expressed her feelings directly, Freud described Dora as having "*acted out* an essential part of her recollections and phantasies instead of reproducing it in the treatment" (op. cit., p.119). Transferences of this kind occurred inevitably and had to be detected and overcome because of the obstacles they raised to treatment; only after the transference had been resolved would the patient accept the analyst's account of the symptoms or, as Freud put it, arrive at "a sense of conviction of the validity of the connections which have been constructed during the analysis" (op. cit., p.117).

Over the next few years Freud came to consider that the most significant figures in transference were the immediate members of the patient's family, especially the parents (Freud, 1912a, p.100). At the same time he placed more and more emphasis on the substitute function of the acting out, on its function of replacing supposed memories, an emphasis that had the consequence of bringing the repetitive quality of the patient's actions more to the fore. Thus he noted that:

> the patient does not *remember* anything of what he has forgotten and repressed, but *acts* it out. He reproduced it not as a memory but as an action; he *repeats* it, without, of course, knowing that he is repeating it. (Freud, 1914c, p.150)

The examples Freud then gave were the same or very similar to those he cited later in *Beyond the Pleasure Principle*: the defiant child who became the defiant patient, the patient who complained that he could not succeed and who had originally felt hopeless in his infantile sexual researches, and so on (ibid.). The compulsive quality of these substitutes for remembering was also noted:

> As long as the patient is in the treatment he cannot escape from this *compulsion to repeat*; and in the end we understand that this is his way of remembering. (ibid. My emphasis, MBM)

Now, Freud did not consider here that the pleasure principle had been overthrown. Quite the opposite. His explanation was based on the *demands* of the pleasure principle:

> If someone's need for love is not entirely satisfied by reality, he is bound to approach every new person whom he meets with libidinal anticipatory ideas .... Thus it is a perfectly normal and intelligible thing that the libidinal cathexis of someone who is partly unsatisfied, a cathexis which is held ready in anticipation, should be directed as well to the figure of the doctor. (Freud, 1912a, p.100)

Repetition in the transference was due to these unsatisfied instinctual demands once again asserting themselves - a renaissance not only not at all inconsistent with the pleasure principle but positively demanded by it.

In *Beyond the Pleasure Principle* Freud did not even refer back to this original explanation. What he did was to re-describe the infantile sexual impulses in a way that made a pleasure-governed explanation virtually impossible. He now said that in the past the child's sexual impulses "could never, even long ago, have brought satisfaction" from the parent. Hence, those drives, having learned "no lesson" from their repeated lack of gratification, produced only unpleasure on revival (Freud, 1920a, pp.20, 21). Is this description of the infantile impulses consistent with the rest of the theory? Do some of the sexual impulses never achieve satisfaction of any sort? It seems unlikely. From the moment of feeding onward at least some of the needs are satisfied fully, others partially. The impulse involving the genitals, such as it is, must share in this satisfaction in some way (if only, for example, through a linkage to the parental imagos concerned in the gratification of the other component drives). Were there not even partial satisfaction, there would be no occasion for that repression of infantile incestuous wishes that Freud supposed to mark the end of active infantile sexual life. And, once repressed, of course, the wishes were immutable, able to learn nothing; forming part of the system *Ucs.*, "time does not change them in any way" (op. cit., p.28). Experiences of an unsatisfying kind could have no effect. Hope always triumphed over experience. To postulate that these infantile drives had never produced pleasure prior to their repression, or that the search for pleasure did not guide their adult strivings, is inconsistent with the theses on infantile sexual

satisfactions contained in *The Interpretation of Dreams*, the *Three Essays*, and with some of the early remarks in *Beyond the Pleasure Principle* itself.

While an alternative explanation for repetition in the traumatic neuroses was at least discussed in *Beyond the pleasure principle* before being dismissed, Freud glossed over the fact that it was actually an older explanation *of his very own* that he was rejecting. When originally discussing the similarities and differences between the traumatic neuroses of war and the ordinary psychoneuroses Freud had explained traumatic repetition by the mechanism of fixation. He said the war neuroses gave:

> a clear indication that a fixation to the moment of the traumatic accident lies at their root. These patients regularly repeat the traumatic situation in their dreams; where hysteriform attacks occur that admit of an analysis, we find that the attack corresponds to a complete transplanting of the patient into the traumatic situation. (Freud, 1916-1917, pp.274-275)

As we have seen, Freud later rejected this mechanism of fixation because he believed it did not explain why the trauma did not occupy the patient's mind during the day-time, its nightly intrusion thereby conflicting with the thesis that dreams attempted to fulfil wishes (Freud, 1920a, pp.13-14).

Absence of a day-time preoccupation is a strange argument. It could be as readily asked why the hysteric's day was not similarly taken up with reminiscences. However, according to Freud, the traumatic memories are repressed in the hysteric precisely because their revival would generate unpleasure. For as long as the repression is intact, the hysteric's memories can only manifest themselves as symptoms. A similarly based explanation holds for the traumatic neuroses of war: only during sleep, when repression-censorship is lowered, will the repressed memory of the trauma return. But to say that is to admit that the repressed consists of other than pleasure-seeking drives - a fundamental although implicit assumption of *The Interpretation of Dreams* and the *Three Essays*. Even though Freud says that a fixation-based explanation of the repetition of the trauma shows a misunderstanding of "the nature of dreams" (Freud, 1920a, p.13) it is clear that what he is really asserting is merely the assumption that only pleasure-seeking impulses can be repressed. And that is to assume, of course, what ought to be proved. Otherwise a fixation explanation is perfectly acceptable. The revival of the trauma would generate unpleasure, and only during the night-time relaxation of repression could it enter consciousness as a dream. We might also note the failure of the explanation that the repetition was a retrospective attempt at mastery. Why should mastery be attempted only in the dream? Here, as with the familiar motives, there is little or nothing in the logical sense to guide one in choosing between explanations based on the pleasure principle and those based on the compulsion to repeat.

Instinctual repetition?

Freud's attributing an instinctual basis to the compulsion to repeat is based on a similar subjective standpoint. Rather than argue that the compulsion had an instinctual foundation, Freud asserted it to be so. The compulsion, he said, proceeds "from the instinctual impulses ... probably inherent in the very nature of the instincts" (Freud, 1919d, p.238) and its manifestations "exhibit to a high degree an instinctual character" (Freud, 1920a, p.35). Any attempted justification of this characterisation through an appeal to the daemonic quality of the manifestations of the compulsion (that might, if the daemonic quality were clearly defined, provide a reasonable argument) founders on the fact that that 'quality' is nothing more than another description of the capacity of the compulsion to override the pleasure principle: "The manifestations ... when they act in opposition to the pleasure principle, give the appearance of some 'daemonic' force at work" (Freud, 1920a, p.35. Cf. Freud, 1919d, p.238). And, of course, the whole attribution simply *assumed* the repetitions were not governed by the pleasure principle.

Something about the behaviours impressed Freud enough for him to suppose they had an instinctual basis, but what it was we cannot be sure. Clearly it involved a subjective and possibly a very personal reaction rather than a logical analysis. When Freud first introduced the compulsion to repeat, in his essay *The Uncanny*, he at once linked it to instinctual processes. What that essay contained was not an analysis of instinct but descriptions of different kinds of repetitive phenomena, all of which allegedly created a feeling of uncanniness. In his discussion of dictionary meanings of the word 'uncanny' Freud mentioned its equivalence to 'daemonic' in both Arabic and Hebrew (Freud, 1919d, p.221). Could it have been through his further equating of 'daemonic' with 'instinctual' that uncanny repetitive behaviours came to be characterised as instinctual? Several times Freud expresses an opinion consonant with an equation of this sort; for example, in his picturing of the relation between the ego and the instinctual drives as a rider guiding his horse in the direction in which the horse wanted to go (Masson, 1985, Letter of 7.7.98. Cf. Freud, 1923b, p.25; 1933b, p.77). But probably the most famous instance is Freud's adoption of Groddeck's view that the mind contains a repository of impersonal forces, an 'it', through which we are lived. Freud adds "We have all had impressions of the same kind" (Freud, 1923b, p.23 and n.3).

Suppose, nevertheless, that we accept Freud's judgements and assertions, and grant that the repetitions were instinctually based. A problem remains of the sense here of the word 'instinctual'. Certainly the instinctual character of the compulsion to repeat is very different from the instinctual processes defined in *Instincts and their vicissitudes*. In no way does Freud argue that repetition of this kind results from the mental representative of some somatic process exerting pressure on the mental apparatus and having as its aim the reduction of a tensions created

continuously within the source of the drive. The compulsion to repeat, it is true, aims at eliminating an excess of excitation but it does not, as did the earlier defined instinctual drives, generate that tension itself. It operates on the tension caused by external trauma or threatening internal demands. Only in one respect, then, is the compulsion 'instinctual'.

Death as an instinctual drive The intimate mechanism of the death drive shares this deficiency of aim with the compulsion to repeat. Psychoanalytic critics have made much of the point when drawing attention to the fact that the death instinct and the outwardly directed aggressive drive, that some take to be independent of it, lack one or more of the defining attributes of source aim, object, and impetus or pressure (Fenichel, 1935/1954b; E. Bibring, 1936/1941; Loewenstein, 1940, and in Lussier, 1972; Simmel, 1944; Cohen, cited in Ostow, 1957; Pratt, 1958; Arlow, 1959; Brenner, 1971; Holt, 1975a, and in Dahl, 1968; Hanly, 1978; Fayek, 1980; Lowental, 1983; Downey, 1984).

Claiming that the source was relatively easy to identify, Gillespie (1971) partly dissented, although his description of it as "the entire body" even if correct, is a little vague. Nevertheless, along with H. Hartmann, Kris, and Loewenstein (1949), Cohen (in Ostow, 1957), and Arlow (1959), Gillespie specifically rejected the musculature as the source of an independent aggressive drive. Obviously the musculature can only ever be the *instrument* of a Freudian-type drive.

An aim has sometimes been allowed, usually some kind of tension reduction (Simmel, 1944; Arlow, 1959; Pleune, 1961; Lebovici and Diatkine, 1972), but beyond this the aim has been said to be very difficult to specify (H. Hartmann, Kris, and Loewenstein, 1949; Cohen, in Ostow, 1957). It is generally agreed that the aggressive drive results in pleasurable satisfaction, quite independent of any libidinal component with which it might be fused (H. Hartmann, Kris, and Loewenstein, 1949; Brenner, 1971; Sandler and Joffe, 1966. Cf. Slap, 1967) especially if there is feedback for successful completion of the act (Sandler, in Lussier, 1972). Cohen (in Ostow, 1957) has however questioned whether, as with libidinal satisfaction, the outcome is pleasurable.

What the mechanism of aggressive pleasure is, is not at all clear. The experimental and other evidence summarised by Holt (1975a) shows the tension reduction model to fit the aggressive drive even less satisfactorily than the sexual and hunger drives. Hanly (1978) also observes that while aggression involves a readiness to respond with a demand for action, it is a reflex-like reaction to an external releasing stimulus lacking the spontaneous and periodic quality of sexuality. Even those who, like Anna Freud (1972), defend the proposition that the earlier criteria of instinctual drive apply to the death instinct and the aggressive drive, nevertheless concede that very little is known about how they apply. Overall, there is, a

"considerable gap" between the clinical phenomena and the hypothetical source of the death instinct (Lowental, 1983).

These limitations, as E. Bibring (1936/1941) pointed out, reflect an essentially changed concept of instinctual drive. In *Beyond the Pleasure Principle* there is no longer a definite process having the aim of removing a state of excitation in the organ from which it originated. This has to be of course. Freud saw the death instinct as acting only to remove the tensions introduced by other drives, a view that has the necessary consequence of preventing the instinct from having a source like those of the life-drives. What is true of the death instinct must also be true of its outwardly directed aggressive manifestations. An aggressive drive deriving from the death instinct can have neither source nor aim. One even inclines to the view that the shadow of Freud's changed conceptualisation falls very darkly upon most of the aggressive drives that have been proposed as alternatives because they also differ markedly from Freud's original concept. If, however, the death drive were to be based on some active process within the organism, as the interpretation of the poisoning of the infusoria and the vague parallel with catabolism begin to suggest, one would have created the not inconsiderable conceptual difficulty of having one active process abolishing a set of tensions by introducing another.

Trauma and pleasure At this point we must return to consider the assumption that the repetitions are not governed by the pleasure principle. Trauma, whether arising from external events or from inner demands, causes an excess of stimuli to flood into the mental apparatus where it has to be mastered by being bound. Binding *is* the conversion of freely mobile cathectic energy into mainly quiescent tonic cathexis (Freud, 1920a, pp.31, 62). While binding is being accomplished "no attention can be paid to the development of unpleasure" (op. cit., p.62). Traumatic dreams are repeated because binding requires the development of the anxiety not present at the time of the unexpected, original traumatic incident (op. cit., p.32). Repetition is also required to master or bind the excessive, freely mobile excitation belonging to the memory traces involved in transference repetition and in children's play (op. cit., pp.34-36). Bound energy is finally eliminated "in the pleasure of discharge" (op. cit., p.62), an end result reflecting the dominance of the pleasure principle. But must not this be equally true of the preliminary to discharge, the act of binding itself? Freud remarked that there seemed to be "no doubt" that unbound processes caused more intense feelings "in both directions" than their bound counterparts (op. cit., p.63). Although binding of the traumatically produced excitation would raise the level of tonic excitation, the overall level would necessarily be reduced and, with it, unpleasure would also be reduced. Clearly the transformation would be governed by the pleasure principle. The reduction would then have the consequence of placing the compulsion to repeat in the service of the pleasure principle - in fact there would be no need to formulate a separate mechanism to explain the repetitions at all. We shall see in the next section that many of the

criticisms of the compulsion to repeat are based on a view rather like this, although the critics do not support it with any explicit or convincing argument. At least it can be said that the critics have capitalised on Freud's failure to establish an instinctual basis for the compulsion to repeat.

The superficial identity of the earlier and later meanings of the term 'instinctual', given by the incorporation into both of a tendency to reduce tension, seems to be *the* reason for Freud's thesis that all instinctual drives seek to restore an earlier state having any plausibility. Elimination of tension necessarily, and in this case trivially, restored the pre-tension state. Because the death instinct, the compulsion to repeat, Eros, and the instinctual drives as defined earlier are all pictured as eliminating a tension, they all *seem* to share the goal of restoration. But, once again, this is only appearance: in *Beyond the Pleasure Principle* it is only the death instinct and the compulsion to repeat that eliminate tension. Unlike the earlier instinctual drives, even the actions motivated by Eros cannot dispose of its excitation. If the death drive and the compulsion to repeat - as well as Eros for that matter - are 'instinctual' it must be in a manner quite different from other instinctual drives. The vagueness of this difference is the real meaning of E. Bibring's (1936/1941) comment that Freud's revised concept of instinctual drive was that of "a directive or directed 'something' which guided the life processes in a certain direction". Introducing a 'something' hardly suggests greater precision and leaves quite obscure the sense in which the compulsion is related to the death instinct.

Before turning to the third of our questions, I should say that I will not discuss the problems posed by the apparent lack of difference between the conservative and restorative tendencies. Freud must have meant these tendencies to be different - the dictionary meanings alone see to it that they are - yet he gave no instances of conservation that were not, at the same time, attempts to restore an earlier state.

When the instinctual drives are considered on Freud's own terms, the hypothesis that they have been acquired historically, are conservative, and act to restore an earlier state, does not hold for all of them. Freud himself was dissatisfied with his attempt to show how the sexual component of Eros had been acquired, which meant also that he could not identify any state that it sought to restore. It is worth noting that there are the same failures in his account of the ego-drives fused within Eros. In fact, when pursuing the logical conclusions of his first premise, Freud made no attempt at all to enquire whether the ego-drives were covered by it. On the positive side, Freud did feel it held for the death instinct, although the only concrete example of a drive based upon it that he could produce was, as he put it, a 'displaced' one. On logical grounds, even this very limited validation can be disputed. Freud's argument was that at the very instant that the life-creating tension was introduced into inanimate matter a counter-tendency attempting to cancel that tension was also created (Freud, 1920a, p.38). Here he clearly assumed that something like Newton's law of action

and reaction applied in the organic sphere. But this assumed what had to be proved. Why should the force that created living matter behave in the same way as a physical force in the inanimate realm or a tension 'introduced' into an already existing organism? But, suppose there were a counter-tendency. Freud had to make the further assumption that it was *stronger* than the very force that created it. If death were to predominate over life, the movement to cancel new internal tensions had to be greater than those tensions themselves. Equivalent force and counter-force results only in balanced life and death instincts. This is, of course, another version of the problem of the mechanism of the death instinct: if that drive acts merely by cancelling the tensions introduced by Eros, how does it come to be the stronger? To ensure that death was the more powerful, that it did win out eventually, Freud had to assume, unlike Newton, a convenient asymmetry of action and reaction. Even for the death instinct, then, Freud's arguments are not especially convincing.

Historical acquisition?

Freud's failures to show what the instinctual drives repeated are consonant with the lack of evidence of a more general trend for such biologically determined phenomena as migration, heredity, and embryological development to manifest repetitions of earlier stages. According to Ernest Jones (1953-1957, *III*, p.277), Brun's 1926 review of the biological literature failed to reveal support for the death instinct, and the repetition theme was one of those he considered. Over thirty years later, Pratt (1958) cited devastating refutations, many of them dating from the 1900's, of the recapitulation "law" Freud used in this aspect of his theorising. In calling on this same "law" to support his thesis that Eros tried to re-establish an earlier hermaphroditic state, Needles (1962) seems to have been unaware of its total rejection by modern biologists.

An internal death?

Neither are Freud's arguments in favour of an internal death any more compelling. Indeed, there is a quite fatal error in his interpretation of the experiments on the successive generations of protozoa. By attributing the deaths of single-celled organisms kept in nutrient admixed with their own waste products to internal factors, Freud had implicitly redefined the organism to include part of its environment. The re-definition subtly shifted attention from processes internal to the organism to those mediating its interaction with the outside world. What were once external factors were necessarily transformed into internal ones. The absurd consequence of this redefinition is, as Silberer (1921) immediately pointed out, that one would have to attribute the death of a deep-sea diver suffocated by his own carbon dioxide in a malfunctioning diving bell to internal factors. Now, if it is illogical to conclude that protozoa died for internal reasons, the observation that conjugation prevents senescence becomes irrelevant, both in

itself and as it purports to bear on the thesis that new substances introduced into the organism create tensions capable of countering the death instinct.

About Freud's preparedness to argue that even if protozoa were immortal they might be such simple organisms that any manifestations in them of the death instinct could be hidden by the life instincts, we first note that it is an "arbitrary application to biology", as Silberer (1921) called it, of the latent/manifest distinction of the dream theory. More importantly, it indicates an unseemly willingness to adopt a position impervious to empirical test. On this point Laplanche (1970/1976, p.110) has observed that it is just when the reader has "the impression that an examination of the various theses would end up refuting the existence of an *internal* tendency towards death", that Freud breaks off his argument to invoke a hidden metaphysical process consistent with his wished-for conclusion. Thus it is not quite true to say, as Pieper and Muslin (1961) have, that Freud simply denied Weismann's conclusions; rather he had put himself in a position from which none of the observations on protozoa could have had any relevance at all. Szasz's (1952) interpretation of Carrel's experiments on the immortality of chicken fibroblasts could have been disposed of in the same way. Nor would anything come from observations of multicelled organisms. As we have seen, in order to avoid the clear but unwanted conclusion that death was a *late* acquisition, Freud was just as ready to apply to manifest-hidden distinction to them. Only by misconstruing what little evidence there was could Freud claim that biological fact did not contradict a natural death and only by retreating to an empirically impervious position could he maintain even the possibility that his second premise held. Here, as elsewhere, it is hard not to agree with Simenauer (1985) that "the stance in which Freud put his arguments" about the death instinct suggests that he was "defending a doubtful proposition".

Finally it is worth remarking that the validity of neither premise is altered by it being true that inanimate things existed before living ones or even that the aim of life is death. First, there is no sense in which it can be held that these conclusions follow from the premises. Second, even if there were a logical connection, the conclusions are not entailed uniquely. Inanimate matter may be prior and death may be the 'aim' of life without there being any commitment to the theses that death is internally caused or that instinctual drives are acquired, conservative, and restorative.

Instinctual binding and mastery?

Turning now to the fifth question, it is not at all clear what Freud meant by 'binding' and 'mastery', the two processes most central to his conceptualisation of the consequences of trauma (Wilbur, 1941; Flugel, 1953; Holt, 1962). Were *Beyond the Pleasure Principle* the only thing to go on, binding and mastery would have to be classed as uncharacterised theoretical terms and those of Freud's explanations drawing on them would have to be dismissed. Little enlightenment is to be obtained from Freud's

other psychological works: neither term is defined explicitly, and their meanings have to be inferred from the relatively few places where they are used. However, using this method of contextual definition, even with the assistance of Holt's (1962) discussion of the distinction between bound and free cathexes and of the relevant sections of Laplanche and Pontalis (1967/1973), only profound ambiguities result. While it might seem possible to clarify the post-traumatic processes through a consideration of the ostensibly neurophysiological concepts of the *Project*, what is illuminated by that work is a contradiction so complete as to defy the possibility of an internally consistent explanation. The difficulty revolves around the question of binding: what is its mechanism and by which mental system is it carried out?

According to the *Project*, neuronal activity was governed by the principle of neuronal inertia, that is, neurones ordinarily divest themselves of excitation as soon as they are charged with it. When neurones did retain some or all of the charge they were said to be cathected and the energy so stored described as being bound, as having been transformed from a freely mobile state into a quiescent one, and as no longer pressing for discharge. A cathected neurone inhibited the transfer of excitation between other neurones because its own charge of energy attracted to itself the energy passing between the others. The greater its charge, the greater the tendency to diversion. Diversion through such a side-cathexis, as Freud called it, minimised or even inhibited transfer completely. What was true for a single neurone also held for a complete system of well-facilitated neurones possessing a more or less permanent store of energy such as Freud believed the ego to be. The ego formed out of those neurones connected with the sources of endogenous excitation. By repeatedly supplying energy to them, the instinctual sources created the facilitations and provided the store of energy for the secondary process function of inhibition-by-diversion. The primary process tendency to press for free discharge was thus prevented or attenuated by the cathexis of what Freud called the nucleus of neurones of the ego attracting incoming excitation to itself and binding it there (Freud, 1950/1954, *Project*, Part I, Sections 14 and 15).

Although by the time *Beyond the Pleasure Principle* was written Freud had long since overtly discarded his purportedly neurophysiological mode of theorising, it is clear that he thought similar mechanisms to be active in the binding that followed a traumatic event. However, what is missing from Freud's account of the traumatic event are even moderately intelligible descriptions of the stemming of the incoming flood of excitation, of the mechanism of binding, of the mental systems in which it occurs, and of the eventual disposal of the excess excitation.

Binding as damming Consider first the halting of the flow of traumatically produced excitation. Freud nowhere says that a counter-cathectic 'dam' is created to stop this flood of traumatic excitation. What he does do, in the paragraph immediately preceding the one on the traumatic neuroses, is to

outline such a mechanism for coping with *physical pain*. One therefore has the impression that he means it to apply to trauma also.

Let us assume that Freud means that something resembling the pain counter-cathexis is constructed in the traumatic neuroses. If a 'dam' made by charging the components of the mental apparatus lying in the path of the excitation with a higher level of energy were to bind the excitation, what need would there then be for the dream repetition of the trauma? Freud's answer could not be that the excitation would be gradually disposed of by successive repetitive discharge because, for him, repetition preceded binding. Repetition generated the anxiety which allowed binding to take place.

A comparison of the bases of the excitation in traumatic neuroses, children's play, and transference repetitions leads to a more reasonable answer. In transference and children's play, instinctual sources were supposed to cause excitation to well up continuously. In both instances, there would be what was virtually a permanent quantity demanding to be bound. In the traumatic neuroses, however, the trauma acted only the once, not even leaving behind the kinds of tissue and other changes that in pain continue to generate excitation in the absence of the stimulus itself. A store of excitation has to be created in the traumatic neuroses equivalent to the instinctual sources in the other repetitions. Binding in the traumatic neuroses is therefore best thought of as being of two kinds: first the creation of a 'dam' by an improvised counter-cathexis, the stemming of the flood of excitation, and a later binding through repetitive discharge. Clearly binding of the first kind does not require explanation by either the compulsion to repeat or the death instinct.

Binding by repetition It is only for the second kind of binding - binding through repetition - that new principles have even to be considered. How, then, is it carried out? Freud's description is so vague that it is possible to read it, as Holt (1962) does to some extent, as implying that it is somehow effected through an anticathectic *energy*. The use of the terms binding and cathection elsewhere oblige us to assume, however, that energy is supplied to some part of the mental apparatus that does the binding. Fleshing out the bones of *Beyond the Pleasure Principle* with some of the ideas from the *Project* suggests how this might be done. According to the *Project*, binding is initiated by the ego when external or internal excitation seeks to recathect the memory traces of an experience whose revival would cause unpleasure. The attempt liberates a small quantity of unpleasure which, after being experienced by the ego as anxiety, signals the ego to cathect or hyper-cathect those systems able to provide a side-cathexis. The ego takes the threatening cathexis into itself and, by so preventing its transfer to the memory traces, avoids unpleasure (Freud, 1950/1954, Part I, Sections 14 & 18, Part III, Section 3).

Now, if there were a store of excitation 'dammed up' in the traumatic neuroses its effects, although not periodic, would be like those of an

ordinary instinctual source, but more intense. Excitation attempting to escape the control of the counter-cathexis would seek to recathect the memory of the traumatic event. The attempt would generate anxiety and the ego would act to re-establish control. During the day, when the ego was fully cathected, one would suppose this mode of defence to succeed each time. Only with the normal withdrawal of the ego's cathexes during sleep would the traumatic memory be recathected completely. Considerable anxiety would be generated, the hallucinating sleeper would wake, and the discharge be terminated. Any excitation taken up by the ego in the process would necessarily be small in quantity and readily disposed of in associative activity. What this mechanism seems to explain admirably is that the memory is re-experienced, that the repetition occurs during sleep, and that those repeated partial discharges eventually result in the dream disappearing altogether. Traumatic dreams do have some tendency to fade in this way, a fact that has led some psycho-analysts to interpret dream repetition as an attempted self-cure.

Provided that one makes a number of other rather doubtful assumptions, this outline explanation can be readily extended to the repetition of children's play and transference. First, one has to assume that what is being repeated in those activities will also generate unpleasure. If we make that assumption, we account for Freud's insisting, against all the sense of his previous writing, that the attempted libidinal gratification of his patients had never produced pleasure in childhood. Second, one has to assume that there are special features about the transference that promote behavioural, day-time repetition. Third, it has to be assumed that although it is unpleasant, the child's repetitive play is not experienced as such. While the first of these assumptions may be only cavalier and not unreasonable at all, the second is clearly *ad hoc*, and the last flies in the face of observation.

My point applies to the explanation of all three repetitions. There is no conceptual gap to be filled by a compulsion to repeat. True, the repetitions are of unpleasant experiences, but they are nevertheless motivated by attempts at reducing an excess of excitation, reductions that have to be thought of as guided by the pleasure principle. The attempted re-cathection of the memory trace of the unpleasant experience succeeds only in the peculiar circumstance of the ego being weakened, caught off-guard so to speak, and even then the discharge is only a limited one. Repetition is thus brought about by the ego's interference with the more or less continuous pressure for discharge. Repetition indicates that all is well with the ego and that the pleasure principle is still able to ensure that binding is carried out. What I have criticised here is not what Freud said, of course, but what it seems reasonable to suppose that he meant. Should it be shown that my criticisms have been directed at a dam of straw, it must be remembered that Freud actually says nothing coherent about binding, mastery, or disposal. Either the compulsion to repeat is not at all necessary or we do not know how it fits into Freud's pseudo-explanations.

Death beyond pleasure?

In one sense our sixth question, that about the relation of the death instinct to the pleasure principle, is quickly answered: Freud began *Beyond the Pleasure Principle* by affirming an absolute opposition between them but ended it by placing the principle in the service of the instinct. Although this change in meaning is complex enough, Freud added further complexity by later reviving the opposition but finally leaving the question unresolved. It is necessary, therefore, to consider in some detail the shifts in his argument. Establishing what Freud really meant in this connection is no trifling semantic exercise. What is at issue is the very basis of the revised instinct theory as well as a good deal of the structural theory that follows it.

Freud begins by arguing that the pleasure principle is violated by the repetition of certain unpleasant experiences, experiences that reveal tendencies beyond the principle, tendencies "more primitive than it, and independent of it" (Freud, 1920a, p.17). Here one naturally takes Freud to mean that the tendencies really do oppose the pleasure principle or at least set it to one side. His further remarks justify this supposition. He asks us to take courage and assume that the behaviours could be governed by such a mechanism (op. cit., p.22), which, he goes on to hypothesise, would seem more primitive, elementary, and instinctual than the principle "which it over-rides" (op. cit., p.23). 'Beyond' here means 'in opposition to'.

Our understanding is soon shaken. Repetitive dreams reveal the presence of a function that although independent of and more primitive than the pleasure principle does not contradict it (op. cit., p.32), or even oppose it, but simply disregards it and then only "to some extent" (op. cit., p.35). Freud then introduces another qualification, one we noted elsewhere, by saying that it is only when the compulsion acts against the pleasure principle that its daemonic quality is manifest (ibid.). The phrasing implies there might be circumstances under which there is no opposition. Transference phenomena are not so qualified - there the compulsion evidently disregards the pleasure principle "in every way" (op. cit., p.36). But then, when Freud discussed binding, the opposition is completely obliterated. Whether in connection with a traumatic event or the instinctual impulses in play and transference, binding is a preliminary act that *introduces* the pleasure principle in order to assure its dominance (op. cit., p.62). At the end of his search, Freud finds nothing beyond the pleasure principle. Indeed, the pleasure principle had even been found to be *in the service of* the death instinct. Opposition to it by either the compulsion to repeat or the mechanism of binding could only be appearance, not reality.

Freud did not adhere closely to this identification of death with the pleasure principle. In *The economic problem of masochism* (Freud, 1924b), a qualified opposition seemed to be revived. Freud there distinguished three principles governing mental life: the reality principle, the pleasure

principle, and the Nirvana principle. The first was unchanged in meaning, the second was considerably modified, and the last was new. The term 'Nirvana principle' was adopted from Barbara Low (1920, p.75), who had used it to describe the desire of the newborn to return to its mother's womb, where all its wishes had been fulfilled. Freud gave it an altogether different meaning. The Nirvana principle was the psychological equivalent of the principle of neuronal inertia, the tendency of the mental apparatus to reduce to nothing or to keep as low as possible the quantities of excitation flowing in upon it (Freud, 1924b, p.159). Freud explicitly aligned the Nirvana principle with the death instinct and the pleasure principle with the libido. Although, according to the revised instinct theory, the death instinct and Eros had to be in real opposition, the conflict between the principles expressing them was muted. The reality and pleasure principles were but modifications of the Nirvana principle: the former brought about by the demands of reality, the latter because the life instincts had seized upon a share in the regulation of the processes of life. These origins meant that:

> None of these three principles is actually put out of action by another. As a rule they are able to tolerate one another, although conflicts are bound to arise occasionally from the fact of the differing aims that are set for each. (op. cit., p.161)

In the two other places where Freud mentioned the matter explicitly, an antagonistic relation is again portrayed, and rather more strongly (Freud, 1923c, pp.117-118; 1933b, p.106). However, in his very last remarks, Freud again left the question open:

> The consideration that the pleasure principle demands a reduction, at bottom the extinction perhaps, of the tensions of instinctual needs (that is, *Nirvana*) leads to *the still unassessed relations between the pleasure principle and the two primal forces, Eros and the death instinct.* (Freud, 1940a, p.198. Last emphasis mine, MBM)

Ultimately the sense in which the activities of the death instinct were *beyond* the pleasure principle was not clear. This is obvious despite the glib description by Ducey and Galinsky (1973) of Freud's position in *The economic problem of masochism* as one from which "he never deviated".

Principled relations?

When trying to understand how the principles of pleasure, constancy, and inertia are related to one another, and so answering our last question, the first thing to note is the fundamental contradiction in all of Freud's formulations of the dominant tendency of mental life. Consider, for example, how it was put towards the end of *Beyond the Pleasure Principle*:

> The dominating tendency of mental life, and perhaps of nervous life in general, is the effort *to reduce, to keep constant* or *to remove* internal tension due to stimuli. (Freud, 1920a, pp.55-56. My emphasis, MBM)

Reducing excitation, keeping it constant, and removing it are not

synonymous but, when treated here and elsewhere as if they were, the pleasure principle, which corresponds to the *reduction* of stimulation, is necessarily equated with two different principles, one that *maintains* a constant level of excitation and one that *removes* the excitation completely. Once the principles have been equated it is impossible to derive oppositions between them. Nor can it be the case that the instinctual tendencies expressed by the principles are different from one another.

Neural confusion

How had this conceptual impasse, if that is what it is, been reached? And does it have a wider significance? I shall argue that Freud's first step, an imperceptible one at the time, was taken in the *Project* when he severed the tension reduction function from the maintenance of a low level of excitation thereby transforming the reduction function into a primary tendency that now tried to dispose of excitation completely. Freud took the second step in *Formulations on two principles of mental functioning* when, as a consequence of the introduction of the concept of narcissism, the sphere of influence of the pleasure principle was extended to regulate excitation arising from *external* as well as internal sources. Little distinction could thenceforth be made between the principles of pleasure and inertia. The impasse was reached in *Beyond the pleasure principle* where the restriction of the death instinct to removing tensions introduced by other drives necessarily equated the aim of the death drive with those of the principles of pleasure and inertia. I shall now examine these three steps in some detail. In doing so, I have explicitly adopted a vocabulary that equates the pleasure principle with reductions in momentary increases in excitation, the constancy principle with the maintenance of some level of excitation, and the principle of inertia or Nirvana principle with the elimination of excitation altogether. While this does some small violence to Freud's usage, it allows for a considerable clarification of the issues.

In Chapter 7 I pointed out that by 1888 Freud had analysed hysterical symptoms in terms of increases in the stable amounts of excitation distributed over the nervous system. By the end of 1892, in his correspondence to Breuer and the sketches for the *Preliminary Communication*, the nervous system was envisaged as maintaining some more or less constant level of excitation by reducing or disposing of those quantities of excitation that increased that level:

> The nervous system endeavours to keep constant something in its functional relations that we may describe as the 'sum of excitation'. It puts this precondition of health into effect by disposing associatively of every sensible accretion of excitation or by discharging it. (Freud, 1892, pp.153-154. Emphasis altered, MBM)

Although this self-regulating view of the nervous system is set out most explicitly by Breuer in his contribution to the *Studies on Hysteria* (Breuer and Freud, 1895, pp.198-201), it obviously also underlies Freud's lecture of

January, 1893 (Freud, 1893a, pp.36-37) and several other of his works. From the paper comparing organic and hysterical paralyses, as well as from the synoptic statement of the mechanism of the neuroses contained in Draft D of the Fliess correspondence, and the discussion of the effects of exogenous excitation in Draft E, it is clear that throughout 1893 and the first half of 1894 Freud continued to hold the view that neuroses were disturbances of equilibrium brought about by surplus quantities of excitation that could not be dealt with by the normal mechanism of reducing the excitation to its previous level (Freud, 1893b, pp.171-172; Masson, 1985, Draft D, possibly of May, 1894 and Draft E, possibly of June, 1894). But, during the five and a half years beginning about mid-1894, the particular self-regulatory model implied in these later sources disappears almost completely from Freud's writings. What replaces it is only partly compatible with the older view of the nervous system.

It is in Freud's *Project* that we first find the rather different conception of the nervous system. From the Fliess correspondence it is evident that from April to September, 1895 Freud was consumed with developing a theory of mental functioning based on quantitative considerations, "a sort of economics of nerve forces" (Masson, 1985, Letter of 25.5.95. Cf. Letters of 27.4.95, 12.6.95, 6.8.95, and 16.8.95). His aim was to represent psychological processes as determinate states of specifiable material particles. The particles were to be the neurones and a quantity of some kind subject to the general laws of motion was to distinguish the system's activity from rest (Freud, 1950/1954, *Project*, Part I, Section 1). Freud first focussed attention on the movement of excitation within the nervous system. "Processes such as stimulus, substitution, conversion, and discharge" had, he said, "directly suggested the conception of neuronal excitation as quantity in a state of flow". The movement of quantity was explained by assuming that each neuronal element of the system had a tendency to divest itself of the quantities of energy that impinged on it. This principle of inertia, as Freud called it, explained movement within the system as a whole by movement within each of the system's components. The first conclusion Freud drew was that the principle of inertia made the reflex intelligible: "the principle provides the motive for reflex movement". The difference between this conception and Breuer's is obvious. No longer was the reflex a simple mediator of the organism's relation with its environment, as it did with breathing and thermo-regulation. Removal of excitation was now the primary aim: "this discharge represents the primary function of the nervous system".

A further or secondary function was also assumed, one from which Freud tried to explain how the specific actions leading to drive satisfaction were selected. This secondary function obliged the nervous system "to abandon its original trend to inertia (that is, to bringing the level [of quantity] to zero)" and to maintain a store of quantity. Nevertheless, the original trend persisted:

> modified into an endeavour at least to keep the [quantity] as low as possible and to guard against any increase of it - that is, to keep it constant. (Freud, 1950/1954, Part I, Section 1)

Constancy, the maintenance of equilibrium in the face of internal demands, was now a secondary acquisition, forced upon a reluctant organism by those exigencies of life known as the instinctual drives. Because, Freud said, we have "certain knowledge" of the trend to avoid unpleasure, he was "tempted to identify" what later became known as the pleasure principle with "the primary trend towards inertia". In that case:

> *unpleasure* would have to be regarded as coinciding with a raising of the level of [quantity] or an increasing quantitative pressure .... Pleasure would be the sensation of discharge. (op. cit., Section 8)

In the *Project* Freud kept the arenas in which the principles operated quite distinct: inertia controlled the primary process, constancy was a secondary process phenomenon, and the pleasure principle operated only on the internal sensations arising from endogenous sources of excitation. Any confusion that might have arisen because of their common origin was minimal. So also in *The Interpretation of Dreams*, where the same basic ideas, shorn of their pseudo-physiological referents, were incorporated into the theory of the mental apparatus. Inertia, although not so named, was the primary principle; at first the apparatus tried to keep itself "so far as possible free from stimuli" (Freud, 1900, p.565). Releases of pleasure and unpleasure automatically regulated "the course of cathectic processes" from within (op. cit., p.574. Emphasis removed, MBM), and the storage of a quantity of excitation, apparently maintained at a constant level, was the province of the secondary process (op. cit., pp.598-599). It is also because the principles are kept distinct that, despite the differences between this model and the earlier one, a certain compatibility was maintained.

Narcissistic confusion

The first signs of confusion follow Freud's introduction of the concept of narcissism. It was then that he made the pleasure principle equivalent to the principle of inertia. In the *Formulations on the two principles of mental functioning* "the governing purpose" obeyed by the very earliest mental processes was "the pleasure principle". And, as if to emphasise the point, Freud introduced the term for the first time (Freud, 1911b, p.219). The initial "state of psychical rest" had been "originally disturbed", it was now said, by internal instinctual drives rather than by external stimulation (ibid.). Motor discharge, once the exemplar of functions regulated by the principle of inertia, was now said to exercise its duty of "unburdening the mental apparatus of accretions of stimuli" by means of the pleasure principle (op. cit., p.221). Freud continued this line of thought in *Instincts and their vicissitudes*. When he there derived the opposition between love and hate, Freud gave equal weight to stimuli from the external world as sources of unpleasure as previously given those from inside. The pleasure

principle was extended to regulate both. Because of the pleasure principle love and hate became opposites (Freud, 1915a, p.138). In primal narcissism the coincidence of the polarities pleasure-unpleasure and ego-external was responsible for the correspondence of the ego with what was pleasurable and the external world with what was unpleasurable (op. cit., p.134). And only the pleasure principle could have been responsible for the identity between "the external world, objects, and what is hated" (op. cit., p.136). The mental apparatus was automatically regulated by "feelings belonging to the pleasure-unpleasure series", a regulation that reflected "the manner in which the process of mastering stimuli takes place" (op. cit., p.120). Here the phrase 'mastering stimuli' summarised in general terms the three functions of reducing or disposing of increases of excitation, maintaining some low level of excitation, and removing excitation altogether (ibid.). What had happened, therefore, was that the pleasure principle had been made responsible for all three of these functions including, most significantly, the principle of inertia itself.

As if to deal with any residual ambiguities about the role of the pleasure principle, Freud's next major reference to the topic in the *Introductory Lectures on Psycho-analysis* was especially clear:

> We may ask whether in the operation of our mental apparatus *a main purpose* can be detected, and we may reply as a first approximation that that purpose is directed to obtaining pleasure. It seems as though *our total mental activity* is directed towards achieving pleasure and avoiding unpleasure - *that it is automatically regulated by the pleasure principle.* (Freud, 1916-1917, p.356. Emphasis altered, MBM)

Once again the "more general way" of describing this main purpose was to say that it involved the mastery and disposal of "the amounts of stimulus and sums of excitation that impinge ... *from outside and inside* (op. cit., pp.356-357. My emphasis, MBM). Although putting the matter in this general way did not, as Freud said, stress the acquisition of pleasure, he indicated the minimal difference between the two definitions by saying that what, from the quantitative or economic viewpoint, was mastery of amounts of excitation and control of their accumulation was, from the qualitative point of view, "an endeavour to obtain pleasure and avoid unpleasure" (op. cit., p.375). Now it was impossible to distinguish between the principles of pleasure and inertia.

A principle of inertia, more fundamental than either the principles of constancy or pleasure, and not the original one was once again placed in the foreground in *Beyond the pleasure principle*. The reversion is most readily understood as an accompaniment to the aim of the death instinct in reducing animate matter to an unstimulable inanimate state. A primary tendency to avoid stimulation altogether could now be held responsible for the removal of excitation. The change was also a partial consequence of the eclipse of the first theory of instinctual drives. No longer was it necessary to derive an opposition between love and hate. That had become

a primary fact in the new theory and the role of the pleasure principle could be correspondingly diminished in importance. With inertia placed more centrally, Freud could now allow constancy and pleasure to resume something like the roles they had had in the *Project* and *The Interpretation of Dreams*. Indeed, the very term "principle of constancy" was brought out of storage for the first time in over twenty-five years. Once more it was responsible for maintaining some degree of tension. Once more it endeavoured "to keep the quantity of excitation present ... as low as possible or at least to keep it constant" (Freud, 1920a, p.9). And, once more, the pleasure principle had the role of regulating the course of mental events. Invariably these were set in motion by an unpleasurable tension but the direction given them resulted in a final outcome of lowered tension - unpleasure was thus avoided and pleasure produced (op. cit., p.7). But, unlike the references in the *Formulations* and the *Introductory Lectures*, the pleasure principle was not described here as a governing or main tendency of mental life.

Although Freud set out the principles of constancy and pleasure on the first three pages of *Beyond the Pleasure Principle*, he delayed reintroducing that of inertia until some eight or nine pages from its end, after the argument for the death instinct had been closed. I think it is not an accident that the precise point of its reappearance is immediately after Freud had interpreted conjugation as introducing new tensions and so countering the tendency of the death instinct to cancel all tensions (op. cit., pp.55-56). The way the principle was reintroduced is also worth examining: it is to be found as part of a redefinition of what Freud called "the dominating tendency of mental life, and perhaps of nervous life in general", a definition that makes no explicit reference to the pleasure principle. The tendency consisted of the efforts "to reduce, to keep constant or to *remove internal tension due to stimuli*" (ibid. My emphasis, MBM). Elimination of excitation (removal) was now included alongside the maintenance and reduction functions. Nor could Freud have done it in any other way. This tendency to remove tension was not quite the same as the original. Had it been, the other two principles might have been deduced from it in the same relatively uncomplicated way as in the *Project* and *The Interpretation of Dreams*. However, the new concept of instinctual drive made those derivations impossible. Elimination of excitation was now much more than a simple expression of inertia - it was *the* mode of action of the death instinct.

Further, because the death instinct acted only in this way, and because it was the pleasure principle that regulated the reduction of tension, it followed that the pleasure principle had no control over the libido, that it regulated only the activities of the death instinct. Freud was therefore practically driven to that paradoxical conclusion that one of his "strongest reasons" for believing in the death instincts was that inertia expressed itself in the pleasure principle (ibid.) and that even the pleasure of orgasm could be interpreted as coming under the death instincts. "The greatest

pleasure attainable by us", as he described it, was associated with "a momentary extinction of a highly intensified excitation" that reflected the "final elimination" of that tension "in the pleasure of discharge" (op. cit., p.62). Freud was thus forced to conclude that the pleasure principle was a tendency operating in the service of a function concerned with "the most universal endeavour of all living substance - namely to return to the quiescence of the inorganic world" (ibid.).

In *The economic problem of masochism*, Freud rejected this "unhesitating" identification of the principle of inertia with a pleasure principle that had allowed Eros to escape its control: "such a view cannot be correct" (Freud, 1924b, p.160). But Freud's belated correction was at an enormous theoretical cost. First, he had to jettison one of his oldest assumptions, namely, that increases and decreases in quantities of excitation caused feelings of pleasure and unpleasure. While still claiming that such quantitative variation had "a great deal" to do with those feelings, pleasure and unpleasure really depended upon some other unknown characteristic "which we can only describe as a qualitative one" (ibid.). Second, Freud tried vainly to align the principle of inertia with the death instinct and the pleasure principle with the life instincts. The life instincts were assigned the role of modifying the principle of inertia, now properly called the Nirvana principle, into the pleasure principle. They had "seized upon a share in the regulation of the processes of life" (ibid.). What that seizure consisted of, and how it brought about the modification, was no more described than the qualitative peculiarities in the variation of excitation that were now supposed to produce pleasure and unpleasure. The correction had placed completely uncharacterised processes at the centre of the theory. Explanations drawing on the pleasure-unpleasure relation or that involved the differential regulation of Eros and the death instinct could be nothing more than shadows of explanations.

Solution by reformulation?

Is a way out of the impasse provided by adopting one or other of the reformulations of the principles, the instincts, or the relations between them that have been suggested by other psycho-analytic theorists? For example, is anything to be gained by separating the instincts from the principles? Although we found no particularly close connection, Loewenstein (1940) said Freud's formulations of the regulative principles were "hampered" by too close an association with the instincts, and there have been two attempts, although only partial, to prise them apart. Simmel (1944) distinguished Nirvana from the state produced by the destructive impulse and Katan (1966) similarly separated it from death but retained the connection of pleasure with sexuality. Ernest Jones' (1935-1936) very peculiar proposition had the compulsion to repeat damping down external stimuli and the pleasure principle attenuating internal instinctual drives. He did not accept the death instinct, of course, but if his suggestion were to be applied to a context including it, the effect would not only be to do away

with the evidence for something beyond the pleasure principle, but would make it difficult to conceptualise the internal processes involved in destructiveness. Apart from completely re-writing the theory of instinctual drives, Simmel's and Katan's revisions simply do not go far enough for one to estimate what their more limited consequences would be. The programmatic statement of Wolfenstein (1985) needs little consideration. He says he wants to argue for aggression and the Nirvana principle as derivatives of the clash between the pleasure and reality principles'', but I find nothing in his paper that constitutes more than assertion.

Reformulations of the three principles themselves have also been suggested, although obviously not for the reasons adduced above. Usually on the grounds that the phenomenology of the two affects differ (Jacobson, 1953), it has been proposed to differentiate a pleasure principle from an unpleasure principle (Eidelberg, 1960, 1962; Kanzer and Eidelberg, 1960; Schur, 1966a, pp.125-152, and in Gifford, 1964; Schur and Ritvo, 1970) and the need for the differentiation is at least implicit in the acknowledged facts that the physiological mechanisms of the two states are distinct, involving rather more than increases or decreases in the amount of excitation or some rhythmical quality of it (Brunswick, 1960; Needles, 1964, 1969; Rangell, 1967; Dahl, in Dahl, 1968). While this suggestion would probably resolve the Nirvana-death instinct difficulty, it would also require a fundamental recasting of the whole of Freudian theory. Every process logically dependent on the notion of tension reduction, including that of the wish as it is currently formulated would need to be thought out afresh (G. S. Klein, 1967; Holt, 1975a). Obviously any such recasting of the theory would have to dispose of Freud's abiological characterisation of the principle of inertia, both in itself and in relation to Freud's picture of the primitive organism. This abiological trend has frequently come under attack. H. Hartmann (1948), for example, indicted the tendency to reduce excitation to a minimal level and what he called the principle of constancy as creating non-adaptive equilibria. Followed through, this judgement requires both principles to be given up. Needles (1969) went part of the way along this path in separating the tendency of ''the psychic apparatus ... to avoid excitation of all sorts'' (which he called constancy) from the pleasure principle and abandoning the former.

Against this has to be weighed the attempts made by such writers as Alexander (cited in E. Jones, 1953-1957, *III*, p.276), Ostow (1958), and Saul (1958) to bolster the concepts of a death instinct and a principle of inertia by appeals to the second law of thermodynamics. The appeals seem to be quite misguided because they are similarly abiological. According to the law, the entropy of a system, usually expressed as a ratio of the quantity of heat in the system to its temperature, tends to increase with time. Entropy increases as energy redistributes itself within the system as an equilibrium is reached and would increase when living organism died. However the death instinct interpretation of the second law must be rejected. It rests on too odd a set of assumptions, including those that the

organism together with its environment constitutes a closed system rather than an open one, that mental energy is homologous with physical energy, and that either or both of Freud's Nirvana and Fechner's state of absolute stability is equivalent to a state of zero entropy (Penrose, 1931; Kapp, 1931; Spring, 1934; Szasz, 1952; Laplanche and Pontalis, 1967/1973, pp.341-347). As Penrose concluded, "Taking all these things into consideration it must be regarded as very doubtful whether the assertion that in the living organism entropy tends to increase is true, or even has any meaning at all". Brenner (1979) has since argued that simply because psychic energy is a *derivative* of a drive it cannot be treated as a physical energy. The belief that it can be "leads to such absurdities as trying to apply the law of entropy to psychology or to questioning whether the mind is what physicists call an open energy system of or a closed energy system". Were these problems to be overcome, there is, from Freud's point of view, a disastrous and unpleasant theoretical consequence, one which Bernfeld and Fietelberg (1931) point out in their generally sympathetic discussion of the death instinct and the second law. A death instinct deducible from the second law leaves no place for a counter-force like Eros: "The theory of energy has no cognizance of any partner, rival or opponent where the law of entropy is concerned." A successful appeal to the second law thus saves one instinct and one principle at the cost of having to discard all the others (Cf. Adrian, 1923).

We may note in passing that this is not to say that some of Fechner's ideas about the different kinds of stability might not be consistent with the second law - after all he was a physicist. However, his views on living systems were a good deal more complex than Freud's. Complete permanent immobility was only *one* of the four grades of stability that Fechner recognised; he did *not* believe that it applied to the biological realm; nor does it seem to have been the state to which processes governed by his version of the pleasure principle tended (Penrose, 1931; Flugel, 1953; Wilbur, 1941; Ellenberger, 1956; Laplanche and Pontalis, 1967/1973, pp.322-325). Consequently Foxe's (1943) rather curious analysis of the relation between Freud's and Fechner's principles may be disregarded, for it rests on the proposition that Fechner proposed only one state of stability. We may similarly dismiss Needles' (1962, 1964) attribution to Fechner of a 'constancy' principle in which stimulus tension was equated with unpleasure and stimulus extinction with pleasure.

Returning to my main theme, it seems to me that the most powerful of the critiques of the principles and their relation to the instincts is that which can be extracted from the thesis of Laplanche and Pontalis (1967/1973, pp.341-349) and later expanded by Laplanche (1970/1976, pp.112-126). According to them, the problems result from Freud's faulty translation of psychological observations into biological terms. They point out that the first theoretical antithesis in Freud's work is between conscious and unconscious processes. Characteristics of the latter - stimulus, substitution, conversion, discharge, and according to Laplanche and Pontalis,

displacement - suggested the conception of excitation as quantity in a state of flow, a conception which in turn suggested, or was explained just as directly by, the principle of neuronal inertia (Freud, 1950/1954, *Project*, Part I, Section 1). Secondary processes were determined by those modifications to the first principle which allowed for a store of excitation to be built up and maintained at a constant level. Discharge was thereby delayed. On the one side then, the principle of neuronal inertia, primary process, and freely mobile energy pressing for immediate discharge and on the other the principle of constancy, secondary process, and bound energy. This fundamental antithesis finds expression, they argue, as successive moments or syntheses as the basic contradiction works itself out during the development of Freud's theory: conscious versus unconscious, repressed versus non-repressed, ego versus sexuality, and life versus death. [2]

While some of the inconsistencies in Freud's theoretical statements reflect the very real difficulty of conceptualising such a complex dialectical process, there is also a real contradiction in the concepts themselves. Neuronal inertia, they point out, is an abiological concept, totally at variance with the requirements of any living system, and quite unsuited as a foundation stone for a general theory of psychology. No doubt motivated by the common nineteenth century tendency to extend the principles of physics as widely as possible, it is based on the faulty method of translating behavioural data directly into physiological terms. For example, displacement of ideas in dreams is called displacement of quantities of neural excitation and these are further translated into the physical terms of displacement of cathexes of physical energy. Once critical attention is focussed on this method of translation and the developmental sequence of Freud's thought, it is possible to resolve the problem of the principles in relation to the instincts. The regulation of the life instincts is to be attributed to the principle of constancy and the death instinct to the pleasure principle. Pleasure, Nirvana, and neuronal inertia all require the reduction of internal tension to zero. Resolution of the more fundamental contradiction requires abandoning those parts of Freud's theory that simply renamed psychological processes with physiological terms and rejecting the whole tendency to infer the purported biological characteristics of organisms from clinical observations of patients.

The primitive organism

One thing that would certainly not be missed were the latter suggestions adopted is Freud's theoretical fiction of the primitive organism being brought into existence by energies impinging from outside. It is not necessary to argue, as Levin (1951) and Holbrook (1971, pp.71-72) have done, that Freud's picture of the primitive organism surrounded on all sides by powerful and threatening energies reflects some personal peculiarity, perhaps even Freud's basic fear of the world. Support for the slogan that

2. At least that is what I think they mean - si ce n'est pas clair, c'est structuraliste.

protection from stimuli is more important than their reception (and its corollary of the organism's tendency to avoid stimuli) is to be found in views common among late nineteenth century writers on child psychology. Bernfeld (1925/1929) was to make this view central to his psycho-analytic account of the developmental process. Of course, at no time was it fully consonant with any wide range of observations on infants and children. Today, in the light of the many studies documenting the active tendency of very young infants to seek out stimulation, it merely seems quaint. Although Bernfeld, and Fenichel (1935/1954b) later, were able, not without difficulty, to reconcile this "craving for stimulus" with Freud's principles, most analysts and non-analysts alike have seen it as quite contrary to the organism's supposed tendency to avoid stimulation (Nuttin, 1956; Needles, 1962, 1964, 1969; Holt, 1975a). Even the nervous system does not act to divest itself of excitation - it is spontaneously active almost all the time, and its activity is modulated by external and internal stimulation (Holt, 1975a; McCarley and Hobson, 1977). No resolution of this matter is possible along the lines suggested by Shapiro (1981): "The concept of the stimulus barrier must also be complemented by knowledge about the infant's stimulus seeking propensity." Shapiro seems not to see that the mere recognition of that propensity is a fatal blow to Freud's basic premise.

What is demanded by these largely psycho-analytical criticisms of the principles and the life and death instincts is nothing less than a complete revision of the whole of Freud's theory. The only other way is to return to the earlier definition of instinctual drive and find a source, aim, and object for the destructive impulses, so allowing them to introduce tensions of their own that challenge the tensions generated by Eros. And that conceptualisation itself contradicts the most central thesis of *Beyond the Pleasure Principle*, the thesis that sought to find destructiveness in the very force that created life out of inanimate matter.

ALTERNATIVES TO DEATH INSTINCT EXPLANATIONS

Three questions have to be answered by those who reject the revised instinctual drive theory. What causes the repetition of unpleasurable behaviour? What are the sources of sadism and masochism? What is the basis of aggressiveness? The answers are relevant in different ways. Formally adequate alternative accounts of sadism, masochism, or aggressiveness do not challenge the death instinct. They simply restrict its explanatory scope. On the other hand, denying that the repetitions are peculiar questions the evidential base of the death instinct completely and so strikes at the very heart of Freud's theory. The alternatives are also worth examining because of the possibility that they resolve the conceptual difficulties we have so far encountered.

Alternative causes of repetition

There are two outstanding features of the psycho-analytic literature on the compulsion to repeat. The first is its one-sidedness - either some kind of more-or-less positive regulatory role is placed in the foreground or the more negative, even maladaptive, compulsive qualities are concentrated upon. Rarely are both aspects considered in relation to one another. The second is that theoretical positions on the compulsion to repeat are relatively independent of the role given the pleasure principle or even whether the theorist accepts the death instinct or not. Alexander's and W. Reich's responses to Freud's new concepts may serve as illustrations. Within a few years of the publication of *Beyond the pleasure principle*, Alexander (1925) took the compulsive quality to reveal a fixation upon methods of tension reduction and mastery. Although at that time he accepted the death instinct, he virtually dissociated repetition from it. It was not "in the strict sense of the term a repetition" but more a protracted attempt "to master stimuli or instinct-excitations". Reich's discussion was from the other position. Sceptical about the death instinct, he saw the compulsion as reflecting the adhesiveness of the libido, that is, its degree of attachment to previously encountered fixation points, and as expressing itself through the pleasure principle (W. Reich, 1926, 1932/1950).

Ego mastery or fixation? Ambiguities in the formulation of the role of the ego, in characterising the pressure of instinctual drive and traumatic stimuli, and in describing binding and mastery make these peculiarities possible. Where the emphasis is placed on pressure from the drive or the trauma, the fixated and conservative - usually instinctual - qualities tend to be given prominence. On the other hand, where the ego's role is stressed the compulsion is seen as aiming at mastery through binding and abreaction. There is also a shadowy middle ground, in which pressure and regulation are uneasily combined.

Waelder's two papers define the ends of the continuum. In the first he suggested that all psychic phenomena resulted from the combination of a *vis a tergo* of the instinctual forces with a pull from the ego. He found it difficult to decide how much each contributed to the repetitions; the two forces could be separated "only by abstraction" (Waelder, 1930/1936). The difficulty may have been the reason for his failing to analyse the two processes further in his later paper (Waelder, 1932/1933). The result was that the middle ground, which he himself took up, provided only a vague stage on which the other more one-sided interpretations figured.

Take as an example E. Bibring's (1943) very influential analysis. After identifying the two components in much the same way as Waelder, Bibring found some role for the ego although he concluded by adopting a fixation view. The seeds of this outcome had been laid in an earlier paper in which E. Bibring (1936/1941) had classed *all* of Freud's clinical examples of repetition as instances of what he called a tendency to abreaction in

fractional amounts. Rather strangely, he identified this tendency with a regulatory principle that arrested and bound stimulus energies by bringing them from a state of activity to a state of rest. Bibring implied that the ego worked according to this same regulative principle. In his second paper he distinguished two main meanings of the repetition compulsion. The first was "the expression of the 'inertia' of living matter, of the conservative trend to maintain and repeat intensive experiences" and the second was "a regulating mechanism with the task of discharging tensions caused by traumatic experiences after they have been bound, in fractional amounts" E. Bibring (1943). He related only the first, the "repetitive or reproductive tendency" to Freud's examples and then only to acting out in transference. None of the remainder of Freud's repetitions fell under it. Neither did they illustrate the second type of repetition, that devoted to the "restitution or re-establishing" of the pre-traumatic situation. In words reminiscent of Charcot, Janet, and Breuer, Bibring described counter-cathexis as "a foreign body ... placed in the psychic organism". The imprisoned excitation periodically broke through the counter-cathexis and was then successively discharged in fractional amounts until complete restitution had been achieved.

Although Bibring had inextricably linked the repetitive or reproductive function with the restitutive he was unable to develop his idea. He thought that what he called the "mechanical trend toward abreaction" seemed not to be consistent with active attempts at mastery and repetition and they did not always lead to tension reduction. He concluded that the compulsion was a property of the instinctual drives themselves that tended to fixate pleasurable and unpleasurable experiences *alike*. Fixation was a facilitation between the instinctual drive and the impressions occurring in association with it. Whenever the drive built up there was a tendency for the memory trace to be revived. The compulsion was thus "an instinctual automatism" and its only significant role was in "the maintenance of impressive experiences" irrespective of their pleasurable or unpleasurable qualities. The ego's role was restitutive. Its task was to find ways in which the compulsion could be managed. With the responsibility so allotted, Bibring then proposed dropping the phrase "restitutive tendency" altogether. Because he had not seen that the ordinary ego function of inhibition was all that was required for the fractional discharge of the restrained traumatic excitation, Bibring's analysis had ended in a complete separation of the two aspects of the function.

Fixation In slightly different ways, Hendrick (1934, p.300), French (1933, 1937), and Nunberg (1932, 1951, 1932/1955), who all took very different positions on the death instinct, also emphasised that the compulsion resulted from a previous fixation or facilitation. Indeed, French (1937) claimed that its more familiar manifestations were "best understood in terms of the conditioned reflex principle". Fenichel's explanation of transference repetitions also belongs here. Unpleasurable repetitions were due to a discharge or binding tendency "stronger than the pleasure principle"

(Fenichel, 1935/1954b). The individual strove again and again for satisfaction but "again and again the ego responds to this striving ... which at a former time caused anxiety" (Fenichel, 1938/1941b, p.69). Loewald (1971) similarly described the unconscious determinants of passive repetition as "automatic and autonomous".

Ego mastery By the middle 1930's the other trend, that represented by Alexander's position that repetition enabled the individual to gain some kind of mastery, gained momentum. Denying that there was any kind of "mechanical association" or "mechanical repetition", Schilder (1938, pp.86-87) claimed the repetitions of his patients were revivals of past situations in which they had previously felt threatened. Kubie's critique was even more emphatic. After a detailed analysis of Freud's examples he concluded that:

> there is neither any need nor any evidence for a 'repetition compulsion' - and that the phrase itself has become a mere descriptive epithet, a psychoanalytic version of the word habit. (Kubie, 1939)

He believed the concept to lack any kind of explanatory power and saw the repetitions as attempted solutions to past injuries. Without making reference to either Kubie or Schilder, both Wilbur (1941) and Lowenfeld (1941) adopted the mastery view to some degree.

Two papers by Hendrick, together with his letter in response to criticism of them, extended the mastery type of explanation by making an ego-instinctual drive for mastery responsible for the repetition of traumatic situations. Mature mastery skills broke down under frustration and anxiety and had to be relearned via repetitive practice (Hendrick, 1942, 1943a, 1943b). While neither Lagache (1953), M. M. Stern (1957), Pratt (1958), Waelder (1967a), Brenner (1971), nor Downey (1984) adopted Hendrick's particular thesis, each endorsed the repetitions as belated efforts to master needs or satisfy earlier unresolved tensions. Stern and Pratt specifically promoted the ego as the vehicle of these efforts, a view soon taken further by Kanzer (in Gifford, 1964), Malev (1969), and Segel (1969). This view can be aligned with Fenichel's (1945b, pp.42-46, 120-121) analysis of repetitions in children's play and traumatic neuroses which also implicated the ego. Embarrassingly enough, Kasanin (1944) then implicitly endorsed this interpretation for transference. Zetzel's (1956) review of concepts of transference recognised the same mechanism there as did Loewald (1971) some years later. Krystal (1978) seems to hold to a mastery view in proposing that the repetition of intense affects is motivated by the need to regain comfort in *having* them. Similarly, Rothstein (1979a) represents the child's play as anything but neurotic or motivated by the death instinct. According to him, the child "joyfully repeats his successful functioning" in controlling and presumably mastering the (symbolised) appearance and disappearance of the mother. But also within the framework of a mastery interpretation is Moses' (1978) very different conclusion that repetition has

part of its basis in the *warding off* of earlier painful experiences.

The whole ego-mastery trend was aptly summed up in Loewenstein's (1969) conclusion to his review of the literature of the previous fifty years on transference. The urge to remember, to master, to undo was, he said, "a powerful ego factor quite independent of the instinctual motivations to which we have been wont to ascribe repetition, reliving, re-enactment in the transference". Fourteen years later Juda began his review by saying:

> We no longer need to construe the repetition of earlier trauma as some sort of 'daemonic' manifestation of the psychic apparatus, nor as a vicissitude of our 'wish' to return to an organic state. (Juda, 1983)

Basing himself on a mixture of Kohut's concept of 'cohesive self' and Piaget's of 'schema', which he substitutes for that of 'protective shield', he added:

> once confronted with new, unassimilated and unaccommodated experience, the human being must relive this experience again and again *ad nauseam* until he has developed a new schema with which to understand it. (op. cit.)

Naturally he disagreed with Silverberg's characterisation of the compulsion as an "enduring monument of man's profound rebellion against reality and his stubborn persistence in the ways of immaturity". It would, he said, be "more fruitful to reconstrue the repetition compulsion as *primarily* a healthy function of the cohesive self".

We must note that the mastery positions are not without difficulties of their own. Dorey (1986) differentiates two distinct meanings of the German words that have been translated by the one word 'mastery'. Mastery-domination has the meaning of dominating or even destroying another, whereas mastery-assimilation places the emphasis on the assimilation of excitation through binding. Even so, when Freud comes to use the concept of an *instinctual drive* for mastery:

> On the one hand, Freud considers this instinct, having the aim of mastery-domination, as a transformation of the death instinct, and hence of destructive trends .... On the other hand, ... he invokes [it] as the driving force behind children's play, as 'the impulse to work over ... some over-powering experience' ... whereas, as few chapters further on, he describes this task of the psychic apparatus by the word [mastery-domination]. (Dorey, 1986)

Dorey suggests that Freud's "highly ambiguous" use of the concept results from his "desire to link mastery-domination to the action of a single instinct - specifically, the death instinct". A similar ambiguity is found in much contemporary psycho-analytic thinking. Thus Plaut (1984) believes Freud's dual instinct theory cannot account for mastery and argues that direct observations and clinical considerations make it necessary to recognise a *third* class of ego-instinctual drives which does. Distinct from

the libidinal and aggressive drives, the new class meet the criteria in having a source in the perceptual-motor system, an aim of directed motor activity, and an object in parts of the body. Plaut offers neither evidence nor argument for the existence of these essential defining characteristics. His one sentence listing of them is mere assertion as is his claim that H. Hartmann and his colleagues came close to a similar postulation.

Lipin (1963) showed the concept was flexible enough for even instinctual formulations to be brought into line with the mastery view. He regarded the compulsion as one of a number of instinctual drive representatives which, regulated by the principles of constancy and pleasure, produced maturational unfolding according to an innate genetic timetable. Repetitions of the kind considered by Freud were inhibitions and distortions of the activity of the compulsion. As Valenstein commented in the American Psychoanalytical Association panel discussion of Lipin's theses (in Gifford, 1964), this made the compulsion favour progression as well as regression. Gifford added: "Ironically the repetition compulsion Freud considered such important evidence for a death instinct has been transformed by Lipin into a phenomenon that has some characteristics of a life instinct". Cohen (1980) approaches Lipin's position for, although he rejects the death instinct and locates the repetition compulsion outside the pleasure principle, he views the compulsion as an analogue of the pleasure principle operating at a lower level of organisation: one of need rather than wish and one involving the primary process of the id rather than the secondary process of the ego. Greenacre's (1967) position is similar in that repetition compulsion results from a positive motivational factor.

Schur Schur's (1966a) analysis is mentioned separately because it is the most comprehensive attempt at clarification since E. Bibring's (1943) of nearly 25 years earlier. Originally Schur (1953) took a kind of fixation view: automatic reflex-like responses developed in the ego under the impact of danger and anxiety. Seven years later he seemed to stress the instinctual aspects by calling the repetitions manifestations of instinctive behaviour patterns released when the executive apparatuses are or become passive in relation to the drives (Schur, 1960). In slightly later panel discussions Schur implied that his concepts remained to be validated (Schur, in Gifford, 1964), and he separated drive-based from defensive repetition, arguing that if the compulsion was extended to the ego it might be given an adaptive-mastery role (Schur, in Blum, 1966).

In his major work, Schur (1966a, pp.129-145) brought these points to fruition by distinguishing between an unpleasure principle as a tendency to withdraw from excessive stimulation and a pleasure principle as a need to re-create situations of satisfaction by approaches to objects of gratification. Denying that concern over trauma was confined to night dreams, he claimed that when such dreams did occur they represented the ego's unconscious wish to undo the trauma, a wish requiring a re-enactment that necessarily generated anxiety (op. cit., pp.177-178). The dream also served

to repair the breach in the unpleasure principle caused by the original failure to withdraw from the stimulus. Traumatic dream repetition was not only not beyond the pleasure principle, it had absolutely no bearing on the death instinct (op. cit., pp.181-182). Nor were transference repetitions manifestations of the compulsion to repeat. Logically and historically it was impossible for the wishes behind them never to have brought pleasure and they remained attempts at satisfaction (op. cit., pp.182-184). Children's play had also been a bad example because Freud was clearly "of two minds" about it (op. cit., pp.189-190). Overall Schur concluded:

> none of Freud's psychological adductions used to substantiate his hypothesis of 'beyond the pleasure principle' and the concepts of the death instinct are valid ... all the examples cited by Freud - certain types of children's play; the reproduction of unpleasurable experiences in the analytic situation and in acting out; and, above all, traumatic dreams - can be explained within the framework of the pleasure and unpleasure principles. (op. cit., pp.192-193)

Schur had reached the same conclusion nearly forty years after Symons: the compulsion to repeat fell "within the sphere of the pleasure principle" (Symons, 1927).

Wallace (1982) has subsequently endorsed the essence of Schur's conclusion. Although he granted that Freud "correctly divined" the repetitive nature of the phenomena he cited, he had built "a very shaky edifice" upon them. Wallace believed the "correct explanation" of repetition was to be found in the "indestructibility" of unconscious wishes. Repetition was caused by their "incessant striving" - a striving which, as Schur had noted, was "entirely compatible with the pleasure principle" and therefore not at all daemonic.

Another approach bringing the repetitions within the purview of the pleasure principle returns to an early view of Freud's and interprets them as nothing more nor less than symptoms of frank neurosis: "All neurotic unpleasure is ... pleasure which cannot be felt as such" (Freud, 1920a, p.11). This trend seems to have begun with Kubie's (1941a) second paper on the compulsion and was extended by Joseph (1959) and Ferreira (1965). Here the ego was necessarily involved through its contribution to the compromise of the symptom. Of all the "neurotic" interpretations of the compulsion to repeat, the unkindest must surely be Weissman's (1956) strong *prima facie* case for the play of Freud's grandson being a neurotic compulsively patterned activity, prototypal of adult obsessive-compulsive symptoms and governed, therefore, by the pleasure principle.

To summarise on repetitions in play. The play that so impressed Freud is completely explicable within the framework of pre-1920 psycho-analytic theory. Neither as a neurotic symptom nor as a belated attempt at mastery does it violate the pleasure principle. Nor does it support a death instinct or a principle of inertia. Hendrick's thesis requires a more radical modific-

ation of instinct theory. Not only would the death instinct be done away with but an independent instinctual drive to mastery would somehow have to be placed alongside other ego drives. Would it have an aim, object, and source? Or would it be a different kind of drive altogether?

Dream and transference repetition

Turning now to repetitive dreams, we find two common views among psycho-analysts. Dreams are repeated in traumatic neuroses because they are ego attempts at mastery of the traumatic situation, because they affirm infantile omnipotence (Stein, 1956), or because they are driven, as Symons (1927) said, by "powerful unabreacted affect". The first two interpretations do not violate the pleasure principle, do not require the postulation of anything lying beyond it, and result in no major modification to the pre-1920 theory. On the other hand, the second interpretation necessitates accepting that there are exceptions to the wish-fulfilment function of dreams - which Freud granted later for other dreams anyway - and that powerful unpleasurable affects arising externally pose exactly the same problems for the mental apparatus as do the more familiar, internally arising, pleasurable kind.

However, any such recognition would require a complete recasting of the relations between the principles if not the development of entirely new ones. Especially would this be so if the self-limiting properties of some of the symptoms of traumatic war neuroses (including the gradual fading of the repetitive dreams) were to be given adequate consideration (MacCurdy, 1918, pp.29-30; Rivers, 1918, 1923, p.27; Lowenfeld, 1941; Grinker and Spiegel, 1943, pp.32-39). These properties are utterly at variance with a self-destructive motivation. This is true even if one accepts the later claim of Grinker and Spiegel (1945, p.365) that the dreams "cannot be dismissed as a general technique to master anxiety". They thought that if that were all to them, the dreams "should disappear ... gradually with time" and that their persistent repetition was due them symbolising other deep-seated conflicts. According to their observations, interpreting this other meaning put an end to the repetition.

Despite Fayek's (1980) contrary opinion, most psycho-analysts bring transference repetitions under the pleasure principle just as readily by thinking of them as attempts to satisfy infantile wishes. Their characteristics are then no longer at variance with those attributed to them in other parts of the theory. For the moment, let us assume that the action which is repeated is one that substitutes for a memory - Boesky (1982) cites instances of patients who act out past events they have no trouble in remembering. Why should this be so? And why is it said to recur in the context of the patient's non-acceptance of a 'reconstruction' of an infantile past? Symons (1927) may well be right in arguing that the reproduction of the old experience in action is less painful than its recollection in clear consciousness, but that does not explain the context. Several writers have

suggested that what determines the regressive aspects of the transference neurosis is the clash between the dependency needs of the patient and the frustratingly rigid emotional neutrality of the analyst. Repetition-in-action, it is implied, is part of the patient's gradual adaptation to an infantile situation (Macalpine, 1950; Marmor, 1962; Kepecs, 1966). Building on this idea only a little further, is it not plausible to suppose that patients, caught between an unwillingness to accept a particular interpretative reconstruction and an equally strong desire to maintain the therapeutic relationship, evolve a compromise? Without having to acknowledge the correctness of the interpretation, they behave in a way that 'pleases' the analyst and so maintain the relation. And would not this be made all the easier because of the very general constellation of factors that are present in the examples Freud gives? Who has not felt scorned by their parents, unable to accomplish big tasks in childhood, and so on? These feelings can be conceptualised more adequately *outside* the psycho-analytic framework than *within* it. Of course, rather than requiring minor peripheral modifications, this social-psychological conception cuts the ground from under a great deal of what is central to Freud's theory. [3]

Sadism and masochism

Considering now the relevance of sadism and masochism to the death instinct, we note that Freud's explanation of how the death instinct generated sadism required his giving up his earlier assumption that there could not be a primary masochism. In *Beyond the pleasure principle*, Freud had had the death instinct forced away from the self by narcissistic libido. Impressed into the service of the sexual function, it then manifested itself as a sadistic drive (Freud, 1920a, pp.53-55). Three and a half years later, in *The economic problem of masochism*, Freud expanded this thesis by having sadism as an outward displacement from an original state of primary masochism. When re-directed on to the self, the drive was observable clinically as secondary masochism. One portion of the death instinct was taken up in the ego, "as an intensification of masochism", and another gave rise to conscience, thereby increasing the sadistic treatment of the ego. So supplementing each other, the two trends united "to produce the same effects" (Freud, 1924b, p.170). Sadism and masochism could be explained provided that one assumed there was a death instinct, a state of primary masochism, an equivalence between sadism and an outwardly directed death instinct, and some mechanism responsible for the re-

3. I am cautious about this speculative explanation because Boesky (1982) begins his review by observing that, despite the amount of literature on the subject, there is "considerable confusion about the nature of acting out" and concludes that it "cannot be defined on empirical clinical grounds". After setting out Freud's original examples of acting out as a substitute for remembering, Boesky asks his readers to notice "how different" they are "from the variety of behaviours we currently associate with the term". He believes problems of the concept of "acting out" are traceable to three sources: its link with memory, its placement in the topographic systems, and its connection with the instinctual definition of transference.

direction. The alternative explanations of sadism and masochism are based on doubts about one or more of these assumptions. The variety of elements available for doubting is, I think, largely responsible for the disagreements, contradictions, and imprecise usage that Bieber (in Masserman, 1959a), Salzman (1959), Spiegel (1978), Mollinger (1982), Maleson (1984), and Grossman (1986) have drawn attention to in the literature.

Masochism without the death instinct One position completely denied the relevance of the death instinct. Given their very different attitudes about the reality of the instinct, one is surprised to find W. Reich, Symons, Alexander, and Federn sharing it. W. Reich (1926) claimed that sadistic behaviour only followed sexual frustration, a fact that led him to conclude that "libido-obstruction is the visible individual source of destructive aggression and that sadism is due to this relation". Although he probably did not mean to use the word "source" quite in the same way as his translator he added that "'primary masochism' is a purely hypothetical source of the death instinct" (W. Reich, 1926). During a later attack on Alexander (1926, 1927/1928) and Reik (1925/1959) for what he saw as their adoption of Freud's concept of primary masochism, he said that the perversion of masochism followed "*directly* from the pleasure principle" (W. Reich, 1927/1928), a point already made by Symons (1927) when questioning whether masochism necessarily implied the reality of the death instinct. Alexander (1929) argued that one could speak of a death instinct only if there were clinical conditions in which a primary masochism united with the sadistic impulses directed against the ego. Although he thought that that conjunction was observable in the melancholias of old age, he could not discern it in masochism or sadism.

Federn's (1932) vigorous defence of the death instinct was along similar lines. Even though he took the same view of melancholia as Alexander and was prepared to accept cruelty gratified solely through pain as one of the manifestations of the death instinct, he excluded sadism: "The death instinct is not necessary to explain sadism. The pleasure aim of gratification is a sufficient motive." Federn seems to have been just as doubtful about the relevance of the death instinct to masochism *per se*. Rado (in Masserman, 1959a) went further. He dismissed Freud's instinctually based approach altogether: "the theory of instincts has outlived its initial usefulness; for decades now, it has proved to be unfruitful". Mollinger (1982) also dismissed the instinctual drives completely. He sought support for this radical surgery in a "new trend", evident in "the last several decades", to remove sadism and masochism from "their instinctual bases". I shall consider Rado's and Mollinger's theses shortly.

Masochism with the death instinct Another position accepted and extended Freud's view. Fenichel (1928), for example, proposed that masochistic behaviour resulted from a need for self-punishment to which the death instinct contributed directly. The unconscious guilt that created the need was "a unique, primitive, ruthless thing, which would not shrink from the

destruction of the patient's own ego''. Moreover, he emphasised, it was ''the clinical representative of the mute death instincts''. At first sight, Nunberg's (1926, 1932, pp.140-156. Cf. Nunberg, 1932/1955, pp.157-171) account seems quite similar to Fenichel's until it is realised that his separation of unconscious guilt from the need for punishment assigned the death instinct only to the latter - the guilt came from unsatisfied libido. Because both W. Reich (1926) and Alexander (1929) also incorporated the sense of guilt into their explanations of masochism, the latter even after he had ceased to believe in the death instinct (Alexander, 1948, p.120), these two positions have a certain independence of the role given the instinct.

Masochism and observation The independence of observation from belief brings us to the most important of the clinical issues - that of the observational status of Freud's new concept. Clearly it, too, is independent of belief. W. Reich, a non-believer, together with Alexander and Federn, both believers, took the view, best expressed by Alexander (1929): ''An unequivocal answer to this question as to the presence of a primary death-instinct cannot ... be obtained by the method of direct clinical observation''. The point is most tellingly illustrated by the bewildering array of causal mechanisms, developmental backgrounds, and psychodynamic factors that have been said to produce the typical masochist. Few of these alternative explanations draw on the death instinct - an oddity indeed if its clinical manifestations are in any sense directly observable. It is to these other views of masochism that I now turn.

Beginning with Sadger (1926), one class of explanations of masochism drew on connections formed in infancy between sexual and painful sensations. Sadger himself implicated the frustration of childhood oral and genital needs after they had been deliberately aroused by the parents, an explanation that partly over laps with that of Van Ophuijsen (1926). Both interpretations are reasonably consistent with M'Uzan's (1973) detailed description of the complex of feelings in his rather extravagant masochist. Sternbach (1975) proposed that during the oral and anal phases heightened pleasurable and genital sensations became connected and that the child learned to tolerate visceral sensations which reached ''the point of physical pain''. These quite different but *genitally* oriented views are all challenged by Friedenburg's (1956) thesis that unpleasure arising during *dentition*, just before the eruption of teeth, becomes associated with the infant's feelings of impotence at his inability to relieve his distress and feelings of hostility toward his mother for failing to help.

W. Reich (1932/1950) introduced a class of explanation centering on responses to castration anxiety. His had sexual tension caused by pain, tension, or threat relieved in a more or less peculiar but pleasurable way once the pain, tension, or threat was removed. As a child, Reich's patient had so feared castration as a punishment that the beating he did receive from his father was experienced as a great relief. When he became an adult, the childhood fear of castration intervened each time he strove for

sexual pleasure but the fear was attenuated by a beating. Gero (in Ostow, 1957) proposed a variation: pain replaced the sexual pleasure that oedipal guilt could not tolerate. Masochism and sadism were expressions of the sexual instinct rather than results of a fusion of aggression and libido. Lewinsky (1944) took masochistic acts to be total denials of castration and Spiegel (1978) saw them as providing a defensive means of escaping castration anxieties. On the other hand, Eissler (in Stein, 1956) claimed that the masochist accepted castration, insisting that despite it he could still have erection and orgasm. Each one of these genital views is very different from its companions and from the role given castration anxiety by W. Reich. More general variants of Reich's thesis are those of Bak, Rado, Romm, and Thompson. Bak (in Stein, 1956) implicated the pleasurable relief of a simultaneously sexually exciting and unpleasant diffused tension. Later the unpleasure became a necessary condition for sexual excitement. Bak pointed out that his view was not unlike that of Loewenstein (in Stein, 1956) who derived prototypal masochism from pleasure experienced by the child whose seductive behaviour reunited him with a parent who had threatened him in a playful way. Only guilt needed to be added for true masochism to form. Rado's (in Masserman, 1959a) alternative was based on learning: punishment and fear of punishment led to sexual inhibition with the consequence that painful experience had to be undergone to free "the inhibited organism for orgiastic release". Romm (1959) thought the patient did not so much wish to avoid an anxiety-free existence so much as "to create an over-all illusion of anxiety" in order to avoid overwhelming real anxiety: "It may represent his inadequate and futile attempt at mastery". Thompson (1959) took it that the behaviour allayed anxiety and gave the illusion of security.

Loewenstein's explanation went part way toward finding the origins of masochism in some peculiarity of the infant's love objects rather than in the pattern of stimulation. Berliner's explanation falls within a subgroup of this class. He concluded from his analyses that the infant experienced hatred quite directly from the mother. Because of its dependence, the infant had no choice but to submit and accept the suffering as a bid for maternal affection. Masochism was thus "the hate or the sadism of the object reflected in the libido of the subject" (Berliner, 1947. Cf. 1940, 1942, and in Stein, 1956). A similar argument was put forward by Menaker (1953, 1956) who differed only by placing the experience of hostility earlier, at the oral level. Avery (1977) saw sadomasochism as "a type of object relationship which serves to defend against the threat of object loss", thus agreeing with Berliner and Menaker. He re-interprets earlier case material as showing primary loss in the pre-genital phase rather than defence against Oedipal phase aggression. Bieber's (1953, 1966) explanation was also of this type but placed more emphasis on masochistic self-injury as a means for avoiding harm from others, for evoking positive affection, and for maintaining a relation with the object. Mollinger (1982) also believed that sadism and masochism were best considered as modes of

relation to love objects and best understood in terms of the stages of development of internal object relations hypothesised by Kernberg.

A further subclass of these object-love explanations is constituted by those like Thompson's (1959) in which the love object has definite positive qualities as well as negative ones. Masochistic behaviour is an attempt to gain the attention, love, and dependency satisfactions that were lacking in childhood. Smirnoff's (1969) analysis of Sacher-Masoch's life and works is reasonably consistent with this view even though he believed the masochist suffered in order to represent to himself his fusion with and separation from the love object rather than to obtain pleasure. For the most part it was the mother's hostility that he implicated. But, according to Grand's (1973) analyses, it was actually the father who hated or was indifferent to the child; the mother was detached, narcissistic, and dominant. As far as I can tell, among the explanations based on the relation to the object, only Parkin (1980) has attributed the hatred solely to the subject. He derives it from the from the child's unsatisfactory relation to the mother.

The last group of explanations is based on very general childhood experiences that are supposed to have resulted in some distortion of ego or self. Masochistic behaviour is thus an attempt to re-establish the ego's loss of the capacity for mastery (R.-J. Eisenbud, 1967), or an effort "to restore and maintain the structural cohesiveness, temporal stability, and positive affective colouring of a precarious or crumbling self-representation" (Stolorow, 1975b). They are broadly consistent with the object-love explanations as well as with Stone's (1971) thesis that the basic connection between sexuality and aggressiveness arises "in the drive to master actual or threatened traumatic helplessness". Stolorow's view is also not very different from Eissler's (in Stein, 1956) who claims that the masochist fears his ego will be overwhelmed during orgasm. It is also consistent with Keiser's (1949) report that his patients feared their sexual sensations, that they avoided unpleasure, and were unable to tolerate painful tension. Keiser felt that at heart the masochist could not manage the passive component of sexual activity. All of these interpretations are very far removed from those based on a deliberately sought dissolution or isolation. For example, Horney (1937, pp.259-280) contended that the masochist actively seeks out the dissolution of self. Salzman's (1959) claimed that the masochist underwent degradation and humiliation in order to support the essential value system of "needing no one - a supreme isolation and separation without, however, the usual despair". Thompson (1959) disagreed: for her the masochist's "deepest motive is the search for intimacy". Weiss (in Masserman, 1959a) endorsed Salzman's discarding "outdated mechanistic concepts" but believed the masochist wished to eliminate the hated self by merging with another. Millet (1959), on the other hand, did not consider that Salzman's isolation motive had provided "a diagnostic concept of sufficiently clear differentiation" and looked to the initial experiences of disappointment of not obtaining absolute love

from the mother in the symbiotic relationship as sowing the "seeds" of masochism.

These strikingly different explanations reveal a basic disagreement about the aim of masochistic activity. The key question was asked by Symons (1927). Does the masochist seek pain as an end in itself or as a means to a pleasurable end? We may also ask: Is the means determined through some relatively simple "association" or by some complex of psychodynamic factors from among unconscious guilt, need for punishment, castration anxiety, affirmation of self, and fear of object or of object-loss? Almost all of the previously cited authors at least imply a complex means of achieving pleasure but their usual scrutiny has been of those neurotic or psychotic conditions in which self-punitive *fantasies* have predominated rather than masochistic sexual *activity*. Because the latter ought to be more closely related to the death instinct or other aggressive tendencies, it is a more appropriate object for study than neuroses or psychoses. Now, when sexual masochism proper is examined, a somewhat different picture emerges. First, the actual tortures may be considerably more painful than those expressed in neurotic fantasies. Unlike the fantasied events there may be actual damage to the genitals, a fact that poses problems for most of the castration anxiety explanations (M'Uzan, 1973). Second, the masochist is bound to his torturer by what amounts to a formal contract specifying the amount of pain to be inflicted and the degree of humiliation to be undergone. The masochist thus retains complete control over the situation (Smirnoff, 1969. Cf. Lewinsky, 1944; Eidelberg, 1968). Third, the pain and suffering are primarily associated with orgiastic activity. Masochists do not have a heightened threshold for pain. As M'Uzan's (1973) patient put it: "it is the pain which releases the ejaculation". Outside of the sexual context he reacted to painful stimuli very much as others; in context, however, pain catalysed and amplified his sexual excitement at the same time as destroying its specifically painful quality (Cf. Bieber, 1966). How the pleasure and pain become associated is the central question of masochism and, as Loewenstein (in Stein, 1956) points out, its answer is not definitely known. Silverberg (1959), who took the view that masochism was "first and foremost" a sexual perversion, considered that until an answer was found to the question why an individual was able to gain pleasure through what he considers to be pain "we will not be able to explain the more figurative types".

Masochism and the need for punishment Finally, the unconscious sense of guilt and the need for punishment supposedly created by it. We have seen how these concepts are used both to support and oppose the death instinct interpretation and that even the believers are not at all agreed how they contribute. Pretty obviously this is because all three concepts lack clear referents - a point well illustrated in comparing the early and late views of those who like Alexander, Fenichel and Eidelberg slid from belief into unbelief. Late or early their clinical observations are the same (Alexander, 1926, 1929; Eidelberg, 1968; Fenichel, 1928, 1935/1954b,

1945b). And it is not at all surprising that Berliner (1947) was able to mount powerful arguments questioning the ultimate role of unconscious guilt and punishment in masochism, that Menaker (1956) and Spiegel (1978) denied the existence of those factors completely, that Salzman (1959) described the unconscious guilt as appearance, that Sternbach (1975) seriously proposed that sadism was neither necessarily connected with hate nor masochism with self-hate, self-destructiveness, and self-punishment and that, according to Spiegel (1978), the interpretation of moral masochism in terms of unconscious guilt "hardly ever generates more than mere intellectual agreement" among psycho-analysts.

It should now be clear that the problems that psycho-analysts have are partly a matter of what is to count as masochism or sadism. For Hoch, for example, the "fundamental question" is:

> do we have a psychopathologic entity which can be explained dynamically in a similar way in all ... or merely phenomenologically similar features of which the causation is not the same? (Hoch, 1959)

Sack and Miller (1975), who believed there were "many and varied" mechanisms underlying mascochism, even allowed there could be masochistic behaviour without masochistic motivation. Much later, Maleson (1984) observed that although "it has often been noted that masochism is not simply or predominately a manifestation of a sexual instinct ... it remains a time-honored part of psychoanalytic instinct theory". Maleson makes the point that in the famous *A child is being beaten* (Freud, 1919a), Freud's attempt to maintain the relation of masochism with the instinctual drives led to a "strained and confusing labeling of the three phases of the beating fantasy". That is, the first phase was sadistic but, although the form of the third phase was also sadistic, the satisfaction in the third phase was masochistic.

Within the fairly narrow confines of moral masochism, Spiegel (1978) distinguished four different but overlapping groups of ideas about its etiology and function. Maleson (1984) found ten different types of psychoanalytic explanations for masochism generally. They derived from three irreconcilable conceptualisations: masochism as a transformation of sadism, masochism as an anal-sadistic fixation/regression, and masochism as developing from masculinity or femininity. Maleson traces these confusions to Freud himself, who described any kind of suffering as well as frankly sexual behaviour as masochistic. Masserman (1959b) was undoubtedly correct to remark that Sacher-Masoch would have been astounded at the use of his name to connote 'a need for suffering'" because his (and de Sade's) search was for "sexual pleasure-in-itself". Maleson also judges Freud's original descriptions of the nature and origin of the masochistic-sadistic component instincts to be "ambiguous" and "tentative", his basic argument to establish them "weak", and its outcome as establishing terms with the dual meanings of instincts and behaviours

which made it "particularly difficult to disentangle the clinical meanings ... whether broad or narrow ... from metapsychological formulations" (Maleson, 1984).

Hoch (1959) favoured a broad meaning. Masochism was much more than a specific sexual deviation. It occurred in almost all disorders. I take it he would even reject the once widely accepted advice, attributed to Brenner (Cooper, in N. Fischer, 1981), to restrict the term to the seeking of unpleasure for the sake of *sexual* pleasure (Brenner, 1959). And, although Grossman (1986) argues powerfully against extending the meaning, he actually goes beyond Brenner in wanting to restrict the term to *fantasies* in which there is an obligatory combination of pleasure with pain. Grossman, like Loewenstein (1957), even took out the sexual quality altogether, recognising some forms of masochism as "desexualized". Maleson (1984) found, not surprisingly, that it could not "be explained by any consistent dynamic or metapsychological formula". He despaired at the possibility of a definitional solution at either the clinical-descriptive or theoretical-explanatory levels (Cf. Cooper, Glenn, and Fischer, in N. Fischer, 1981). I would add that until the psycho-analytic couch produces basic facts about sadism and masochism it is silly to pretend that what psycho-analysts like to call their observations have any bearing at all on any explanatory concepts, including those deriving from the death instinct.

Alternative explanations for aggression

Dissatisfaction and disagreement over Freud's new explanation of aggressiveness was if anything more marked than with the explanation of sadism and masochism. The dissatisfaction was expressed almost immediately by the concept of the death instinct being denied clinical utility. For example, when discussing the usefulness of the new instinctual concepts in child psychology, Bernfeld observed that they made:

> certain aspects less clear; they are biological, philosophical ... they belong to metapsychology with which the present psychology of infants had better not concern itself yet. (Bernfeld, 1925/1929, p.99)

Spilling over into a footnote, Ernest Jones (1926) expostulated, "Freud's 'death instinct'. I find myself unable to operate with this philosophical concept in a purely clinical discussion." Many other analysts subsequently expressed similar opinions (e.g. Berliner, 1940; Ostow, 1958) or they at least noted that Thanatos and Eros were "largely left outside the mainstream of psychoanalytic theory and practice" (Valenstein, in N. Fischer, 1981). More recently, Downey (1984) has judged Freud's death instinct to have "placed a metaphorical obstruction in the path of psychoanalytic inquiry" and Werman (1985) said bluntly "there are few psychoanalysts today who support the concept of the death instinct". All this even though an externally directed aggressive-destructive drive is widely accepted (Cf. M. I. Klein, 1983). I now examine the alternative accounts of aggressiveness which developed out of this widespread dissatisfaction.

One of the most popular non-death instinct explanations of aggressiveness derives it from some other biological drive that is not itself primarily aggressive but has secondary destructive aims or a mode of action generating aggressiveness as a side effect. For example, some type of self-preservative instinctual drive having its source wholly or partially in oral or gastro-intestinal predatory, or devouring, or incorporative tendencies has been postulated as the basis of aggressiveness by Loewenstein (1940, 1969, and in Lussier, 1972), Simmel (1944), and Ostow (1957, 1958). This kind of basis has been criticised, however, on the very good grounds that predatory aggression is not and probably never was a significant factor in inter-human conflict. Lantos (1958) therefore located what she called subjective aggression (which accompanied the affect of hate) in instinctually determined conflicts over territory, sexual rivalry, and the like. Somewhat similarly, Brunswick (1954) saw human aggression as manifestations of the innate defensive-aggressive responses that man shared with animals. Although of this same general type, Winnicott's (1950-1955/1958) derivation has a quite different basis. For him, aggressiveness comes from infant motility drives having more fundamental determinants in a life force active in the very tissues themselves. Perhaps more debatably to be included in this class is Parens' (1973) thesis that motor activity resulting from some unknown physiological source generates a *non-destructive aggression* (!) equivalent to the drive for mastery, as well as the more familiar destructive kind. A major stumbling block for all these conceptualisations is Werman's (1985) objection that there is a good deal of evidence from anthropology against the innateness of an aggressive drive independent of the death instinct and no good evidence from biology for its having that origin.

Aggressiveness as a mode of instinctual operation, reflecting merely the way in which instincts seek their satisfaction, and therefore independent of any particular drive, was first proposed by Fenichel (1935/1954b). He adopted the very argument psycho-analysts had first used *against* Adler's concept of an independent aggressive drive (E. Jones, 1935-1936). Fenichel's suggestion seems to have been accepted by Szasz (1952) and has been explicitly endorsed by Gillespie (1971. Cf. Fenichel, 1945b, p.59).

Contributing to two further types of explanation is a distinction which has been made between instinctual drives as Freud originally defined them and the biological or philosophical forces they became in the new theory. Seemingly first made by Bernfeld (1925/1929, p.99), the distinction eventually gave rise to distinct biological and psychological explanations. Bernfeld had noted that "the life- and death-instincts are biological forces ... which extend beyond the domain of the individual" and later, in conjunction with Feitelberg, made the distinction more explicit (Bernfeld and Feitelberg, 1931). However it was once again Bibring who produced the most well-known formulation of the issue, the one we have already noted, in which he said that calling the life and death drives 'instincts' had

turned them into a vague 'something' which gave direction to the life processes (E. Bibring, 1936/1941). In his influential comments on the concept of instinctual drive, H. Hartmann (1948) claimed that the life and death instincts added little to the psychological understanding of drives. He fell in with the view that hypotheses deriving from them could only be tested biologically and even suggested that Freud's earlier concept had to be extended "beyond the physiological substratum traceable today". Consequently, a year later, in conjunction with Kris and Loewenstein, he introduced the concept of a primary aggressive drive, independent of the death instinct, and one that paralleled the sexual drive even though its source was unknown (H. Hartmann, Kris, and Loewenstein, 1949).

Although not all ego-psychologists agreed with Hartmann's critique and particular solution (M. I. Klein, 1983), the concept of a primary aggressive drive, not usually thought of as having any relation to Freud's death instinct (or at best an uncertain one), has been widely accepted (Waelder, 1956a; Arlow, 1959; Sandler, in Lussier, 1972). Perhaps the most interesting recognitions of this concept, admittedly falling short of its actual endorsement, are those of Segal and Anna Freud. As a member of the Kleinian School (which emphasises the role of aggression and even looks kindly upon Freud's revised theory), Segal (in Lussier, 1972) moved considerably toward the Hartmann position in insisting that she had to use some concept of a primary aggressive drive and was prepared to think of it as separate from the death instinct. While not moving as far as Segal, Anna Freud (1972) acknowledged the gap between clinical fact and biological speculation by accepting the kind of distinction made by Bernfeld and Bibring. To some extent the adoption of this distinction was aided by the argument of Lantos (1955, 1958) and Lampl-de Groot (1956). After recognising the practical importance of the distinction, they both went on to propose terminological revisions that effectively restricted the term 'instinctual drive' to Freud's first meaning and only allowed Eros and the death instinct to be described as 'forces' or 'tendencies'. It was from this position that Lantos derived aggressiveness from conflicts over sexuality, territoriality, and the like. H. Hartmann (1956), on the other hand, endorsed the Lantos-Lampl-de Groot proposal, dismissed Freud's revised theory as speculative and difficult of validation, and restated his thesis of primary aggression. Clearly the Bernfeld and Bibring distinction had allowed contemporary psycho-analysts to use the earlier concept of drive in a variety of ways, mostly inconsistent with one other, while comfortably ignoring Eros and Thanatos altogether. An exception is Downey (1984) who, while recognising the negative qualities of a death instinct-derived aggressive impulse, believes it to be "a *positive* force in psychological development and subsequent functioning" [!].

H. Hartmann's (1948) emphasis on the psychological inadequacies of Freud's life and death instincts led to the third of the methods of dealing with the theory - that of removing its biological foundations completely. No doubt this may not have been intended, but it is an obvious influence in

Pleune's (1961) argument that drives "should be conceptualized as a psychological rather than a biological phenomenon". Pleune himself did not actually dispose of the somatic sources. That was left to Brenner. As Brenner (1979) said later, "it was Freud's need to anchor the aggressive drive in physiology, in the soma, that led him to relate it to a protoplasmic death drive", a concept he considered "neither useful nor defensible". Brenner (1971) began his argument by questioning the claim of H. Hartmann, Kris, and Loewenstein (1949) that the validity of the death instinct was a matter for biologists. Were not "psychoanalytical data by themselves ... sufficient evidence for the theory of aggression?" Brenner then defined a drive as "a theoretical construct which serves the purpose of explaining the nature of basic motivation", arguing further that psychological evidence provided "an acceptable basis for the concept of aggression as an instinctual drive" (Brenner, 1971. Cf. Brenner, 1979).

One advantage of adopting Brenner's position is that disputes over sources, aims, and objects either disappear or are resolvable by psychoanalysts or psychologists rather than by biologists, or physiologists, or philosophers. From this psychological trend two quite different specific suggestions have resulted: those of Stone and Holt. As we have seen, Stone (1971) postulated aggression to arise from the drive to master traumatic helplessness. This drive was not a drive in an inborn, constantly acting, autochthonous sense, however, even though it had an *anlage* in the hostility, rage, and aggression of the prolonged helpless stage of infancy. Holt, on the other hand, wanted to do away with the concept of drive altogether, replacing it by the concept of 'wish' found in much of Freud's early writing. Analysts would thereby avoid being committed "to a great deal of pseudoexplanatory mythology that does not have satisfactory grounding in fact" (Holt, 1975a). Although having a different basis, a similarly monolithic position is seen in the trend, which Blanck and Blanck (1977) welcome, in which the very foundation of Freud's theory of instinctual drive - the ubiquitous bipolar contrast - is abandoned altogether.

Taken together these alternative explanations of aggression, of sadism and masochism, and of the compulsion to repeat show that the major problem, if not the central one, is that the death instinct has no characteristics which manifest themselves in unique ways. The very essence of the drive is that it is mute; unlike any other drive its source is not located in a particular organ; it produces no tension of its own and acts solely by reducing the tensions introduced by other drives (Cf. Fenichel, 1935/1954b); and it reveals itself more or less openly, if at all, only after a complex series of transformations. It is true that at various times it has been proposed to have clear or reasonably clear and unique manifestations (Fenichel, 1928; Alexander, 1926, 1929; Federn, 1932; Nunberg, 1932, pp.62-66, 1932/1955, pp.84-87; Heimann, 1952; Garma, 1971; M'Uzan, 1973; Fayek, 1980). However, these claims have been rejected fairly explicitly or implicitly by the vast majority of psycho-analytic writers. Among those who are reasonably direct in their rejection are W. Reich

(1926, 1927/1928, 1932/1950), Fenichel (1932/1934, pp.68-69, 272-273), E. Jones (1935-1936), E. Bibring (1936/1941), Berliner (1940, 1947), Reik (1941), Simmel (1944), Lewinsky (1944), H. Hartmann, Kris, and Loewenstein (1949), Flugel (1953), Lampl-de Groot (1956), Ostow (1958), Pratt (1958), Arlow (1959), Eissler (1969), Brenner (1971), Stone (1971), and Sandler, Segal, Loewenstein, and Rosenfeld (all in Lussier, 1972). Those who reject it indirectly include Bernfeld and Feitelberg (1931), Loewenstein (1940), Kubie (1941a), Alexander (1948, p.68), Waelder (1956a), Markovits (in Ostow, 1957), Saul (1958), Pieper and Muslin (1961), Pleune (1961), Eidelberg (1962), Katan (1966), R.-J. Eisenbud (1967), Gillespie (1971), Lebovici and Diatkine (1972), and Sternbach (1975). The conceptual arm required to reach from a self-destructive tissue process and land a punch on someone's nose is far too long.

The weight of these opinions is very consistent with the results of Cain's (1961) search for evidence of an aggressive drive in childhood being turned back upon the self prior to the formation of the super-ego. He believed he had been successful. But he found no manifestations of the death instinct upon which, in Freud's view, that drive rested.

CONCLUSION

Condensing our conclusions about Freud's concept of the death instinct: the clinical data from which it was inferred were capable of better and more familiar interpretations, the arguments adduced in its support were so riddled with faults they were not compelling, and the biological evidence was so defective or misinterpreted that a logically shoddy and empirically impervious prop had to be used to support its relevance. Neither the concept nor the explanations it generates are at all necessary.

Why then is the theory still taken seriously? Why has it not been discarded? First, the new theory does fill an explanatory hole. By providing an opponent for the sexual drive the death instinct gives the theory symmetry. And, by explaining aggressiveness without recourse to the tortuous logic through which Freud had previously derived the opposition between love and hate, the new theory also gained in simplicity. How much a simpler explanation of aggressiveness is valued can be seen in the fact that not one of the multitudinous critics of the new theory has sought to revive the complex nonsense of *Instincts and their vicissitudes* as an alternative even though it may be as Ikonen and Rechardt (1978) claim that the current psycho-analytic theory of aggression rests more on *Instincts and their vicissitudes* than on *Beyond the Pleasure Principle*. Ironically, one of the few proposals to revive the concept of the ego-instincts as a distinct class is based on their being needed to account for self-care and *counter* self-destructive tendencies (Khantzian and Mack, 1983).

Second, none of the alternatives has any clear advantage over Freud's. Whether aggression is thought of as a drive in its own right, as do Hartmann, Kris, and Loewenstein, or as the side-effect of another drive, as does Fenichel, the very difference of such constructs from the psychoanalytic concept of drive rules them out. Aggressiveness does not resemble the sexual and hunger drives subjectively or objectively. It is neither governed by tension reduction nor the production of pleasure. Objectively there is an even greater difference. It has no definite source and its underlying processes cannot even be guessed at. Did the aggressive drive seem to possess characteristics described by Freud, Holt's objection, that the physiological properties might not be the same as those sensed subjectively, would still have to be met.

A psychological concept of drive, like that proposed by Brenner, has other problems, mostly those centering around the defining criteria. The problem has been recognised by Plaut (1984). Part of his argument for reviving the concept of ego instincts, is that the only alternatives are "to abandon the concept of instincts altogether ... or to postulate an unlimited number". He cites R. W. White (1959) and Kohut and Wolf (1978) as having already opted for the first and Yankelovitch and Barrett (1970) for the second solution. [4] As the history of the concepts of instincts and drives in psychology shows, without strict criteria, the number of essential or basic drives multiplies well beyond any reasonable necessity. I can do little better than cite Ikonen and Rechardt (1978) who say the "unsatisfactory state of psycho-analytic aggression theory is clearly revealed by recent publications, panels and congresses". Neither do they find support for the psycho-analytic theory of aggression in behavioural approaches, and the search in biology has been "equally fruitless".

Third, most of the alternatives provide only partial or fragmentary solutions. Presumably this is because they are based on criticisms that for the most part deal with isolated, separate difficulties with Freud's concept. Nowhere does there seem to be a moderately comprehensive critique of Freud's revised theory. Consequently, there is nowhere a comprehensive alternative. Nor is it the case that any of the alternatives possesses the grandeur (or grandiosity) of Freud's tracing the basic tendencies governing mental activity to the cataclysmic events that brought life itself into being. While this explanatory modesty is not at all a bad thing, it is the wider view that has the greater intellectual attraction. No less a critic of the death instinct and the compulsion to repeat than Waelder (in Gifford, 1964) admitted that he accepted the general sense of the opposing tendencies of conservation and progress in human life being somehow related to life and death. Lowental (1983), who objects to the death instinct for its unsound conceptualisation in drive theory, has a similar feeling. After separating it from aggression he placed the death instinct in Freud's etiological equation

4. I am not sure that this plea would actually be endorsed by all of the authors Plaut cites.

(!) as a pre-condition for "death motivation". He also said he was "convinced of the driving momentum of the death instinct" as the motivator of a regressive striving for death (Lowental, 1981). Freud's theory thus positively forces itself into the minds of psycho-analysts. I suggest it does so there and elsewhere because of the way it seems to mesh with a much larger intuition about life.

Although this last consideration may do for mythologists and literary critics, it is not to be taken too seriously. It is relevant only in the sense of the relevance of John Donne's striking anticipation of Freud's picture of death as an internal enemy:

> Who then is this enemy?... An enemie that is so well victualled against man as that he cannot want as long as there are men, for he feeds upon man himselfe.... he fights with our weapons, our own faculties, nay our calamities, yea our own pleasures are our death. (Donne, *Sermon*, Preached at Whitehall, March 8, 1621)

Or as Donne's better known thought about the relation between birth and death which he preached just before his own death:

> deliverance *from* ... the death of the *wombe*, is an *entrance*, a delivering over to *another death*, the manifold deathes of this *world*. Wee have a winding sheete in our Mothers wombe, which growes with us from our conception, and wee come into the world, wound up in that *winding sheet*, for wee come to *seeke a grave*. (Donne, *Sermon at White Hall*, the beginning of Lent, 1630)

The icy grip of Donne's words comes from the suddenness with which they reveal the central truth about our existence: our deaths begin with our lives. The command Freud's concept exercises over us is because it reveals the same truth. However, while Donne's thought aspires to no status other than that of theological poetry, Freud's version represents it as a scientific truth.

Freud's instinct theory also maintains its currency because of the intellectual torpidity of those who sleepily insist that it is better to have ideas that glow even feebly in the light of a thoroughly bad theory than admit complete ignorance.

13 THE STRUCTURES OF THE MIND

> ... whenever she looked hard at any shelf, to make out exactly what it had on it, that particular shelf was always quite empty, though the others round it were crowded as full as they could hold.
>
> Lewis Carroll: *Through the Looking Glass.*

What I consider in this Chapter is how major alterations to Freud's theory of the mind were made necessary by his introducing the concept of a death instinct. An *id*, now containing destructive as well as sexual drives, was regulated by an *ego* and a *super-ego*. This new structural theory supplanted the old topographic theory, originally set out in the *Interpretation of Dreams*, in which simple repressed sexual ideas imprisoned in *Ucs* had been controlled by *Cs./Pcs.*

The basic concepts of Freud's new structural theory can be briefly characterised:

the ego controls the perceptual and motor apparatus, lays down memories, makes judgements, and selects possible courses of action. Only in the ego does consciousness arise and is anxiety experienced. The ego uses its functions to initiate repression or to control and delay instinctual discharge until realistic modes of need satisfaction have been found. Normally the ego is governed by the reality principle and operates according to the secondary process.

the super-ego is the vehicle of the ego-ideal, the repository of the individual's standards and values, the location of the conscience, the function that scrutinises his behaviour, forever measuring it against the standards of the ideal, and home of the mechanism which punishes violations of those standards.

the id is the reservoir of the psychic energy deriving from the twin drives of Thanatos, or death, and Eros, or life. Activity in the id is governed by the primary process, the tendency for instinctual drives to press for immediate discharge, and for their energies to be freely mobile, capable of condensation and displacement. The id is said to be timeless, to know nothing of logic, contradiction, or negation. This seething cauldron of instinctual drives is an original, inherited endowment of energy.

It may seem as if there is a simple isomorphism of the old with the new: *Ucs.* corresponding with the id and *Cs./Pcs.* with the ego and super-

ego. But that overlooks the death instinct. What will be argued in this chapter is that the essential purpose of Freud's final theoretical revision was to incorporate the death instinct into the mind. It was precisely because that force could not be accommodated in the old that new structures had to be found to house it.

I begin Chapter 13 with an examination of Freud's arguments in *The Ego and the Id* for replacing the topographic theory with the structural. After outlining how Freud found a place for the death instinct in the new structures, I go on to show how this required him to stress the cognitive functions of the new ego, especially its capacity to sense and generate anxiety as a precursor to initiating repression, and how those functions had to be based on a neutral, disposable energy. The relation between the id, *Ucs.*, and the death instinct is then explored before I examine Freud's toxic theory of neurosis and his changed view of primal repression as these relate to the anxiety that he now supposed to cause repression.

WHY THE STRUCTURAL THEORY?

Freud's explicit arguments for the structural theory did not include the difficulty of finding a place for the death instinct. His campaign was conducted on two seemingly different fronts. First, he adduced terminological difficulties with the words 'conscious' and 'unconscious' as one ground. Second, he announced two new discoveries - those of unconscious ego resistance and an unconscious need for punishment - that rendered the topographic theory obsolete and demanded its replacement. Both propositions must be challenged. The terminological difficulty, although real, was unimportant, while the discoveries were not made through uncomplicated observation and at least one of them was definitely not new.

The terminological problem

Freud noted three ways in which the word 'unconscious' was used in psycho-analysis: *descriptively*, *dynamically*, and *systematically*. A mental event could be described as 'conscious' if one were immediately and presently aware of it. In contrast, the idea might not be present in consciousness but be readily capable of becoming conscious. Unconscious ideas like these could be described as *preconscious*, as latently conscious, and were unconscious only in the *descriptive* sense. Freud claimed psychoanalytic investigation had shown many ideas could not become conscious so easily - their entry into consciousness was prevented by an active force maintaining them in a state of repression. Ideas unconscious in this way were so in the *dynamic* sense as well as descriptively. Freud had already differentiated these two meanings before 1912 and, in that year, he added the third or *systematic* meaning when discussing the topographic theory originally set out in Chapter 7 of *The Interpretation of Dreams* (Freud, 1912c, p.266). According to whether ideas belonged to the systems *Cs.*,

Pcs., or *Ucs.*, they were conscious, preconscious, or unconscious respectively. Furthermore, ideas in *Cs.* or *Pcs.* were governed by the secondary process. They were rational and the energy investing them was able to tolerate the postponement of immediate discharge. Contrariwise, ideas in *Ucs.* lacked structure, were illogical and contradictory, and their energy pressed for immediate discharge. In a word, they were ruled by the primary process. Thus the characteristics of ideas and their capacity for consciousness depended upon the system to which they belonged.

Reduced to essentials, Freud's main argument in Chapter 1 of *The Ego and the Id* was that there was a need to describe both preconscious and repressed ideas as unconscious but that only the repressed was unconscious in the dynamic sense. As he put it, "in the descriptive sense there are two kinds of unconscious, but in the dynamic sense only one" (Freud, 1923b, p.15). Three things attest to the unimportance of confusion so created. First, when these different senses of the terms conscious and unconscious had been formally delineated some eleven years earlier, these problems in usage were not even hinted at (Freud, 1912c). Secondly, in 1915, when Freud did draw attention to them, he made no especial case for abandoning the topographic theory; rather he seems to have resigned himself to the ambiguity being inescapable (Freud, 1915c, p.172). Lastly, in *The Ego and the Id* he indicated implicitly the degree of importance of the descriptive-dynamic distinction:

> For purposes of exposition this distinction can in some cases be ignored, but in others it is of course indispensable. At the same time, we have become more or less accustomed to this ambiguity of the unconscious *and have managed pretty well with it.* (Freud, 1923b, p.15. My emphasis, MBM)

By itself the ambiguity had not required a major theoretical revision. Something more was needed.

The first discovery

At this point Freud revealed the first of his discoveries. "The further course of psycho-analytic work" had rendered the dynamic-descriptive distinction inadequate "in more ways than one". He claimed as "the decisive instance" his discovery of an unconscious resistance located in the ego (op. cit., pp.16-17). In each individual, said Freud, there was a coherent organisation of mental processes called the ego responsible for repression. During a psycho-analysis the resistances toward repressed mental contents had to be removed. But Freud had found:

> when we put certain tasks before the patient, he gets into difficulties; his associations fail when they should be coming near the repressed. We then tell him that he is dominated by a resistance; but he is quite unaware of the fact, and, even if he guesses from his unpleasurable feelings that a resistance is now at work in him, he does not know what it is or how to describe it. (op. cit., p.17)

Because there was "no question" but that the resistance emanated from the ego:

> We have come upon something in the ego itself which is also unconscious, which behaves exactly like the repressed - that is, which produces powerful effects without itself being conscious and which requires special work before it can be made conscious. (ibid.)

For Freud, the practical consequences of this recognition were momentous enough. Only after "endless obscurities and difficulties" could the neuroses now be derived from a conflict between the conscious and unconscious. Neuroses had rather to be seen as reflecting a different antithesis, that between "the coherent ego and the repressed" (ibid.). However, the theoretical implications were the most far-reaching. The unconscious portion of the ego, the part from which unconscious resistance arose, demanded the recognition of a third type of unconscious, one more than latently unconscious but not one co-extensive with the repressed.

The second discovery

What Freud claimed as his second discovery - an unconscious need for punishment - also required a theoretical emendation. The discovery was best appreciated, he said, by reconsidering some well established clinical facts:

> There are certain people who behave in a quite peculiar fashion during the work of analysis. When one speaks hopefully to them or expresses satisfaction with the progress of the treatment ... their condition invariably becomes worse. (op. cit., p.49)

This "negative therapeutic reaction" seemed to Freud to be determined by something over and above the usual forms of resistance such as defiance of the analyst, narcissistic inaccessibility, and the secondary gain from the illness itself:

> In the end we come to see that we are dealing with what may be called a 'moral' factor, a sense of guilt, which is finding its satisfaction in the illness and refuses to give up the punishment of suffering. We shall be right in regarding this disheartening explanation as final. (ibid.)

For the patient, however:

> this sense of guilt is dumb; it does not tell him he is guilty; he *does not feel guilty*, he feels ill. This sense of guilt expresses itself *only* as a resistance to recovery. (op. cit., pp.49-50. My emphasis, MBM)

It was essentially in hysteria where the unconscious need for punishment (as this sense of guilt was more correctly called) remained so completely unconscious. In other disorders, especially in melancholia and obsessional neuroses, there were conscious manifestations, sometimes quite strikingly

so. As Freud saw it, this peculiar form of resistance was caused by the need for punishment powered by the death instinct operating unconsciously within the ego. There was thus another reason for recognising an unconscious portion of the ego. Moreover it was a part in direct communication with the instinct.

Recent? Discovery?

The idea of unconscious ego resistance was no more a *recent* one, as Freud implied, than it was a *discovery*. Notice the sequence of Freud's arguments: it is after having discussed the three meanings and the concepts of the topographic theory as if they were the usual currency of psychoanalytic discourse that he said it was "in the *further* course of psychoanalytic work" (My emphasis, MBM) that unconscious resistance had been "come upon" or "discovered" (op. cit., pp.16-17). An abstract, probably written by Freud himself, of a paper he gave some eight months earlier foreshadowed both claims: unconscious ego resistance was there also described as a new discovery (op. cit., p.4). However, one of the cases reported in 1895 in the *Studies on Hysteria* was the very first in which this kind of resistance was described. The behaviours from which the resistance was then inferred were identical with those described in 1923. The patient was Elisabeth von R. and it was of her inability to produce ideas and visual images of etiological significance in response to the pressure of his hand that Freud first used the term resistance (Breuer and Freud, 1895, pp.153-154). Freud conceptualised the failures of his other patients to complete the associational trains he was following similarly (op. cit., pp.269-281, 287, 292-295, 301). In the model of the pathogenic memory structure outlined in the *Studies*, the closer memories were to the nucleus, the greater was the resistance to their recovery (op. cit., pp.288-289). Although perhaps put more succinctly in the *The Ego and the Id*, the behaviour was the same: the closer the patient approached the repressed, the more the associations failed.

Freud's very earliest remarks about the patient's *consciousness* of resistance are not at all inconsistent with what he later said about *unconscious* resistance in *The Ego and the Id*. When Freud overcame resistance by repeated pressure, and a definite idea or memory emerged, the patient would frequently say something like "I could have said it to you the first time" (Breuer and Freud, 1895, p.154. Cf. pp.269-270, 279). Some of the resistance was therefore a conscious unwillingness to report rather than an inability to recall. But this was not true of those breaks in the chains of associations that had first caused Freud to use the pressure method repeatedly. What happened there was much more like what he later reported. In the *Studies*, Freud briefly described a special series of five cases in which each link in the chain was recovered by separate, single pressures. These links appeared as isolated words, images, or ideas, which the subject had little or no hesitation in reporting (op. cit., pp.273-278). These accounts read differently from those of conscious withholding and

the patients seem to have been quite unaware of any motive for the associational failure. Indeed, Freud described one of these patients as "quiet and co-operative" (op. cit., p.274) and another as putting up "only a remarkably small conscious resistance" (op. cit., p.275). At most, patients seemed to have experienced painful emotions but not the specific feelings of aversion and repulsion supposed to motivate resistance (op. cit., pp.166, 269, 303-304). Other statements, including Freud's descriptions of his own mental state, clearly show the same kind of unawareness to be more usual (op. cit., p.117, n.1.).

There was nothing new in 1923 in the idea of unconscious resistance - it had been so thought of, as H. Hartmann (1956) says, "long before" (Cf. Freud, 1896b, p.162; Masson, 1985, Draft N of 31.5.97). At neither time was it really a discovery. Although Freud had pictured repression as initiated by a conscious act, the process itself was unconscious. Once he had combined that conceptualisation with the view that both resistance and repression resulted from the same "aversion on the part of the ego" (op. cit., p.269), it was logically necessary for resistance to be an unconscious ego process too. An unconscious portion of the ego, a portion responsible for resistance, is a simple logical outcome of Freud's locating repression there. Even if the idea is not to be dated as early as 1895 there is no doubt that by 1923 it was neither new nor a discovery.

It is also difficult to accept the unconscious need for punishment as a discovery. First, what was observed was the negative therapeutic reaction. Complex interpretations were required before Freud could conclude that the reaction was a resistance or that it was due to an unconscious need for punishment. He had to weigh and eliminate the more usual forms of resistance in an intricately subjective manner before deciding there was a residual type. Attributing resistance to an unconscious need for punishment was also an interpretation - a point emphasised by Freud himself, although in an odd way. Patients did not feel guilty, their guilt was manifest "only as a resistance to recovery" (Freud, 1923b, p.50). Indeed it was particularly difficult to convince them their behaviour was motivated by a need for punishment.

Interpretations at least as complex are involved in Freud's claim that the unconscious need for punishment was also present in obsessional neuroses and melancholia. In them the sense of guilt was actually conscious, even "over-strongly" so (op. cit., p.51). While a logical derivation of both conscious and unconscious senses of guilt from the same unconscious need for punishment might not be impossible, it seems to me very doubtful that these complex interpretations ought to be represented as discoveries. As to their novelty, the fact of guilt in obsessional neuroses and melancholia had been recognised from the times of their first description. Moreover, it is peculiar in the extreme that nowhere in Freud's earlier writing, nor in the other psycho-analytic literature as far as I can ascertain, is there even a hint at a behaviour as striking as the negative therapeutic

reaction. Is one to believe it had previously escaped the scrutiny of Freud and the whole of the first generation of psycho-analysts?

A theoretical motive?

At this point we have every right to be suspicious of Freud's motives. His argument rests on the twin pillars of an admittedly unimportant need for terminological clarification and two inferences paraded as discoveries. When one considers Freud did not even allude to the really important criticisms of the topographic theory - including the crucial problem of structured fantasies in *Ucs.* he himself had raised some eight years earlier - one is quite unable to avoid the suspicion that something else was hidden in the theoretical woodpile.

Let us assume Freud was really faced with the problem indicated at the beginning of this chapter, of fitting the death instinct into his theory of the mind. It would then follow that under the pretence of pursuing a line of *empirical* enquiry Freud was really attempting a *logical* reorganisation of some of his concepts. Only slightly, if at all, was he adjusting those concepts to newly discovered facts. My views here agree with that part of Deigh's (1984) assessment which has it that the structural theory resulted from Freud's reorganising his views and bringing "to fruition several ideas the germs of which he had cultivated in earlier writings" rather than from new discoveries. I disagree with Deigh in seeing the structural theory precisely as a consequence of the introduction of a novel theoretical entity - a factor he dismisses. However, let us look at the theoretical needs from which I am certain the reconceptualisation sprang.

Superficial consideration alone shows Thanatos simply cannot be fitted into the old topographic theory. The death instinct cannot be located within *Cs./Pcs.*, for its manifestations would then have to be logical and rational, directly accessible to consciousness, and not subject to repression - requirements quite inconsistent with the instinct's projected role and in its having been so long overlooked. On the other hand, were it to be placed in *Ucs.*, dynamic considerations would require the existence of some other *Cs.* force to repress it. Further, the unstructured and primary process characterisation of *Ucs.* would make it difficult for Thanatos to come into organised conflict with sexuality there. The very concept of a death instinct occasioned a new kind of unconscious, one more unconscious than the merely latently unconscious but not forming part of the repressed unconscious as that system had been previously understood. A new kind of unconscious agency had to be invented, one inaccessible to *Cs./Pcs.* but one that could oppose the demands of Eros. The super-ego and the structural theory obviously met these requirements.

What basis is there for supposing Freud was undertaking a theoretically based reorganisation? We have two pieces of positive evidence. In July of 1922 Freud wrote to Ferenczi that he was "occupied with something

speculative, a continuation of *Beyond the Pleasure Principle*'' (cited in E. Jones, 1953-1957, *III*, p.99), and he repeated this thought in April, 1923 in the preface to *The Ego and the Id*:

> The present discussions are a further development of some trains of thought which I opened up in *Beyond the Pleasure Principle* and to which, as I remarked there, my attitude was one of a kind of benevolent curiosity. (Freud, 1923b, p.12)

The speculations toward which Freud had then been so indulgent were precisely that there might be a death instinct and that it might conflict with the demands of Eros.

We can be sure Freud envisaged the connection between the new mental structures outlined in *The Ego and the Id* and the instinctual theory of *Beyond the Pleasure Principle* as an especially intimate one even though only a small number of writers - Schafer (1960, 1970) and Ricoeur (1970, pp.281-309) among them - has remarked on it. I think Freud did not get beyond sensing the relation and it may be that this implicitness is responsible for the connection being overlooked. Certainly Freud did not establish it as a logical necessity or an empirical finding. The new theory was neither justified by his terminological critique, which was weak, nor by his observations, for there were none.

SUPER-EGO, DEATH INSTINCT, AND IDENTIFICATION

The super-ego was the structure through which the death instinct controlled and sometimes punished the ego's expression of the sexual drive. Most of the materials from which Freud constructed his new concept are to be found in the older concepts of ego-ideal and conscience. However, in the new building the ego-ideal was almost totally reconstituted. Most importantly, the functions of the conscience extended to punishment.

Originally Freud had thought the ego-ideal formed when children found it impossible to retain their early narcissistic perfection. They sought to recover a lost state of bliss by reviving in themselves the ideal ego originally belonging to the phase of primary narcissism:

> What he projects before him as his [ego] ideal is the substitute for the lost narcissism of his childhood in which he was his own ideal. (Freud, 1914b, p.94)

Critical admonitions first voiced by the parents prompted the formation of this ego-ideal. As the individual developed, the voices of others, especially teachers, peers, and public opinion were added to the individual's own critical judgements (op. cit., pp.94, 96). Conscience, which embodied the criticisms, ensured narcissistic satisfaction by constantly scrutinising the actual ego and measuring its adequacy against the ideal (op. cit., p.95).

The super-ego [1] of the new theory included standards as well as a conscience. But there were two crucial differences: one in the functions of the new structure, the other in its mode of formation. The functions were extended beyond mere scrutiny. Freud now said the normal conscious sense of guilt or conscience was "The expression of a *condemnation* of the ego by its critical agency" (Freud, 1923b, p.51. My emphasis, MBM). In various pathological conditions the ego was described as being treated with "extraordinary harshness and severity" (op. cit., p.53). For example, in melancholia "The whole of the sadism available in the person" raged against the ego (ibid.).

Ten years later, in the *New Introductory Lectures on Psycho-analysis*, Freud clarified the functions of the super-ego by assigning each to a specific substructure within it. First, the standards were carried within the super-ego by the ego-ideal. Second, the scrutiny of the actual behaviour of the ego, its measurement against the standards, was carried out by a special observing agency. Lastly, punishment for infringements was meted out by the conscience. The term super-ego referred to the comprehensive structure covering these three distinct functions (Freud, 1933b, p.66. Cf. pp.58-65). Of them, the most novel, and the one differentiating the super-ego from any of its precursors, was the function of self-punishment. Through this punishment Freud gave the death instinct a place in mental life.

The origins of the super-ego

What were the origins of the new structure? Narcissism barely came into it. Freud now envisaged the super-ego forming during the demolition of the Oedipus complex. By a process he called *identification* a substructure based on the values and standards of the parents formed within the ego. Simultaneously a process he termed *defusion* provided the energy of the death instinct to this altered part of the ego.

Defusion Considering defusion first, we note it was actually one of two related assumptions forced on Freud by his characterisation of the death instinct. It will be recalled that the death instinct was initially directed at the very fabric of the organism; it sought to silence every sign of life and to return each cell to its original, inanimate condition. For the organism to survive for even a minimal period this tendency to self-destruction had to be countered. As Freud saw it, instinctual drives like sex and hunger interfered with this tendency. Eros neutralized the immediate self-destructive

1. In *The Ego and the Id* Freud used the term ego-ideal as synonym for super-ego (e.g. Freud, 1923b, pp.28, 34, 36). Only in his later writing did he differentiate the ego-ideal as a part of the super-ego, describing the super-ego as the vehicle or bearer of the ego-ideal (Freud, 1933b, pp.64-65 and n.1). The distinction involves more than the relatively minor semantic point it is sometimes represented as being (Editor's Note, *Standard Edition*, *19*, pp.9-10; Laplanche and Pontalis, 1967/1973, pp.144-145). For the present I write as if Freud had already made and meant something important by this finer differentiation and will discuss it more fully later.

efforts of Thanatos by first gathering individual cells into colonies and then combining with Thanatos to create an externally directed impulse of aggressiveness or destructiveness.

Conceptually speaking, the first step in placing the death instinct within the organism had to be to find ways of mitigating its effects. Freud's conception of Thanatos fusing with Eros was an absolutely necessary consequence of the revised instinct theory. As he himself put it, the fusion of these instincts was "an *assumption* indispensable to our conception" (Freud, 1923b, p.41. My emphasis, MBM). But, once the assumption had been made, a second became just as necessary: Thanatos had to be separated from Eros. Defusion had to be assumed if the super-ego were to be invested with a portion of the death instinct:

> Once we have admitted the idea of a fusion of the two classes of instincts ... the possibility of a - more or less complete - 'defusion' of them *forces itself upon us.* (ibid. My emphasis, MBM)

As we shall see, Freud further assumed that there was relatively free communication between the ego and the id. Through that channel the defused portion of the death instinct was made available. Now charged with the might of Thanatos, the super-ego could oppose the tumultuous strivings of Eros.

Identifications How did identification bring about these consequences? While Freud used the term identification to mean different things at different times, his common meaning was that identification was an alteration of the individual's behaviour such that it becomes more like someone else's. At the level of interpretation, one ego becomes more like that of another. Freud had initially made most use of this notion of identification as a change in the ego in explaining some of the symptoms of those disorders that, as in some depressions (or melancholias), had been preceded by the loss of a sexual object. He believed libido was then detached from the mental representation of the object and returned to the ego where it was used to establish an identification of the ego with the lost object. The self-reproaches of the melancholic, which might sometimes culminate "in a delusional expectation of punishment" (Freud, 1917c, p.244), were nothing more than the reproaches that the patient had originally directed against the object. Now however, they were directed to his own ego:

> Thus the shadow of the object fell upon the ego, and the latter could henceforth be judged by a special agency, as though it were an object, the foresaken object. In this way an object-loss was transformed into an ego-loss and the conflict between the ego and the loved person into a cleavage between the critical activity of the ego and the ego as altered by identification. (op. cit., p.249)

The 'critical activity' was, of course, the conscience. In melancholia, the modifications produced in the ego by its incorporation of the lost object were sufficient to direct the reproaches of conscience against it.

It seemed to Freud that the lost object must have had two contradictory properties. First, the fixation to it had to have been very strong - otherwise it would not have become so important. On the other hand, it had been given up relatively easily - its cathexis would not have had much power of resistance. Freud resolved the apparent contradiction by assuming the original basis for choosing the object had been a narcissistic one. He proposed a mechanism of oral incorporation, a concept modelled on the act of eating:

> The ego wants to incorporate this object into itself, and, in accordance with the oral or cannibalistic phase of libidinal development in which it is, it wants to do so by devouring it. (op. cit., pp.249-250)

This identification, said Freud, "represents, of course, a *regression* from one type of object-choice to original narcissism" (op. cit., p.249). He assumed super-ego formation partly involved a similar regression to narcissism with a similar identification-by-incorporation.

Not every identification from which the super-ego formed incorporated an object. In *Group Psychology and the Analysis of the Ego*, Freud had already recognised another kind of identification not based on object-choice at all. There he declared that what may be called primary identification was "the earliest expression of an emotional tie with another person" (Freud, 1921, p.105. Cf. p.107). It was not at all the same thing as an object-choice:

> It is easy to state in a formula the distinction between an identification with the father and the choice of the father as an object. In the first case one's father is what one would like to *be*, and in the second he is what one would like to *have* .... The former kind of tie is therefore already possible before any sexual object-choice has been made. (op. cit., p.106)

Compared with this descriptive distinction, a meta-psychological or theoretical representation of it was, Freud wryly noted, "much more difficult" (ibid.).

In *The Ego and the Id*, Freud brought both kinds of identification together in the following simplified account of the origins of the super-ego:

> At a very early age the little boy develops an object-cathexis for his mother, which originally related to the mother's breast and is the prototype of an object-choice on the anaclitic model; the boy deals with his father by identifying himself with him. (Freud, 1923b, p.31)

As the boy's sexual wishes for the mother grew more intense, so the father was more frequently perceived as an obstacle to them, and the Oedipus complex developed. Thereupon the boy's identification with the father took on a hostile, aggressive colouring and after that his relation to him was based on an ambivalent mixture of love and hate. With the demolition of the Oedipus complex, the object-cathexis of the mother was given up and

its place taken by either an identification with her, an identification analogous with that in melancholia, or by an intensification of the identification with the father (op. cit., pp.31-32). This latter outcome was, said Freud, "the more normal" (op. cit., p.32).

Bisexuality Outcomes were not usually simple. An omnipresent constitutional bisexuality caused the Oedipus complex to have a negative or inverted form as well as a positive. The positive complex in the boy, the one just discussed, is the combination of sexual desire for the mother with an ambivalent but basically hostile death wish toward the father. In it there is an object-choice of the mother and a primary identification with the father. The negative or inverted complex was based upon the boy's *femininity*. It led him to choose his father as object and make a primary identification with his mother. When the Oedipus complex was dissolved, the four trends grouped themselves to produce:

> a father-identification and a mother-identification. The father-identification will preserve the object-relation to the mother which belonged to the positive complex and will at the same time replace the object-relation to the father which belonged to the inverted complex: and the same will be true, *mutatis mutandis*, of the mother-identification. (op. cit., p.34)

He had his summary printed with emphasis:

> *The broad general outcome of the sexual phase dominated by the Oedipus complex may, therefore, be taken to be the forming of a precipitate in the ego, consisting of these two identifications in some way united with each other. This modification of the ego retains its special position; it confronts the other contents of the ego as [a] ... super-ego.* (ibid.)

Identification thus gave the super-ego its content as well as the energy necessary to execute its destructive functions.

THE EGO AND THE TWO INSTINCTS

We have seen how the death instinct broke down the isomorphism of the structural with the topographic theory. It required, in essence, an unconscious portion of *Cs./Pcs.* to be separated off and invested with the completely new function of punishment. Nor was there a match between what remained - the new ego operated in a fundamentally different manner from *Cs./Pcs.* Again the difference was due to the death instinct. Because the basic conflict was now between Thanatos, housed in the super-ego, and Eros, located in the id, rather than between an active repressing force in *Cs./Pcs.* and a sexual instinctual drive in *Ucs.*, the ego had to become a passive agency, almost a kind of arena on which the battle took place. As a consequence, the cognitive functions of perception, memory, judgement, and motor control were emphasised much more in the ego of the structural theory than they had been in *Cs./Pcs.* They enabled an essentially cogni-

tive basis for the decision to repress or not. And, although in one sense the ego's decision initiated repression, it was not the ego itself that carried it out, but the pleasure principle operating under the aegis of the death instinct in the super-ego.

In *The Ego and the Id*, Freud's proposals, although fragmentary, point up the changed role of the ego. First was the passivity of this new ego. It was:

> a poor creature owing service to three masters and consequently menaced by three dangers: from the external world, from the libido of the id, and from the severity of the super-ego. (Freud, 1923b, p.56)

However, Freud gave this ego the power of apprehending anxieties arising from these sources. Where there was danger, the ego emitted anxiety and, if the danger came from the id, repression was initiated (ibid.). But the initiative was not the ego's:

> as a rule the ego carries out repressions in the service and at the behest of its super-ego. (op. cit., p.52. Cf. Freud, 1924a, p.150)

Subordination was logically inevitable. If Freud were to continue asserting that repression derived from the individual's moral standards, and if he had transferred those standards to the super-ego, nothing remained in the ego from which repression could proceed. Very early Freud had made the ego's standards the point of departure for repression and, while that line of thinking seems to have been temporarily eclipsed by the stress he put on the organic theory of automatic repression in the period 1900-1912, the standards were re-emphasised from 1912 onward (Chapter 11). When, then, he re-iterated that repression "took its stand on aesthetic and ethical motives", as he was to do in an historical account of psycho-analysis contemporaneous with *The Ego and the Id* (Freud, 1924c, p.197), it had to be the case that repression could really be instigated only by the structure housing those motives, namely, the super-ego.

There was a second respect in which the new ego was passive: it had no energy of its own that could be sensibly conceptualised as opposing and controlling the sexual instinctual drive. Nothing corresponding to the energy of the self-preservation drive had been given it. All Freud provided was a form of libido so emasculated it could hardly be thought of as standing up to and directly denying the demands of its parent. But it was enough to allow the ego to perceive and evaluate instinctual danger and, if necessary, produce a signal of anxiety. Through the death instinct, the pleasure principle did the rest. Consequently, it was possible:

> to picture the id as under the domination of the mute but powerful death instincts, which desire to be at peace and (prompted by the pleasure principle) to put Eros, the mischief-maker, to rest. (Freud, 1923b, p.59)

Meaning had been given to the otherwise peculiar conclusion of *Beyond*

the Pleasure Principle that the pleasure principle seemed "actually to serve the death instincts" (Freud, 1920a, p.63).

Given its passive nature and its reliance on the super-ego, what the new ego required in order to carry out its tasks were functions that can broadly be referred to as cognitive. Given Freud's style of theorising, it also had to be endowed with some energy enabling those functions to be exercised. It is to these two aspects of the new concept that I now turn.

The ego's cognitive functions

Not all instinctual pressures necessitated repression - drives had sometimes to be satisfied. Nor did all danger arise from within. That which came from outside required a choice between flight or active defence. The ego had therefore to be able to perceive stimuli, to assign an external or internal origin to them, to assess them for potential danger, and then to institute appropriate action. In his earlier theorising, Freud had derived these judgemental and motor functions from the perceptual, motor, and memory systems. In *The Ego and the Id*, and several other works from between 1923 and 1925, Freud synthesised the systems slightly differently from the way he had done earlier. For the most part, the functions themselves had already been described or suggested in the *Project* of 1895, in *The Interpretation of Dreams* of 1900, in the papers on metapsychology of 1914, and in *Beyond the Pleasure Principle* of 1920. What had now to be done was to make them consistent with the new theory of repression, especially with the signal role given to anxiety.

Energy and protection Freud began the new synthesis by returning to his fictional primitive organism suspended in its world of potentially lethal energies. In the *Project* he had already argued that the sense organs protected the nervous system from intense quantities of excitation (Freud, 1950/1954, *Project*, Part I, Sections 5-9 of 1895) and in *Beyond the Pleasure Principle* he developed this notion into the concept of a protective shield absorbing and attenuating most of the energy before passing it on (Freud, 1920a, pp.27-29. Cf. Freud, 1940a, pp.145-146). Five years later, in a short paper exploring an analogy with some of these concepts, Freud claimed he had *shown* that there was such a shield (Freud, 1925b, p.230). Although he may sometimes have given the impression that the shield was a mere passive membrane, a kind of baked crust, Freud clearly meant it to have active properties:

> The protective shield is supplied with its own store of energy and must above all endeavour to preserve the special modes of transformation of energy operating in it against the effects threatened by the enormous energies at work in the external world. (Freud, 1920a, p.27)

The first purpose for which Freud required the shield to have its own energy was to enable it to transform freely flowing energy into a bound or

quiescent state. Binding reduced the intensity of potentially damaging external excitation or held it back altogether.

Energy and perception The second purpose for which energy was required was to give perception a protective function. Freud had already supposed the sensory receptors - collectively the system *Pcpt.-Cs.* - to lie beneath the protective shield (Freud, 1950/1954, *Project*, Part I, Sections 3, 5, and 9). He now went on to suppose they were supplied with an energy:

> cathectic innervations are sent out and withdrawn in rapid periodic impulses from within into the ... system *Pcpt.-Cs.* So long as that system is cathected in this manner, it receives perceptions (which are accompanied by consciousness) and passes the excitation on to the unconscious mnemic systems; but as soon as the cathexis is withdrawn, consciousness is extinguished and the functioning of the system comes to a standstill. (Freud, 1925b, p.231. Cf. Freud, 1920a, pp.27-28; 1925d, p.238)

As we saw in Chapter 12, Freud believed the traumatic effects of excessive stimulation were reduced by the individual's expectation of danger or preparedness for it (Freud, 1920a, pp.13, 33). Because he believed that preparedness to be an "increased sensory attention and motor tension" (Freud, 1916-1917, pp.394-395), and sensory attention to be a cathection of the sense organs, the periodic innervation of *Pcpt.-Cs.* had a protective function. This is what Freud seems to have meant when he said:

> preparedness for anxiety and the hypercathexis of the receptive systems constitute the last line of defence of the shield against stimuli. (Freud, 1920a, p.31)

And it seems Freud was referring to the periodic sampling when he said the sense organs also included:

> special arrangements for further protection against excessive amounts of stimulation and for *excluding unsuitable kinds of stimuli.* (op. cit., p.28)

Small amounts of cathexes periodically supplied to *Pcpt.-Cs.* allowed it to sample the external world, to take in specimens, as it were, and to determine the nature and direction of any external stimulation and, if necessary, to subdue it by binding (op. cit., pp.27-31. Cf. Freud, 1925b, p.231; 1925d, p.238).

Pcpt.-Cs. also received stimuli from within. Freud had always believed the most important internal stimuli to be the feelings of unpleasure and pleasure caused by the pressure and discharge of instinctual drives (Freud, 1950/1954, *Project*, Part I, Section 8. Cf. Freud, 1900, pp.598-603; 1915a, pp.120-121). Building on an argument first advanced in *The unconscious* (Freud, 1915c, pp.177-178) he reiterated that these feelings of pleasure and unpleasure became conscious directly (Freud, 1923b, pp.21-22. Cf. Freud, 1940a, pp.145-146).

Energy, judgement, and reality testing Freud's co-ordination of this explicit sampling-protective view of *Pcpt.-Cs* with the motor functions hypothesised earlier allowed him to attribute judgemental functions to the new ego. In his very earliest theorising, Freud had supposed there to be a direct connection between the motor and perceptual systems and that the consequences of movement differentiated external sources of excitation from internal. Withdrawal, for example, caused external stimulation to cease but had no such effect upon endogenous sources of excitation (Freud, 1950/1954, Part I, Section 1. Cf. Freud, 1900, p.565). As we saw in Chapter 10, this differential result was incorporated into the definitions of stimulus and instinctual drive. The former could be "disposed of by a single expedient action" such as flight or withdrawal but the latter could not (Freud, 1915a, p.118).

By these distinctions the organism also differentiated external from internal and the real or objective from the unreal or subjective. Freud argued that in what he termed "the efficacy of its muscular activity", the organism had "a basis for distinguishing between an 'outside' and an 'inside'":

> On the one hand, it will be aware of stimuli which can be avoided by muscular action (flight); these it ascribes to an external world. On the other hand, it will also be aware of stimuli against which such action is of no avail and whose character of constant pressure persists in spite of it; these stimuli are the signs of an internal world, the evidence of instinctual needs. (Freud, 1915a, pp.119. Cf. p.134)

Then, slightly later, he made the additional point that this same movement made judgements of reality possible:

> A perception which is made to disappear by an action is recognized as external, as reality; where such an action makes no difference, the perception originates within the subject's own body - it is not real. (Freud, 1917a, p.232)

'Reality-testing', Freud's term for this judgemental function, had been developed by him from the connection between the perceptual and the motor apparatus.

Note the active role that reality-testing imposed on the organism and the need it created for an energy with which to carry it out. *Cs.(Pcpt.)* said Freud:

> must have at its disposal a motor innervation which determines whether the perception can be made to disappear or whether it proves resistant. (op. cit., p.233)

After stressing that "Reality-testing need be nothing more than this contrivance", Freud assigned that function squarely to his then concept of the ego (ibid.), although he later vacillated and placed it temporarily within the

ego-ideal (Freud, 1921, p.114). When he reinstated reality-testing as one of the major institutions of the new ego (Freud, 1923b, p.55), it was again through the connection of *Pcpt.-Cs.* with the motor system. That connection itself had to be placed within the new ego (Freud, 1925d, pp.237-238).

Connecting memory with *Pcpt.-Cs.* followed in a similar way from Freud's earlier theorising. In both the *Project* and *The Interpretation of Dreams* the memory systems had been placed immediately behind the perceptual, a proximity that seems to have been based on Freud's twin beliefs that stimuli causing perceptions necessarily left traces of their passage and that memory elements were of the same basic type as the perceptual (Breuer and Freud, 1895, pp.188, n1; Freud, 1950/1954, *Project*, Part I, Section 3; 1900, pp.538-540). Whatever the reasons, once memory had been so situated, the ego had been given a function enabling it to make ready comparisons between current stimulation and traces of previous experience.

Between them, these connections between the motor, the memory, and the perceptual systems gave the primitive organism the potential for perceiving stimuli, distinguishing them from instinctual drives, differentiating between its inner and outer worlds, testing the reality of its percepts, and remembering the effects of its actions. But its actions were very circumscribed. Really expedient action was limited - instinctual drives could be satisfied only in the hallucinatory mode and external stimuli could only be avoided. More was required. Freud reasoned that, if immediate primary process discharge could be delayed, earlier experiences could be scrutinised for appropriate courses of realistic action. He therefore gave the nucleus of the ego - the systems *Pcpt.-Cs.* - its own cathectic energy as well as the power to use it in any way at all. With these cathexes, Freud had given the ego the ability to control what he variously termed the approaches to or access to motility, that is, to delay or initiate motor discharge.

The ego's power over its cathexes and its control of movement also enabled it to think. However, even that function was not new nor was its attribution to an ego or ego-like structure. From the time of the *Project*, Freud had conceptualised thought as an experimental kind of *action*, an exploration of the mnemic traces of the movements that had led to previous satisfactions (Freud, 1950/1954, *Project*, Part III, Sections 1 and 4 of 5.10.1895. Cf. Freud, 1900, pp.566-567, 598-599). He therefore retained this control over the motor system as one of the two essential characteristics of the new ego (Freud, 1923b, p.55. Cf. Freud, 1925d, p.238).

The other essential characteristic of the ego also derived from its omnipotent control of its own cathexes but, unlike the ego's control over the motor system, it was absolutely novel. Freud's sudden announcement in the concluding pages of *The Ego and the Id* that "The ego is the actual seat of anxiety" (Freud, 1923b, p.57) was quite unheralded. Freud meant

two things. First, only the ego could generate anxiety, that is, emit it as a signal (ibid.). Second, as he added in a later clarification, only the ego could experience anxiety (Freud, 1926a, p.140). He later summarised the two functions by saying "the ego alone can *produce* and *feel* anxiety" (Freud, 1933b, p.85. My emphasis, MBM). Disposable cathexes enabled the ego to judge whether an instinctual danger was present and to generate the anxiety causing the super-ego to repress the drive.

The ego's energy

How, then had the ego acquired its displaceable store of energy and precisely how did it use it? Taking the question of origins first, Freud argued that the energy was a special form of libido created during identification. Identification altered the ego so it resembled the parents and could be loved in the same way as them. The ego thus represented itself to the id as the original love objects and thereby attracted libido to itself (Freud, 1923b, pp.29-30). In doing so, Freud asserted, the libido became narcissistic and lost its sexual quality:

> The transformation of object-libido into narcissistic libido which thus takes place obviously implies an abandonment of sexual aims, a desexualization - a kind of sublimation, therefore. (op. cit., p.30)

Desexualised, sublimated Eros was the ego's neutral and displaceable psychic energy.

Despite its transformation sublimated energy retained:

> the main purpose of Eros - that of uniting and binding - in so far as it helps towards establishing the unity, or tendency to unity, which is particularly characteristic of the ego. If thought-processes in the wider sense are to be included among these displacements, then the activity of thinking is also supplied from the sublimation of erotic motive forces. (op. cit., p.45)

Consciousness itself was pictured as arising from displacements of energy. Freud here drew on some concepts he had first outlined in *On Aphasia* (Freud, 1891/1953, pp.77-78. Cf. *Standard Edition, 14*, pp.209-215), and expanded upon in correspondence with Fliess (Masson, 1985, Letters of 30.5.96, 6.12.96, 22.12.97; Freud, 1950/1954, *Project*, Part III, Section 1). In *The Ego and the Id*, Freud proposed as a precondition for an unconscious idea becoming preconscious, and so acquiring the potentiality for consciousness, was that it become connected with the words corresponding to it. An unconscious idea was a 'thing-representation', a direct mental representation of an earlier perception, particularly of its visual components. It was "the cathexis, if not of the direct memory-images of the thing, at least of remoter memory-traces derived from these" (Freud, 1915c, p.201). Verbal mnemic residues deriving primarily from the auditory perceptions corresponding to the thing-representations formed what Freud called 'word-representations'. The ego raised unconscious ideas to preconsciousness by using its energy to link thing-representations with word-

representations (Freud, 1923b, pp.19-23). An additional attentional hypercathexis then brought them to consciousness proper. Consequently, Freud said it was to the ego that consciousness was attached (op. cit., p.17). [2]

We can now appreciate Freud's summary propositions. The ego:

> starts out, as we see, from the system *Pcpt.*, which is its nucleus, and begins by embracing the *Pcs.*, which is adjacent to the mnemic residues. (Freud, 1923b, p.23. Cf. Freud, 1926a, p.92)

The motor connection was stressed because:

> the functional importance of the ego is manifested in the fact that normally control over the approaches to motility devolves upon it. (Freud, 1923b, p.25)

And the significance of what Freud called the important functions entrusted to the ego were:

> By virtue of its relation to the perceptual system it gives mental processes an order in time and submits them to 'reality-testing'. By interposing the processes of thinking, it secures a postponement of motor discharges and controls the access to motility. (op. cit., p.55)

Given its store of displaceable energy and its particular components this ego could sense dangers, inhibit discharge, and act rationally. When it generated anxiety and called the pleasure principle into operation instinctual drives were repressed. However, before examining how repression took place, we have to consider the forces the ego had to control.

UCS., THE DEATH INSTINCT AND THE ID

To his revised concept of the ego Freud now opposed not simply the repository of repressed ideas constituting the topographic *Ucs.* but a new structure containing the totality of those impersonal and uncontrollable forces that gave people the impression they were creatures of obscure powers, lived by alien drives and urges, and acted upon as passive objects. Freud derived the name for this new structure from Georg Groddeck, a German physician of speculative persuasion and friend of psycho-analysis. Groddeck held the view that:

> man is animated by the Unknown, that there is within him an 'Es', an 'It', some wondrous force which directs both what he himself does, and what happens to him. The affirmation 'I live' is only conditionally correct, it expresses only a small and superficial part of the fundamental principle, 'Man is lived by the It'. (Groddeck, 1923/1949, p.11)

Groddeck's term 'das Es' ['The It'] has been rendered by the Latin 'Id' in

2. Like Compton (1986a), I prefer 'representation' to Strachey's 'presentation' and will use that term as much as possible.

English translations of Freud's works. Freud almost completely disregarded Groddeck's meaning in annexing the term. He made his id the repository of the death as well as the sexual instinctual drives and attributed all the primary process characteristics of *Ucs.* to it. As he remarked some ten years later:

> This impersonal pronoun seems particularly well suited for expressing the main characteristic of this province of the mind - *the fact of its being alien to the ego.* (Freud, 1933b, p.72. My emphasis, MBM)

Naively, "The ego represents what may be called reason and common sense, in contrast to the id, which contains the passions" (Freud, 1923b, p.25). The id was everything the ego was not.

If the ego was that part of the mind that started out from *Pcpt.*, the id was the unconscious part into which it extended. This notion of the ego merging with the id constitutes another of the main differences between the structural and topographic theories. Whereas the more-or-less absolute barrier of repression had prevented contact between *Ucs.* and *Cs.-Pcs.* in the earlier theory, direct communication between ego and id was possible. As Freud put it, "The ego is not sharply separated from the id; its lower portion merges into it" (op. cit., p.24). Freud so allowed for the effects of the third kind of unconscious, the non-repressed kind. Moreover, this direct avenue was available to the repressed mental contents themselves; they also merged with the id, forming part of it and communicating with the ego "through the id" (ibid.).

Now, the absence of a distinct barrier between the repressed and the id (and hence the ego) did not mean repression and the storehouse of repressed ideas had declined in importance. So drastic a modification would have required Freud to rewrite almost every page of his previous explanations of mental life. What was being allowed for was the retention of a part of the old in the context of the new. Repression separated the repressed contents only from the ego. As Freud's diagrams show, on one side of the storehouse of repressed ideas was the wall of repression, while on the other, there was an open archway, leading to the id and from thence directly to the ego.

The idea that some unconscious processes might have direct access to higher structures was not new to Freud's thinking in 1923. As early as the metapsychological paper on *The Unconscious*, where the role of word-representations in bringing unconscious ideas to consciousness had been argued most fully, Freud had insisted there were no such things as unconscious affects (Freud, 1915c, pp.177-179). Freud repeated this argument in *The Ego and the Id*, adding that, when emotions and feelings became conscious, they did so directly, without requiring word-representation linkages at all. His reiteration came just before his description of the id, a description that emphasised the absence of a sharp division between it and the ego (Freud, 1923b, pp.22-23). Perhaps it was Freud's purpose to suggest that

what might be true of emotions in the unconscious was true of the whole range of contents in the id. Although it is a not-very-plausible analogy, the point does foreshadow part of Freud's explanation for the second of his claimed discoveries. An unconscious need for punishment positively demanded direct communication between the id and that part of the ego from which punishment proceeds. It also allowed for instinctually related anxieties to become conscious directly and to serve as signals of danger and so precipitate repression.

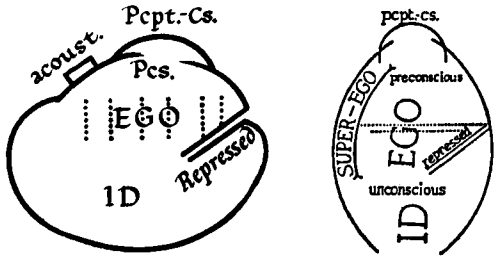

Figure 13.1. The structures of the structural theory (Freud, 1923b, p.24 and 1933b, p.78)

ANXIETY AND REPRESSION

A central proposition of the structural theory was that anxiety caused repression. Freud several times apologised for this about face - his earlier formula had had repression causing anxiety (Freud, 1926a, pp.93, 109. Cf. Freud, 1933b, p.89). More than a simple modification to the mechanism of repression was involved however. Freud had to abandon or drastically change his propositions about the essence of neuroses, to find a source of anxiety other than libido, and, in order to incorporate castration and super-ego anxieties, the distinction between primal repression and repression proper had to be rethought. I take these matters up sequentially.

Toxicity and the essence of neuroses

Almost from the time he began writing about neuroses Freud stressed that their essence was a toxic state. Quite early he drew parallels between neuroses and conditions of what he called auto-intoxication like the over-

activity of the thyroid gland in Graves disease or exopthalmic goitre (Freud, 1888a, p.41; 1892-1894, pp.139-140, Note to Charcot's p.237). He cited this parallel frequently, usually as part of his argument that intoxication produced by accumulated sexual substances or their break-down products were basic to both the actual and the psychoneuroses (Masson, 1985, Draft I, undated, possibly of 8.10.95, Letter of 1.1.96; Freud, 1950/1954, *Project*, Part I, Section 12; 1905b, pp.215-216 and 1920 modification; 1906a, pp.278-279; 1908c, pp.185-186; 1910e, p.218; 1912b, p.248; 1916-1917, pp.388-391; 1925c, pp.214-215; 1925a, pp.25-26). We have seen how Freud believed anxiety in anxiety neurosis was caused by the deflection of the libido into the autonomic nervous system in an unsuccessful attempt to discharge sexual toxins (Masson, 1985, Draft E, possibly 6.6.94; Freud, 1895a; 1895b; 1916-1917, pp.401-403). He believed he had discovered a similar process in the psychoneuroses:

> Analyses of hysteria and obsessional neurosis yield the ... conclusion that a similar deflection with the same outcome may also be the result of a refusal on the part of the *psychical* agencies. (Freud, 1916-1917, p.404)

Anxiety in the psychoneuroses could be seen as a similar attempt to discharge toxins. Repression could do away with neither libidinal energy nor the substances from which it derived.

Toxicity and birth Freud combined his toxic theory with the ego's signal function by searching for an original danger situation having toxic accompaniments the threatened recurrence of which could be signalled by the ego. He found this prototype of neurotic anxiety in the universal experience of birth. As early as 1908-1909 he had drawn attention to *anxiety* during birth:

> the act of birth is the first experience of anxiety, and thus the source and prototype of the affect of anxiety. (Freud, 1900, p.400-401, n.3 of 1909 written in 1908. Emphasis altered, MBM. Cf. Nunberg and Federn, 1962-1975, 2, Minute 87 of 17-11-1909, pp.323-324)

Two years later he connected this first anxiety with *danger*. Birth was:

> both the first of all dangers to life and the prototype of all the later ones that cause us to feel anxiety, and the experience of birth has probably left behind in us the expression of affect which we call anxiety. (Freud, 1910d, p.173)

What was the danger? About six years later Freud specified it as the toxins which accumulated in the infant's bloodstream between birth and its independent respiration being established:

> The immense increase of stimulation owing to the interruption of the renovation of the blood (internal respiration) was ... the cause of the experience of anxiety; the first anxiety was thus a toxic one. The name 'Angst' - 'angustiae', 'Enge' [Latin and German for 'narrow place' and 'straits'] - emphasizes the characteristic of restriction in breathing which

> was then present as a consequence of the real situation and is now almost invariably reinstated in the affect. (Freud, 1916-1917, pp.396-397. Cf. Nunberg and Federn, 1962-1975, 2, Minute 60 of 25.11.1908, pp.71-72)

Neurotic anxiety could find its prototype in birth anxiety because both were reactions to toxins. For the same reason birth was the basis for the most common anxiety of childhood - separation from the mother or longing for her. During separation, libidinal desire for the mother led to toxicity and from thence to anxiety, just as in the actual neuroses (Freud, 1916-1917, pp.405-408).

Toxicity and normal anxiety If the ego's reaction to current threats could be put in toxic terms, a little juggling made it possible to link non-neurotic anxiety to birth. During 1916-1917, when Freud was still thinking in terms of an instinct theory that pitted libido against self-preservation, the only things that could be discharged in response to a current threat were the energy of the self-preservation instinct (more correctly, the egoism of the ego-instincts) or the narcissistic libido invested in the ego, the ego-libido. Freud's choice had then been monistically clear:

> the state of anxiety is in every instance inexpedient, and its inexpedience becomes obvious if it reaches a fairly high pitch. In such cases it interferes with action, whether flight or defence, which alone is expedient and alone serves the cause of self-preservation. If, therefore, we attribute the affective portion of realistic anxiety to ego-libido and the accompanying action to the self-preservative instinct, we shall have got rid of the theoretical difficulty. (op. cit., p.430. Cf. p.405)

Being the energy of the self-preservation drive, egoism could not be toxic. Ego-libido, on the other hand, had all the properties of sexual libido, including its toxicity. After all it was only ordinary libido lodged in the ego. Normal anxiety could be given the same toxic source as neurotic anxiety if it resulted from the discharge of ego-libido.

Actually whenever Freud had discussed anxiety during the first ten to fifteen years of his work his references were always to neurotic anxiety. During the whole of that period there was only one mention of normal anxiety and it is a quite unimportant, non-technical and passing one (Freud, 1895a, p.93). More or less suddenly in the *Introductory Lectures* he differentiated realistic from neurotic anxiety. Normal anxiety was the reaction of the ego to the perception of an external threat and was expedient to the extent it was a signal to prepare for danger rather than being a massive and overwhelming trauma. On this ground, normal anxiety seemed rational and intelligible (Freud, 1916-1917, pp.393-394). But, why were the discharge patterns identical? The only alternatives Freud seems to have considered were that the two kinds of anxiety might have a similar toxic basis or that they might be provoked by similar external conditions. He chose the first kind of explanation. We have seen that Freud had a similar choice in accounting for the resemblance of birth to

other anxieties. Was it because they were all reactions to toxins or because they were provoked by similar dangers? Initially, Freud again chose toxicity - accumulated ego-libido caused realistic anxiety.

Toxic conceptualisations had to be given up in the structural theory. By then, Freud was not insisting on the special connection between anxiety and libido required by the thesis that repression caused anxiety. Nor, by then, was there any distinction between self-preservation and sexual drives - both were coalesced in Eros - and egoism could now no longer be distinguished from ego-libido or any other form of libido. Consequently, the revisions of the structural theory forced Freud to choose the second alternative: neurotic and objective anxiety now had in common the fact they were provoked by similar conditions of danger. Even birth anxiety, although still a reaction to toxicity, was not linked to other anxieties simply because of that fact. What anxiety situations had in common was excessive amounts of stimulation. What, then, was the source of the excess?

The sources of anxiety

It was in *Inhibitions, Symptoms and Anxiety* (1926a) that Freud rethought the problem of the sources of anxiety. There he proposed that certain very early traumatic experiences left behind them memory traces that included traces of the affect. When the ego later compared new stimuli with its earlier records it might judge the same situation was about to recur. It would then inhibit the primary process discharge and revive an attenuated form of the earlier anxiety as a signal.

Defence and flight Freud's explicit model for defence against unwelcome internal stimuli was the organism's flight from external danger. At first the organism merely withdrew its cathexis from the dangerous perception; it made only a token attempt to flee. As the ego matured, the cathexis was used to produce a movement that took it away from the danger. Repression was:

> an equivalent at this attempt at flight. The ego withdraws its (preconscious) cathexis from the instinctual representative that is to be repressed and uses that cathexis for the purpose of releasing unpleasure (anxiety). (Freud, 1926a, pp.92-93)

But the preconscious ego cathexis was not itself transformed into anxiety. Withdrawing it reduced the total cathexis and:

> according to our assumptions, unpleasure and anxiety can only arise as a result of an *increase* in cathexis. (op. cit., p.93)

What the ego cathexis was used for was to reproduce the affect:

> Anxiety is *not newly created* in repression; it is *reproduced* ... in accordance with an already existing mnemic image. (ibid. My emphasis, MBM)

Neither could Freud argue that this reproduced anxiety was explicable in economic terms. Reproduction required only a trivial increase in the total quantity of energy - just enough to generate the signal. Freud was therefore forced to propose that anxiety was generated in two different ways. Excessive stimulation breaking through the protective shield caused a profound economic disturbance experienced as a very intense anxiety. Freud termed the breakthrough a *traumatic situation* and the anxiety produced in it *automatic anxiety*. The impending recurrence of an original trauma placed the organism in what he called a *danger situation* during which the original memory might be used to produce *signal anxiety* (op. cit., pp.137-138, 166. Cf. Freud, 1933b, pp.93-95).

The components of anxiety What then *was* anxiety? Freud thought it had three characteristics. First, there was the specific quality differentiating anxiety from other affects, especially from other unpleasant emotions. Second, there were the motor innervations of the various bodily organs - the processes of discharge - particularly those that increased the heart-rate and breathing. Lastly, there were the perceptions of the discharge. Of the connection between the characteristics, Freud said:

> In accordance with our general views we should be inclined to think that anxiety is based upon an increase of excitation which on the one hand produces the character of unpleasure and on the other finds relief through the acts of discharge already mentioned. But a purely physiological account of this sort will scarcely satisfy us. (Freud, 1926a, p.133)

Leaving aside the doubtful proposition that this account was a physiological one (excitation and discharge had ceased to be 'purely physiological' as early as *The Interpretation of Dreams*), in what sense did it 'scarcely satisfy' Freud? What Freud meant was that he could see no particular reason for the excitation, the unpleasure, and the pattern of discharge to occur together. He was therefore:

> tempted to assume the presence of a historical factor which binds the sensations of anxiety and its innervations firmly together. We assume, in other words, that an anxiety-state is the reproduction of some experience which contained the necessary conditions for such an increase of excitation and a discharge along particular paths, and that from this circumstance the unpleasure of anxiety receives its specific character. In man, birth provides a prototypic experience of this kind, and we are therefore inclined to regard anxiety-states as a reproduction of the trauma of birth. (ibid. Cf. pp.93-94)

Although this was not his original thought about birth at all, it was still the case that he could assert the innervations belonged to an action that had once been expedient:

> at birth it is probable that the innervation, in being directed to the respiratory organs, is preparing the way for the activity of the lungs, and, in accelerating the heart-beat, is helping to keep the blood free from toxic substances. (op. cit., p.134)

Toxins continued to constitute the specific traumatic situation at birth. But, according to Freud's new argument, birth was important primarily because the purposive reaction to the excessive stimulation generated in it brought the three distinguishing characteristics of anxiety together. Later traumatic increases in stimulation resulted in similar feelings and the same perceptions of discharge even when toxins were not present. Irrespective of the physiology of the other traumatic situations, birth provided a prototypal purposive affective response for them. A complete recasting of his distinction between primal repression and repression proper now allowed Freud to link the various traumatic situations to one another and to relate them to signal anxiety.

Primal repression and repression proper

Pretty clearly what signal anxiety caused was repression proper or after-pressure. What was signalled was the danger of an earlier traumatic situation recurring and it was really that threat that caused the repression.

The pathogenic nucleus It had always been a central point with Freud that later repressions had to be related to an earlier or primary and more pathogenic impression. In positing that Anna O.'s initial snake hallucination was "the root of her whole illness" Breuer had pointed the way (Breuer and Freud, 1895, p.40). Independently of Breuer, the French school of psychopathologists had proposed an elementary *condition seconde* as the core of all hysteria. The first experiences occurring in it formed a seed around which later symptoms precipitated. Similarly, it had been crucial to Freud's view of his patients' pathology in the *Studies on Hysteria*. At the centre of the pathogenic memory structure was a "primary impression" in which the traumatic factor "found its purest manifestation" (op. cit., pp.75, n.1, 288). At that time he believed that the impression had to have been "intentionally repressed" and he persevered until he found it (op. cit., p.116). A focus around which later pathogenic experiences crystallised was a *sine qua non*.

Freud never really dropped this notion of a central traumatic event as a prior focus from which later events derived their traumatic force. We find it again in the topographic theory as the repressed unconscious memory that attracted later expelled mental contents to itself. It is also recognisable during psychosexual development as the passive lagging behind or fixation of a libidinal component. Its final manifestation is as the concept of primal repression first introduced in the two stage theory of repression set out in *Repression* in 1915. Repression proper supplied an "after-pressure" linking later pathogenic experiences to those succumbing to the earlier primal repression. At that time Freud thought of primal repression as a psychosexual fixation (Freud, 1915b, p.148. Cf. Freud, 1905b, p.175 and n.2 of 1915; 1911a, pp.61-62, 67-68).

Trauma and primal repression Where in the structural theory were the parallels with traumatic and danger situations? After reiterating that a

repression dealt with in therapy was an instance of after-pressure which presupposed a primal repression, Freud offered the brief speculation that it was "highly probable":

> the immediate precipitating causes of primal repressions are quantitative factors such as an excessive degree of excitation and the breaking through of the protective shield against stimuli. (Freud, 1926a, p.94)

Primal repression was the response to the automatic anxiety caused by the excessive stimulation arising from a trauma; after-pressure, or repression proper, was the response to the signal of its possible recurrence.

While it might have been possible for the automatic anxiety of birth to cause *that* primal repression, none of the other traumatic situations had the slightest resemblance to birth in content. Nor was it the case that later signals necessarily conveyed messages that that particular danger or one like it was about to recur.

The structural theory also demanded a very specific place for castration anxiety. Otherwise there could be no super-ego and consequently no self-punishing tendency or moral anxiety. It would be an advantage in constructing a monolithic theory if birth were the first anxiety experience, if one of the early primal repressions were a response to castration threats and if the super-ego, which then formed, was the sole agent of repression proper. But that conceptualisation was clearly not viable. If his earlier case histories were to be believed, Freud had evidence of repression proper well before the super-ego had formed. After-pressure presupposed:

> the operation of earlier, *primal repressions* which exert an attraction on the more recent situation. Far too little is known as yet about the background and preliminary stages of repression. There is a danger of over-estimating the part played in repression by the super-ego. We cannot at present say whether it is perhaps the emergence of the super-ego which provides the line of demarcation between primal repression and after-pressure. At any rate, the earliest outbreaks of anxiety, which are of a very intense kind, occur before the super-ego has become differentiated. (Freud, 1926a, p.94)

Further, by itself the response to the threat of castration failed to explain how the three components of anxiety were joined.

In addition to birth, Freud eventually distinguished *four* traumatic situations: separation of the infant from its mother, threatened castration, losing the love of the chosen object, and threats from the super-ego (Freud, 1926a, pp.136-140, 1933b, 87-88). In each situation the immature ego was especially helpless and readily overwhelmed by stimulation. Chronologically the traumas matched the infant's development. Loss of the mother was most potent during the total dependence of infancy, castration during the phallic stage, while loss of the object and super-ego threats had their effects during latency. Being of such varied content and covering such a wide age range the four traumatic situations generated enough foci to attract any

ideas forced out of consciousness by repression proper. Female development could be accommodated by assuming castration anxiety was replaced by anxiety over the loss of the object's love, a kind of prolongation of the infant's anxiety over the loss of the object itself (Freud, 1926a, p.143. Cf. Freud, 1933b, pp.87, 88).

Of the traumatic situations, birth was undoubtedly the most important. The response to its toxins was the prototype of the responses to the other excessive amounts of excitation. Without birth anxiety there could be no anxiety of any other kind. In the event of being discarded as a prototype, Freud's assumption that there always had to be an earlier core experience would require an equally central trauma as a substitute.

EVALUATION OF THE STRUCTURAL THEORY

The structural theory is sometimes represented as resulting from Freud's resolving some of the minor problems of the topographic theory. My account shows there were more than just a few unimportant loose ends to be tied. The structural theory has considerable deficiencies, many of which it shares with the topographic theory (Gill, 1963; Arlow, 1975; Sandler, Dare, and Holder, 1972, 1982; Sandler and Sandler, 1983) and others peculiar to itself (Van der Waals, 1952; Fayek, 1980). My evaluation is organised around five issues: anxiety, the prototype of the anxiety experience, anxiety as a cause of repression, the ego's acquisition of objects and neutral energy, the mechanism of identification, identification and the Oedipus complex, and the formation of the female super-ego.

ANXIETY

Freud insisted the anxiety of signal anxiety was not newly *created* but *reproduced*. Because the difference seems to require two mutually exclusive sources of anxiety, it has provided the basis for a good deal of psychoanalytic criticism and evaluation. Would it not be better to find a single source for the automatic anxiety of the traumatic situations and the signal anxiety of the danger situations? Before considering whether a more parsimonious explanation is possible I shall consider the rather more important issue of whether any satisfactory explanation of the means by which the signal is generated can be given at all.

The mechanism of signal anxiety

As Freud put it, the anxiety signal was reproduced "in accordance with an already existing mnemic image" (Freud, 1926a, p.93). The problem with this apparently simple proposition is that it is not possible to derive the reproduction in a way consistent within itself or consistent with the rest of psycho-analytic theory. I take up the three main aspects of the problem

in turn: first, anxiety and the nature of affects; second, the reproduction of anxiety; and third, the attenuated form in which Freud supposed the signal to appear.

Anxiety and affects Freud saw two absolute differences between affects and ideas. Ideas existed at an unconscious level as actual structures, that is, as memory traces of things or events - as thing-representations - and could become conscious only by being linked to words. An affect, on the other hand, was not a structure at all but a process of discharge having only a "potential beginning" (Freud, 1915c, p.178). Affects were "transmitted directly" (1923b, p.23) to consciousness and became conscious as soon as the discharge commenced, without requiring linkage to word-representations. Even:

> when they are attached to word-presentations, their becoming conscious is not due to that circumstance, but they become so directly. (Freud, 1923b, p.23. Cf. Freud, 1915c, pp.177-178; 1916-1917, pp.409-410)

Consequently:

> there are no unconscious affects as there are unconscious ideas .... The whole difference arises from the fact that ideas are cathexes - basically of memory-traces - whilst affects and emotions correspond to processes of discharge, the final manifestations of which are perceived as feelings. (Freud, 1915c, p.178)

Until the end Freud maintained that affects were processes of discharge and that ideas were structures (Freud, 1933b, p.81). Nor did Freud deviate from his view that ideas became conscious via their verbal linkages. Neither did he give way on the directness with which affects became conscious. Now all of this had to be so, of course. In the structural theory the difference between repressed and non-repressed ideas was the difference between thing- and word- representations. And, for instinctual drives to communicate directly with ego and super-ego, there could be no impediment, certainly not a word-representation barrier, to even their slightest stirrings being sensed.

Nevertheless, several psycho-analytic writers have argued that affects can be unconscious (e.g. Pulver, 1971; Brenner, 1974). Their arguments are based on gross misunderstandings. For example, Pulver cites descriptions by Eissler (1953), G. S. Klein (1967), and Joffe and Sandler (1968) of what they say are unconscious affects but it is clear, as those authors themselves indicate, that the affect is so only preconsciously. Freud did allow, of course, that the individual need not be conscious of the affective signal initiating repression. Because the absence of an attentional cathexis accounts sufficiently for this 'unconsciousness', I do not believe, as G. S. Klein (1967) seems to, that Freud was here being inconsistent. Pulver also bases his argument explicitly on a confusion between the descriptive and dynamic. He says, "if an affect can be preconscious there is no theoretical

reason why it should not be able to be unconscious'' (Pulver, 1971). But it is just the difference between the attentional cathexis, which raises preconscious ideas to consciousness, and the anticathexis, which denies consciousness to repressed ideas, that provides a very good theoretical counter to his proposition. Neither is Pulver's clinical evidence any more cogent - it is quite clear that what ordinarily passes for repression in psycho-analysis is not involved in any of his examples.

Schafer (1964) seems almost to fall victim to another terminological confusion in that he appears to allow that discharges could be barred from consciousness by being repressed rather than simply because they are processes. Siegal (1969) puts the matter correctly, however, by saying that, as discharge processes, ''it does not make sense'' to speak of repression denying them access to consciousness. Brenner (1974) developed a rather more subtle argument by extending the definition of affect to include the idea associated with it. Since the ideational component can be repressed, *ergo*, so can the affective. Brenner's understanding of affect is, of course, quite at variance with Freud's and overlooks the point I have previously stressed, one with which Green (1977) seems to agree, that in many instances Freud has it that repression *is* the stripping of the affect from its accompanying idea. Green (1977, and in Jaffe and Naiman, 1978) also makes essentially the same criticism of Brenner's redefinition as I have and Limentani (in Jaffe and Naiman, 1978) supports him. Green grants that affects are 'represented' in some way but, clearly consistent with Freud's usage, he restricts the term 'repressed' to ideas and uses 'suppressed' and 'inhibited' to apply to affects. Like most of those who have written on the subject, Brenner also fails to treat the topic theoretically, and like everyone else, he does not consider affects in relation to the thing- and word-representation distinction.

Reproducing anxiety What then of the reproduction of anxiety? Even though it cannot be reproduced through the re-cathection of the trace of an earlier affect (there being no structural record to be revived) Freud himself sometimes seemed to suggest the traumatic situation did leave a trace of the affect behind and the signal was produced by its re-cathection. Thus, although he was careful to speak of the reproduction of the affect *in accordance with* an existing memory image rather than *from* it, Freud did assert:

> Affective *states* have become incorporated in the mind as precipitates of primaeval traumatic experiences, and when a similar situation occurs *they are revived* like mnemic symbols. (Freud, 1926a, p.93. My emphasis, MBM)

A *state* is hardly the same thing as a *process*. Two of Freud's later comments also seem to imply the revival of some kind of affective trace. After describing the ego noticing the recurrence of a situation similar to an earlier trauma, he went on to say it signalled danger:

> by an abbreviated repetition of *the impressions* one has experienced in connection with the trauma - by *an affect* of anxiety. (Freud, 1926b, p.202. Emphasis altered, MBM)

Again, the ego brought about:

> the reproduction of *the unpleasurable feelings* at the beginning of the feared situation of danger. (Freud, 1933b, p.90. My emphasis, MBM)

For a trace to be so revived and act like a mnemic symbol it would have to have a rather more permanent structure than a *potential beginning*.

Consider now the possibility that the signal is somehow generated by the recathection of a memory-trace of the event rather than of the affect. First, we note an essential vagueness about what Freud thought was recorded during the trauma. Second, we find him to be completely silent about how that record is related to the affect allegedly revived. In *The Ego and the Id* he referred only to "dangers" that threatened the ego and to "the menacing perception" or "the similarly regarded process in the id" from which the ego's cathexis was withdrawn (Freud, 1923b, p.57). In *Inhibitions, Symptoms, and Anxiety* he was not much clearer. When discussing castration anxiety Freud said:

> as soon as the ego recognizes the danger of castration it gives the signal of anxiety and inhibits ... the impending cathectic process in the id. (Freud, 1926a, p.125)

An equivalent vagueness is found in his discussion of separation anxiety:

> It is the absence of the mother that is now the danger; and as soon as that danger arises the infant gives the signal of anxiety. (op. cit., p.138)

Nor are his concluding remarks much more definite:

> the ego subjects itself to anxiety as a sort of inoculation, submitting to a slight attack of the illness in order to escape its full strength. It vividly imagines the danger situation, as it were, with the unmistakable purpose of restricting that distressing experience to a mere indication, a signal. (op. cit., p.162)

Within Freud's theory, one certainly imagined an event by reviving the memory of it through re-cathecting its memory trace. But Freud was vague about what the trace might be of and silent about the mechanism by which its recathection reproduced the associated affect.

In Freud's theory, the only alternative to producing the signal by recathecting an affective trace was to generate it through a partial discharge. That this was probably what Freud really had in mind (even though it also poses grave theoretical difficulties) is suggested by the parallel he frequently drew between affects and hysterical attacks (Freud, 1926a, pp.93-94, 133-134) and his treatment of pain in the *Project* (Freud, 1950/1954).

According to the hypnoid state conceptualisation of the mechanism of the hysterical attack, the intrusion of the *condition seconde* into the primary consciousness could occur spontaneously, as in the stage that Charcot had described as *attitudes passionelles*, or it might be provoked:

> just as any memory can be aroused in accordance with the laws of association ... by stimulation of a hysterogenic zone or by a new experience which sets it going owing to a similarity with the pathogenic experience. (Breuer and Freud, 1893, p.16)

Hence Anna O.'s paralysis when the bent branch for which she was reaching reminded her unconsciously of the hallucinatory snake her temporary paralysis had prevented from warding off (Breuer and Freud, 1895, pp.38-39). Similarly, Freud supposed Emmy von N.'s attacks of arm and leg pains to be based on well-remembered rheumatic pains:

> originally associated only accidentally with those experiences, [but] were later repeated in her memory as *the somatic symbol* of the whole complex of associations. (op. cit., p.71, n.1. Cf. pp.90-91. My emphasis, MBM)

Whatever the origins of the motor symptoms, Freud thought:

> they all have one thing in common. They can be shown to have an original or long standing connection with traumas, and *stand as symbols for them* in the activities of the memory. (op. cit., p.95. My emphasis, MBM)

Symbolism was sometimes achieved through words and ideas. Thus Frau. Cäcilie's *penetrating* head pain was said to be based on a look of suspicion her grandmother gave her that was "so *piercing* that it had gone right into her brain" (op. cit., p.180. My emphasis, MBM). For physical and mental symbols the "long standing connection" was provided by the facilitation created when the affect or sum of excitation of the original traumatic idea first discharged along an abnormal pathway. During any later attack, the affect of the provoking experience re-created the symptom by being directed along the same somatic or 'mental' pathway.

Whether the symptom seen in the hysterical attack is *produced* or *reproduced* is a nice point. Clearly the motor or other innervation underlying the symptom can be described as being reproduced in accordance with the trace of the original traumatic idea. Just as clearly, each attack requires a fresh discharge of excitation - the symptom has no existence outside of the attack. The symptom does not recur simply because a memory trace of the idea has been re-cathected (indeed it cannot) but because its re-cathection leads to another discharge along the original pathway and a subsequent re-innervation. So, it seems to me, with affects. The "potential beginning" of the process of discharge lies not in a trace but in the facilitatory pathway between the affective memory and the organs into which the discharge occurs. And, just as with the symptom, it is a nice point which description is more correct: is the signal of anxiety reproduced in accordance with a memory or is it produced by a fresh discharge?

Attenuating the signal Emphasising the fresh discharge of excitation in this way immediately brings to the fore the third major problem: Freud's description of signal anxiety as an *attenuated* form of the original. Hysterical symptoms are not attenuated. An attack of paralysis is just as complete or an hallucination just as frightening on the subsequent occasions as on the first. Some difference must therefore exist between an hypothesised discharge mechanism allowing a symptom to recur with its original intensity and one which attenuates the level of anxiety to that of a signal. Freud does not say what this difference is but, as Compton (1972a) and Green (1977) have also noted, his discussion of pain in the *Project* suggests what it might be (Cf. Glick, 1966).

Freud thought that pain, like anxiety, was produced when excessively large quantities of excitation broke through the protective shield. The increase was felt as an unpleasure having the qualities peculiar to pain, an "inclination" to discharge, and a facilitation between that inclination and the memory image of the object causing the pain. Any subsequent perception cathecting the image caused a state to arise:

> which is not pain but which nevertheless has a resemblance to it. It includes unpleasure and the inclination to discharge which corresponds to the experience of pain. (Freud, 1950/1954, *Project*, Part I, Section 12 of September, 1895)

It was impossible for this unpleasure to come from a simple re-cathection of the memory trace. To begin with, memory-traces were without quality. Further, a recathection:

> is in the nature of any other perception and cannot have as a result a general raising of [quantities of excitation]. (ibid.)

Freud therefore assumed that, in addition to the motor neurones involved in the discharge of painful excitation, there were also "key neurones" connected to the group of well-facilitated neurones constituting the ego. Stimulation of these hypothetical key neurones caused:

> the production of endogenous [quantities of excitation], and accordingly do not *discharge* [quantity] [by way of the musculature] but *supply* it in roundabout ways .... As a result of the experience of pain the mnemic image of the hostile object has acquired an excellent facilitation to these key neurones, in virtue of which [facilitation] unpleasure is now released in the affect. (ibid. My emphasis, MBM)

Allowing for the moment that "roundabout ways" actually refers to something fairly definite - a point reasonably disputed by Amacher (1965, pp.71-72) - the amount of *this* unpleasure could be readily attenuated. By disposing what he called a side-cathexis on the facilitatory pathway, Freud pictured the ego as diverting the potential discharge to itself and binding it there. It was then, said Freud, easy:

> to imagine how, with the help of a mechanism which draws the ego's *attention* to the imminent fresh cathexis of the hostile mnemic image, the ego can succeed in inhibiting the passage [of quantity] from a mnemic image to a release of unpleasure by a copious side-cathexis, which can be strengthened according to need. (Freud, 1950/1954, *Project*, Part I, Section 14)

In this mechanism there is no doubt, as Compton (1972a) and Green (1977) would agree, that if the unpleasure occurs at all it has to be generated anew by a minor excitation of the very key neurones involved in the original experience and not simply by the reproduction of a memory.

At the time of the *Project* there was neither physiological nor clinical evidence for the existence of Freud's mechanism (Amacher, 1965, p.72). Nor has any accumulated in the years that followed. After observing, not quite correctly, that Freud never referred to these supposed neurones again (Cf. Freud, 1900, pp.468, 582 and Freud, 1915c, p.179, n.1), Levin notes that Freud never offered an "alternative explanation of how a recathected memory might release unpleasure" (Levin, 1978, pp.176). Were it not for the generally low level of psycho-analytic writing, I would otherwise find it odd that in discussions of this most central point of the new theory, Freud's non-explanation has not been picked up. But, because it is glossed over, we can be sure the problem has been identified. For example, Glover (1926) equated the revived memory with the affect itself and H. Hartmann (1956) misrepresented the process by incorporating the signal into the *anticipation* rather than leaving it where Freud had so clearly placed it: among the *consequences* of the sensed danger.

What is at issue here is nothing less than the psycho-analytic theory of affects. In relation to the concept of signal anxiety, Applegarth (1977a) remarked that "affect theory represents one of the most total failures" [of psycho-analytic theory]. Garza-Guerrero (1981a) went further and argued more generally that an adequate psycho-analytic theory of affects "is long overdue" noting as he does so the paradox that psycho-analytic work "has at its very heart the task of dealing with feeling states and emotions". The central issue is identified by Green in his comments on the notion of unconscious affect:

> the whole question is to know how the 'idea' which forms an integral part of the nature of affect differs from what is conventionally called an idea, the content. (Green, 1977)

Underlying this question is the broader one, which I discussed in Chapter 11, of the essential lack of characterisation of the notions of instinct and instinctual representative. Consequently, even at the definitional level, almost anything can be said about affect. For example, although the three elements by which Kernberg (in Lester, 1982) defines an affect seem to be the same as Freud's, what he calls an affect is a *structure* rather than a process.

Two other things should be especially noted about Freud's analysis. First, there is the emphasis placed on a cognitive appreciation of the possible recurrence of the painful situation. It is an appreciation identical to that supposed to precede the generation of the signal of anxiety. Second, precisely because this mechanism of Freud's inhibits discharge, it cannot be a device for generating a signal. What Freud describes in the *Project* is how an ego, *after already recognising the imminence of another painful situation*, might attempt to control the amount of unpleasure to be released. But, had he tried to conceptualise signal anxiety in the same way he would have ended in an impasse. For the ego to produce a small amount of anxiety it would have to inhibit partially the facilitatory pathway between the about-to-be-revived trace of the traumatic situation and the key neurones. To take that action its appreciation of the potential danger would have to have been concluded *before* the hypothesised key neurones could produce their small quantity of anxiety. Rangell puts the point forcefully in his colourful comparison of the ego to a water-tap. What the ego/tap does:

> controls the amount, but [the ego] does not and cannot know whether the water will come out hot or cold. If the water comes out hot, it experiences it, and acts further accordingly. (Rangell, 1968)

The ego's appreciation cannot be other than cognitive and that evaluation renders a subsequent signal quite unnecessary.

We now see clearly why the three choices of mechanism open to Freud were equally unsatisfactory: first, re-cathection of the trace of a prior discharge could be brought about only at the expense of his theories of affect and repression; second, a re-cathected image of the event would not in itself reproduce the affect originally accompanying it; and third, the blocking of any facilitation between a recathected image and potential discharge would do away with the need for a signal at all. Descriptively there is also more than a hint of inconsistency. How can imagining a situation "vividly" reproduce "a mere indication" of the original anxiety? (Freud, 1926a, p.162). Attractively simple as it may seem, the mechanism of signal anxiety has to be rejected. If it is not uncharacterised, it is, at the very least, so inadequately characterised that it can explain nothing.

Psycho-analytic resolutions From this vantage point we may now consider the issues raised by those psycho-analysts who have concerned themselves with Freud's theory of anxiety. Although almost all of them accept that anxiety occurs in traumatic situations and that it somehow acts as a signal of danger, very few accept Freud's toxicological theses, most are dissatisfied with the two sources he proposed, and practically none accept his view of the ego's signalling role.

One line of attack has centred on traumatic anxiety. Intimated earlier by others (e.g. Fenichel, 1937/1945a; Oerlemans, 1949), the main salvo was fired by Brenner (1953), with Schur (1953) providing close support, and Flescher (1956) attempting the mopping up. Claiming the support of

clinical observations, critics taking this line variously deny that anxiety is produced from libido, deny the existence of the entity Freud described as anxiety neurosis, and deny that an influx of excitation always precedes automatic anxiety or the traumatic neuroses. Each of these points has subsequently received wide endorsement (Compton, 1972a, 1972b). However, as there undoubtedly are traumatic situations as well as traumatic neuroses, some other characterisation of them had to be made and, where anxiety does occur, another explanation found for it. Some form of instinctual energy has usually been proposed for anxiety. Aggression, either by itself or in a sadistic mix with libido and sublimated to some degree, is the candidate of Zetzel (1955a), Flescher (1956), H. Hartmann (1955), and possibly Brunswick (1954), while an opposite, masochistic trend is favoured by Rado (1933) and Ernest Jones (1929) derived it from the fear instinct. Melanie Klein (1932, pp.182-185, 1948, 1952) favours a fear of the death instinct but Rangell (1955) postulates a special defensive instinctual energy as does Schur (1958, 1960). In complete opposition to these instinctual energy formulations, Brenner (1953, 1974) and Stewart (1967) have proposed that experience gradually transmutes a purely psychological unpleasure, a conception requiring no energy at all. The evaluation of each of these alternatives goes beyond what I can reasonably undertake here. Most are so thoroughly embedded in larger revisionist frameworks they cannot readily be wrenched from them for separate scrutiny. But it can be said that one way and another they fail to maintain consistency with other parts of Freud's theory (and sometimes with their own).

No one seems to believe that the ego *produces* anxiety as a signal, not even those who believe anxiety has a signalling role or that some kind of smaller version of the trauma might be involved (Kubie, 1941b; Fenichel, 1937/1945a, 1941a; Oerlemans, 1949; Spitz, 1950; Brenner 1953, 1974; Schur, 1953, 1958; Rangell, 1955, 1968; Pumpian-Mindlin, 1958-1959; Benjamin, 1961; Stewart, 1967, pp.173-174; Waelder, 1967b; J. H. Smith, 1977). Were it to do so, for the child at least, the signal may produce the same reaction of the trauma (Sandler, 1967). Laplanche (1981) reaches a similar conclusion. He disputes the possibility that fear, considered as an adaptive reaction to danger, can be experienced before anxiety and, therefore, that anxiety develops from fear. He concludes that anxiety cannot be a signal of internal danger but is that danger itself.

Practically nothing remains of Freud's notion: the ego merely *experiences* anxiety or *uses* it in response to danger. Waelder aptly summarises this whole trend:

> The ego *anticipates* future events and samples the unpleasure of a future catastrophe in small doses - the anxiety signal- which then, through the pleasure-pain principle, sets the avoidance reaction into motion. (Waelder, 1967b)

This is, of course, exactly as Freud had it for pain in the *Project*. Waelder goes on to note that:

> the implicit ability of the ego to anticipate still contains the whole secret: how does the ego do the anticipating? (op. cit.)

Consequently, "the idea of an 'anxiety signal' is in need of revision". It is hard to imagine a more thorough demolition of Freud's concept of signal anxiety than this one carried out by his loyal disciples and followers.

Even were it possible for the ego to produce its signal there are the fatal inconsistencies that J. H. Smith (1977) and Bush (1978) have pointed out. If the primary processes eschew anything unpleasant, what accounts for the tendency of the id to either attract or propel into consciousness those unpleasant memories of danger situations that caused the ego to generate signal anxiety? And, if the ego's production of unpleasure in generating the signal is governed by the pleasure principle, how does that principle come into operation only after the unpleasure of the signal?

The prototype of the anxiety experience

The question now to be asked is where Freud's theory of anxiety stands if birth anxiety is not the prototype of the later kinds. Clearly it is very seriously impaired. In the structural theory birth anxiety was much more than the starting point for symptom formation. Normal development also began there. Rejecting the prototypal role of birth anxiety therefore undermines the very foundations of Freud's personality theory.

Psycho-analytic data on birth anxiety Neither clinical nor observational data allows birth anxiety to be accorded the status Freud proposed. Greenacre (1941, 1945, 1967) concluded from her own psycho-analyses of adults and from direct observations of children that birth *per se* was not even an organic trauma. None of the studies of neonates confirm that early infantile anxieties are in any way based upon birth. Direct observations, many of them conducted by psycho-analysts, as well as analytical interpretations of the observational studies of non-analysts, show that infants experience physical distress or unpleasure rather than anxiety (Spitz 1950; Ramzy and Wallerstein, 1958; Benjamin, 1961, 1963; S. Brody and Axelrad, 1966; Izzard, in Lester, 1982). Even before most of that work had been reported, Brenner (1953) had concluded that infantile 'anxiety experiences' were best described as unpleasurable. Furst later actually questioned whether acute psychophysiological stress situations in early infancy were "traumas in the psychoanalytic [i.e. Freud's] sense of the term." He went on to say:

> A breach in an existing stimulus barrier, followed by a feeling of helplessness on the part of the ego, is a central feature of the psychoanalytic concept, while the early months of life are synonymous with *pre-ego* and *pre-stimulus* barrier. (Furst, 1967. My emphasis, MBM.)

Because birth occurs "outside object relations", Neubauer (1967) judges the birth experience to have "limitations" as a model for understanding

trauma. Or, as Compton (1972b) explains, there can be no content to the anxiety experience prior to the development of object cathexes.

Most psycho-analysts have concurred with the Neubauer-Compton position. The truly infantile ego is unable to experience anxiety of any kind. Although Benjamin (1961) was actually prepared to allow some usefulness to untestable propositions (!), he really delivered the *coup de grace* to Freud's by observing that the hypothesis of a prototypal birth experience was a universal proposition of just that kind. Many years ago, Greenacre (1941) saw the difference between herself and Freud over birth as revolving around phylogenesis. For her the importance of birth was as an individual experience; for Freud the prototype of birth assimilated "the endless procession of the births of our forefathers". So, although Greenacre interpreted her observations as showing that birth organises the anxiety pattern by bringing together the genetic and individually determined elements, it is little wonder she makes no use of the concept of signal anxiety in relation to it (Greenacre, 1941, 1945).

Even were birth as important as Freud supposed, he offered, as we have seen, only a very brief and general description of the primal repression it generated and his discussion focussed on demarcating primal repression from after-pressure. It would have been most convenient had Freud been able to argue that after-pressure only appeared subsequent to the formation of the super-ego. I have no doubt that in *Beyond the Pleasure Principle* and *The Ego and the Id* Freud was aiming at an all-embracing explanation of the neuroses, the origins of civilization, and the very beginnings of life. The elements of his explanation were the omnipotent trend to return to the inanimate state, the tendency of instincts to repeat previous experiences, a uniformly present prototype of anxiety, and a super-ego acquired after a prehistoric parricide. The individual's super-ego, created in response to a threat of castration, not only explained normal and neurotic development but how civilization itself was maintained. But no development could take place without the universal trauma of birth to explain why anxiety was the reaction to the castration threat. Only birth anxiety provided the basis for the grandiose recapitulation Freud had envisaged.

Primal repression Without necessarily endorsing the place Freud gave birth in his vision, primal repression is nevertheless granted extremely high status. Nunberg (1932/1955, p.234), for example, has *all* later repression being "but a repetition of the primal repression". In various ways Glover (1939, pp.70-80, 153), Rapaport (1951, pp.694-698), Lantos (1955), Eissler (1962), Whitman (1963), Anna Freud (1967), Loeb and Carroll (1966), and Grotstein (1977a, 1977b) have all attested to its importance. But, partly because Freud used the term in different ways at different times, the usual psycho-analytic confusions abound. There are positively silly comments. For example, primal repression should not be taken to refer to time because "Freud was considering the mind of the adult when he wrote on repression" [!] (Pearson, 1953, cited in Frank and Muslin, 1967).

Again, after reviewing and revising Freud's theory of repression, Cohen and Kinston claim they have given primal repression a precise place within repression theory. The "precise place" (Kinston and Cohen, 1986) at which primal repression operates is "at any stage of life" and its basis is "an absence of structure" which is, metaphorically, "a hole in the mind" (Cohen and Kinston, 1983). How is at 'any' time to be equated with a 'precise' place and how is an absence of 'structure' also a process? They arrive at these spectacular self-contradictions by glossing Freud's almost self-evident point that no repression can take place until a sharp cleavage has developed between conscious and unconscious mental activity as meaning that that separation takes place *throughout* the life cycle. It would, on their gloss, possibly be excluded in conditions like the psychoses in which, they say, the separation has disappeared. Were it not for the grandiosity of their claim, what they have to say is, I think, best thought of as another example of the merely silly.

A number of substantial questions has nevertheless to be answered. As Madison put it:

> the mechanism of 'traumas' and 'primal repression' is a puzzle. Seemingly Freud was saying that primal repression was a direct, automatic response to traumas ... apparently if a state of too intense stimulation occurs ... the infant's *automatic* response is to repress the impulses responsible .... How does this happen? What is the mechanism ... ? Freud left the point undiscussed. (Madison, 1961, p.114)

In a partial answer, Parkin (1983) differentiated "developmental" from "defensive" primal repression. The former is *ad hoc*, is of relatively short duration, and occurs in response to trauma. The defensive variety opposes the entry into consciousness of wishful primary process impulses. But Parkin's case rests on a very arbitrary reading of Freud, which he places in a footnote, and has few consequences.

Frank and Muslin (1967) have made the only substantial attempt at finding answers. They opened their paper with an acknowledgment that primal repression occupied "an uncertain niche in psychoanalytic psychology" before going on to pose other equally significant questions:

> How are we to understand primal repression as distinguished from repression proper? If we assume that primal denotes early repression, how early? When, how, and why does it begin? When, how, and why is it superseded by ... repression proper? And in what ways do the earlier ... and later ... forms ... differ? (Frank and Muslin, 1967)

Frank and Muslin went on to distinguish two kinds of primal repression. One was an early and passive form that caused fixations and had pathological consequences when it occurred in response to overwhelming stimulation. Passive primal repression was found, they thought, with the primary processes that "are in the foreground" up to eighteen months (Frank, 1969). The other form, the active process Freud described in his later

works, was only made possible by the development of the ego and the secondary process. In this ahistorical solution, Frank and Muslin simply juxtaposed two of Freud's meanings without attempting to answer the question of how early primary repressions came about. They left their revision of Freud's developmental theory without a starting point.

Three other important consequences follow the removal of birth from its position as the first of the traumas. First, without birth, Freud had no explanation for the three components of anxiety being found together - and we have seen the emphasis he placed on that point. Second, none of the other traumas would *in themselves* generate anxiety. Their power came from their resemblance to birth in their toxicity or in their psychological content. Finally, none of them could provide the uniform cause for which Freud was searching - none were truly *universal* experiences. Even though it happened very frequently, not everybody underwent an actual separation or was really threatened with castration.

Castration and phylogenesis Even with birth as the starting point, anxiety over castration had to be universal. Otherwise no super-ego could form, no neurosis could develop, and there could be no perpetuation of civilization. But, if not every single individual were threatened, how could universal castration anxiety be accounted for? Freud proposed that individuals came into the world carrying the trace of a memory of an actual castration carried out on their forebears. Whether or not the individual was really threatened with castration, this phylogenetically inherited memory or archaic inheritance ensured that even the most gently expressed parental disapproval of childhood sexuality would be magnified:

> When we study the reactions to early traumas, we are quite often surprised to find that they are not strictly limited to what the subject himself has really experienced but diverge from it in a way which fits in much better with the model of a phylogenetic event *and, in general, can only be explained by such an influence*. The behaviour of neurotic children towards their parents in the Oedipus and castration complex abounds in such reactions, which seem unjustified in the individual case and *only* become intelligible phylogenetically - by their connection with the experience of earlier generations. (Freud, 1939, p.99. My emphasis, MBM. Cf. Freud, 1940a, pp.190 and n.1, 200-201, 206-207)

The case of the patient known as the Wolf Man was paradigmatic:

> the boy had to fit into a phylogenetic pattern, and he did so, although his personal experiences may not have agreed with it. Although the threats or hints of castration which had come his way had emanated from women, this could not hold up the final result for long. In spite of everything it was his father from whom in the end he came to fear castration. In this respect *heredity triumphed over accidental experience*; in man's prehistory it was unquestionably the father who practised castration as a punishment. (Freud, 1918, p.86. My emphasis, MBM. Cf. pp.97, 119)

Freud was quite explicit that what was passed on was much more than an

innate, constitutional disposition. More was involved than even a tendency to develop or behave in a certain way (Freud, 1918, p.98). What was inherited was a subject-matter: "memory traces of the experiences of earlier generations" (Freud, 1939, p.99). This must be so, of course. As Satinover (1986) observes, if there is any such thing as an 'archaic heritage' it must be "undistorted" and not transmitted in symbolic form.

Freud also made it clear that the acquisition of individual moral standards and the perpetuation of the demands of civilization depended upon the inheritance of this memory of a prehistoric castration. Castration anxiety, which brought the Oedipus complex to a close, coincided with:

> the most efficient way of mastering the archaic, animal heritage of humanity .... that heritage comprises all the forces that are required for the subsequent cultural development of the individual, but they must first be sorted out and worked over. (Freud, 1919b, p.262. Cf. Freud, 1923b, pp.37-38; 1925c, pp.220-221; 1940a, pp.205-207)

Freud was aware there was no evidence for the notion of the inheritance of acquired characteristics, especially for the inheritance of specific memories. He had, he said:

> no stronger evidence for the presence of memory-traces in the archaic heritage than the residual phenomena of the work of analysis which call for a phylogenetic derivation, yet this evidence seems ... strong enough to postulate that such is the fact. (Freud, 1939, p.100)

His "confession" (ibid.) that he could not do without a postulate for which there was no "evidence" other than the gaps in his speculative reconstructions is at once a measure of the weakness of the developmental schema and of the strength of the grip the uniformity assumption had on him. Both derive directly from the faulty causal assumptions Freud had made in his very earliest work on the actual neuroses, and from the concept of the pathogenic memory structure and the principles of interconnection he had devised in the *Studies on Hysteria*.

Anxiety as the cause of repression

When Freud reversed his formulation of the relation between anxiety and repression and announced that repression was caused by anxiety he adduced his interpretations of the cases of 'Little Hans' and the 'Wolf Man' as evidence. The one thing that stands in the way of accepting these interpretations is that these very cases had been previously construed as showing that anxiety was caused by repression. What was the real significance of the cases? Were Freud's re-interpretations justified?

The cases and the re-interpretations Little Hans was a five year old boy with a fear of being bitten by a horse (Freud, 1909a). He was the first child patient to be treated according to psycho-analytic principles, although most of his treatment was actually conducted by his father acting under Freud's

direction. The phobia responded most markedly to a direct interpretation given by Freud, on the one occasion he saw the boy, that the child was actually afraid of his father (op. cit., pp.41-43). The Wolf Man was about 25 years old when Freud began treating him for an obsessional condition (Freud, 1918). During his psycho-analysis it was found that some twenty years earlier he had feared that a wolf would devour him. Again, Freud's interpretation was that the patient had really been afraid of his father (op. cit., p.32). It is worth noting that Freud originally proposed the identification of the father with the wolf partly because of a *possibility* that the father *may* have playfully threatened to "gobble up" the boy (Freud, 1918, p.32). In the second discussion, Freud represented this possibility as an *at least highly probable* component source of the phobia (Freud, 1926a, p.104).

In the re-interpretations Freud argued that a hostile impulse directed at the father had been repressed and that that repressed impulse was the basis of the phobias (Freud, 1926a, p.106). The motive for the repression was fear or anxiety over castration. In both cases the son feared the father would respond to the hostile impulse with an actual castration. The anxiety or unpleasure so created caused repression. The affect of anxiety came not from repressed or transformed libido but from the agency initiating repression - the ego itself. As Freud put it:

> The anxiety belonging to the animal phobias was an untransformed fear of castration. It was therefore a realistic fear, a fear of a danger which was actually impending or was judged to be a real one. It was anxiety which produced repression and not, as I formerly believed, repression which produced anxiety. (Freud, 1926a, pp.108-109)

But what was the sequence of events? Had castration anxiety really preceded and caused repression?

Little Hans In the case of Little Hans, anxiety had occurred a few days before the phobia. At that time, however, Hans' anxiety was about being separated from his mother (Freud, 1909a, pp.23-24). In fact, Freud had initially adduced this very aspect of the sequence as evidence for repression having caused anxiety. He had supposed that, when away from his mother, Little Hans' libidinal longing had increased up to some level of intensity after which it was repressed and then transformed into anxiety. True, a castration "threat" had been made some months earlier (op.cit., pp.7-8) but it had had such little effect at that time that Freud was driven to one of his characteristic assertions:

> It would be the most completely typical procedure if the threat of castration were to have a *deferred* effect. (ibid. Cf. pp.35-36)

Freud thought the reason for the phobia being of a horse was because Little Hans had repressed his hostility to his father before the phobia developed (op. cit., pp.138-139). Although it had sought expression, it had been caught up in the repression of the libidinal impulse toward the mother and

provided the content of the phobia - the fear of the father-horse (ibid.).

Now, all that was directly observable was that Hans had been anxious over his mother's absence before the phobia developed and that the interpretation of the fear as really being one of his father had preceded its disappearance. The 'repression' of Little Hans' hostility toward his father was an obvious *post hoc* postulate accounting for, at most, the phobia being of a horse. Freud's re-interpretation commends itself even less. The history shows no anxiety about 'castration' *prior to the onset of the phobia* and what Schur (1953) called Rado's (1950) "mockery" of Freud's interpretation seems to have been based on Rado's explaining the fear without reference to castration at all!

The Wolf Man The Wolf Man's phobia began when he was four years old and followed an anxiety dream in which he had fancied a tree full of wolves outside his bedroom window. Freud assumed the dream had been preceded by the boy hearing a fairy story in which a wolf had had its tail pulled off. Freud then interpreted this assumed story as containing a castration theme that revived a much earlier memory of a time when, at the age of one and a half years, the boy had seen his parents having sexual intercourse and had sensed the mother was somehow being given pleasure by the father. Not only that: their *a tergo* position let him see that his mother lacked a penis (Freud, 1918, pp.29-47). Freud believed the Wolf Man's reaction to this revived memory was interpretable in the context of a negative Oedipus complex. It was as if the scene meant that the boy would have to be deprived of his penis if he were to gratify his feminine libidinal wish by satisfying his father sexually. The result of this possibility of castration:

> was terror, horror of the fulfilment of the wish, [and] the repression of the impulse which had manifested itself by means of the wish. (op.cit., p.36)

The fairy story merely provided the occasion for the repression to begin (op.cit., pp.41-42). The real "motive power" was the fear of castration (op. cit., p.36), although the phobic anxiety itself was transformed, repressed libido. Once again repression had caused anxiety (op. cit., pp.46, 112).

Evidence and reconstruction There is little direct evidence in the history for either of Freud's interpretations. Perhaps this is not surprising. First of all, Freud's investigation of the Wolf Man's infantile past is, as Jacobsen and Steele point out, "clearly anything but direct, since he reaches the infantile material through a very circuitous route". After noting that Freud judged the Wolf Man's story of having been seduced by his sister to be true because his elder cousin also recalled having been seduced by her, they say:

> It is unclear what evidential value there is to the testimony of a relative in the form of a recollection by the patient. (Jacobsen and Steele, 1979)

They also question another of Freud's constructions, one based on the

principle of deferred action, because "it appears impervious to any questioning or attack." Blum (in Isay, 1978) contradicts Freud directly, claiming there was no evidence that these traumas served as the single shocklike events Freud supposed. A more detailed analysis by Jacobsen and Steele (1979) of the "recollections" of the castration threat (upon which Freud relied so heavily) are shown by them to result from Freud *having added* the really important elements to the Wolf Man's limited recollection. Freud (1918, p.92) should hardly have been as impressed as he was by the material fitting together "spontaneously" and filling "the gaps in the patients memory". In the recapitulation he even staked a methodological claim on the reconstruction:

> The gaps were filled ... in a fashion which must be regarded as unexceptional, if any value is to be placed on psycho-analytic work. (op. cit., p.112)

On does not know whether to regard this claim as an exceptional one or merely rather quaint. Should we extend either of these appreciations to Little Hans? According to Schimek (1987), reconstruction plays the same role in his case as in the Wolf Man's: "the crucial early events (castration threat, primal scene) are never remembered but only reconstructed."

A second consideration is that the Wolf Man was, after all, recalling events from some twenty years earlier. What is surprising, however, is that while he recalled other direct castration 'threats' from about the same time they had *not* produced any untoward reaction. By that age, of course, he had already observed his parents and also knew females lacked the penis (op. cit., pp.24-26). Were it then to be argued that these other threats of castration could have no effect because they occurred before the Wolf Man's childish feminine genital impulses had been directed toward his father, the problem then arises as to why those threats had not prevented those impulses from being directed toward his father in the first place. The Wolf Man first experienced anxiety during the dream about the wolves. It was, at best, *simultaneous* with the repression (op. cit., p.28). In both instances we see that the case for repression being the cause of the anxiety required Freud to make the same very judicious use of the principles of deferred action and summation of traumas as he had in the *Studies on Hysteria.*

In any case, both the first and the second explanations are tautological. As Compton has said of Freud's explanation that the Wolf Man's repression was actuated by fear of castration:

> But what is castration fear if not a form of anxiety? And, if so, what has been gained by 'explaining' anxiety as a result of anxiety? (Compton, 1972a)

Compton makes the same objection to the second explanation (Compton, 1972b) and it also applies, of course, to Little Hans. Compton notes further

that in his first interpretation of Little Hans' case:

> Freud does not allow his theory of anxiety [as caused by repressed libido] to interfere with the accumulation of observations that are not entirely convenient. (Compton, 1972a)

Overall, Compton concluded that Freud's theory of anxiety as transformed libido was:

> an illustration of how very far it is possible to go by making assumptions which cover without contradiction what cannot, at the given time, be understood - a successful exercise in ambiguity. (Compton, 1972b)

Nearly twenty years earlier Zetzel had asked some pertinent questions about anxiety. Because they had not been answered, Compton repeated them:

> (1) To what extent is anxiety a response to a danger situation, internal or external? (2) To what extent is anxiety produced by the frustration of an instinct, again whether internal or external? (3) Should anxiety be regarded as a subjective awareness of instinctual tension, or, and to what extent, does anxiety represent a mode of instinctual discharge? (Zetzel, 1955a)

When Compton introduced this quotation from Zetzel he expressed doubt that "psychoanalysis has a working anxiety theory". And, in his concluding remarks, he does not seem to have classed Freud's revised theory as one of the contributions that had decreased "the areas of ambiguity" (Compton, 1972b).

THE EGO'S ENERGY AND ITS OBJECTS

Freud fails to account for five things about the ego and super-ego: why the ego rather than the super-ego acquires neutral energy and becomes masochistic, how sublimation is able to provide energy, where the super-ego's sexual characteristics come from, how identification produces a critical superego, and the contribution of narcissism to the super-ego.

Energy and masochism in the ego

It is not clear that sublimated energy should be lodged in the *ego* or, if it is, how that makes the ego masochistic.

Freud implied that the ego as a whole was altered by Oedipal identifications but the change was really only to that part of the ego incorporating the essence of the parents - the super-ego-to-be. Consequently, sublimated energy should have been stored in it and not in the ego proper. Freud had to see it otherwise. The super-ego was "independent of the ego for its supply of energy" (Freud, 1933b, p.60). Having liberated Thanatos from Eros to make it available to the super-ego, Freud was not about to put the

two instincts together again, and especially not in the super-ego. There they would have to be again fused and so unable to conduct their battle.

As to masochism in the ego, Spiegel (1978) drew attention to the contradiction of the destructive drive appearing as "an intensification of masochism in the ego and at the same time ... an increase of sadism in the superego". Brenner (1982) thought he had solved the problem by allowing both libidinal and aggressive drive derivatives into the super-ego, picturing the remorse and self-punishment deriving from the super-ego as providing a masochistic gratification. On the other hand, Arlow (1982) effectively drained the super-ego of Thanatos and filled it with neutralized energy instead. He was able to do this by saying nothing about fusion and defusion during identification. Laplanche (1981) attempted to resolve the issue by having "sexual-life drives" in the ego opposed to a sexual death-drive as "an extreme of sexuality ... working according to the principle of free energy and the primary process" in the id. Maleson therefore noted that it was "not at all clear why masochism was described as residing in the ego rather than the id". He traced much of the controversy to Freud's lack of clarity about the consequences of identification (Maleson, 1984).

Sublimation and the ego's energy

Freud is also unable to explain how energy is acquired by the ego. What he postulates as happening is that the ego is altered by identification to become like the parents and chosen as object by the defused portion of Eros also made available by identification. The new choice was "an abandonment of sexual aims, a desexualization - a kind of sublimation" (Freud, 1923b, p.30. Cf. pp.45-46, 54).

Three questions can be asked. How could the energy be non-sexual? What is the mechanism of sublimation? What does the ego operate with prior to the dissolution of the Oedipus complex?

Sexualisation or sublimation? In every other one of Freud's descriptions of the sexual instinctual drive being directed to non-sexual objects, or to instinctual trends, or to various activities the consequence is that that object, trend, or activity is *sexualised.* Thus, once it became admixed with libido each one of the following was said to be sexualised: social instincts (Freud, 1911a, p.62), non-sexual instincts in general (Freud, 1913d, p.323), memories (Freud, 1909b, p.206, n.1; 1918, p.96) phantasies (Freud, 1918, p.103), thought (Freud, 1909b, p.245; 1910b, p.80; 1912-1913, p.89), actions like looking, touching, or exploring (Freud, 1916-1917, p.309), as well as the ego itself during the secondary narcissism of megalomania (Freud, 1911a, p.72).

Ernest Jones (1926) observed that "narcissistic libido is still sexual". More recently Schafer noted that Freud's previous explanation of the "sexualization of ego functions" and "all the clinically observed varieties of sexualized secondary narcissism as well as the hypothesis of infantile

primary libidinal narcissism'' stood against transformation by sublimation. Schafer was forced to reject desexualisation so produced: ''Taken on its own terms, Freud's proposition that desexualization occurs *through* Oedipal identification is untenable'' (Schafer, 1968, p.205). It is no solution to shelter behind Ernest Jones' (1926) proposition of ''degrees of desexualization'' or H. Hartmann's (1955) that there is ''a certain limit'' beyond which sexualisation of ego functions occurs and which implies sublimation to take place below that limit.

The mechanism of sublimation What does the psycho-analytic literature have to say about sublimation? From the time the concept of sublimation came under scrutiny there has been absolutely no agreement about the behaviour to which the mechanism refers or the underlying processes on which it might be based. According to Glover (1931a), there had been ''a good deal of confusion'' about the concept before 1923, a confusion that had ''increased rather than decreased'' by the time he wrote (Cf. Ernest Jones, 1941). Nine years later Deri (1939) gave the impression that there was considerable agreement about the way in which the term was used, but Levey (1939), who used what he took to be Freud's own standards, concluded his contemporaneous and influential analysis by saying that sublimation was ''confused, obscure, incomplete, redundant, static, and lacking in objective verification''. Eight to nine more years saw Brierely (1947) described it as an ''omnibus term'' and H. H. Hart's (1948) opinion that ''our notions of sublimation are none too clear''. Seven years later again H. Hartmann said ''despite many efforts to free [sublimation] from ambiguities'' there was no doubt there was ''a certain amount of discontent'' among psycho-analysts with the concept (H. Hartmann, 1955. Cf. Hartmann in Arlow, 1955). Eleven years further on Kaywin (1966) opened his analysis by saying there were ''still many ambiguities and disagreements'' about the meaning ''and even the usefulness'' of the notion. After a lengthy analysis, one sympathetic to Freud's aims, Ricoeur (1970, pp.483-492) concluded that sublimation was an empty concept.

The points especially singled out in the discussions include this doubt about the usefulness of the concept (R. W. White, 1963), as well as whether an instinctual drive transformation *per se* can result in a socially valued activity (Bernfeld, 1931, cited by Lantos, 1955; Glover, 1931a; Levey, 1939; H. Hartmann, 1955; Kubie, 1962), whether the libido sublimated was genital or pre-genital (Deri, 1939), and the absence of characteristics defining sublimated activity and differentiating it from other mechanisms and processes (Levey, 1939; Bergler, 1945; Kubie, 1962; Kaywin, 1966; Hacker, 1972). Even the question of whether only libido can be sublimated has also been raised and disputed (H. H. Hart, 1948; H. Hartmann, Kris, and Loewenstein, 1949; H. Hartmann, 1955). The limitations of defining sublimation as mainly or fundamentally a change in the aim of the sexual drive were recognised early by Glover (1931a). He argued that the ''permanent neutrality'' of the desexualised energy supposedly formed during oedipal identifications demanded that energy transform-

ations had to be included in the definition. H. Hartmann (1955) later based his own approach to the essence of sublimation on Glover's point.

Many psycho-analytic theorists regard the energy transformation view as so inadequate or untenable that there must be serious doubt that sublimation can provide anything at all with which the ego can work (e.g. Colby, 1955, pp.1-36; Kubie, 1962; Sandler and Joffe, 1966; Kaywin, 1966; Holt, 1967/1968; Schafer, 1968, Ch.3; Applegarth, 1971). The most powerful line of criticism has been directed against the aphysiological notion of instinctual drive *energies* having different and variable aims. Begun by Colby (1955, pp.30-35) and extended by Holt (1967/1968) the argument was put most succinctly by White:

> an instinctual drive does not function with its own *kind* of energy, but with neural energies released in particular *places* (centers) and organized in particular *patterns*. Energy can be called sexual, for instance, only by virtue of the fact that certain somatic sources or hormonal conditions activate certain nerve centers which in their turn activate a characteristic pattern of excitations in skin, genitals, and elsewhere .... Aggressive energy is differentiated from sexual by the places and patterns that are central in the excitation. An ego interest, such as learning the skills necessary for an occupation, is neutral in the sense that its places and patterns are not those of either eroticism or aggression. (R. W. White, 1963, pp.178-179. Cf. R W. White, 1960)

Perhaps Freud's view was a hangover from Müller's and Helmholtz's notion that each sensory modality operated with its own kind of energy. But, given that that view had been completely discredited by the latter part of the last century, it was an especially old-fashioned neuro-physiology for even Freud to have adopted.

Early ego functions If neutral energy is only created through Oedipal identifications, what does the ego operate with before the dissolution of the complex? Are we to believe there can be no realistic modes of thought and action before the age range of five to seven years is entered? Where the shield gets its energy, where perceptual and attentional cathexes come from, and the source of the anticathexes that bring about and maintain the very early primal repressions are also of more than a little importance.

In the absence of a satisfactory account of the acquisition of its energy, the ego itself must remain, quite literally, an empty theoretical construct. Without desexualised energy the ego cannot judge incoming stimuli, bind traumatic excitation, think, or generate the signal of anxiety. The process of binding is crucial. In the words of Freud's last major work, it is the ego's *psychological* function. The ego's other main function, the *constructive* one of placing thinking between the instinctual demand and the satisfying act, was equally dependent upon neutral energy. The ego:

> after taking its bearings in the present and assessing earlier experiences, endeavours by means of experimental actions to calculate the consequences

> of the course of action proposed. (Freud, 1940a, p.199. Cf. Freud, 1911b, p.221; 1925d, p.238; 1933b, p.89; 1950/1954, *Project*, Part I, Sections 16, 17, 18 of September, 1895 and Part III, Sections 1, 2, 3 of October, 1895)

Neutral energy is absolutely crucial to Freud's conception of how the ego functions and he is quite unable to give a consistent account of how it is obtained.

Sex and the super-ego

One immediately obvious problem to which Freud himself drew attention was that the identifications from which the super-ego formed were:

> not what we should have expected, since they do not introduce *the abandoned object* into the ego. (Freud, 1923b, p.32. My emphasis, MBM)

The analogy with melancholia leads to a super-ego of the wrong type. Having lost the maternal object, the boy should have built his super-ego around his mother's qualities and the girl, having lost the father, should have had his masculine qualities reflected in her super-ego.

Freud's own attempt at filling the hiatus was embarrassingly inadequate:

> Analysis very often shows that a little girl, after she has had to relinquish her father ..., will bring her masculinity into prominence and identify herself with her father (that is, with the object which has been lost), instead of with her mother. This will clearly depend on whether the masculinity in her disposition - *whatever that may consist in* - is strong enough. (ibid. My emphasis, MBM)

Freud was silent about any such strengthening in the boy, merely remarking that the relative strengths of the two parts of the bisexual disposition determined which of the identifications was the stronger (op. cit., p.33).

Bisexuality had another role. Each child chose both parents as objects, lost both, and incorporated both. The boy, for example, chose his mother because of the masculine component of his sexuality and his father because of the feminine (op. cit., pp.33-34). But while bisexuality made the choice of the same sex parent possible it did this at the cost of creating the paradox that it was then not the child's biological sexuality that determined the essence of its psychological sexuality but its complement. For example, the boy's femininity led to the choice of the father and so determined one of the most essential of his male characteristics: the harsh, unforgiving, and law-ridden super-ego. There was a further complication for the girl. Her masculine component determined the choice of the mother but, if it were strong enough, it caused her to identify with the father.

Few psycho-analytic writers have even remarked on the hiatus. Ernest Jones (1926), early on the scene, proposed a particularly tortuous resolution. Without mentioning primary identification at all, he based the super-

ego on the replacement of an initial hostile rivrivalry with the father by a "homosexual love" for him. As he later made explicit:

> the super-ego is in all cases of predominantly homosexual origin, i.e. it arises from identification with the parent of the same sex as the child feels itself to be (not necessarily its actual sex). (E. Jones, 1928-1929)

Freud was unconvinced by Jones' ingenuity. He responded:

> All the obscurities and difficulties you describe really exist. But they are not to be removed even with the points of view you emphasize. They need completely fresh investigations, accumulated impressions and experiences, and I know how hard it is to obtain these. Your essay is a dark beginning in a complicated matter. (Freud to Jones, Letter of 20.11.1926. Cited in E. Jones, 1953-57,*III*, p.285)

Etchegoyen (1985), apparently more easily satisfied, accepted Jones' suggestions as "more convincing" than Freud's. Relatively few psychoanalysts have even drawn attention to or alluded to the hiatus. (e.g. Fenichel, 1926/1954a; Fuchs, 1937; Jacobson, 1954; Koff, 1961; Arlow, 1982). None has bridged it. The two explicit attempts I have found have been based on combining identification with introjection. They are neither consistent nor successful. Widlöcher (1985) has primary identification alter the *ego*, with a narcissistic introjection-identification bringing the right-sexed into the super-ego, while Deigh (1984) has the child identify with both parents and then introject both. Nearly a quarter of a century after Freud introduced the structural theory, Fenichel noted that "The attempts at solving these problems have not yet advanced beyond Freud's formulation" of a bisexual involvement (Fenichel, 1945b, p.104). It is evident that no progress has been made since.

It is, of course, precisely because the terms 'masculine disposition' and 'feminine disposition' ("whatever that may consist in") are so completely uncharacterised that they can be put to such use. Even so, Freud was unable to explain how an erotic attachment to the mother was replaced by a strengthened identification with the father (Abend and Porder, 1986). What must not be overlooked in these heroic theoretical endeavours of Freud's are that they are forced upon him because of the very untoward consequences of identification-by-incorporation. It is a quite inappropriate mechanism for explaining super-ego formation.

The critical function

In melancholia, the lost object re-established in the ego was supposed to be *reproached by* an already existing critical agency, the conscience. But, when the super-ego formed, it was the lost *object* itself that *became* the critical agency, directing *its* complaints against the unaltered part of the ego. We may therefore ask about the adequacy of explanations based on identification-by-incorporation as well as about the non-punitive functions psycho-analysts (and perhaps Freud himself) believed the super-ego had.

The critical agency Although it is obvious that the object incorporated in melancholia is *not* the critical ego, I have found Etchegoyen (1985) to be one of the few psycho-analytic writers to see this point. However, it is fairly widely recognised that during the formation of the super-ego, identification-by-incorporation has such different consequences from those in melancholia, if not opposite ones, it is difficult to reconcile the two mechanisms (Jacobson, 1954; M. W. Brody and Mahoney, 1964; Meissner, 1970). Freud gives no hint about the conditions under which one rather than another consequence ensues. To this problem we should add the one that Deigh (1984) has seen: why does identification lead to a part of the ego being split off rather than just being changed?

Super-ego v. ego-ideal Many psycho-analysts have also found it difficult to reconcile the super-ego as a whole being harsh and punitive with its also containing the much less severe ego-ideal.

Freud first described the ego-ideal in *On narcissism* and it is the ideal of narcissistic perfection described there that causes the theoretical difficulty. After being devalued by parental criticism, the infant's previous blissful narcissistic perfection was re-established as an ego-ideal, an agency sharply differentiated from the critical agency of conscience. Conscience was the agency that *embodied* the parental criticisms and watched the actual ego, measuring its behaviour against the standards of the ideal (Freud, 1914b, p.94. Cf. Freud, 1916-1917, pp.428-429). However, by 1921 Freud virtually claimed the opposite: the psycho-analyses of melancholia were now supposed to have shown that the ego-ideal itself was the critical agency (Freud, 1921, pp.132-133. Cf. Freud, 1917c, pp.247-248). Freud built on this reversed conception when he announced the structural theory two years later. There the ego-ideal was explicitly equated with the super-ego and given punitive functions in addition to the earlier ones of criticism, scrutiny, and standard bearing. In his very last formulation, in the *New Introductory Lectures*, only the names were changed. The super-ego exercised the function of conscience, it was "the vehicle" of the standards of the ego-ideal, and it was responsible for punishing behaviour. As Holder (1982) observed, this ego-ideal was not only very different from that portrayed in *On narcissism* but had little relation to narcissism at all.

By the time of the structural theory Freud had also pretty well inverted the source of the ideal's perfection:

> There is no doubt that this ego-ideal is the precipitate of the old picture of the parents, the expression of admiration for the perfection which the child then *attributed to them*. (Freud, 1933b, p.65. My emphasis, MBM)

No longer is the child's own lost narcissistic perfection the model. The ego-ideal is clearly modelled on the parents.

Now, although the parental qualities Freud emphasised were harshness and severity (Freud, 1923b, pp.54-55; 1924b, p.167; 1933b, p.62), he also described the ego as seeking the 'love' of its super-ego (Freud, 1923b, p.58; 1926a, pp.139-140; 1930, p.126; 1939, p.117; 1940a, p.206). For many psycho-analysts, this loving quality seemed to give the ego-ideal characteristics so different from the super-ego proper that they do not even accept it as "one of the functions of the super-ego" (H. Hartmann and Loewenstein, 1962). According to Loewenstein, these theorists were unable to reconcile:

> the early formulations of Freud ... according to which the ego ideal is the substitute for the lost infantile narcissism, with his later proposition that the parents are taken as models for the child's ego ideal. (Loewenstein, 1966b. Cf. Blos, 1974; Hammerman, 1965).

From the difference was born the notion of the ego-ideal as a separate structure having benign properties balancing the super-ego's punitiveness. After outlining some of the history of this notion, Sandler, Holder, and Meers (1963) concluded "There can be little doubt, on clinical and theoretical grounds, that some such step is necessary" (Cf. Novey, 1955; Loewenstein, 1966b; and Grunberger and Chasseguet-Smirgel cited in Mancia and Meltzer, 1981).

The proposal for separation is not new. Within ten years of Freud's new theory, Nunberg (1932/1955, p.146) had differentiated the ego-ideal as "an image of the loved objects" from the super-ego as "an image of the hated and feared objects". Lampl-de Groot claimed the earlier work of Alexander and Flugel as support for her rather similar conceptualisation in which a loving, wish-fulfilling ideal was contrasted with a prohibiting and punishing super-ego (Lampl-de Groot, 1947, 1962). Murray put the issue in its most general form. The super-ego and ego-ideal were "distinct and separate elements ... each ... born in its own distinctly different manner" (J. M. Murray, 1964). Even though Arlow (1982) called on Waelder's principle of multiple function to argue against separating the two agencies, saying that "the idealization 'ought to' was the other side of the coin 'must not'", the division is now widely accepted.

There is little doubt that as well as these differences about the super-ego and ego ideal, most of what has been referred to as "problems and inconsistencies" (Lampl-de Groot, 1962), "loose ends" (Turiell, 1967), "basic differences of opinion" (Steingart, 1969), "vigorous discussion" (Furer, 1972), "terminological confusion" (Kernberg, 1982), and the four distinct meanings of the term 'ego-ideal' that Sandler, Holder, and Meers (1963) distinguish between, result from the distinctly different functions ascribed to the two agencies (Cf. Hägglund, 1980).

Two variant resolutions of the separation position can be distinguished. In the one, the two structures are kept separate, with the ego-ideal usually seen as forming part of the ego (Piers and Singer, 1953; A. Reich, 1954;

Bing, McLaughlin, and Marburg, 1959; J. M. Murray, 1964; Hendrick, 1964; Bressler, 1969; S. Ritvo, 1971), but sometimes kept completely apart (Novey, 1955). In the other, the ego-ideal eventually becomes part of the super-ego but only after its relatively independent earlier development (Nunberg, 1932/1955, pp.137-147; Jacobson, 1954, 1964, pp.186-189; Schafer, 1960; H. Hartmann and Loewenstein, 1962; G. L. Bibring, 1964; Deutsch, 1964; Hammerman, 1965; Loewenstein, 1966b; Blos, 1974). Other variants are also possible (Mancia and Meltzer, 1981; Brickman, 1983; Kramer, 1986). Of these the oddest must be that of Hägglund (1980) who has the ego-ideal as a "developmental extension of the negative Oedipus complex" and the positive "based more on incest wishes" and "therefore the main *result* of the super-ego".

None of the resolutions is widely accepted and we must conclude that the differences in function remain unexplained. The problem is not, however, where the ego-ideal should be located or of any "intrinsic ambiguity" in the ideal being at once a component and a content of mind, as Blos (1974) contends. The problem is narcissism. Narcissism was "never ... adequately integrated with the later theoretical concepts" and no place at all could be found for it in the topographic theory (Spruiell, 1979, 1981).

Narcissism and the character of the super-ego

Most theorists concerned with the differences between the super-ego and the ego-ideal took it that the ideal had to have formed out of some kind of early narcissistic perfection. The problems that result from deriving the ideal from narcissism are partly empirical and partly theoretical.

The empirical problems of narcissism Infantile forms of narcissism have doubtful empirical status in the psycho-analytic literature. Granting, as Loewenstein proposes, that the shift in the new theory to a secondary narcissism fits better with "the over-all concept of the mental apparatus", it remains the case that there is still an:

> extreme difficulty of stating clearly what happens in the mind of the child in the first few months of life. For that earliest period, *neither reconstructions* from later analyses *nor direct observations* will yield definite answers to questions regarding the interaction between primary and secondary narcissism. (Loewenstein, 1966b. My emphasis, MBM)

Balint (1960) showed, for example, that the observations supposedly supporting the concept of primary narcissism were relevant only to the secondary form and there has always been a number of analysts taking a similar view (Wisdom, 1968; B. E. Moore, 1975; Pöstényi, 1979). It is this issue that lies behind Laplanche's and Pontalis' (1967/1973, p.338) conclusion that, empirically speaking, the existence of primary narcissism is "highly problematic" and Etchegoyen's (1985) more recent opinion that "the theory of primary narcissism is only a myth about origins". It was

never the case, as Hartmann, Kris, and Loewenstein (1946) once asserted, that "Freud's theory of 'primary narcissism' seems still best to account for facts immediately observable after birth".

After 1923, Freud made only one mention of primary narcissism (Freud, 1940a, p.150) and none at all of the secondary form. But if neither form can be observed, it is, as Loewenstein says:

> hardly possible to circumscribe the respective roles, at a given age, of narcissistic and object cathexes in the ontogenesis of the ego ideal. (Loewenstein, 1966b)

By speculating that the ideal formed from some kind of mix of parental and ego- or self- idealisations, psycho-analysts settled for a very poor second and did not reach a consensus about where the ingredients came from (Hägglund, 1980). If we accept Loewenstein's point that observations are just as opaque as reconstructions, even those kinds of sources can never be known with any certainty.

The theoretical problems of narcissism It is agreed that Freud left the relation of his conceptualisation of libido-narcissism to the ego-id-superego model far from clear (Compton, 1981d. Cf. Compton 1981a, 1981b, 1981c) or "quite puzzling" (Jacobson, 1954. Cf. Jacobson, 1964).

Instinctual fusion, a concept absolutely demanded by the structural theory, means there cannot be a purely libidinal investment of a primitive ego or self. Narcissistic cathection can only mean "the relative predominance of libidinal over aggressive investment" (Kernberg, 1975, p.318. Cf. Kernberg, 1982; Jacobson, 1964, pp.14-16; Mahler, 1971). And, as Spruiell notes, it is the aggressive component that "has never been satisfactorily included" in formulations of narcissism (Spruiell, 1975). Nevertheless, the authors of one of the main theoretical discussions of narcissism restricted their consideration to libidinal energy and indicated only in passing the need for studying aggressive drives (Bing, McLaughlin, and Marburg, 1959). Even those of the few who note the importance of aggression or of the death instinct for understanding narcissism are unable to propose solutions (e.g. B. E. Moore, 1975; Duruz, 1981a, 1981b).

A minimal theoretical solution comes from those psycho-analysts who alter the target of libidinal investment and make structures and functions other than the ego, usually the self or the self-representation, the narcissistic focus (H. Hartmann, 1950, 1955). It is, of course, hardly a solution; any resulting idealised qualities must still have aggressive characteristics. Consequently, as Furer (1972) observed, the shift to ego-psychology leaves the problems of super-ego formation unresolved. Another solution first separates libido from its aggressive companion before supposedly finding the essence of narcissism in an idealising quality of the libido itself (Kohut, 1971). Here one can only admire the sleight-of-mind that so cleverly disposes of both aggressive investment and primary narcissism.

Complex developmental schemas have also been constructed within which the interactive effects of aggressive and libidinal drives on ego, self, or self-representation are positioned. However, for the most part these schemas are inconsistent with each other and within themselves (e.g. Jacobson, 1964, pp.119-135; Kernberg, 1975, 1976, 1982; Teicholz, 1978; Tyson and Tyson, 1984). Finally, and as might be expected, dissatisfaction of a more general kind with Freud's energy-investment concept has led some to discard wholly or partially that mode of conceptualising narcissism (e.g. Joffe and Sandler, 1967). Some of the "functional" alternatives proposed move so far from the basic tenets of psycho-analysis that they can barely be considered extensions or amplifications of Freud's theses as, for example, Stolorow's (1975a) which does away with ego to some extent and with drive altogether.

The core problem for the ego-ideal, then, is that once Freud had incorporated a death instinct into his theory, his original notions of primary and secondary narcissism should have been done away with altogether. A death instinct or other primary aggressive drive necessarily ruled out the kind of narcissistic state or mode of existence required to produce a completely or even predominately positive ego-ideal.

IDENTIFICATIONS

Neither of the two different kinds of identification from which the super-ego forms is based on assumptions consistent with other aspects of psycho-analytic theory. Primary identification ('being like') allowed the child to form emotional ties on a basis other than the satisfaction of its instinctual drives and identification-by-incorporation ('having') took place when the childhood drive was supposed to be without objects.

Drives and emotional ties

While it is true that Freud allowed for non-sexual feelings of affection, these are either aim-inhibited sexual drives (e.g. Freud, 1923a, p.258; 1925a, p.39) or feelings arising as self-preservative needs are satisfied concurrently with sexual ones (Freud, 1913e, pp.180-181). Nowhere does Freud find a place for emotional bonds not deriving from instinctual drive satisfaction. In slightly different ways, Widlöcher (1985) and Abend and Porter (1986) have all drawn attention to this aspect of the problem of identification. Padel (1985) noted that ego-change through primary identification was inconsistent with the drive-satisfaction formulations required to explain introjection. He argued for a theory of ego formation built almost solely on a non-drive satisfaction concept of primary identification. It would seem that Ricoeur's conclusion of there being "more questions than answers" in relating primary identification to an instinctual energetic view has been amply confirmed (Ricoeur, 1970, p.219).

Objects and primary identification

If primary identification were direct and quite different from object-choice it seems very odd that Freud should so persistently describe it as based on the oral incorporation *of an object*. For example, after opening his discussion of the ego-ideal in *Group Psychology and the Analysis of the Ego* with the clear distinction between identification with an object (being like) and choosing it (having), Freud based what followed completely on identification through oral incorporation of the object, that is, on an oral object-choice (Freud, 1921, p.105). Similarly in *The Ego and the Id* he wrote that the origin of the super-ego lay hidden in the individual's:

> first and most important identification, his identification with the father .... This is apparently *not* in the first instance the *consequence or outcome* of an *object cathexis*; it is a *direct and immediate* identification and takes place *earlier* than any *object-cathexis*. (Freud, 1923b, p.31. My emphasis, MBM)

Two pages earlier, however, Freud had voiced the opposite thought:

> *At the very beginning*, in the individual's primitive oral phase, object-cathexis and identification *are no doubt indistinguishable*. (Freud, 1923b, p.29. My emphasis, MBM)

No wonder Ricoeur (1970, p.219) placed the supposed oral origin of identification as first among the many unanswered questions about it.

Identification as a mechanism

In the psycho-analytic literature there is agreement that the core meaning of identification is a simple one - to be like or to become like another (Abend and Porder, 1986). There is also agreement on its importance as a "nodal point in general psychoanalytic thinking and conceptualization" (Simenauer, 1985. Cf. Etchegoyen, 1985). Yet as recently as 1984 Rangell judged identification to be "the most perplexing clinical-theoretical area" in psycho-analysis, an opinion that Carneiro Leao (1986) has subsequently quoted and endorsed.

The fact is there are simply no agreed upon definitions of identification nor of the related concepts of incorporation, introjection, and internalisation at either the theoretical or clinical levels. Fuchs (1937), Knight (1940), Greenson (1954), Sandler (1960), H. Hartmann and Loewenstein (1962), and Meissner (1970, 1971, 1972) all drew attention to the 'complications', 'confusion', and 'chaos' about identification and advanced their own conceptual and terminological reforms. For example, H. Hartmann, Kris, and Loewenstein (1946) distinguished a pre-Oedipal identification that was both a defence mechanism and a means by which personality was formed from a second that contributed to the super-ego. Of the first they say its impact on ego development "is not known in detail". The only

explanation they offer for it, an obviously incorrect one, is that it is based on incorporation. And, although the second is "different", all they specify about it is the "concomitant change in the economy of psychic energy" they see it as involving. But Etchegoyen (1985), among others, rejected the distinction between the two kinds of identifications and others, like S. Ritvo and Solnit (1960) simply adopted one of the related terms (in their case internalisation) as a blanket term.

Ferenczi (1909/1952a), who invented the concept of introjection, thought introjection was brought about by identification (Ferenczi, 1912/1955a, 1913/1952c). Fenichel (1926/1954a) recognised Ferenczi's usage but treated introjection and identification as being the same, an "unjustified" identity according to Hendrick (1951). Fenichel also reluctantly accepted that incorporation could be equated with introjection and this latter equation was adopted in whole or in part by Zilboorg (1938), Knight (1940), Nunberg (1955, pp.141-142), M. W. Brody and Mahoney (1964), and Schafer (1973). What amounted to the converse of Ferenczi's hypothesis was also proposed: incorporation resulted in identification (Knight, 1940; H. H. Hart, 1947; Greenson, 1954). Incorporation was regarded as a physical process and introjection as psychological by Glover (1947) and Greenson (1954) but Schafer (1973) treated the terms as synonyms and saw both processes as psychological in that both referred to fantasy.

Fuchs (1937) held identification to be an ego-term and introjection an id-term and Meissner (1979b) similarly categorised identification as mature and introjection as immature. On the other hand, in Waelder's (1936) analysis, introjection was an ego mechanism and presumably mature.

Knight's (1940) solution was to restrict the various forms of identification to the result of some action or mechanism and not to allow the term to be used for the act itself, a suggestion partly anticipated by Fuchs (1937). Knight's proposal was substantially endorsed later by Greenson (1954), although Schafer (1968) argued against it and Sandler (1960) and H. Hartmann and Loewenstein (1962) kept the dual reference. Simenauer virtually put all the meanings together by making identification more than an assimilation and more than one mechanism among many. Indeed, for him, identification used "a whole series of cohorts of psychic mechanisms ...[to]... integrate the individual with external objects" (Simenauer, 1985).

Koff's (1961) original picture of a Humpty-Dumpty created confusion has not changed. Meissner (1970) also saw the looking-glass arbitrariness and similarly prefaced his lengthy evaluation with a reference to Carroll. But, whereas Koff (1961) had attributed the confusion to some of the terms having antithetical meanings, Widlöcher (1985) to its referring to "a complex of mental operations whose boundaries are indistinct", and Abend and Porder (1986) to identification being used to refer to both a process and

a product, Meissner laid the responsibility on Freud himself. Freud had "used the term with considerable flexibility", having included under it "a variety" of ego alterations that really covered "a set of metapsychologically distinct processes" (Meissner, 1972). Some even take the identificatory jumble to be a mark of progress. Widlöcher judged the confusion to reflect challenges to Freud's 1933 "explanatory theory and descriptions", and actually says that if they could not be challenged in this way, "what could be their scientific value fifty years later?" (Widlöcher, 1985).

Primary identification

By and large, psycho-analysts grant the importance and centrality of primary identification in psycho-analytic theory even though they also grant that the concept varies "according to each author and his ideas, its meaning, in consequence, being far from precise" (Etchegoyen, 1985). Actually, the problems associated with primary identification pretty well defeated the earlier critics. Fuchs (1937) gave up: primary identification did not involve introjection, the term should be dropped altogether and replaced with an (unspecified) description of what happened. Greenson (1954) simply accepted Freud's ambiguity and it is not surprising he admitted to knowing more about identifications that *resolved* the Oedipus complex than about "the earlier forms". Like Meissner, Sandler (1960) dismissed primary identification as not being "the sort which leads to superego formation". By then Jacobson (1954) had been able to describe super-ego formation by avoiding Freud's "somewhat vague" term altogether. Holder (1982) later outstripped even that feat by accounting for super-ego formation without drawing on identification of any kind (Cf. McIntosh, 1986) and Brenner (1982) failed to mention either the repression or dissolution of the Oedipus complex when he argued that the process of super-ego formation had not been understood.

Narcissism and primary identification Primary identification is, Etchegoyen says, "anything but clear" and if attempts are made to relate it to the stage of primary narcissism, as he himself does, his caveat about the mythical nature of that origin must be recognised (Etchegoyen, 1985). There is another problem in that identification and narcissism are incompatible concepts which, Meissner's analyses show, cannot be separated. Meissner (1970) found that identification had five different meanings - dream, hysterical, primary, narcissistic, and partial identification - and was based on as many processes. From Freud's descriptions of primary identification, Meissner differentiated an "early objectless form", which was not based on introjection (presumably the 'being like'), and one that was "a function of an object relation" which was (presumably 'having'). Although both were narcissistic, he recommended the two were "better kept separate". Only the introjected object relation kind had, he said, "primary application to superego formation" but made no clear further reference to the very early form. Despite his critique, Meissner ended by accepting Freud's primary identification as a pre-object emotional

attachment brought about by *oral incorporation*. He thus fell victim to the very ambiguity between identification "which required object loss" and other forms which "did not depend upon object relation or object loss" he had himself discerned in Freud (Meissner, 1972).

In this connection, Compton raises the possibility that the concept of primary identification was "perhaps a substitute" for that of narcissistic identification. He remarked that Freud "did not much explain" primary identification or its relation to later forms (Compton, 1985a). Elsewhere Compton notes that the prior concept of primary narcissism "almost disappeared" from Freud's work after its introduction in *Mourning and melancholia* (Compton, 1986c). Perhaps Freud's coyness was deliberate. How could either concept be explained, especially the new one, when its characterisation was so at variance with the rest of the theory? Compton (1985a) also described narcissistic identification as "cumbersome", as "unclear", and complained that it "did not make sense" when "put in terms of transposition of libido". In fact, as Abend and Porder (1986) suggest, Freud's original invention of narcissistic identification may have been because the Oedipal part of his theory had true object-cathexes coming into existence only at that much later stage.

Much contemporary discussion continues to founder on the ambiguity. Parkin (1983), for example, says primary identification is a description of "the primary narcissistic union" but he does not see the contradiction that in that state any kind of emotional tie with another, even the most primitive, is an impossibility. The same objection applies to Simenauer (1985) and Widlöcher (1985), who both have primary identification occurring in a state of fusion, even though Widlöcher clearly differentiates it from the oral incorporation of an object and insists it is not narcissistic, and Simenauer equates primary identification or 'being' with narcissistic identification and object choice as 'having'. Even Carneiro Leao (1986), who in my view asks the most sensible questions about primary identification, ends by making it a form of narcissistic identification quite distinct from secondary identification. Defining all identifications as primary (the 'to be like' kind), as does Deigh (1984), is of course not a solution. It immediately creates the problems of allowing no role for introjection and also making it difficult to differentiate permanent ego alterations from temporary ones. But it would allow some use for Kramer's (1986) uncharacterised - and ugly - term 'dis-identification'!

Resolutions Resolving the ambiguities and inconsistencies at least requires, as the analysis by Belmonte Lara and colleagues (cited by Etchegoyen, 1985) shows, that primary identification be placed at the only place where it makes sense: at the beginning of the phallic phase. But even that is not enough. In concurring with Etchegoyen's negative judgement about primary identification, Blum (1986) not only concludes it is "so diffuse a concept that it has probably outlived its usefulness" but goes on to apply the criticisms of primary identification and primary narcissism

to secondary identification. It "does not seem to make sense" to invoke an identification "when there is not yet an emotional tie to a differentiated object with which to identify". One cannot avoid Brenner's description of the current theory of the super-ego forming through identification as "inadequate" (Brenner, 1982). Even without my criticisms, disagreements about the meaning and usefulness of primary identification of the magnitude we have seen among psycho-analysts precludes accepting Freud's explanation of how the father comes to form the essence of the super-ego.

Why did Freud postulate a mechanism so confused as primary identification, one so lacking explanatory power, and one so at variance with the rest of his theory? Because only through it could the super-ego be made sexually appropriate. Bisexuality had paradoxical consequences and identification by incorporation provided an object of the wrong sex, *ergo* some other mechanism had to be found. Primary identification filled the bill nicely, perhaps all too nicely - it needed only to be 'intensified' to give the super-ego sexually appropriate psychological characteristics. Thus the boy's super-ego was masculine because of a simple strengthening of the already existing resemblance of his ego to that of his father. Plausible as it is, this mechanism has to be rejected. Not only is primary identification inconsistent with the rest of the theory but it is itself uncharacterised, its strengthening is unexplained, and its capacity to contribute more powerfully to the super-ego than the opposite-sexed object positively mystifying.

OEDIPUS AND THE SUPER-EGO

The strong trend among contemporary psycho-analysts to make the Oedipus complex less central than did Freud to development and to psycho-pathology may be an attempt to find solutions to some of the preceding problems. Although not brought out explicitly in these 'new wave' discussions, it follows that the importance of the Oedipus complex to the development of the super-ego must be reduced.

Oedipus a "shibboleth"

In his contribution to a recent panel discussion re-evaluating the Oedipus complex, Basch (in Sacks, 1985) represented psycho-analysis as defining itself as a science through the "particular construct" of the Oedipus complex rather than through its method of investigation. He described the complex as a "shibboleth" which "limited what it was permissible to discover with the clinical method of psychoanalysis" and went so far as to identify and praise Ferenczi, Balint, Bowlby, Erikson, Fairbairn, Guntrip, Hendrick, Winnicott, and Kohut for being among the "distinguished psychoanalytic thinkers" who had freed themselves of its constraint and "uncovered new data which modified and expanded Freud's early conclusion that the Oedipus complex was central for all of development". Stressing that the re-evaluation was a modification of Freud's

position, Basch opined, "No one is challenging infantile sexuality and the oedipal phase as *one aspect* of human development" (my emphasis, MBM). Modell (in Sacks, 1985) disagreed only slightly. He thought "the Oedipus complex is a universal biological predisposition of probable genetic origin" but did not consider it was necessarily "a universal contributor to psychopathology". Similarly Brenner granted Oedipal conflicts the largest role in super-ego formation while denying the super-ego was "the heir" to the Oedipus complex and asserting it to be one of many resulting compromise formations (Brenner, 1982). According to Burgner (1985) in recent years "many analysts" had "questioned a number of issues classically connected with the Oedipus complex, and sometimes even the centrality of the concept itself".

Oedipus and the clinic The matter is not only or even mainly a theoretical issue. Basch (in Sacks, 1985) asked if it were not time to change psychoanalytic theories about the Oedipus complex "so that they are in keeping with our actual clinical experience (as opposed to the sanitized versions that appear in the literature)". However, Laufer (1982) has endorsed Freud's view of the Oedipus complex as central, saying it remained "one of primary importance" to his own clinical work and Lebovici (1982) claimed the Oedipus complex to be universal and at "the heart of the mental life of man".

Oedipus and the clinical facts The 1985 panel discussion also showed that clinically obtained facts are too muddied to resolve the issue. Modell (in Sacks, 1985) said the analyses of "many patients" were for years dominated by self and actualization issues "to the exclusion of oedipal conflicts". Despite this endorsement of the importance of self and actualization, Modell disagreed fundamentally with the conceptualisation of them in Kohut's psychology of the self. On the other hand, Basch (in Sacks, 1985) characterised Kohut's findings as challenging Freud's theory that "the Oedipus complex is focal and the universal gateway to normal maturation" and Simon (in Sacks, 1985) described them as an "attack on the unique and privileged position of the Oedipus complex". In later general discussion Wolff (in Sacks, 1985) described Kohut as having moved the Oedipus complex "from the center ... to a more peripheral position". Loewald (in Sacks, 1985) disagreed, however. There was, he said, no dichotomy between Kohut's and Freud's views.

How early is the Oedipus complex?

One of the major controversies about the Oedipus complex concerns its time of occurrence. For Laufer (1982) the controversy centres around the theories of Melanie Klein who holds that the super-ego begins to develop in the first year of life through the mechanisms of projective identification and the introjection and projection of 'good' and 'bad' objects. In the phallic phase when, "according to classical theory" it becomes the heir of the Oedipus complex, it simply reaches its developmental climax (M.

Klein, 1952). Even with that qualification, Klein's concept of the super-ego and its formation is markedly different from Freud's.

Mancia and Meltzer (1981) note that Klein pays little attention to the ego-ideal and where she does she seems to confuse it with the ideal ego. I have also noticed that she either does not use concepts like identification, sublimation, neutralization, or repression at all or she uses them in ways very different from Freud. As to Klein's own notion of projective identification, Etchegoyen (1985) comments that it erases the essential distinction Freud made between "being like" and "having". Together with other commentators Etchegoyen also rejects its compatibility with the notion of a symbiotic union between mother and child. In that state, there is insufficient differentiation between ego and not-ego. In contrast, Silverman (1986) bases what he sometimes terms "primal" and at other times "primary" identification precisely on that union. He thinks the Kleinian mechanisms allow for the development of the separation. There is a further problem with the claim of compatibility if it is true that the harshness of the super-ego can actually be derived from projective identification, as Mancia and Meltzer (1981) argue. Deigh makes the point that this is a different source from that proposed by Freud who had it that super-ego severity depended on the child's own aggressiveness. Deigh also finds no evidence for Freud's endorsement of Melanie Klein's thesis that the severity of the super-ego reflects the child's view of how the parents treat it, as Wollheim claims (Deigh, 1984).

Precursors to the Oedipus complex?

Rangell (1978) claimed that psycho-analysts had pushed the frontier of the genital phase to the second year of life so that 'genital' - 'oedipal' and 'pre-genital' - 'pre-oedipal' were no longer synonyms. If so, a major change is required to Freud's thesis that the development of and resolution of the Oedipus complex is a phenomenon of the phallic phase. The second of the controversies Laufer (1982) identified is this one over the relative contribution of pre-Oedipal components and which he associates with the views of Mahler, Kohut, Winnicott, and Jacobson.

A number of other clinicians has also opted for early placement. Burgner (1985) reported that his group at the Hampstead Clinic - the centre of anti-Kleinian thought - "did not accept the classical concept" of the superego forming only after Oedipal resolution. Although offered as an alternative to Melanie Klein's theses, Holder (1982) also has the super-ego forming before the phallic phase. He claims in addition that the resolution of the Oedipus complex is not an essential pre-condition for the super-ego's later autonomy. There are many more, probably even some who are silent on the issue. Gillman (1982) describes two fears amongst psycho-analysts who propose that there are pre-Oedipal precursors to the super-ego: they might be construed as blurring the significance of the Oedipus complex or as endorsing Melanie Klein. The link was brought out clearly

by Etchegoyen (1985) who, along with others whom he cited, saw Klein's very early placement of the Oedipus complex as stripping the Oedipus complex of its "nodular" function in normal and pathological development. Etchegoyen prefers an early placement because it explained "the clinical facts better" and did away with "the *ad hoc* hypothesis of deferred revision" (i.e. deferred action).

Although Laufer (1982) noted that for Freud the Oedipus complex "as a developmental landmark" formed only in the phallic phase he also represented Freud as believing that it was the culmination of a very long process. But not much that is specific is said about the precursors. Garza-Guerrero (1981a) found that although there are "allusions" to the effects of "'biological roots', 'constitutional endowments', 'inherited potential', 'instinctual givens', and 'drives' on super-ego development in the psychoanalytic literature, these are mentioned "only in passing or in footnotes, or with the author's promising to elaborate later on". Holder (1982) is a good example. He says there are phylogenetic contributions but does not say what they are. The situation is similar with the non-phylogenetic components. Even though his paper is entitled "Problems of the superego concept" and he grants their existence, Arlow (1982) says practically nothing about them.

For the most part, Klein's theses are too deficient in other respects to provide strong precursors to the phallic phase. Gillman (1982), who does not accept the Kleinian view, cites Spitz, Furer, Gould, and Parens in support of his proposal that the precursors are the inhibitions and restraints of the child by its parents. On the other hand, Brickman (1983) sees them as the affective, cognitive, and behavioural differentiations that occur at the different stages of psychosexual development. It is then easy for him to incorporate Klein's ideas into the affective group of precursors. Loewald (in Sacks, 1985) has the precursors as an initial transindividual phase that he discerns in the work of Mahler and Kohut.

What importance do psycho-analysts place on these pre-Oedipal events? Of the Kleinians, we might note that Garza-Guerrero (1981a) cites Winnicott as saying of analysts who claim too much for Klein's notion of the precursor of "depressive position" at six months of age: "what a pity to spoil a valuable concept by making it difficult to believe in". And, by themselves, without integration into a larger process, Laufer (1982) does not grant that any precursors provide answers to psychopathology. But that is the problem: psycho-analysts simply do not agree on the nature of that long process. Kramer (1986) and Kanzer (1985), among many, lump together, in the most superficial of ways, almost all the contradictory formulations into one or another sponge-like developmental schemata, presumably in the hope that the differences will be soaked up. Garza-Guerrero (1981a) similarly observes that most of the discussions of the concept of the super-ego, including Flugel's (1945) encyclopedic contribution, are "additive" rather than "integrative". Nevertheless he extends

his own mini-encyclopedia by merely adding Jacobson to Kernberg before placing the resulting hybrid within the most simplistic of frameworks, one generated from the list of the unresolved problems: the role of pre-Oedipal components, the relation of the ego-ideal to prohibitions, Oedipal resolution, and post-oedipal development (Garza-Guerrero, 1981b).

Without, I am sure, trying to be ironical, Simon (in Sacks, 1985) refers to the "full elaboration" of the Oedipus complex as "a rich, elastic, and subtle theory with a great deal of explanatory power" before going on to say that however little it has to do with primordial history it is "definitely applicable" to explaining the schisms that mark the history of the psychoanalytic movement.

THE FEMALE SUPER-EGO

Freud's explanation of super-ego formation in the female, which I have barely alluded to so far, has the useful function of bringing together an number of issues arising from the various inadequacies of the concepts of identification and sublimation.

The female as sexually masculine For Freud the sharp distinction between male and female sexual characteristics was not established until puberty: until then clitoris and penis were both phallic in function, the vicissitudes of psycho-sexual development were the same, and the mother the first sexual object for both sexes. Libido and sexuality were essentially masculine (Freud, 1905b, pp.219, 222). It was within the constraints posed by this conception that Freud had to explain how the female child came to be possessed by the twin desires of sexual love for the father and murderous hostility toward the mother or how she developed a feminine super-ego.

When he began his theorising about the female super-ego, Freud pictured the little girl as choosing the father, forming a primary identification with the mother, and emerging from the Oedipal situation in a way "precisely analogous" to the male (Freud, 1923b, p.32). There are several problems with the analogy. First, the girl could not choose the father as a sexual object anaclitically because he satisfied no self-preservation drives. Second, the resolution of the Oedipus complex required that the father be identified with by incorporation; as a consequence, the girl's super-ego would have had masculine characteristics - a patently absurd result. Thirdly, it was implicit in this description that the girl's sexuality was feminine, not masculine. There was no basis for Freud's claim that the outcome of the Oedipus complex in the girl was "precisely analogous".

Female sexuality Within essentially the same period that saw the completion of *The Ego and the Id*, Freud began the first of five works (four papers and a Lecture) which bore on the origins of female sexuality over which he continued to worry during the next eight years. The works are as relevant

to Freud's attempt to complete the account of super-ego formation as they are to femininity as such.

Throughout the papers and the Lecture, Freud insisted that the female child's sexuality was initially masculine. In the first paper, *The infantile genital organization*, he implied that he had previously underestimated genital primacy. The dominance of the genitals in the infantile genital organisation was not far removed from that of the mature form but it was a primacy of the *phallus* rather than of the genitals (Freud, 1923e, p.142). Because only maleness existed, Freud had no doubt that the girl also went through the phallic phase (op. cit., p.145).

In the second paper, *The dissolution of the Oedipus complex*, Freud noted that the girl could not experience the fear of castration which destroyed the boy's Oedipus complex and established his super-ego (Freud, 1924d, pp.175, 177). What essentially differentiated the girl from the boy was her acceptance of castration as "an accomplished fact". After becoming aware that a boy had a penis and she did not, she explained the deficiency by assuming that an earlier castration had robbed her of it. Without the fear of castration, "a powerful motive" dropped out for the formation of the female super-ego (op. cit., p.178). Nevertheless it was set up.

In the third paper, *Some psychical consequences of the anatomical distinction between the sexes*, Freud described what turned the little girl away from the mother, and made her choose the father, as a passive "loosening" of the tie (Freud, 1925f, p.254). But, for the analogy to hold properly, there had to be hostility toward the mother. Although in the fourth paper, *Female sexuality*, Freud described the attachment as being broken because of active feelings of hostility on the girl's part (Freud, 1931, pp.231-204, 240-242) it took him some time to propose the hatred was because the mother was responsible for the absence of a penis. Girls never forgave their mothers the life-long disadvantage to which they had thus been put (Freud, 1925f, p.254; 1931, pp.234-235. Cf. Freud, 1916b). Freud could then argue in the lecture, *Femininity* in the *New Introductory Lectures*, that the source of the super-ego, as with the boy, was the castration complex (Freud, 1933b, p.124).

The choice of the father How was the father chosen? Freud drew on two of his very much earlier notions: the male as a provider of the penis and the symbolic equivalence of penis with baby (Freud, 1917d, p.129. Cf. Freud, 1913c, p.282). He brought these notions together by describing the little girl as attempting to compensate for the assumed loss of her penis by symbolically transforming her wish for that organ into a wish for a baby and turning to her father for its gratification (Freud, 1924d, pp.178-179). What brought the little girl's female sexuality into being, then, was the stealing of her masculinity and what consolidated it was her attempt at restitution.

Consequently, Freud's final resolution of the problem posed by the constraints retained the postulate of an initial male sexuality for the girl but supposed a developmental transformation which allowed the girl to enter the Oedipal situation with the right complement of feelings. Her situation was then truly analogous to that of the boy. She could choose the father and model her super-ego on his.

Femininity The process by which Freud imagined the girl's Oedipus complex to be dissolved resulted in a super-ego which was less harsh and more forgiving than the boy's. Lacking castration anxiety she had no real motive to surmount the Oedipus complex. Girls thus stayed in the Oedipal situation for an indeterminate length of time, the complex was demolished only incompletely, and the resulting super-ego was less like that of the male on which it was modelled (Freud, 1925f, pp.256-257; 1931, p.230; 1933b, p.129). In a word, she had acquired feminine characteristics. The female super-ego was less inexorable, less impersonal, and less independent of its emotional origins than the male's (Freud, 1925f, p.257). Related traits - woman's lesser sense of justice, her greater unwillingness "to submit to the great exigencies of life", and her being more readily swayed by feelings of affection and hostility - were all, Freud thought, "amply accounted for" by the incompleteness of the Oedipal situation (op. cit., pp.257-258. But see Freud, 1933b, p.134 for a different explanation of the sense of justice).

Apart from peculiarities in the formation of the female super-ego, it seemed to Freud that there were two other ways in which feminine psychological characteristics were created. Some appeared to be residuals from the pre-Oedipal phase: repeated alternations between masculinity and femininity, the failure of the libido of females to incorporate an aggressive component, the frequency of sexual frigidity, and the peculiarities in the choice of husband and typical attitudes to him and his male children (Freud, 1933b, pp.131-133). Other characteristics seemed to derive directly from penis-envy although Freud was rather less certain about them. He thought they included narcissism, vanity, shame, as well as jealousy and envy itself (op. cit., pp.124, 131-132. Cf. Freud, 1925f, p.254 for jealousy).

Freud briefly summarised these various results of the girl remaining in the Oedipus situation for an indeterminate period and of her late and incomplete demolition of the complex:

> In these circumstances the formation of the super-ego must suffer; it cannot attain the strength and independence which give it its cultural significance. (Freud, 1933b, p.129)

It is Freud's claim of lesser cultural significance which I examine before concluding this discussion of the super-ego.

The female and cultural development According to Freud, in the remote prehistoric past, some of the young members of the primal horde, in which

people then lived, collectively killed the horde leader. The remorse which arose in the killers had two momentous and simultaneous consequences: a religion was established which centered around the worship of a totem animal ancestral figure, representing the slain leader, and systems of taboos were set up forbidding the killing of the totem animal and denying sexual relations between the remaining members of the horde (Freud, 1912-1913, pp.140-146). The killing was the "great event with which civilization began" (op. cit., p.145) and Freud repeatedly emphasised its significance (Freud, 1925a, p.66. Cf. Freud, 1927, p.13 and 1930, p.101).

Because the leader of the horde was the father and his killers were his sons, civilization was solely a male creation. True, Freud did also assert that love, including woman's love, had to be recognised as one of the foundations of civilization (Freud, 1912-1913, pp.101, 103), but it was pretty obvious to him that woman's contribution could not be of the same magnitude as that of the man. Woman's love was fundamentally passive and narcissistic and led her to acquiesce in the desire of the male to keep his sexual objects near him. What contribution even that made was before the males had taken the really decisive action of killing their father (op. cit., pp.99-100).

However, for Freud there was a much more fundamental basis for woman's limited contribution to civilization. He believed her to have less capacity than man to sublimate her instinctual drives, that is, to have a slighter ability to redirect her libido on to cultural ends (Freud, 1930, p.103; 1933b, p.134). She could not contribute to the growth of civilization to the same extent as man. The very limited sublimation also led her to make sexual demands on man that prevented him from deploying his libido for cultural purposes as fully as he might otherwise have done. And, to the extent that he did not meet her sexual needs, so she became hostile to man, to his civilizing mission, and to its end product. Woman thus doubly restrained and retarded the development of civilization (Freud, 1930, pp.103-104).

Woman's deficiencies came about during the formation of the super-ego. When the Oedipus complex was overcome, identifications replaced object-cathexes and a defusion of Eros and Thanatos took place. Because the female surmounted the Oedipal situation only partially, the defusion has also to be only a partial one. Less of the sexual energy can therefore liberated in her than in the male and less made available to be sublimated. And if, as Freud also asserted, every identification was a sublimation (Freud, 1923b, pp.30, 45, 54), the incomplete identification has to mean that the female makes less use of what has to be a smaller store of sublimated libido. Further, because less of the death instinct was freed, there has to be less of it available to be taken up into the super-ego. Woman's super-ego was necessarily less harsh and less opposed to sexuality than man's (Freud, 1924b, p.167). A final consequence of the partial defusion is that more of the remaining alloy of Eros with Thanatos has to be left behind in

the female than in the male. To the extent that it was internally directed, her erotogenic masochism - what Freud saw as the entire basis for feminine masochism - had therefore to be stronger. [3]

The female super-ego could not therefore be other than as Freud described it and woman's contribution to civilization could not be other than minor. So, after the killing of the primal father, when the super-ego formed, Freud could not allow that it was even through that agency that woman might have made one of her few contributions to civilization. Plaiting and weaving, which Freud grudgingly conceded that she "may have invented", sprang directly from genital deficiency; they were techniques based on an unconscious imitation of the way in which, at puberty, her matted pubic hair came to conceal her deficient genitals (Freud, 1933b, p.132).

Civilization was, it must be repeated, a *male* creation - woman had no part in it. Similarly, the remorse, and later the guilt which came to sustain it, was substantially a *male* feeling. What moral standards woman had, what capacity she had to resist instinctual demands, she acquired from the male by "cross-inheritance" (Freud, 1923b, p.37; 1925f, p.258). When, then, in the course of her own development, Freud pictured the little girl as creating yet another incomplete super-ego, he was also picturing her as reaffirming the masculine foundation of civilization, her own relatively trivial later contributions, and the paucity of her moral standards. Fundamentally it is because Freud considers her to be a male, although an incomplete one, that he ends by representing her so abjectly. We see, however, that her supposed inadequacies are much more a result of Freud's postulate that her infantile sexuality is masculine than because of any supposed anatomical deficiency. Even were Freud correct, however, we also see that the mechanisms of identification and sublimation he proposes would be unable to bring about any change in her.

STRUCTURES, AND FUNCTIONS

I think it is worth examining the uncertain status which Freud's structural formulations have been granted by psycho-analytic theorists and clinicians rather than simply summarise my criticisms of them. There are those who accept Freud's 1923 arguments and welcome the structural theory as resolving the ambiguities of the topographic theory. Others are no less enthusiastic in seeing it as a recasting of psycho-analytic concepts in a way that emphasises conflict more and generally fits the data of clinical observation better (e.g. Arlow and Brenner, 1964, pp.43-55; Arlow, 1975).

3. Despite the importance so given erotogenic masochism, its basic weakness as a concept is that it accepts pain as the source rather than the condition of pleasure. Further, its theoretical underpinnings are said to be "strong but dubious" [!] and, although it was Deutsch who proposed it as the basis of feminine masochism, that relation has since been rejected by most writers (Maleson, 1984).

Among those who have reacted in a generally positive way are some who regard the theory as significantly incomplete and either extend it along lines they regard as essentially similar to Freud's (e.g. H. Hartmann, 1939/1958) or argue that concurrent use should or has to be made of some parts or all of both theories (E. Kris, 1950; Eissler, 1962; Fayek, 1980; Sandler, Dare, and Holder, 1972, 1982; Sandler and Sandler, 1983).

Negative responses are to be found alongside the positive and, strangely enough, many of them are based on the opposite contentions. The structural theory is thus criticised for *not* being new, that is, for not really being different from the topographic or as *sharing* the deficiences of the earlier theory (e.g. Gill, 1963). Others say the abstract systems of the structural theory are *too removed* from the realm of clinical experience and its emphasis upon conflict (e.g. G. S. Klein, 1976, pp.7-8 and Ch.4; Schafer, 1976). In line with this, the Lacanists argue that the American housed ego-psychology adaptation of Freud's theory is also too removed from conflict. Others again, including some whose attitude is generally positive, attack it for it conserving too much of the topographic view, thereby perpetuating at least some of the original problems (Arlow, 1975).

Anthropomorphic structures?

Many critics join one or other of these arguments with the point that the structures are not real entities but mere groupings of psychological functions if not unfortunate anthropomorphizations or reifications (Nacht, 1952; Beres, 1958, 1965, and in Marcovitz, 1963; Hayman, 1969; Bieber, 1972; Wiedeman, 1972). H. Hartmann, Kris, and Loewenstein (1946) have clearly not been successful in convincing other psycho-analysts that strict adherence to their functional definition of the structures effectively meets the charge that the structures are mere anthropomorphic metaphors for immediate experience. Pseudo-entities like these, the critics continue to say, should be purged from the theory. Opposing this demand, at least to some extent, are Laplanche and Pontalis (1967/1973, pp.437-438, 452), Grossman and Simon (1969), and Kernberg (1982) who see some virtue, necessity even, in anthropomorphic formulations. Recently Stolorow (1978) has formulated a third point of view. He sees anthropomorphism as inherent in the conceptions of ego, super-ego, and id "as structures" and, while wanting to do away with them even as groups of functions, he grants their usefulness as "a symbolic representation of the tripartite structuration of the subjective experiential world".

Ellman and Moskowitz (1980) have argued that in the psycho-analytic criticisms of the structural theory the terms 'anthropomorphic' and 'reification' tend to be inadequately defined or not defined at all. They show that whether a term is anthropomorphic or not is not a matter of the word itself. For an explanatory statement not to be anthropomorphic, Ellman and Moskowitz say the terms in it "must fit into a theoretical

context that implicitly defines the explanatory terms and independently characterizes them." The real question is whether this can be done.

Realistic structures?

It is as well to begin our considerations of this point by noting that Freud himself never spoke of the structural theory. According to Anna Freud, it was Ernst Kris who coined the phrase 'structural theory' and it may have been through his use that others adopted it (Nagera, 1967). Freud hardly ever even used the term "structure", usually speaking of the components of the "psychical apparatus" as "systems" or "agencies" or, less frequently, as "organisations", "formations", or "provinces".

My first point is that Freud's statements about the systems or agencies show him to have taken a realist stance - he clearly believed the structures to have a real existence. Thus, in introducing the 1923 'ego', he had said:

> We have formed the idea that in each individual there is a coherent *organization* of mental processes; and we call this his ego. (Freud, 1923b, p.17. Emphasis altered, MBM)

As an organisation, this ego was more than a set of functions. It was:

> the mental *agency* which supervises all its own constituent processes. (ibid. My emphasis, MBM)

And of the term 'id':

> I propose ... calling the *entity* which starts out from the system *Pcpt.* and begins by being *Pcs.* the 'ego', and ... calling the other part of the mind, into which this *entity* extends and behaves as though it were *Ucs.*, the 'id'. (op. cit., p.23. My emphasis on entity, MBM)

Later he expressed the hope that the readers of his *New Introductory Lectures*:

> will by now feel that in postulating the existence of a super-ego I have been describing a genuine *structural entity*, and have not been merely *personifying an abstraction*, such as conscience. (Freud, 1933c, p.92. My emphasis, MBM) [4]

Ego, super-ego, and id, then, were organisations or entities as real as the systems *Pcpt.*, *Cs.*, *Pcs.*, and *Ucs.*

4. Because it brings out Freud's thought so accurately, I have here quoted from the Sprott translation (Cf. Wilson, 1973; Beres, 1965). The original German is "Ich hoffe, Sie haben bereits den Eindruck empfangen, dass die Aufstellung des Über-Ichs wirklich ein Strukturverhätnis beschreibt und nicht einfach eine Abstraktion wie die des Gewissens personifiziert" (Freud, 1933a, p.71. Cf. Freud, 1933b, p.64). In the *Standard Edition*, *Aufstellung* is rendered as 'hypothesis' rather than as 'postulating the existence', and *Strukturverhätnis* as 'structural relation' rather than 'genuine structural entity'. While the accuracy of the latter translation is debatable, there is no doubt that, albeit inadvertently, Strachey provided positivist fuel for the functional fire.

My second point is that Freud's realism has two consequences. On the one hand, by itself and without any further evaluation, it completely undermines the argument, prosecuted most vigorously by the ego-psychologists, that the structures should be defined by their functions. On the other, it provides the basis for circumventing the trivially silly criticism that the structures Freud describes are mere reifications or anthropomorphizations.

Structures as functions?

It seems to have been the ego-psychologists in Hartmann and his collaborators who first adopted the non-realist criterion of defining "psychic systems" like the ego, the super-ego, and the id "by the functions attributed to them" (H. Hartmann, Kris, and Loewenstein, 1946; H. Hartmann and Loewenstein, 1962. Cf. H. Hartmann, 1939/1958). When they did this, H. Hartmann, Kris, and Loewenstein (1946) explicitly attributed the definition to Freud himself. I think it significant they did not produce any statement of Freud's in support. Neither in my reading of his works nor in the extensive psycho-analytic literature on psychic structures have I been able to find a single remark of Freud's that would remotely class him among those who define structures by their functions. Despite its non-Freudian origins, psycho-analysts have widely endorsed it (Rapaport, 1951, 1959; Beres, 1958, 1965; Rapaport and Gill, 1959; Gill, 1963; Marcovitz in Marcovitz, 1963; Arlow and Brenner, 1964; Hammerman, 1965; Loewenstein, 1966b; Moore and Fine, 1967, cited in Wilson, 1973; Hayman, 1969; Arlow, 1975).

With what I shall call this functional version of Freud's structural theory there is a number of problems. The criterion for assigning functions to 'structures' is probabilistic, making "it difficult to maintain that explicit definition is involved", and the rules relating the structures to clinical data are "quite imprecise and loosely stated" (Wilson, 1973). Possibly this is why even the same functions are sometimes defined differently by different authors, as for example, in the differences between Nacht and Hartmann about whether there are any ego functions that are not concerned with drive satisfaction (Nacht, 1952). Nor is what is to count as a function, and therefore as a structure, agreed upon. Rapaport (1959), for example, has affects and delayed discharge as structures - a proposition that Wiedeman (cited in Abrams, 1971b; Wiedeman, 1972) and Beres (1965) quite properly reject as self-contradictory or meaningless.

The functional theory has also been criticised from within by Beres (1958, 1965. Cf. Schwartz, 1981). According to him, those adopting the functional version have not been radical enough. Defining structures by a relative stability of functions (H. Hartmann, 1964, p.xii), or by abiding patterns in the flux of processes (Rapaport and Gill, 1959), or by recurrent patterns of functions, especially regulatory ones (Gill, 1963, p.113), is to

define them too rigidly and inflexibly (Beres, 1965). On Beres view, even ideas and memories cannot be the *contents* of structures. Instead, they have to be *products* of the functions.

The nub of the problem lies in the functionalist position itself. If functions are to be the criteria for defining structures, what are the rules that restrict them multiplying beyond necessity, that prevent arbitrariness in the ways in which they are grouped, and that guide their allocation to the 'correct' structures? Not only are there no guiding principles, but the functionalist view has been interpreted to mean that one invents as many functions as are necessary to produce the behaviour to be explained (Wilson, 1973, citing Fodor). It is the lack of constraint on this inventive fertility that causes the significant differences in the different functional definitions of the three structures of ego, super-ego, and id. And they are irreconcilable.

It is all too easy to show there is no agreement about the functions supposedly defining any of the structures. For example, the often expressed need for re-evaluating the theory of the super-ego (e.g. Stein, cited in Goodman, 1965) has been attributed to an overall problem of:

> analysts [finding] it difficult *to distinguish conceptually between functions of the superego and those of ego or id* when they attempted to apply the theory to clinical situations. (Loewenstein, 1966b)

Loewenstein went on to raise issues such as whether anxiety, anticipation, and self-observation were solely ego functions or were in some measure shared with the super-ego. Sandler (1960) spoke of an "apparent 'conceptual dissolution' of the super-ego" in the face of this reassignment of functions and, from the other end, so to speak, Modell (1975) noted a growing tendency to attribute id functions to the ego.

It has not been possible to arrive at a functional definition of any of the three structures. Beres (1958) found there was "no conclusive list" of functions defining the super-ego and "many difficulties" in assigning specific functions to it. The id has fared even worse. On the grounds that many ego tendencies were wrongly ascribed to it and that it drew attention away from the repressed unconscious, Van der Waals (1952) questioned "whether the introduction of the concept of the id has been profitable in every respect". As compared with the other structures:

> The concept of the id has been perhaps the most consistently misused, misunderstood, and criticized of Freud's postulates. (V. H. Rosen, 1968)

Examining the three main works devoted to the id show the misunderstandings result from basic ambiguities about its functions. Is it concerned only with energy pressing for immediate discharge, or does it 'contain' real structures of primitive perceptions, memory traces, and repressed ideas? (Brierley, 1951 cited by Hayman, 1969; Schur, 1966a). Does the id

express instinctual drives as *needs* or does it press for the gratification of *wishes* stimulated by drives? (Beres v. Schur in Marcovitz, 1963). Even after Schur's (1966a) massive re-evaluation of the id, Hayman (1969) found it impossible to make a simple list of its functions. Central to her difficulty was the fact that id and ego are defined in terms of each other. Lack of specification of the functions of the one is necessarily associated with indeterminacy about the other.

The same points hold for one of the most resolutely functional approaches, the ego-psychology created by Hartmann and his collaborators. H. Hartmann (1939/1958) argued that the ego had its sources in processes other than conflicts over drive satisfaction and that the impact of a harsh reality upon the instinctual drives was only one of the bases of ego formation. In fact, he believed it was not possible to derive the ego from the conflict between drives and reality. Subsequently, he and his colleagues (H. Hartmann, 1947; H. Hartmann, Kris, and Loewenstein, 1946; H. Hartmann and Loewenstein, 1962; Loewenstein, 1966a) proposed that the ego differentiated itself from an original undifferentiated ego-id matrix and that functions such as perception, thinking, memory, motor development, and the comprehension of objects - what they term primary autonomous ego functions - developed outside of the sphere of conflict. These functions were thus autonomous and independent of drive satisfaction. They were fuelled by a neutral and primary "mode of energy different from that of the drives" available from the very beginning of life. Instinctual needs thus activated but did not create "the apparatus serving perception, motility, and others that underlie ego functions" (H. Hartmann, 1952). Ego-psychology has always drawn some fire on the very grounds of whether these so-called autonomous, conflict-free ego functions exist or not and in what ways they differ from the functions subserving conflict. In short, ego-psychology is criticised for difficulties in the very matter it was partly designed to overcome: that of assigning functions to structures.

Structures as structures?

The simple fact is that structures cannot be defined by their functions. Structures are different in nature from functions. Structures are combinations of mutually connected and interdependent parts, elements, or components making up some whole. Functions are the activities proper to the structure or by which it fulfils its purpose (Rosenblatt and Thickstun, 1977; McIntosh, 1986). Structures thus *have* functions but they are clearly not the functions themselves. That is:

> the structure of a thing determines its functions and, hence, the structural definition takes primacy over the functional definition. Without structure, function is impossible. (Berrien, 1968, cited in Wiedeman, 1972)

Wiedeman puts it even more sharply: "Structure defined by its functions ... is a contradiction in terms" (Wiedeman, 1972. Cf. Eissler, 1962; Nagera, 1967). The contradiction is well-illustrated in the misleading

analogy Schwartz (1981) drew between a telephone and functional definition. Schwartz represented the communication transmission *functions* of the telephone as its *structure* rather than seeing that those functions are the result of the way the component structures of the telephone - such as the dialler chips, the electret condenser microphones, and ceramic sounders - work, operate, or function.

In the case of physical or organic structures these elementary definitional points are almost self-evident. But they apply equally to other realms, including the mental. There we note it is not at all necessary for adequate definitions of psychological structures to have or include physical referents. One may speak as readily of simple mental structures, such as memories or ideas, or of complex ones like ability, conscience, or self without referring at all to either physical or organic elements or substrates (Cf. Grossman and Simon, 1969; Schwartz, 1981).

If the essence of a structure is that it is a structure of something, then in psycho-analysis the prototype of a psychic structure is a mental representation. From my discussion of theoretical terms in Chapter 6, it follows that the real question is how to characterise the structures of psychological theories. What are the components out of which the structures of the structural theory are built and how do those structures give rise to the functions assigned them? The question is worth an answer. Freud was a realist in his theorising and is done no service at all by those of his followers who have tried to turn him into a player of positivist functional pursuits.

Rapaport and Gill (1959) proposed the most widely adopted answer. For them structures were "configurations of a slow rate of change" but, apart from adding that they were "abiding patterns in the flux of processes" from which the structures were inferred, they did not mention what the configuration consisted of. W. C. Lewis (1965) endorsed Rapaport's innate discharge-regulating thresholds as the basis of psychic structures at the same time as defining a structure as "an ordered arrangement of elements, which may be perceptions, thoughts, reactions, et cetera of sufficient stability to give predictability". There is a number of problems with Rapaport's energic definitions of structures as Schwartz (1981) has pointed out. 'Structure as process' leads to the ambiguity of the same concept being a structure controlling a process as well as process itself. For example, defence is an ego process as well as a structure controlling drive discharge. 'Structure as energy', involves the use of a quite explicit and unacceptable analogy of psychic energy with physical energy. 'Structure as internalisation of environmental contingencies' leaves external events having no connection with the physical structure of the organism. 'Structures as operations' lumps together elements that are too disparate, such as memories, which may be specific, and operations as general and abstract as cognitive skills.

Structures as memory traces?

What Freud had meant by a structure in the *Project* was clear: the ego, for example, was a group of well-cathected neurones. The difference between the ego as that structure and its functions was no less clear: functions resulted from the movement of quantities of excitation within the neuronal network. Ego functions like the perception of stimuli, making decisions about their origin, delaying motor responses to them, laying down memories of them, and preventing unpleasure from their attempted revival result from the disposal of cathexes stored within the ego as a structure. After abandoning the pseudo-physiological elements, Freud could have retained a realist meaning for the structures by rethinking the problem in psychological terms. But he did not do this. The result was that the relations between structure and function became implicit. Although it was clear that the structures of the structural theory were more than a group of functions, it was not at all clear what they consisted of.

At various earlier places I have argued that the implicit elements of many important structures in Freud's theory are memory traces. Entirely in accord with Freud's mode of thought Glover has also noted:

> The moment two sets of memory traces concerning experiences of gratification and frustration are linked (associated or merged) we have the makings of an ego nucleus. (Glover, 1961)

Similarly, Freud's account of the development of the reality-ego from the pleasure-ego through the incorporation of objects or their projection from it requires the formation or reorganisation of the memory traces of objects with those comprising the ego (Chapter 11). Super-ego formation as we have described it in this Chapter really only makes sense if thought of in the same way: primary identification reorganises the existing ego-traces to match the model of the person the child wishes to be like and Oedipal identification links up existing traces in new ways. Despite Beres (1965) reservations, there is no doubt that Glover was being perfectly consistent with Freud's own thinking when he identified groups of memory traces as the elements of structures (Glover, 1943, 1947).

Of course, mounting a rescue operation for psycho-analytic theory forms no part of my intention. Psycho-analysts interested in structures must invest in that effort themselves. But should they try, they will almost certainly end in a *cul-de-sac* as barren as that in which we left the functionalists. Suppose they decided it was sensible to think of structures as being composed of something like memory traces. They will then have to face the fact that memory structures are just as inert as were Freud's neuronal structures. What explicit *psychological* meaning can be given to the quantities of excitation that once enlivened the neuronal net? Psychic energy? Yet, of all Freud's concepts, that of psychic energy must be about the most severely criticised. No substitute for it has been proposed and without it a trace-type structural theory is doomed.

CONCLUSION

Whether or not one agrees that the final version of Freud's personality theory is the consequence of the introduction of the concept of instinctual drive, one has to agree that there are grave deficiencies in the components of the theory as Freud formulated them and as others have tried to correct them. First there is the totally unsatisfactory nature of Freud's concept of instinctual drive. Its inadequacies are already evident in the sexual instinctual drive, especially in its pseudo-physiological referents and supposed infantile components, and becomes more striking in the ludicrous propositions about the ego-drive, particularly as an opponent of sexuality and a source of hatred. The death instinct, both in itself and as a source of aggression, simply carries the original inadequacies, contradictions, and lack of characterisation to a higher stage. Second, there are the completely empty theoretical structures of id, ego, and super-ego. Even were it possible to establish other than in the broadest and most metaphorical manner what functions these structures perform, it is absolutely impossible find an intelligible account of how any of them function. Freud is also unable to give even a moderately coherent account of how the ego emerges from the id, how its functions are acquired, how it exercises those functions, especially how it regulates the dangerous id drives. Freud's description of the formation of the super-ego is, at best, the description of a process which gives the wrong properties to it. Third, to the extent that mechanisms like identification, repression, and sublimation are central to the formation and function of ego and super-ego we find Freud unable to explain the relation between primal repression and repression proper, the relation between anxiety, both the traumatic and the signal kind, and either kind of repression, or how the ego and super-ego acquire their stocks of neutral and destructive energy respectively.

If we ask what can be done to remedy this parlous situation we are either fobbed of with reassurances that psycho-analysis is as yet too young to have developed a fully systematic account of its discoveries or we are told that what is needed is some new kind of framework within which those discoveries can be reinterpreted. We are never told that the so-called discoveries are dependent upon methods of inquiry and interpretation so defective that even practitioners trained in their use are unable to reach vaguely congruent conclusions about such things as the interpretation of a dream or a symptom let alone the basic clinical characteristics of infantile or perverse sexuality or the reconstruction of the early stages of an individual's development or on the functions that make up a given structure.

While the basic methodological deficiencies remain, it will not matter how great an effort is made or what perspective is adopted, there is no way in which either Freud's original form of psycho-analysis or any modern derivative will ever lead to a satisfactory personality theory. This is, of course, the quite explicit message of Schafer's proposal for the use of

action language: after nearly one hundred years of psycho-analysis, psycho-analysts must begin all over again. They have to clarify what it is that requires explanation. Because psycho-analysts see only the need for a reformulation of the theory, the methodological point has been missed altogether by those who want a metapsychology something like that which Freud proposed; they tamper with the structures or alter the nature and status of the drives but their own concepts of drive and structure are inferred from facts gathered by a defective method. My point also holds for those like G. S. Klein and Holt who think it possible to save what they refer to as the clinical theory at the expense of the metapsychology. The psychoanalytic method is not capable of discovering clinical facts that can be agreed on.

I believe the conclusion I have just drawn is sufficiently documented from within psycho-analysis, so to speak. But it is, as we shall see, also confirmed by those investigations made outside of that particular clinical setting. This additional evidence will be evaluated in Chapters 14, 15, and 16, which constitute Part IV.

PART IV

EVALUATION

Chapters 14 to 16

14 A Theory? A Therapy?

15 A Method?

16 A Science?

14 PSYCHO-ANALYSIS AS THEORY AND THERAPY

Rosencrantz: Why don't you go and have a look?
Guildenstern: Pragmatism? Is that all you have to offer? You seem to have no conception of where we stand.

Stoppard: *Rosencrantz and Guildenstern are Dead.*

Part IV consists of Chapters 14, 15, and 16. In it I set out an evaluation of psycho-analysis which follows on from the previous discussions. It is organised around the three components into which psycho-analysis is conventionally dismembered and for which there is some warrant in Freud's own writing: a theory of personality, a method of investigation, and a type of psychotherapy (Freud, 1913b, pp.207, 210; 1924c, pp.200, 205; 1925a, p.30; 1926b, pp.248, 252-253; 1927a, p.36; 1933b, pp.156-157). Chapters 14 and 15 deal with the three components. In Chapter 16, which brings *Freud Evaluated* to a conclusion, I consider the senses in which psycho-analysis is a science.

Chapter 14 is devoted to evaluating psycho-analysis as a personality theory and as a type of psychotherapy and I begin with a discussion of the basis used for the evaluation. I argue that whatever kind of theory one construes psycho-analysis to be - scientific, humanist, clinical, metapsychological, or hermeneutic, - the explanations it generates must be deductive in form and adequately logical. This standard is then used to examine what I take to be the most central concepts of Freud's theory of personality. The most abstract (e.g. excitation, stimulus barrier, narcissism, mental structures) are considered before the lower-level personality characteristics. Some new criticisms are advanced alongside the briefer repetitions of some of those made in earlier chapters. Where it is appropriate, data is cited relevant to the validity of the concepts. I conclude the chapter with an assessment of the effectiveness of psycho-analysis as a therapy and an evaluation of the bearing of that effectiveness on its validity as a personality theory.

THE PERSONALITY THEORY

The core of Freud's theory of personality is about the transformation of an original inherited endowment - the id - into a set of mental structures which regulate the individual's relations with the surrounding world. Primary process gives way to secondary and ego and super-ego form out of the id, defence mechanisms begin operating and character traits are laid down. This conglomerate of structures and processes constitutes the personality.

Personality results from a complex process in which the infant's original polymorphous sexual disposition is modified by having some of its libidinal aims changed and its auto-erotic tendencies replaced by hetero-sexual object-love. Non-reproductive aims are brought under the sway of genital activities and object-choices that do not lead in the direction of non-incestuous heterosexuality are abandoned. For the most part, personality development follows biologically determined pathways in which repression is the main agent of change.

The ego and super-ego develop from the id to play important roles in the two different kinds of repression. Some early traumas produce enough anxiety to bring about repression automatically. Usually this is when the ego is weak and before the super-ego has developed. At other times, usually later, a threatened re-activation of the memory traces of an earlier, even archaic, trauma or a threat to the values and standards of the individual causes anxiety to be experienced by the ego. Repression as an after-pressure is then initiated at the behest of the super-ego.

Differences in personality result from variation in this complex development. A normal outcome may come about despite peculiarities in the constitution and the effects of repression. Failures to complete some part of the process cause the development of fixations on particular libidinal aims or objects and on modes of ego and super-ego functioning. Depending on their strength, libidinal fixations result directly in perversions or in character traits. Fixated modes of libidinal satisfaction which are carried over into normal adult life as character traits are prolongations-continuations, sublimations, and reaction formations which formed during psychosexual development. Neuroses and psychoses result from later regressions to these fixation points when earlier libidinal aims and modes of satisfaction are revived together with more primitive functioning of the mental structures.

The standard I adopt in evaluating Freud's personality theory is whether or not it allows for the deductive explanation of the facts with which it is concerned. As I argued in Chapter 7, for an explanation to be genuine, it must be possible to deduce the facts to be explained from the propositions of the theory.

THE BASIS OF EVALUATION

Insisting that adequate explanations are deductive is not to make a demand unique to scientific explanations. Explanations in all domains are governed by it. Thus when I say that a particular narrative or case-history provides a genuine explanation, I am saying that *if* the factors in the case or in the narrative were really as described, *then* the behaviour to be explained can be deduced logically from them. Again, when I say that I

can explain someone's behaviour because of the empathic understanding I have of it, I am saying that *if* I were in that person's situation, with his or her particular complement of feelings and experiences, *then* it would be logical for me to do the same, whether I was conscious of those feelings at the time or not. It is the *if-then* structure of these explanations which marks them as deductive. That structure requires they be evaluated in the same way as explanations which begin with the *if* assumptions of scientific theory and are followed by a *then* deduction of the facts to be explained. Naturally the point holds for explanations deriving from theories of high as well as low levels of abstraction. Consequently, my evaluation will not be restricted to any particular type of explanation or level of theorising.

It is necessary to stress this elementary point because the past 15 to 20 years has seen a number of attempts by psycho-analysts to meet the demand, made for the most part from within psycho-analysis, to jettison Freud's theoretical assumptions and replace them with others. Two kinds of arguments can be distinguished. One is based on what is claimed to be a scientific standpoint. In it, concepts like the instinctual drives or the structures on the mental apparatus are criticised for not being in line with contemporary knowledge of the physiology or psychology of drives or with current thinking about executive and controlling processes in the mind (e.g. Holt, 1967/1968, 1975a). Depending on the critic, physical field theory, modern information processing concepts, systems theory, or real contemporary neurophysiology, or a purely psychological theory of drives are suggested as substitutes (e.g. Pumpian-Mindlin, 1958-1959; Peterfreund, 1975; Rosenblatt and Thickstun, 1977; Reiser, summarised by Wallerstein, 1985; G. S. Klein, 1976). Basically this line of argument accepts Freud's scientific aims but says they have to be realised with different theoretical concepts. The second argument pretty well dismisses Freud's theorising altogether. According to it, psycho-analysis is either not a science at all or, if it is one, it is not a science in the mold of the natural sciences. An implication is that the usual standards for evaluating theoretical explanations do not or should not apply.

Cutting across these two positions, although overlapping with them to some extent, is a set of arguments about the level and function of Freud's theoretical statements. Because theoretical statements can often be arranged hierarchically, with the most abstract at the top and the most concrete at the bottom, it is argued that the standards or techniques of evaluation should differ according to level. Some also argue that explanations couched in the higher level terms, like psychic energy and cathexes for example, are too remote from what is experienced in the clinical or treatment situation to be useful. What these critics say is that psycho-analysis needs primarily or solely a *clinical* theory (e.g. G. S. Klein, 1976) Some critics taking this line also imply that the standards for evaluating a new theory of this type have to be different from those used previously. Although both the level and function arguments may be put independently of the argument about the correctness of the higher level propositions,

those making them tend to argue that almost all of Freud's theoretical statements are faulty as well as irrelevant (e.g. Schafer, 1976).

Basic to these two kinds of criticisms is a confusing set of arguments about what is known as Freud's *metapsychology*. Unfortunately there is so much variation in the way this term is used, I have to clarify it before estimating the relevance to my own evaluation of the arguments about metapsychology and the criticisms based on it.

PSYCHO-ANALYTIC METAPSYCHOLOGY

Freud gave his only formal definition of metapsychology in his 1915 paper on *The unconscious*. There, after outlining how ideas were unconscious, either in the dynamic sense of being opposed by other forces or in the topographic sense of belonging to the system *Ucs.*, he went on to discuss unconsciousness in terms of the withdrawal of cathexes of energy and the imposition of anticathexes. So explaining unconsciousness meant, he said, that he had adopted an economic point of view in addition to the dynamic and topographic. Consequently:

> I propose that when we have succeeded in describing a psychical process in its dynamic, topographical and economic aspects, we should speak of it as a *metapsychological* presentation. (Freud, 1915c, p.181)

A metapsychological description was therefore a comprehensive one which drew on three kinds of psychological concepts: forces, systems, and energy respectively. In *A metapsychological supplement to the theory of dreams* written at the same time as *The unconscious* but not published until two years later, Freud gave precisely such a comprehensive, three-fold description of dreams (Freud, 1917a).

Three viewpoints or five?

Freud himself recognised only the three metapsychological viewpoints of the dynamic, topographic (or systematic), and economic. By the early 1940's Glover had distinguished an 'adaptive' and a 'genetic' viewpoint (Glover, 1943) and by the late 1950's, mainly through the work of Rapaport and Gill, these two additional perspectives had been accepted by a number of psycho-analysts. Not everyone approved of the extension, as Rapaport and Gill found when they began their attempt at systematising psycho-analytic theory. They had themselves been doubtful about the genetic perspective and reported that the least widely accepted of the five was the adaptive; ''almost every reader'' of the early drafts of their paper questioned its inclusion (Rapaport and Gill, 1959). Years later, Warme continued to question the metapsychological status of both the genetic and adaptive positions (Warme, 1981).

Reservations about a genetic or adaptive metapsychology are well-founded. Although it is undoubtedly the case that all clinical practitioners

- not only psycho-analysts - find it important to understand the adaptive function of behaviour and the way it developed, neither the genetic nor adaptive perspectives are based on specific assumptions from which adaptive or genetic explanations can be derived. Rather, what exists are sets of principles against which the developmental or adaptive adequacy of the behaviour can be judged. Consequently, descriptions based on the genetic and adaptive perspectives are different in kind from the three which Freud proposed and explanations cannot be generated from them. I shall therefore disregard them.

What is beyond or meta to psychology

Freud clearly meant 'metapsychology' to refer to something that went beyond the psychology of consciousness (Masson, 1985. Letter of 10.3.1898). The problem is to know what that something else is. In the correspondence with Fliess, Freud had several times used the term 'meta-psychology' explicitly and many of his references to 'psychology' are also really to it. One set of references is in the letters between 1895 and 1896, the other in those between 1898 and 1899. While many of the remarks are impossible to interpret, there is no doubt that the overall context in which the first set occurs is that of Freud's difficulties with the *Project* and that of the second is the completion of what became the theoretical chapter of *The Interpretation of Dreams*. In both contexts, most of Freud's pre-1915 references or allusions are to explanations of conscious psychological phenomena through his economic, pseudo-physiological concepts or via psychologically transformed versions of them (e.g. Masson, 1985. Letters of 28.3.1895, 27.4.1985, 25.5.1895, 16.8.1985, 23.9.1895, 13.2.1896, 2.4.1896, 4.6.1896, 17.12.1896, 9.2.1898, 23.2.1898, 10.3.1898, 26.8.1898, 31.8.1898, 22.7.1899). There is only one post-1915 paper in which Freud's use gives clear meaning to the term metapsychology. In it Freud classed quantitative variation in the strength of the instinctual drives as part of the economic approach (Freud, 1937a, pp.224-227, 234).

Psycho-analysts disagree about the meaning of 'meta' in 'metap-sychology'. It does not seem to refer to phenomena that are simply not conscious (H. Hartmann, 1959; Brenner, 1980). And, even allowing that Freud never really abandoned his pseudo-physiology or his attempts to develop explanations consistent with it, Freud's formal definition clearly rules out Pribram's and Gill's opinion that neurophysiology lay behind psychology (Pribram and Gill, 1976; Gill, 1977). But it is equally obvious that some metapsychological explanations of psychological phenomena could be based on purely psychological forces. Thus, as Rapaport and Gill observe, a dynamic description or explanation may use propositions about drives, ego-interests, and conflict, for example, "without recourse to an organic substrate" (Rapaport and Gill, 1959).

Neither can metapsychology be restricted to the assumptions on which the metapsychological viewpoints depend, as the widely accepted definition

of Rapaport and Gill (1959) has it. Assumptions obviously underpin each of the viewpoints, but are themselves only of interest to the extent that valid descriptions or explanations can be generated from them. Nor can metapsychology be broadly defined as psycho-analytic theory in general or, as Brenner does, as "psycho-analytic psychology as a whole" (Brenner, 1980; Reppen, 1982. Cf. Holt in Chattah, 1983). A metapsychological explanation or description is a comprehensive statement from three different viewpoints each of which is based on a set of slightly different assumptions. For psycho-analysis, there can no more be a single metapsychology than a single theoretical explanation or description. Consequently, while Gill is correct to describe the broad definitions as the least defensible, one must also reject those narrower definitions, like his own, which has it that metapsychology is the theory which explains the clinical theory (Gill, 1987), or as the theory that "explains psychology in another universe of meaning than that of a person's aims", that is, biologically, neurophysiologically, or in terms of forces (Reppen, 1982).

Metapsychology and the levels of a theory

The most persistent and pernicious definition of metapsychology is that of Waelder who placed it at the highest level of abstraction in an hierarchy of theoretical statements. Brenner noted that Waelder's placement implied metapsychological propositions were the most speculative, the least useful, and the most difficult to prove, if provable at all. Brenner also pointed out that many of the criticisms of metapsychology made by Gill and Holzman were of the same kind (Brenner, 1980. Cf. Waelder, 1962; Gill and Holzman, 1976). All three of Freud's metapsychological viewpoints are sufficiently abstract and 'high' enough in level to require complex deductive structures to link them to observations. In this respect there is little difference between a psychological force, an ego, or a quota of psychic energy. However, because the assumptions underlying each of the metapsychological points of view are different, the adequacy and validity of each requires its own evaluation. While these differences do cause some difficulties, they are not evaded by defining metapsychology functionally, that is, as what metapsychology does rather than what it is (Modell, 1981). Three different viewpoints generate three different descriptions or explanations and necessarily require three different evaluations.

Levels and singularity

The error that metapsychology is singular, is sometimes confounded with the error that it is the highest level of abstraction. Meissner's (1981b) placement of metapsychology at the highest of the six levels of generalisation and conceptualisation he distinguishes provides an illustration of this confounding. Meissner's levels are:

1. Empirical observation. The analyst here is a participant observer who uses empathy.

2. Empirical generalisation. Regularities in the behaviour of the single patient are established.
3. Clinical interpretation. Motivated and meaningful connections are postulated to account for the empirical generalisations.
4. Clinical generalisation. The motives of the single patient are extended to groups of patients or phenomena.
5. Clinical theorising. Explanations of the regularities of behaviour are made in terms of mechanisms and processes like conflict, repression, and developmental vicissitudes.
6. Metapsychology or the theory of psycho-analysis. This level includes a method, attitudes (!), and a set of resources for specifying relevant data, for establishing generalisations, for providing means of testing, for explaining, understanding, and predicting, and for concept elaboration.

Meissner's levels are unremarkable in that they or something like them can be distinguished in the psychological sciences generally. But how misleading it is to characterise metapsychology by a level can be seen by considering his Levels 2 to 4. Only if generalisations made at them are purely descriptive can metapsychology be avoided. To go beyond them involves metapsychology. Consider Level 3, for example. How are motivated and meaningful connections to be established without drawing on concepts like the force of a motive or on the psychological processes that make connections meaningful? And, how do 'conflict' and 'repression' at Level 5 avoid being *dynamic* metapsychological concepts?

What makes a theory useful and dependable is not how abstract or concrete it is but how well it is supported by its data (Brenner, 1980). Tests of some psycho-analytic propositions, like those about behavioural regularity and generality located at Meissner's levels 1 to 4, for example, might not need to draw on metapsychology at all. In line with these fairly obvious points, my evaluation will not be restricted to theoretical statements at any one level or to any particular metapsychological perspective. Although my main criticisms will be of the economic and structural viewpoints, it will also include material relevant to Levels 4 and 5.

WHERE PERSONALITY STARTS

What is the point from which Freud postulated that personality began? Here we are faced with the concept of the stimulus barrier and with Freud's confusions over narcissism and auto-erotism.

The stimulus barrier

There is a number of self-evident reasons for not being too critical of Freud's picture of the primitive organism surrounded by lethal energies,

but that immunity cannot be extended to the concept of the protective shield or stimulus barrier, which has, or is supposed to have, important practical consequences. The objections are conceptual and evidential as well as practical.

Esman (1983) refers to the confusion surrounding the concept, a characteristic he derives from Freud's merging of the mental and physical levels of discourse (Cf. Peterfreund, 1978). Furst notes that most psycho-analysts see the shield in psychological terms, as "a complex *ego* function" (Furst, 1978. My emphasis, MBM). That formulation is not accepted by all (e.g. Peterfreund, 1978), and is, in any case, certainly not how Freud envisaged it. Esman (1983) also thought it was "unclear" whether Freud thought the shield was active or passive. To make it active, it had to have its own source of energy and Freud could not provide it other than through some libido sublimated very much later in the individual's development. Possibly it was this difficulty which led him to his holding simultaneously passive and active conceptualisations.

A number of psycho-analysts has also concluded that the evidence is against the compatibility of the shield with contemporary knowledge of the nervous system, even though it obviously partly derives from Freud's neurophysiological ideas (Benjamin, 1965; Tennes, Emde, Kisley, and Metcalf, 1972; Esman, 1983). Actually, the shield was never consistent with any real nervous system. Both the stimulus-seeking of the neonate (Tennes, et al., 1972; D. Stern, 1977; Spielman, 1986; Furst, 1978) and spontaneous activity in the nervous system and its receptors (Benjamin, 1965) speak very loudly against the tendency to quiescence encapsulated in the notion of the shield. Nor have those longitudinal studies beginning at birth and conducted for the most part within a psycho-analytical framework provided any evidence for it (e.g. Benjamin, 1965; Wolff, 1966; Tennes, et. al., 1972; D. Stern, 1977; Lichtenberg, 1981).

Psycho-analytically oriented studies of the infant's post-natal development show it to be tri-phasic. First there is an initial passive insensitivity resulting from lack of functional connections in the nervous system. A second phase follows in which there is marked sensitivity consequent upon a maturational spurt. In the third phase the infant is able to regulate stimulus input actively. Within this sequence, Freud's stimulus barrier can only be saved if it is thought of as part of the *mother's* protective behaviour in the second or sensitive phase (Esman, 1983). If this interpretation of the evidence is consistent with the concept of a stimulus barrier at all, it makes for a barrier very different from the one Freud envisaged (Wolff, in Schafer, 1965). The direct studies have been no more successful in confirming the existence of a protective shield than were Greenacre's earlier psycho-analytic reconstructions of the experience of trauma in childhood (Greenacre, 1941, 1945).

Finally, the practical value of Freud's concept of a protective shield has been impugned directly, for example by Greenacre (1967), Neubauer

(1967) and Krystal (1978), and indirectly in the many discussions of trauma in which, if the concept is mentioned at all, it is to provide a background rather than a basis for explanations (e.g. Greenacre, 1941, 1945; Rangell, 1967; Sandler, 1967; Solnit and Kris, 1967). The point holds even for those who place the concept more central (A. Freud, 1967; Furst, 1967, 1978; Waelder, 1967a). Nor do any of the writers I have mentioned discuss the role of the anticathexis, despite its importance in the barrier-based theory of trauma. Where solutions have been proposed, they are often peculiar. For example, Ikonen and Rechardt (1978) provide the shield with energy and anticathexis deriving from Thanatos. They also represent the excitation that floods in through the stimulus barrier during a traumatic situation as narcissistic libido detached from its object!

Peterfreund (1978) had wondered "why the concept should be retained at all" and, after showing it was inconsistent with developmental data, Esman (1983) concluded it was "no longer tenable". Where the concept has been retained, it refers to simple individual differences in thresholds for tolerating stimulation and tension (e.g. A. Freud, 1967), or it is used as a mere metaphor, a kind of experiential equivalent of "indifference and 'thick-skinnedness'" (Ikonen and Rechardt, 1978). A far cry indeed from the grandiose evolutionary significance Freud gave it.

Narcissism

Etchegoyen has recently stressed that the basis of much of the confusion about auto-erotism and narcissism I considered in Chapters 10 and 11 respectively, lies in the consistency with which Freud adhered to the notion that auto-erotism was the first stage of development and primary narcissism the second while simultaneously asserting that sexual impulses remained auto-erotic when they were discharged through objects (Etchegoyen, 1985).

The term 'narcissism' was introduced into psycho-analysis by Sadger (Nunberg and Federn, 1962-1975, *II*, p.312) and we have seen how Freud first used it publicly in the 1910 alterations and additions to the *Three Essays*. For both Freud and Sadger the word meant 'love of self' and Freud used it technically to describe a homosexual identification although, even then, Freud foreshadowed his notion of a narcissistic developmental stage located between auto-erotism and object-love.

In Chapter 11 I set out the inconsistencies in Freud's formulations and cited psycho-analytic evaluations that supported my own. Smith has recently drawn similar conclusions. He notes, for example, that Freud put forward "three distinct and mutually exclusive" models for the development from narcissism to object-love (D. L. Smith, 1985. Cf. Teicholz, 1978). Just as I did, Smith describes the motive of the first as economic - anxiety caused by the build up of internal tension - and goes on to make four crucial points about the model. Of them, the most important is that narcissism was never properly integrated with the theses about the psycho-

sexual stages of development. Smith also notes that Freud does not really succeed in explaining the transformation of narcissism into object-love, that *only* in the Schreber analysis did he posit homosexual object-choice as a stage between narcissism and heterosexual choice, and, lastly, that he could not adopt Ferenczi's account of omnipotence without abandoning his own anal-fixation/regression explanation (D. L. Smith, 1985).

Together with Freud's failure to relate narcissism adequately to aggression in his instinct theories, the fact that he never "explicitly repudiated" the internal tension explanation is responsible, as Smith says, for almost all of the confusion about narcissism so many writers have observed in the psycho-analytic literature (D. L. Smith, 1985. Cf. Pulver, 1970; B. E. Moore, 1975; Teicholz, 1978; Meissner, 1981a). In my view it is precisely the limitations, confusions, and internal contradictions of Freud's theses about narcissism which are also responsible for the evident confusions in the current psycho-analytic literature about psycho-sexual development and the formation of the ego-ideal and the super-ego (Teicholz, 1978; Tyson and Tyson, 1984). What is especially notable about that literature is how many contributors to it treat the mutually exclusive notions as non-contradictory, or use only one of Freud's three approaches without noting that none of them is internally consistent (e.g. B. E. Moore, 1975; Rothstein, 1979a, 1979b).

From auto-erotism to object-love

The movement from auto-erotism to object-love poses the second major problem for the development of personality. The recent analyses of the problem by Compton (1985b) and Erlich and Blatt (1985) confirm the conclusions I set out in Chapter 10. Compton shows how Freud's "apparently straightforward" distinction between the auto-erotic and object-directedness of drives "proved difficult" to apply to the component drives of sado-masochism, scopophilia, and exhibitionism. Erlich and Blatt (1985) endorsed Balint's account of the pitfalls of primary narcissism and of the apparent contradictions between it, primary object-love, and auto-erotism before making the more general point that what is "left obscure ... is the nature of the process" by which libido in both anaclitic and narcissistic object choices is remodelled into object relations. Compton (1985b) puts it slightly differently. Within the notion of object-directedness the confused status of the mother's breast "persists".

Compton also notes that the difference between object-love, which is sexual, and the loving/affectionate current, which is not, "points to a considerable gap" in the 1905 theory. Neither are the determinants of the affectionate current "much pursued", apart from Freud's indicating modelling, or a derivation from the suckling relation. In summary, Freud "contradicted himself" on whether infantile drives were auto-erotic or directed toward objects, made "incompatible statements about whether or not object-choice occurs before puberty", and, by restricting himself to

drive constructs, used object-choice and object-directedness with an incomplete set of explanatory constructs (Compton, 1985b).

In a further analysis, Compton (1986c) confirms that two separate developmental schemata can be distinguished in Freud's work. One is of stages of libidinal organisation (oral, anal, genital) and the other of stages of object-directedness (auto-erotic, narcissistic, homosexual object-directed and heterosexual object-directed). He goes on to observe that Freud "never quite resolved his equivocation about whether there is object choice in infancy" emphasising that the apparently unequivocal references Freud did make to the mother's breast as object, mainly in discussing the Oedipal situation, involve "*a different hypothesis altogether*". Compton also makes an absolutely overwhelming case for Freud holding to an objectless beginning, even after his formulating the final anxiety theory.

Development in the female

In Chapters 10 and 13 I brought out some of the difficulties which Freud's theory of psychosexual development had in explaining female object-choice and super-ego formation. Basic to Freud's theory is the assumption that at the very beginning of the developmental process the sex of the female child is masculine. The girl's sexual attachment to her mother weakens because of the hostility she feels at being deprived of a penis. Penis envy then develops. The father is chosen as an object because the symbolic equivalence of baby=penis means that he can supply both to her. Having already been castrated, both her entry into the Oedipal situation and her exit from it are less complete than in the male. As a consequence her super-ego is less well-formed and weaker. Important psychological characteristics of women, including femininity itself, form as a consequence of the peculiarities of this process.

What light does research throw on these aspects of Freud's theory? First, the assumed male starting point requires that the developmental 'stages' through which females were supposed to pass have to be secondary to it. Consequently, for the most part what literature there is revolves around characteristics which are developmentally secondary and the tests themselves are not direct tests of Freud's basic assumption. Second, when the assumption of masculine sexuality is sometimes questioned it is only by its being mentioned in a derogatory way. For example, by merely referring to it as "sexual phallic monism", Chasseguet-Smirgel (1976) fails to come to grips directly with the basic issue and D. Bernstein (1983) and Cereijido (1983) do not come any closer. Avoidance is also true of those in the past like Horney, who questioned Freud's assumption, as well as those like Ernest Jones, who made the opposite assumption (Horney, 1923/1924, 1926, 1928; E. Jones, 1927, 1933, 1935). Third, the clinical observations of psycho-analysts other than Freud fall broadly into two groups: those which seem to confirm what Freud said he had seen and those which do not. Men and women analysts are found in both camps so

that the division does not seem to be related to the sex of the analyst.

To judge from the summary reviews of Fliegel (1982) and Chehrazi (1986) a number of the differences over the secondary features have been settled. While some modern psycho-analysts adhere strictly to Freud's original descriptions of the developmental consequences (Mitchell, 1974; Nagera, 1975), sometimes with a greater tenacity than he did (Lacan, 1966/1982), most do not now believe the female child masturbates only by rubbing the clitoris (Barnett, 1968; Kestenberg, 1975; Kleeman, 1977), or enters the Oedipal situation via the formation of a negative Oedipus complex centered on the mother (Edgcumbe, Lundberg, Markowitz, and Salo, 1976; Parens, Pollock, Stern, and Kramer, 1977) and, where the term 'penis envy' is still used, it is taken to mean something other than what Freud described (Chasseguet-Smirgel, 1976; Galenson and Roiphe, 1976; Roiphe and Galenson, 1971, 1981). The components of the developmental sequence seem to be very different from those which followed from Freud's assumed starting point.

However, as the discussions of Fliegel and Lampl-de Groot illustrate, the nature and paucity of the data makes it difficult to know if analysts can (or wish to) reach agreement about either the starting point or how the development occurs (Cf. Fliegel, 1973; Lampl-de Groot, 1982). Some who do have data believe the developmental sequence and outcome to be significantly different from that described by Freud. For example, Melanie Klein (1928, 1932), describes a quite different process with a super-ego *more rigid* than the male's as its outcome. Others, like Lacan have, as Fliegel puts it, placed Freud's developmental sequence "out of reach of the 'absurdities of empirical refutation'" by reasserting it in metaphorical terms and elevating it "to the status of axiomatic truth" (Fliegel, 1982. Cf. Lacan, 1966/1982).

The psychological characteristics have also been questioned. Again very few data are reported, even of a clinical psycho-analytic kind, and what can be gleaned from the literature of either camp is limited. Thus, although Lowery (1985) introduced her discussion of women's supposed lesser capacity for sublimation, which she accepted, with the acknowledgement that the topic had received "little consideration", she discussed the supposed inferiority without mentioning any observations at all. Nor did Schafer (1974) cite any data in reaching his conclusion that "Freud's estimates of women's morality are logically and empirically indefensible" and none of the analyses subsequent to his is empirically any stronger (e.g. D. Bernstein, 1983). Schafer believed he saw the influence of Freud's "traditional patriarchal and evolutionary values" on his "flawed" ideas about the development and psychological characteristics of the female in:

> questionable presuppositions, logical errors and inconsistencies, suspensions of intensive inquiry, underemphasis on certain developmental variables, and confusions between observations, definitions and value preferences. (Schafer, 1974)

My impression of the literature is that most of these criticisms would be accepted by contemporary psycho-analysts. If so, explanations of female development cannot be derived from Freud's psycho-sexual theory.

The *goal* of Freud's explanations provides, I believe, a more fundamental ground for rejecting Freud's theses. It seems to me obvious that Freud was not describing his female patients so much as putting forward the stereotyped view of the female typically held by males of his time and social outlook. The 'facts' he wanted to explain were certainly not clinical facts and were hardly facts at all. The secondary developmental transformation has a similar status. Given a 'masculine' starting point, the changes Freud described were more-or-less demanded by the end point and the failure to confirm them was almost inevitable. Freud's account of the psycho-sexual development of the female is not so much wrong as totally unnecessary. [1]

WHAT DEVELOPS

The ego and super-ego are the central components of Freud's personality theory. Because of my detailed examination of these structures in Chapter 13, I here summarise the developmental aspects of those criticisms and add such psycho-analytic opinion and argument as seems relevant. Issues about the development of functions like realistic secondary process thinking will also be briefly mentioned.

The structures

We have seen that two of the main problems with the structures of the structural theory are that psycho-analysts are not at all certain what it is that constitutes a psychological structure and what it is that the particular structures of the structural theory are. At the purely descriptive level they are not clear about which functions belong to which structure. For example, they cannot list which processes are appropriate to the id and they cannot decide whether the ego-ideal's 'goodness' allows it to be part of the super-ego or not. Nor can they explain *how* a given structure exercises its functions. In other words, the characterisations of the structures are at least uncertain and it may be that they are not characterised at all.

That some of these problems are very old ones is evidenced by the

1. Being as curious as Schafer (1974) about "what sense, if any" Freud could have been making in his statements about women's lesser morality, I have informally asked graduate students and acquaintances of both sexes unfamiliar with Freud's descriptions to characterise the typical differences between the moral standards and behaviour of men and women. For what these data are worth, the question usually draws responses deeply uncomprehending of the question and explanations do little to make it meaningful.

difficulties which psycho-analysts had when they made their first attempts to bring order to the structural concepts. Thus, in 1947, Glover implicitly disregarded the metapsychological aspects of the super-ego by saying that it was "from first to last a clinical concept" (Glover, 1947). Ten years later, Rapaport concluded his 1957 listing of the many unsolved theoretical problems about the super-ego with an outline of what he meant by metapsychology and a frank acceptance of defeat:

> Clearly, I am not in a position today to give a metapsychological analysis of the superego concept and of the psychoanalytic propositions pertaining to it. (Rapaport, 1967)

Three years later, he was still unable to include more than the ego in his ambitious attempt to systematise psycho-analytic theory:

> the structural treatment of the id and superego is still so inadequate that the lengthy discussion it would require is beyond the scope of this presentation. (Rapaport, 1960, p.54, n.14)

As Beres found, there was not even a conclusive list of super-ego functions (Beres, 1958). More recent discussions show how little progress has been made over the past thirty years. Metapsychological descriptions of the super-ego are either termed "less-than-well systematized" and not in keeping with clinical understanding (Garza-Guerreo, 1981a) or are not even alluded to (e.g. Arlow, 1982; Brenner, 1982).

What Rapaport said about the metapsychology of the id in the late 1950's was confirmed throughout the 1960's in the 1963 Panel discussion of the American Psychoanalytical Association (Marcovitz, 1963) and in the attempted clarifications by Schur (1966a) and Hayman (1969). It seemed impossible to decide whether the id had any structure, or to arrive at any consensus of its functions, or to be certain whether all of it was unconscious in the descriptive sense, or to know what the relation between 'id' and *Ucs.* was (Marcovitz, 1963; Hayman, 1969). Since then there seems to have been no further attempt to clarify the concept either by describing its functions more adequately or resolving the theoretical problems associated with it (Shulman, 1987). Until the functions are clarified, the concept itself can make only a pseudo-contribution to Freud's personality theory and its metapsychological explanation will continue to elude psycho-analysts.

The situation of the ego is similar. One searches in vain through the numerous papers celebrating the fiftieth anniversary of the publication of *The Ego and the Id* for anything definite about its functions (e.g. Arlow, 1975; Holt, 1975b; Modell, 1975). Recent dissatisfaction about the uncertain functions of Freud's concept, has led to calls to replace 'ego' with other concepts, for example, the 'self' of Kohut (1971, 1977) or 'schema' (Slap and Saykin, 1983; Slap, 1987), or to modify the ego so substantially that it is included in the concept of 'self' (Meissner, 1986) or

the 'self' or some major function is included within it (e.g 'self', Kernberg, 1982; 'representational world', Rothstein, 1981). Many of the proposed alterations are, as Spruiell says, "subtly or not so subtly antithetical to Freudian psychoanalysis" (Spruiell, 1981), a point also brought out by Patton and Sullivan (1980). All are based on the assumption that Freud's original definition of the ego had a reasonable basis in observation but, some time ago, Holt argued that neither the ego nor the id had ever fitted the data of observation "in any usable way" (Holt, 1975b).

The processes

I considered fairly fully in Chapter 13 the generally unsatisfactory nature of Freud's account of how the main structures of the personality develop. One point not raised there was how the ego developed its realistic, secondary process mode of thinking. In Chapter 9 I did characterise what Freud said in *The Interpretation of Dreams* as a description (and an inadequate one) rather than an explanation. There seems to be nothing in the subsequent psycho-analytic literature which attempts to transform the description into an explanation. Gill (1967) tried to avoid the problem altogether. After describing the primary process as "an ingrained shibboleth of psychoanalytic thinking" he went on to give it secondary process characteristics from the beginning.

Deficiencies in Freud's theory of the development of the secondary process are very occasionally commented on in the psycho-analytic literature. In 1969 Noy had observed that Freud never really explored the theory of the primary process after its initial description at the turn of the century and that it had "remained frozen" in its original economic formulation. Nor had he "revised the concept" so as "to adjust it" to the new concepts of the structural theory, especially to the ego (Noy, 1969). Noy noted that by 1956 two distinct arguments were being made about the primary process. One was that it should be restricted to the economic view and the other that it be widened to include the various chaotic processes Freud said he had found in the dream work. The opposition is a rather odd one. It was the chaos from which the economic perspective was inferred and the chaos was supposed to be explained by the ways psychic energy was distributed.

Nevertheless, by gradually attributing more and more secondary process characteristics to the primary, the wideners pushed the two formulations even further apart. For example, Holt (1967) argued that an economically defined primary process had to have a structural basis. It required structures within which to work and had to have structures in the form of ideas on which to perform that work. Consequently, the primary process could not be a fixed unchanging given but had to have a developmental history. Holt took thinking which had autistic, magical, and wishful properties to be primary process thought and attempted to outline how it developed. On my reading of him, he seems to have thought of the

history of the primary process as being part of what Freud would have probably described as the history of the secondary process.

Without referring to Holt, Steele and Jacobsen (1977) took a similar line when they noted that the assumption of primary process functioning made it difficult to see how an hallucinated object could be formed during primary narcissism because "the perception of an actual object is an essential prerequisite for any subsequent hallucinating". They also showed it was precisely this prerequisite that forced Freud into the otherwise peculiar postulation of an "original reality-ego" which had to give way to a "pleasure-ego".

So fundamental are differences like these that it is hard to accept the arguments of Gill (1967) that there is some degree of continuity between primary and secondary processes. The attempt by Zern (1968) to represent Freud's view of the mental processes as "an integrated and coherent whole" is based on a similar overlooking of the distinction between the imperative primary process and a delaying secondary. There is, of course, a relation between the polarities described by Freud (secondary-primary, conscious-unconscious, reality-pleasure, etc.) in that one member of the pair always derives from the other. But what Zern does is to turn those relations into similarities and base an argument for integration on them. He simply leaves out the very differences that were postulated initially.

Steele and Jacobsen concluded that because of the contradiction which Freud created in having the secondary process function of binding located *prior* to the primary process one of discharge, and because of the "elegant, though implausible idea" of the presence of a pleasure-ego and a primary process at the beginning of life:

> The problem of Freud's developmental explanation of mental functioning is not in *how* the primary process changes into the secondary process but in how *anything at all* emerges after the infantile primary process. (Steele and Jacobsen, 1977. My emphasis, MBM)

They regard Freud as having treated "fact as fiction" and, like Holt, believe that secondary process thought must exist at the beginning of life. Perhaps it is the damaging nature of comments like these, brief as they are, which makes comprehensive discussion of the transformation of primary into secondary process unnecessary.

HOW DEVELOPMENT IS POWERED

The development of personality is powered by psychic energy provided from the instinctual drives. A number of critical points was made in Chapters 11 and 12 about both concepts. Of them, the most important are that the sources of neither the ego- nor the death drives can be specified, that an aggressive drive, however thought of, has no source or aim, that

psychic energy is never consistently derived from an instinctual source, that psychic energy can not provide energy for the mental structures through sublimation, and that it can not exist in different forms or, if it can, those forms could not mix with or separate from one another.

So powerful have these criticisms of libido and the drives been that Werman almost casually grants that:

> Contemporary psychoanalytic views of the instinctual drives ... are markedly heterogeneous. In regard to libido ... the opinions include considering libido as a purely neurophysiological phenomenon; accepting the drive but rejecting its energic qualities; conceptualizing libido as a wish, with or without a physical substrate; and rejecting libido, psychic energy, and all other related concepts as irrelevant, unverifiable, and of no explanatory value. (Werman, 1985)

Having noted there was "more agreement" about libido than about aggression, Werman added there were "few ardent advocates of psychic economics today". I will nevertheless take up some of the more basic features of these concepts.

The limitations of an excitatory energy

We noted in Chapter 7 that Freud's conception of nervous system functioning lacks a genuine inhibitory component and is predominately excitatory in nature (Mancia, 1983). McCarley and Hobson (1977) contrasted this aspect of the theory of the *Project* unfavourably with the theory proposed at about the same time by Exner, one of his colleagues. Weiner (1979) has subsequently included in this unfavourable contrast Sherrington's theory, which also incorporated an active inhibitory process. Instead of basing himself on the facts of the inhibition of neural function known at the time, Freud chose to use an entirely speculative "lateral cathexis" in order to explain repression and, as Weiner (1979) also observes, the development of the secondary process.

Why did Freud not allow for an inhibitory process? Weiner (1979) writes as if Freud's concepts about neurones and connectivity "came from nowhere". However, at the time Freud began the *Project*, a good deal was known about both but, as I pointed out in Chapter 7, Freud chose to disregard that knowledge. It is well known, as Kanzer (1973, 1981) confirms, that the "neurological systems" Freud invoked in the *Project* were drawn from clinical observation. Now it was clinical observation which required active *excitatory* processes to be repressed into the unconscious and maintained there by an equally active conscious *excitatory* process. An *inhibitory* repressive process would have involved a considerable self-contradiction. Attributing the limited usefulness of the theory in the *Project* to the neurophysiology of the day not being advanced enough and describing it as "too far removed from clinical material, too abstract to be useful", as Applegarth (1971) does, is only part of the

explanation. The fact is, as Solms and Saling (1986) imply, the conceptual nervous system Freud settled on was too simple to do the job. The lack of a genuine inhibitory mechanism was part of that simplicity.

We might note in passing that Freud seems to have had a real ability for adopting out-dated conceptualisations of nervous system functioning. In addition, within about ten years of Freud's first drafts of the *Project*, and at about the same time as he was formulating his concept of instinctual drive, Sherrington saw that it was but a partial truth to group "all motor reflexes ... into those that tend to prolong the stimulus and those that tend to cut it short" (Sherrington, 1906, p.329). Consequently, even Freud's simple reality-testing function could not have the reflex basis he proposed.

There are some differences between the theory of the mental apparatus in Chapter 7 of *The Interpretation of Dreams* and that of the *Project* (Bush, 1978), but their basic properties are almost identical. The similarity derives, McCarley and Hobson (1977) and Weiner (1979) note, from Freud's basing both on the same simple reflex model of tension-reduction and using the same pseudo-physiological constructs in both (Cf. Swanson, 1977). I would add that the resemblance each has to the structural theory has the same basis and that Kanzer (1981) also observes that Freud's propositions about consciousness in his posthumously published *Outline of Psycho-analysis* (Freud, 1940a) are "reminiscent of an updated *Project*".

The concept of psychic energy

When commenting to psychologists in 1923 on mental energy, Adrian rightly concluded that it was an "impossible" concept. The facts of neural transmission made the notion of a nervous energy "unnecessary" and the less said about it and a mental energy "the better". He illustrated his point with a remark about "neurin", an hypothetical nervous 'fluid' that McDougall had proposed. He thought neurin was "more welcome" to psychologists than to physiologists for explaining phenomena like fatigue, inhibition, and hypnosis (Adrian, 1923). Just over twenty years later, in a paper delivered to psycho-analysts, Adrian noted that the early criticism of Freud's psychic energy concept had been based on the vagueness of its analogy with physical energy and referred, rather prematurely, to those criticisms as "ancient history" (Adrian, 1946).

There can, of course, be only limited objection to the concept of psychic energy on the grounds that no physical evidence can be found for it. Mental concepts lack extension and the 'reality' of psychic energy cannot be established in the same way as physical energy (Swanson, 1976, 1977). But, if psychic energy is considered as a neurophysiological concept, or one that derives from neurophysiology, or a neurophysiological concept in disguise (Holt, 1967/1968; Pribram, 1962, 1965; Applegarth, 1971, 1977a), the evidence is against it having the external source Freud and his predecessors seem to have supposed (Amacher, 1965). Nor can a neuro-physiological type of energy be limited in amount (Sherrington, 1906,

p.325), function within a closed system (Pumpian-Mindlin, 1958-1959; Rosenblatt and Thickstun, 1977), or derive from an instinctual drive (Lashley and Colby, 1957, cited in Holt, 1968). And, of course, it can never exist in different forms such as sexual, aggressive, and neutral (R. W. White, 1963), or vary quantitatively in ways consistent with the systems it energises (McCulloch cited in Kubie, 1953; Holt, 1967/1968).

The major conceptual problems with truly mentalistic concept of psychic energy proper are independent of its 'real' existence. They include the failure to find a measure of it independent of the phenomena it is meant to explain (Holt, 1967/1968), the proposition that different amounts of it can be added or subtracted (Rosenblatt and Thickstun, 1977), the indefiniteness of its relation to the instinctual drives and the obscurity of its relation to structures (Applegarth, 1971), the inconsistent and largely descriptive characterisation of its capacity for neutralization and binding (Holt, 1962; Applegarth, 1971), the contradiction of it being directionless but having aims (Apfelbaum, 1965; Rosenblatt and Thickstun, 1970, 1977), and the proposal that it exists in different forms (W. C. Lewis, 1965), especially as the only change in it can be in its pathways (Sandler and Joffe, 1966; Applegarth, 1971). This postulate of a changing energy, so critical to so many psycho-analytic processes and explanations is, as Lewis noted, "something of a theoretical nightmare" (W. C. Lewis, 1965).

None of these conceptual problems is overcome in any alternative formulations, including those of the ego-psychologists (Apfelbaum, 1965). Thus, while V. H. Rosen (1965) agrees that White's concept of a neutral and independent ego energy offers some theoretical advantages, there were no "unambiguous criteria" for its identification. Rosen also believes, with good reason, that those interested in retaining some concept of energy have not come to grips with the fact that the identification of different types of energy requires a social value judgement of their alleged aims - a problem logically identical to that I pointed out earlier about sublimation.

Psychic energy as a metaphor Adrian's reference to 'neurin' highlights the metaphorical way in which the concept of psychic energy is so often used. As a metaphor it stands primarily for the subjective sense of effort or energy that mental work seems to entail and in this respect it has the same origin as its physical counterpart (S. W. Jackson, 1967, 1970; Kubie, 1975). However, no concept of psychological energy has so far been realised through the experimental investigations and mathematical transformations that have resulted in our modern conceptions of physical work and energy. Nor could it ever be; its 'reality' is different (Shope, 1971).

Rosenblatt and Thickstun observed that many psycho-analysts nevertheless seem happy to use the concept to translate subjective experience directly into what they feel are more meaningful terms and in so doing to give it a physical 'reality' (Rosenblatt and Thickstun, 1970). Thus Shevrin (in Chattah, 1983) described a patient whose experience of her own actions

and thoughts in energic terms seemed to involve a real expenditure of energy. Shevrin also defined psychological work as the exercise of motive force over time and explicitly equated it with the physical definition of work as energy expended over distance. The 'physicalist' definitions of psychic energy given by Rapaport and Gill (1959) and Loewenstein (in Calder, 1970), anticipate Shevrin almost exactly and illustrate how entrenched the habit of subjective translation is.

The applications made by psycho-analysts today are also similar to Freud's and, despite Adrian's optimism, metaphorical redescriptions posing as explanations are still invoked for fatigue-inhibition type phenomena. For example, Lustman translated alterations in the neonate's responsiveness into changes in the distribution of energy between different processes (Lustman, 1957), and in a 1969 Panel Discussion there was a similar abundance of metaphorical redescriptions (Calder, 1970). Psychic energy was used to 'explain' conflict (Treurniet), repression (Wexler), megalomania (Wexler, Garma), dreams and hallucinations (Garma), actual and/or traumatic neuroses (de M'Uzan, Mitscherlich-Nielsen), childhood autism (Greenson), momentary changes in a patient's state of consciousness (Rubinfine), the recovery of a patient who had used his own psychological forces during analysis (Shor), and the neurotic absorption of a child in its own problems, as well as transference, free association, trauma, and displacement (Lustman). And, although Schur warned the Panel of the dangers of using economic explanations exclusively, his own list of candidates was as large as that of the rest of the Panel put together (Calder, 1970). Starke (1973, p.39) subsequently extended its scope even further, defending its usefulness "in elucidating the mental processes conditioned by the instincts".

The scope of these 'explanations' illustrate what Nagel pointed out nearly thirty years ago:

> in Freudian theory metaphors are employed without even half-way definite rules for expanding them ... in consequence ... metaphors such as 'energy' or 'level of excitation' have no specific content and can be filled in to suit one's fancy. (Nagel, 1959, p.41)

A good illustration of Nagel's point is provided by Rapaport's two quite different explications of Freud's term 'binding'. In 1951 he equated the binding of psychic energy with neutralization but later with the formation of structure (cited in Holt, 1962; Applegarth, 1971).

Almost all of the translations are also open to the more general objection made by Warme (1981) to metapsychological explanations in general: the movement from clinical data to inferred metapsychological process and to the use of clinical data to confirm the metapsychological process is circular.

Alternatives to psychic energy Many psycho-analysts tacitly accept Nagel's criticism and are not enthusiasts for metaphorical energic explanations. Nor are they much keener on 'real' energy. Over twenty years ago Apfelbaum (1965) observed "it is mainly on the clinical level that the deficiencies of the quantitative approach are most serious". This was at about the time Holt (1967/1968) changed his previous view and began to wonder if the concept of psychic energy had been clinically more misleading than helpful. The convergence of opinion is of interest because Holt, unlike Apfelbaum, was then prepared to grant the potential usefulness of some as yet unknown but measurable kind of energy. The doubt they expressed was an influential one. Applegarth (1971) had concluded that it was a useful concept but wondered if it generated good explanations. Later she shifted her position: it was not whether it was useful but whether it was "indispensable" (Applegarth, 1977a), a view with which Ellman and Moskowitz (1980) appear to agree.

During his attempt to justify a concept of psychic energy having neither physical nor metaphorical reference, Brenner (1980) pinpointed the explanatory problem. He argued that the effects of wishes had led Freud to characterise an instinctual drive as something that drove or impelled the mind into action. That quality led in turn to the concept of psychic energy as the "capacity" of a drive to impel, a concept which contained "the idea that some wishes ... are stronger than others". Hence:

> Unless one is ready to assume that the drives never fluctuate in intensity, one must attribute a dimension of magnitude to psychic energy; there must be some quantitative or economic aspect. (Brenner, 1980)

Brenner made his plea for a concept of psychic energy because he believed it to be "a valid and useful generalization" about the variable capacity with which the mind is brought into activity.

Wallerstein (1977) has asserted that, for Freud, as for most psycho-analysts, psychic energy had always been "a mental construct used metaphorically". It is true that it can be so interpreted. Years after Adrian's first critique, Apfelbaum (1965) pointed out that, in addition to Freud's concept matching the common understanding of fatigue, exhaustion, and the 'running down' of an energy store, it also matched the common pseudo-energy view of relief from tension being obtained by working off tension by "direct energy expenditure". After remarking that "as commonsense notions often do, these metaphors mirror conscious experience", Apfelbaum also stressed how well "abreactive expression lends itself to physical explanations". As we saw in Chapter 7, it was similar ordinary experiences from which Freud and Breuer drew their theoretical notions. And, we know Hughlings Jackson's quoting the common view contributed to Freud's energic concept of catharsis.

However, I would dispute that Freud's original concept of psychic energy was mental and its use metaphorical. In the *Studies on Hysteria*,

Freud initiated the psycho-analytic tradition of directly translating subjective impressions and clinical phenomena into economic terms by translating his subjective response to resistance into an ego-force. The opening sentences of the *Project* tell us that condensation, displacement, substitution, and stimulus "directly suggested the conception of neuronal excitation as quantity in a state of flow" and, consequently, that at that time Freud's fundamental economic propositions referred to a physical process (Freud, 1950/1954, Part I). Stripped of their pseudo-physiological referents they can equally well refer to psychical processes. Gill is absolutely correct that there is "no evidence" Freud intended psychic energy to be a mere metaphor or that he used it that way (Gill, 1977).

One of the few things upon which there is agreement among psycho-analytic theorists is that psychic energy readily allows for the explanation of the psychological processes of displacement and condensation. However, since it was from those very phenomena that the concept was inferred, any faith that contemporary psycho-analysts might have in those phenomena confirming the reality of the concept is only touching. The relation is, as Applegarth (1971) says, "not surprising". But Gill makes an even more fundamental objection: energy is not necessary to explain condensation and displacement (Gill, 1977). What is left of Freud's concept if stripped of its conceptual usefulness and its evidentiary base?

PERSONALITY AND PSYCHOSEXUAL DEVELOPMENT

Because I have not previously discussed the means by which personality is formed, I now outline briefly Freud's theory of psycho-sexual development before considering the mechanisms by which he thought personality characteristics were formed.

Stages of psycho-sexual development

We have seen in Chapter 10 how Freud believed infantile sexual behaviours were polymorphously perverse. Originally he referred to the oral, anal, and phallic (then called genital) *modes* of satisfaction of the sexual impulse, describing each as auto-erotic. By this he meant that the object of the impulse was the subject's own body and that its aim was the stimulation of a given erotogenic zone (Freud, 1905b, pp.182-183).

Neither were these auto-erotic modes of satisfaction originally seen as sequentially related. Freud first thought of them being equally important in producing pleasure and in contributing to an overall auto-erotic stage which preceded the stage of object-love (op. cit., p.207). Some time later he proposed narcissism as a developmental stage separating auto-erotism and object-love. Auto-erotic manifestations then underwent a metamorphosis after which none were regarded as auto-erotic.

Freud eventually termed the whole set of distinct patterns of infantile sexual behaviour the *pregenital stages of libidinal organisation*. They were the auto-erotic, first described in the *Three Essays* (Freud, 1905b), the narcissistic (Freud, 1911a), the anal-sadistic (Freud, 1913d), the oral (Freud, 1915a), and the phallic (Freud, 1923e). Each stage was one of a predominant mode of libidinal satisfaction and each necessarily involved the predominance of a particular relation of libido to objects. [2]

The oral stages

Two sub-stages of the oral stage were eventually distinguished: oral sucking and oral sadistic (or oral biting). In both, the object was the mother's breast, but in the first sub-stage the aim was to suck at it while in the second it was to devour it. The second stage was referred to variously as oral-sadistic or oral-cannabalistic. Freud thought the destruction of the object derived from a fusion of sexual and destructive impulses; he therefore described the attitude toward the object as *ambivalent*. [3]

Freud's original description of the oral mode of satisfaction drew heavily on the work of Lindner, of course (Freud, 1905b, pp.179-181), but it was Karl Abraham's clinical observations that provided the basis for differentiating the two oral sub-stages. K. Abraham (1916/1927b) described an adult schizophrenic patient who drank milk heated to body temperature with a pronounced sucking action and who tended to wake with strong sexual desires, which milk-drinking satisfied. However, if he was unable to find milk he would masturbate. The patient also had a number of phantasies with markedly cannibalistic themes. Abraham took these and similar behaviours in other patients as confirming Freud's proposal that there was an infantile stage in which oral sexual and aggressive impulses were fused. K. Abraham (1924/1927d) later stressed how ambivalent these behaviours and phantasies were, especially in depressed patients.

Abraham's evidence for the infantile oral-sadistic sub-stage was not based on any direct observation. Indeed, Abraham disavowed the possibility that direct observations could supply evidence about it: "we are concerned with developmental processes which are hardly accessible to direct observation" (K. Abraham, 1916/1927b). His evidence was a reconstruction of an infantile past based on a complex interpretation of disordered adult behaviour. For the oral-sucking stage the role of observation seemed more direct. Freud's picture of sucking as sexual did

2. The changes in the concept of auto-erotism, which cannot be detected in any of the editions of the *Three Essays* or the later footnotes to it, has a complex history which finds no explicit acknowledgement in either Freud's 1932 systematic account of the theoretical revisions of the previous sixteen years (Freud, 1933b, pp.99-102) or in the last major outline of his views written six years later (Freud, 1940a).

3. Originally Freud had used this term to refer to the active and passive forms of oral erotic activity (Freud, 1915a, p.138).

not include an aggressive tendency. Consequently, although Abraham's reconstruction only implied there had been an initial non-ambivalent stage, that implication was consistent with Freud's interpretation of Lindner's observations view that early sucking gave an unalloyed sexual pleasure.

The anal stages

Freud inferred the existence of anal erotism as a mode of libidinal satisfaction from the sexual pleasure in defaecating he supposed children to experience (Freud, 1905b, pp.185-186). Eight years later he described a distinct stage with two discernable but not completely separate forms: a passive anal-erotism and an active anal-sadism, the latter being activated by the "instinct of mastery" (Freud, 1913d).

Freud put forward no direct observations to support the existence of either form but like Abraham he offered a complex reconstruction based upon interpretation of a single case. A female patient had developed an obsessional neurosis with a:

> compulsion for scrupulous washing and cleanliness and extremely energetic protective measures against severe injuries which she thought other people had reason to fear from her - that is to say, reaction-formations against her own *anal-erotic* and *sadistic* impulses. (Freud, 1913d, p.320)

The onset of the disorder had been preceded her husband losing his potency. Freud linked that fact to the "well known fact" that the character of women was often peculiarly altered after such a loss of sexual function;:

> They become quarrelsome, vexatious, and over-bearing, petty and stingy; that is to say, they exhibit typically sadistic and anal-erotic traits which they did not possess earlier, during their period of womanliness .... We can see this alteration of character corresponds to a regression of sexual life to the pregenital sadistic and anal-erotic stage. (op. cit., pp.323-324)

Freud's interpretations of these behaviours as reaction-formations was a reconstruction that drew explicitly on the concept of regression. In the case of the sadistic component Freud assumed it was activated by an unspecified instinct for mastery.

K. Abraham (1924/1927d) differentiated two sub-stages of the anal stage through some rather curious reasoning. He first argued that the *similar* symptoms of melancholia and obsessional neuroses meant that both were be based on a *similar* regression to an anal-sadistic stage (as it was then called). But, because there were *differences* in the symptoms, the stage had to consist of different components. He then argued for the existence of two different anal pleasures, faecal retention and faecal expulsion, as well as two different sadistic tendencies, control of the object and destruction of it. Then, drawing on parallels in speech, folklore and mythology, as well as upon symptom interpretation, he linked the two

impulses together: anal expulsion and sadistic destruction marked the first anal sub-stage and anal retention and sadistic control the second. Once again, most of Abraham's evidence for these anal-expulsive and anal-retentive sub-stages were reconstructions based on complex interpretations of symptoms rather than on direct observation.

The phallic stage

Genital erotism in children was also described by Freud in the original edition of the *Three Essays* (Freud, 1905b, pp.187-189). That both male and female children obtained pleasure from manipulation of the sexual organs was directly observable and became the primary fact from which the stage was inferred. Differences between child and adult attitudes to the organ led Freud later to rename the stage as *phallic*, a definition that reflected the childish belief that both sexes possessed a phallus and that it had primacy (Freud, 1923e, p.142).

Freud's evidence that both male and female children believed they had a penis appears to be based substantially on the beliefs of "Little Hans". Certainly the phrases with which he described the belief have a striking correspondence with his account of that case.

Narcissism

Although the term narcissism was used by Freud in a 1910 footnote added to the *Three Essays*, there was, at best, only an implication that a stage of sexual development was being referred to (Freud, 1905b, p.144, n.1). Narcissism was proposed as a pre-genital development stage as part of Freud's later endeavour to explain Schreber's delusionary psychosis (Freud, 1911a, p.72). The meaning of megalomania and delusions of persecutions were clarified by assuming that Schreber's own ego had become grossly overvalued when it was chosen as the object of his own sexual instinctual drive. In the general context of Freud's thinking about regression, this required that the ego had had such a role previously. Again, the narcissistic stage was a reconstruction based on the interpretation of adult behaviour rather than on direct observation.

THE FORMATION OF PERSONALITY CHARACTERISTICS

The three mechanisms by which Freud believed personality traits formed are prolongation or continuation, sublimation, and reaction formation.

The mechanisms of trait formation

Freud recognised that there could be *prolongations* or *continuations* of component instinctual drives well before either of those terms were used. In the *Three Essays* he had remarked that if the erotogenic significance of the mouth persisted:

> these same children when they are grown up will become epicures, in kissing, will be inclined to perverse kissing, or, if males will have a powerful motive for drinking and smoking. (Freud, 1905b, p.182)

Although Freud did not then mention any other prolongations, the essence of the process is clear. A prolongation or continuation carries over a largely unchanged version of an infantile mode of component drive satisfaction into adult life.

Freud first used the term *sublimation* to refer to a re-direction of a component sexual instinctual drive. Thus, visual sexual curiosity:

> seeks to complete the sexual object by revealing its hidden parts. It can, however, be diverted ('sublimated') in the direction of art, if its interest can be shifted away from the genitals on to the shape of the body as a whole. (op. cit., p.156)

Freud (1908b, p.171) later made it explicit that the redirection was to a new aim and proposed a more formal definition. The sexual impulse:

> places extraordinarily large amounts of force at the disposal of civilized activity, and it does this in virtue of its especially marked characteristic of being able to displace its aim without materially diminishing in intensity. This capacity to exchange its originally sexual aim for another one, which is no longer sexual but which is psychically related to the first aim, is called the capacity for *sublimation*. (Freud, 1908c, p.187)

The new activity was no longer sexual but the drive itself was unchanged.

Freud's earliest uses of the term *reaction-formation* tend to confuse it with sublimation. However, reaction-formation involves the replacement of the impulse itself by its opposite. Thus, in speaking of obsessional patients, Freud said that during the repression of the component:

> a special *conscientiousness* is created which is directed against the instinct's aims; but this psychical reaction-formation feels insecure and constantly threatened by the instinct which is lurking in the unconscious. (Freud, 1907b, p.124)

Although the aim of a reaction-formation was the opposite of the original impulse Freud also noted that the original component impulse could be satisfied. For example, cleaning and washing the anus satisfied the impulse without the satisfaction being conscious (Freud, 1908b, p.172).

In 1908 Freud also made an explicit but slightly ambiguous reference to feelings of shame, disgust and morality as "reaction formations, or counter-forces", an ambiguity he disposed of later by describing them explicitly as reaction-formation (Freud, 1908b, p.171; 1925a, p.37. Cf. Freud, 1913f, pp.298-299).

In none of these works did Freud propose a basis for identifying reaction-formations nor did he explain how they were created.

The traits by stage

I now describe the typical personality characteristics Freud associated with each of the stages of libidinal development, emphasising those said to be responsible for the formation of character traits.

The oral stage Prolongation, sublimation, and reaction-formation produce similar results in both sub-stages of the oral stage. The prolongations include eating, drinking, smoking, and oral sexuality. The sublimations from this stage either take socially acceptable forms of oral indulgence, such as food or wine tasting, or non-nutritive oral activities such as wind instrument playing, even including some without specific oral content, such as debating or public speaking. The reaction-formations include fads over foods and drink as well as, for example, concern with oral hygiene.

Important psychological characteristics supposedly deriving from the oral sucking stage are passivity and dependence. The ambivalence and narcissism which develop in the oral sadistic stage are the source of other important characteristics. Throughout this stage, the ego develops and with it the commencement of the reality principle. The beginnings of the super-ego are seen in the pre-Oedipal choice of the mother as sexual object by children of both sexes.

The anal stage The prolongations, sublimations and reaction-formations are again similar in both stages of the anal stage. Prolongations include all those behaviours related to the eliminative function (such as concerns with bowel regularity and constipation) and the actual attainment of pleasure through defaecation or retention. All the graphic arts (painting, sculpture, photography), the collection of things (e.g. stamps, coins) as well as money-making and its disbursement are seen as sublimations arising from the anal stage. Reaction-formations include a classical triad of cleanliness, orderliness, and parsimony as well as the traits related to them.

Emotional ambivalence became stronger, partly because aggression could now be expressed in a second way, and partly because of a growing awareness of the father as a possible rival for maternal affection. This latter factor was also responsible for the further development of the foundations of the super-ego.

The phallic stage Prolongations included non-social and narcissistic forms of sexual behaviour such as masturbation and petting. Sublimation in the phallic stage gave rise to non-genital but erotic artistic endeavour: pornography, love poetry, themes of sacred love, and so on. Reaction formations were directed against the obvious expression of sexuality and included excessive modesty and shame and concern over standards of sexual behaviour.

Castration anxiety, which had begun developing in the latter part of the second anal stage, led to the male child beginning to repress the Oedipus

complex. The acceptance of castration led the female to choose the father as object. With these choices, the reality principle finally dominated the pleasure principle and the formation of the super-ego was completed for the boy but for the female a long period of renunciation of phallic sexuality commenced.

Later developments Following the repression of the phallic stage, Freud believed that a latency period set in during which the expression of sexual impulses diminished. Various character traits were strengthened, the mechanism of sublimation becoming of particular importance for the development of social feelings. Puberty marked the onset of the genital stage with, at first, a revival of homosexual object choices. These were brought about by the male child's identification with the father and the female's with the mother, both identifications being determined by the castration complex. Adult normal heterosexuality developed gradually until, by the late teens, the personality with its structures, object choices and characteristic modes of behaviour could be said to be complete.

Pathological outcomes Even though I am not primarily concerned with "abnormality", the three deviant behavioural outcomes that Freud linked to his theory of personality should be mentioned. They are the psychoses and the neuroses, the sexual deviancies ("perversions"), and the character defects. The psychoses and neuroses involved a libidinal and/or structural regression to an earlier, fixated stage. Fixation at and regression to the oral stage produced the schizophrenias as well as the manic and the depressive psychoses. At the anal stage the psychosis was paranoia and the neuroses were the obsessive-compulsive disorders. Various forms of hysteria evolved from regression to the phallic stage.

Deviant sexual behaviour was seen as resulting from particularly strong fixations at the various pre-genital stages which had not been repressed. Deviant oral and anal practices were fixations at their respective stages. Fixation at the phallic stage was said to be substantially responsible for exhibitionism and voyeurism as well as for some disorders of the sexual function (nymphomania, satyrisis, impotence and some types of frigidity). Other disorders, especially the so-called character disorders, were related to the stages of psychosexual development but in a more complex way.

Freud had hoped to associate particular disorders with particular modes of defence, for example, hysteria with repression and obsessions with reaction-formations. From this it might seem that certain types of defence should be associated with certain developmental stages, repression with the phallic stage, for example. While Freud did recognise some connections of this kind, he saw the matter as much more complicated. However, he did think the modes of defence used by the individual were characteristic of him. The typical modes of defence, the personality characteristics, and characteristics of the mental structures, especially the

balance between them, constitute the main aspects of personality covered by Freud's theory.

THE BASES OF THE MECHANISMS

As to the determinants of the mechanisms, Freud left no room for doubt: they were constitutional. Thus, for prolongations, it was a "constitutional intensification" of the significance of a given erotogenic zone (Freud, 1905b, p.182) and it was "the innate constitution of each individual which decides in the first instance how large a part his sexual instinct it will be possible to sublimate" (Freud, 1908c, p.188). Although Freud does not seem to have said so explicitly, there is little doubt that he thought of the energy used by the reaction-formation as desexualised libido. The neutral energy stored in the ego was appropriated by the trend to be reinforced and added to its own. Consequently, reaction-formation also had a constitutionally determined basis. Although Freud did write as if development was determined jointly by these constitutional bases and environmental experiences, he gave no guidelines for the identification of the innate factors. His discussion of the interplay of accidental and constitutional factors is therefore almost totally devoid of meaning (Freud, 1905b, pp.239-240).

If we sense some vagueness in Freud's definition of sublimation and if he only hints at the conditions under which it takes place it is because of his belief that the capacity for sublimation was innately determined. Freud also granted that some direct sexual satisfaction of a sublimated impulse might be required (ibid.), a qualification which raised the question of the nature of the change in the impulse. In order to account for apparent transformations of sexual impulses into aggressive ones and *vice versa*, Freud postulated the existence of a desexualised or sexually neutral psychic energy which could combine with either the sexual or aggressive impulses (Freud, 1923b, p.44). Freud extended this concept of desexualisation by linking it with sublimation; adding desexualised energy not only changed the aim of the sexual impulse but altered its content. The intensity of the sublimated impulse was thus the intensity of a desexualised one.

As we saw in Chapter 13, another of the major problems with the concept of sublimation was that Freud was unable to explain how the sexual impulse was emasculated and how its desexualised energy was supplied to the ego. Because the super-ego draws on the energy of the death instinct liberated at the same time, the main structures of the personality have no fuel to drive them. Anxiety cannot be experienced and repression cannot take place. So great are the difficulties of the concept of a desexualised libido that we saw there is a substantial body of psychoanalytic opinion in favour of abandoning the concept. There is a further and equally fatal difficulty in using sublimation to explain the formation of

personality traits. Unless the content of the impulse changes, sexual satisfaction must result from the new activity - a consequence not at all what Freud wanted.

Apart from the theoretical difficulties there is the empirical problem of how sublimated behaviour is to be identified. As we also saw in Chapter 13, its defining characteristics have never been described. If sublimated behaviour cannot be identified, the conditions under which it occurs will be difficult to describe and a mechanism virtually impossible to formulate.

Freud attempted a clarification of some aspects of reaction-formation when he formulated his last hypothesis of the relation between repression and anxiety, which has a bearing on evaluating his personality theory. Reaction-formations were seen as arising out of situations of ambivalence, that is, situations where the individual had both positive and negative feelings toward an object. One trend, usually the affectionate one, would become intensified and repress the other. That which remained was the reaction formation. Although it was a permanent alteration in the ego and required a permanent expenditure of energy it did avoid the need for the repression to be repeated (Freud, 1926a, p.102). Freud was able to characterise behaviour motivated by reaction-formation by its exaggerated and compulsive quality. There are practical difficulties here but the real difficulties are theoretical. Reaction-formation draws on desexualised or neutral energy in the ego and therefore rests on sublimation, instinctual defusion and Oedipal identification. Further, if neutral energy creates new impulses in the ego directed against the original sexual impulse, how are sexual needs also satisfied through its discharge?

EVIDENCE ABOUT THE CHARACTERISTICS

Evidence about the traits postulated by psycho-analytic personality theory comes from behavioural rather than clinical observations. The question is whether there are distinguishable constellations of adult personality traits regularly associated with particular types of behaviour at the different stages of psychosexual development. Do the traits cluster together and are they related to the original dispositions as postulated by the theory? For example, is there a constellation of traits defining the oral character and is it associated with behavioural signs of fixation at the oral stage?

The procedure for answering these questions is essentially that relied on originally by Freud (1908b) and by E. Jones (1918) and K. Abraham (1921/1927c) when they elaborated Freud's initial formulations on the anal mode of satisfaction into the anal character. Although their observations were made during treatment and the childhood behaviours they referred to were most frequently reconstructions based on patients' recollections, there is no reason to suppose that non-clinical methods of enquiry should not confirm their inferences. Factor and cluster analyses of

data on traits collected by questionnaire and other methods ought to reveal the groupings of traits postulated by psycho-analysts and direct enquiry ought to produce material on childhood fixations.

Much psychological energy has been expended in investigating trait clustering but little or no support has found for their grouping in the ways demanded by psycho-analytic personality theory (Sears, 1942; P. Kline, 1972, 1981; S. Fisher and Greenberg, 1977; Fonagy, 1981). However, for the results of these studies to be acceptable, the measures of the traits themselves must be reliable and valid. In most studies they are not. Nevertheless they are included in a number of reviews, for example, that of S. Fisher and Greenberg (1977). The only writer who insists that these methodological requirements be met is P. Kline (1972, 1981) and his analysis of the many studies on the clustering of character traits is really the only one worth considering.

In both editions of his book Kline summarizes the findings as showing there is 'some' evidence to support the existence of a grouping of oral traits, 'good' evidence for the anal, but 'no evidence' for any other hypothesised psycho-sexual dimension, for example, the phallic (P. Kline, 1972, p.93; 1981, p.128). These fairly guarded judgements are later transformed. In his summary conclusions, Kline says that the evidence for *both* oral and anal characters is 'good' and in a later reference describes both as being supported by a 'considerable body of evidence' (P. Kline, 1972, pp.94, 335; 1981, pp.129, 421. Cf. P. Kline, 1984, pp.64, 156).

Evidence on the relation of the personality characteristics to the original disposition is also a matter for direct non-psycho-analytic investigation. Freud held that variations in the strength of the innate components of the sexual constitution were reflected in variations in the intensity of childhood oral, anal, and phallic behaviours. Although, he has frequently been taken to mean that childhood experiences like weaning and toilet training actually create fixations, his belief was that childhood experience merely intensified existing tendencies for modes of libidinal satisfaction to become fixated. However, whichever of these interpretations is correct, a relation must exist between adult traits and childhood behaviours. Of all of the acceptable studies of the relation between adult personality characteristics and childhood behaviours (retrospective, cross-cultural, current, longitudinal and projective) which Kline found, he noted in the first edition of his review that only two, and in the second only four, gave 'even slight support' to the relationship between personality, infant behaviours, and rearing practices (P. Kline, 1972, p.93, 1981, p.128).

Kline argument that the failure to relate the personality characteristics to childhood behaviours are due to technical difficulties is suspect and it is ingenuous to conclude as he does that:

> The aetiological hypotheses may not ... be rejected until better techniques of investigation have been devised. (P. Kline, 1972, p.94; 1981, p.129)

The action is inappropriate. Given the suspect bases of the original hypotheses in the limited number of cases, the curious logic, and the reliance on reconstruction and interpretation, the failure to find strong evidence for the personality characteristics to group together and for them to be associated with childhood behaviours is what might have been expected. Because they were proposed without adequate supporting evidence, Freud's hypotheses deserve as much (or as little) consideration as any other idle speculation. They should be discarded.

Were one or other of Freud's hypotheses genuinely 'aetiological', as Kline represents them, their confirmation would require rather more than the demonstration of a correlation between adult traits and childhood behaviours. What is hypothesised is that various mechanisms carry into adult life those fixations which arise in childhood, largely from constitutional tendencies. All that the confirmation of the association between the characteristics and the earlier behaviour provides is confirmation of the accuracy of the original observations. By themselves they throw no light on how the traits develop. The real question to be asked is what purpose the etiological hypotheses serve: are there any facts to be explained?

Part of the difference between psycho-analytical opinions about trait clustering and the very slight support provided by later work can be explained by the retrospective nature of the psycho-analytic observations. In samples of patients studied in this way, antecedent factors are often wrongly identified and inflated estimates of their prevalence usually obtained. Thus Stott's (1961) retrospective study of Down's syndrome incorrectly identified maternal emotional shock as its cause. On the other hand, Pitt's (1961) prospective study of the prevalence of intellectual disability in children affected with rubella found normal rates rather than the much higher ones reported in retrospective studies of rubella-deaf children seen at paediatric clinics. Similarly, the non-patient enuretics Lovibond (1964) studied showed few of the personality characteristics supposed to be associated with enuresis and he was able to exclude a causal role for those which were present.

The errors to which retrospective studies of patients are prone arise independently of investigator bias. They reflect peculiarities of the method itself and of the samples of patients studied. The causal factors in samples gathered for retrospective study are different in kind and intensity and they may operate differently than in the general population. Glover's (1955, pp.43-46) response to Eysenck's (1952) evaluation of the evidence (some of which Kline also considered) suggests that these factors were present to some extent when the original psycho-analytic observations were made. At the very least the observations were unsystematic and sparse:

> it is gratifying and not a little significant to find that *correlations* arrived at by two analysts round about 1922, *on the strength of a few uncontrolled*

> *observations of a few cases*, have been confirmed. (Glover, op. cit., p.45. My emphasis, MBM)

Although Glover was wrong in arguing that the original correlations had been confirmed, what he says about the early psycho-analytic work is consistent with my points about retrospective studies.

Lastly, can fixation and the mechanisms of sublimation, reaction-formation, and prolongation/continuation explain how traits form? The answer has to be 'No'. Freud left fixation without a definite place in his theory and continuation or prolongation is based on an absolutely uncharacterised process of "normal repression". Sublimation is probably the least clear of all the mechanisms and, like reaction formation, has few if any agreed on behavioural correlates. Neither is there agreement about the economic and dynamic factors upon which sublimation depends. It is not really surprising that there is so little support for the explanatory aspect of Freud's theory.

Some psycho-analytic writers dismiss the investigations of trait grouping and trait formation as being studies of concepts of personality that are outdated or not representative of psycho-analytic thought. There is no evidence that this is so. Discussions of the consequences for normal development of fixations are either central or of great importance in such texts as those of Hendrick (1934), Nunberg (1932/1955), Fenichel (1932/1934, 1945b), Fliess (1948), Brenner (1955, 1973), and Sterba (1968). R. E. Fischer and Juni (1981) have recently emphasised the continuing importance given anal characteristics and Baudry (1983) the concept of character itself. If Freud's trait theory is not representative or is outdated, we may reasonably ask what replaces it. Is it really being said that there never was any substance to Freud's trait psychology or to his theory of psycho-sexual development? Part of the 'outdated' response is quite silly. Fonagy's, for example, is the irrelevant comment that the phrase 'anal character' is so out of date that it is "restricted almost exclusively to a term of abuse between analysts"! (Fonagy, 1982).

PHYLOGENESIS

Many of the developmental aspects of Freud's theory of personality rest on the twin notions that species characteristics have been acquired through individual experience and that individual development repeats or recapitulates the development of the species. In spite of having no stronger evidence for them than "the residual phenonema of the work of analysis" (Freud, 1939, p.100), Freud strongly defended his need for these two ideas. In a letter to Groddeck, he remarked of the inheritance of experiences that "a consistent continuation of Lamarck's theory of evolution coincides with the final outcome of psychoanalytic thinking" (cited in Satinover, 1986). Sulloway has subsequently demonstrated very impressively the centrality of

the inheritance of acquired characteristics and the biogenetic law to all of Freud's explanatory schema (Sulloway, 1979).

In the theory we are now considering, not only did recapitulation and acquired inheritance pre-determine the stages of personality development but the strength of the fixations was also brought about by similar hereditary factors. Further, the disposition to react to a fixation by using one or another of the defence mechanisms was a matter of the archaic heritage.

It is not inappropriate here to recall that one of the major points of difference between Janet on the one hand and Freud and Breuer on the other was over the role of hereditary factors in hysteria. Freud and Breuer both regarded them as of much less importance than the accidental environmental factors. By the time that one can speak of a psycho-analytic personality theory, Freud's thinking had been reversed. In fact, the further one goes in the development of Freud's theory, the larger is the importance of innate, hereditary factors and the smaller the role of the environment.

The dispositions Freud appeals to are inferred from the very behaviours they are said to explain. As we saw in relation to Mesmer's theory of animal magnetism and Beard's syndrome of neurasthenia, explanations of this kind fail because they are circular. In the case of a disposition to fixate a mode of satisfaction or a disposition to use a particular mechanism of defence, the very existence of the disposition cannot be established independently of the behaviour or the use of the mechanism. In any case the prolongations are not clearly related to any events occurring within a stage, the concept of sublimation is almost devoid of meaning, and the tendency to reaction-formation is derived from another indistinctly defined dispositional concept. To the extent that Freud's personality theory rests on dispositions of these kinds, neither the essential determinants of personality can be identified or the personality characteristics explained.

Most psycho-analysts regard Freud's phylogenetic propositions as an embarrassment, flying as they do in the face of all fact and reason. When critically evaluating the use to which Freud put them in *Civilization and Its Discontents*, Werman (1985) judged them to be such "important weaknesses" that he passed over Freud's "untenable anthropological theories, his biologism, and his Lamarckism". This kind of criticism is not new. In his review of Wallace's *Freud and Anthropology*, Almansi (1986) compares the ideas in Freud's *Totem and Taboo* that were favourably received when it was published with those which were not. The concepts accepted included those of the incest taboo, spirits as projection, magic as wish fulfilment and omnipotence of thought, and ambivalence toward the dead. On the other hand, "the tenets of cultural evolutionism" which were particularly psycho-analytical and more central to Freud's explanations "had already come under strong attack" by the turn of the century. Nor, he adds, are Freud's notions of "the hereditary transmissibility of culture, the recapitulation doctrine, the equation of primitive and prehistoric man

and the concepts of parricide and the totemic meal'' accepted today.

The structure of the personality most dependent on phylogenetic factors is the super-ego. What can be said about inheritance in relation to the Oedipus complex to which it is heir? As we saw in Chapter 13, the majority view of contemporary analysts is that the contribution of the Oedipus complex to the super-ego is less central and less important than Freud proposed. There are a few traditionalists, like Holder (1982), who soldier on in Freud's footsteps by making the formation of the super-ego dependent on pre-Oedipal components supplied by the archaic heritage, or like Rubenstein and Levitt (1957) use Freud's phylogenetic hypothesis to explain the fears their male child patients had of their non-threatening fathers. The majority view is, however, more in accord with Almansi's summary of Wallace's opinion that Freud ''went astray in his phylogenetic view'' about the transmission of the Oedipus complex and its ubiquity (Almansi, 1986).

Major problems remain with most of the alternative explanations psycho-analysts have put forward. For example, after implicitly rejecting Freud's phylogenetic explanation of the intensity of the boy's castration fear, H. Hartmann and Kris (1945) propose that, although the child might not be threatened with castration, ''the veiled intensity of the aggression of the adult against the child may still produce the same result''. One sees very clearly that the problem of the 'residual' Freud referred to is still present; something other than the child's own experience determines the anxiety. But an aggression of *veiled* intensity is no advance logically on Freud's own explanation. The proposition is just as difficult or impossible to test as the phylogenetic original.

PSYCHO-ANALYSIS AS A PSYCHOTHERAPY

I turn now to the matter of whether psychoanalysis is effective as a therapy. Although the answer is undoubtedly important to the question of whether it can be recommended as a treatment for so-called mental illnesses or as a help with ordinary problems in day-to-day living, those practical matters are not the main reason for taking the issue up. The question is really the bearing of the effectiveness or ineffectiveness of psycho-analysis as a therapy on the validity of psycho-analysis as a theory of personality.

Eysenck's critique

Until the publication of Eysenck's (1952) well-known review of the effects of psychological therapy, little attention had been given to problems of evaluating the effectiveness of psycho-analytic therapy. Among the studies Eysenck reviewed were reports of psycho-analytic treatment of neurotics from the Berlin Psychoanalytic Institute (Fenichel, 1930, cited more fully in Bergin and Garfield, 1971), the London Institute for Psycho-

analysis (E. Jones, 1936, cited more fully in Bergin and Garfield, 1971), the Chicago Institute for Psychoanalysis (Alexander, 1937, cited more fully in Bergin and Garfield, 1971), the Menninger Clinic (Knight, 1941), and the study by Kessel and Hyman (1933). None included base data against which to judge the claimed effects. Eysenck therefore derived a baseline from studies of the effects of non-systematic treatment. This had:

> some two-thirds of severe neurotics showing recovery or considerable improvement without the benefit of systematic psychotherapy. (Eysenck, 1952)

After drawing attention to various defects in the studies, including problems of how outcome had been assessed, Eysenck concluded that the comparison of outcome rates of systematic with non-systematic treatment:

> fail to prove that psychotherapy, *Freudian or otherwise*, facilitates the recovery of neurotic patients. (op. cit.. My emphasis, MBM)

Depending on whether those who broke off psycho-analytic treatment were counted as "not improved" or excluded from the calculations altogether, the analytic recovery rates arrived at by Eysenck were either "44 per cent" or "approximately 66 percent". The latter figure was not different from that for either the other forms of psychotherapy or from his baseline estimate. At best, psycho-analysis as a therapy was the equal of other formal psychotherapies; at worst, it was less effective than non-systematic treatment.

Eysenck's unpalatable conclusion stimulated a number of re-analyses of the data. The most notable of these was undertaken by Bergin (1971). After also emphasising the inadequacies of the original data and implicitly agreeing with Eysenck about the difficulty of classifying the outcomes, Bergin concluded that, depending on how the outcomes were classified, improvement rates as low as about 40% or as high as about 90% could be calculated for the five psycho-analytic reports.

Eysenck qualified his conclusion:

> The figures ... do not necessarily disprove the possibility of therapeutic effectiveness .... Definite proof would require a special investigation, carefully planned and methodologically more adequate than these *ad hoc* comparisons. (Eysenck, 1952)

Although Bergin clearly distanced himself from Eysenck on other matters, I doubt that he would have disagreed with Eysenck's so characterising the early work.

Post-Eysenck critiques

None of the reasonably numerous post-1952 studies cited by Bergin (1971) met the criteria proposed by Eysenck but, even so, they generally returned rates falling below 50%. Lambert's (1976) analysis of waiting list

remissions yielded a median rate of 43%, a baseline possibly closer to that of a true no-treatment control than Eysenck's. Lambert nevertheless regarded even that rate as misleading: depending upon the disorder, remission rates as high as 70% were reported. [4]

Similarly, of the six studies they had been able to find comparing psycho-analysis with no treatment, S. Fisher and Greenberg (1977, p.321) noted all six had such "obvious methodological flaws or deficiencies" that the positive outcomes "could be as easily attributed to specific therapist characteristics as to the analytic treatment method employed". When compared with alternative therapies, psycho-analysis did not lead to markedly different outcomes. They concluded:

> Our review of studies of the outcome of psychoanalysis seems to come down to two general conclusions: (1) psychoanalysis has been shown to be consistently more effective than no-treatment with chronic neurotic patients, and (2) psychoanalysis has not been shown to be significantly more effective than *other* forms of psychotherapy with any type of patient. (op. cit., p.341)

Kline's later review reached a similar conclusion:

> Studies of the outcome of psychoanalytic therapy where even the minimum standards of methodology ... are satisfied simply do not exist .... Thus ... the net result of the studies ... of the outcome of psychoanalytic therapy is very small and little is yet known about its efficiency. (P. Kline, 1981, p.398)

In his more popularly written work, Kline repeated this assessment: "most studies" of the outcome of psychoanalytic therapy, "are too deficient in research design and measurement to stand rigorous scrutiny" (P. Kline, 1984, p.123). He concluded:

4. In this literature, much is made of the need to use standards for assessing outcome that are more complex than simple ratings of symptomatic change. The demand is unwarranted. Mintz (1981) had two non-professional naive judges use precisely such simple ratings of symptom change ('maximum recovery'to 'no change' or 'worse') to assess outcome and compared their ratings with ratings of the same patients made by Malan's (1976) experienced psycho-analytical Tavistock psychotherapist-judges who had used complex psychodynamic standards. The correlations between the two measures were of the order of 0.8 to 0.9. Thus the symptom description contains the same information as the complex formulations which psycho-analysts and their friends urge should be adopted and, which they insist, renders Eysenck's studies of non-systematic and behavioural treatments irrelevant. Mintz's finding is so robust that I have been able to confirm it repeatedly in student practical exercises over the last seven or eight years. Well over half a class of second year students of psychology, relatively naive even as to the meaning of the word neurosis, can generate judgements of the amount of change shown by Malan's patients that correlate 0.70 or more with the Tavistock ratings. Yet the students base their judgements *only* on Malan's descriptions of the symptoms at presentation and follow up. Wallerstein's conclusion that structural changes were not dependent on the mode of intervention is also consistent with Mintz's view (Wallerstein, 1986b, pp.720-721).

> Sadly I have been unable to report any studies which clearly demonstrate the success or failure of psychoanalytic therapy. (op. cit., p.131)

Justified melancholy indeed. Not only is it now about one hundred years since Freud pioneered his form of psychotherapy, but Kline is probably even more sympathetic to psycho-analysis than are Fisher and Greenberg! Neither do more recent reviews of psychotherapy research compel great dissent from Kline's conclusion (Bergin, 1978; Strupp, 1986; Stiles, Shapiro, and Elliott, 1986). In fact, VandenBos indicates how tenuous our knowledge remains by saying:

> By about 1980 a consensus *of sorts* was reached that psychotherapy, as a generic treatment process, was demonstrably more effective than no treatment. (VandenBos, 1986. My emphasis, MBM)

In adding, "some ... are less convinced that the controversy has yet to be fully resolved", VandenBos makes the qualification even more apparent.

Of course, to the extent the conclusion is warranted at all, it has been studies of non-analytic therapy and, at best, psycho-analytic psychotherapy rather than *psycho-analysis* proper, that have cleared away such doubts as have been resolved. There is, consequently, still no evidence to suppose VandenBos' consensus includes psycho-analysis as therapy. Fonagy (1982), it seems to me, has tried hard to bolster the claims of psycho-analysis to be effective, but even he cannot go beyond saying that it had "little to be ashamed of in the area of therapeutic outcome" or was "at least as successful as behaviour therapy", a conclusion at one with that of H. B. Lewis (1984). In addition, as Erwin (1980) shows, the fact that no alternative to the average improvement rate of two thirds for those treated non-systematically has been determined does nothing to rebut Eysenck's conclusion that "there is no firm evidence that psychoanalysis is therapeutically effective".

The Columbia Center Studies

No controlled comparisons of the outcome of psycho-analytic therapy have appeared since the above reviews. However, data relevant to outcome is reported in the most important of the studies of psycho-analytic therapy which has been published since, that from the Columbia Psychoanalytic Center (Weber, Solomon, and Bachrach, 1985; Weber, Bachrach, and Solomon, 1985a; Weber, Bachrach, and Solomon, 1985b; Bachrach, Weber,and Solomon, 1985). The improvement rates reported provide, at best, equivocal support for an effectiveness of psycho-analytic therapy greater than that of unsystematic treatment. Between 1945 and 1962, 9,000 patients were referred to the Center of whom 1,348 were accepted for treatment and who constituted the population from which Sample 1 of the study was drawn. Patients were either treated wholly at the Center or transferred to private practice after beginning at the Center. 588 were seen 4-5 times per week in psycho-analytic therapy and 760 others, comprising

two sub-groups of patients of different types, were seen twice a week in psycho-analytically oriented psychotherapy. Complete data or data in which the evaluators had confidence was available for 295 of the 588 and 286 of the 760 in both groups who had completed treatment, that is, for slightly less than half of the total.

Confining ourselves for the moment to the 295 in whom a full psycho-analysis was undertaken, 91% of the 77 who completed their analyses as private patients were given ratings of "much improved" and "improved". 86% of smaller group of 28 of these private patients who completed their treatment with analytically oriented therapy were given similar ratings. A "better" overall improvement rating was given to 56% of the 158 seen entirely at the Center and "no change" or "worse" to the other 44%. These data are broadly consistent with other more complex data. Thus 7% of private patients broke off treatment without gain as compared with the 41% of the Center patients who terminated their treatment unilaterally and were rated "unimproved".

However, in the Columbia study, treatment type, length of treatment, and independence of judgement are confounded. Before treatment even began, those patients eventually selected for psycho-analysis had been assessed as functioning at significantly higher levels than those allotted to psychotherapy, and those who were eventually transferred to private practice showed a non-significant trend to be at the highest levels of all. Patients in psycho-analysis were also seen for longer than the psycho-therapy patients, with the private patients being seen for about twice as long as those seen only in the Center. For those seen entirely in the Center the outcome was judged by psycho-analysts *not involved in the treatment* but for those transferred to private practice, the outcome was assessed *by the analyst conducting the treatment.* Thus the patients who did so exceptionally well were the less impaired private patients whose treatment took much longer and whose final status was judged by their therapists. However, for those treated with psycho-analysis in the Center and judged independently, the 56% "much improved" rate is broadly consistent with Eysenck's and later findings. So are the rates of 61% and 48% for the two subgroups of patients treated with psycho-analytically oriented psycho-therapy where the outcome was also not judged by the therapist. Treatment time for these two groups was much shorter - over 96% treated for less than one year - than for those treated with psycho-analysis.

There were virtually no differences between these findings and those of Sample 2 of the Columbia study. Sample 2 consisted of 77 cases drawn from an initial pool of 237 subjects seen at the Center between 1962-1971. 36 were treated with psycho-analysis and 41 with psycho-therapy. While there were some differences in the methods of investigation, the findings from Sample 2 cross-validated those from Sample 1. According to the Columbia investigators, there were no differences between their findings and the outcome of a number of smaller studies, reported since the middle

1970's, but not reviewed by Kline or Fisher and Greenberg. Nor were the results different in the small intensively studied sample of the Menninger Foundation Psychotherapy Research Project (Wallerstein, 1986c, Ch.23)

Therapeutic outcome as the test of a theory

Even were psycho-analysis an effective *therapy*, that fact has little bearing on the truth of psycho-analytic *theory*. Suppose that the positive changes that were supposed to take place in a therapy could be strictly deduced from a given theory of psychopathology. Under those circumstances, the therapeutic outcome could be thought of as like any other hypothesis that could be deduced from the theory: *If* the problem has the characteristics which the theory supposes it to have, *and* a particular kind of intervention that can change or modify those characteristics can be deduced from the theory, *then* the characteristics should be observed to change as the therapy is applied.

Four points need to be made about the practical and logical consequences of the therapeutic test. First, the logic of testing any theory is such that a positive result does not *prove* the theory to be true but only possibly true. This is because the result may have come about for reasons entirely unrelated to the truth of the theory. Consequently, even if psycho-analysis were an effective therapy, that fact would not prove the truth of the theory of psycho-analysis. There might be processes at work in the therapeutic situation having nothing whatever to do with psycho-analysis as such, for example, the relationship with the therapist, or the possibility for the patient to learn new techniques for coping, or some change in the patient's ability to regain control of his or her life. Second, a therapy derived logically from a theory has to bring about its changes in a completely unique way if the theory is even to be confirmed. As far as one can tell, what effects psycho-analysis does have as therapy are not unique and do not confirm it as a theory.

The third point flows from the basic asymmetry of the logic of testing a theory. A positive result only confirms the theory but a negative one may *disprove* it. However, a negative therapeutic outcome is not quite as unproblematical as it might seem from Scriven's argument that "negative results count heavily against [psycho-analytic] theory, positive ... count weakly, if at all for it" (Scriven, 1959). A disorder or disease may be fully understood but be completely resistant to treatment. For example, many years elapsed between the identification of the causes of diseases like tuberculosis or syphilis and the development of effective treatment for them. The lack of a therapy for AIDS provides a modern example. Thus, it is perfectly possible to imagine psycho-analytic theory giving a correct etiological account of a disorder but having little or nothing to offer in the way of effective treatment of it. A negative therapeutic outcome would be irrelevant to the truth or falsity of psycho-analysis as a theory in that case.

The fourth point concerns the practical consequences of a positive outcome and is not germane to the logic of the test. Those who use, plan, or administer clinical-psychological and psychiatric services need to be able to choose between therapies on the grounds of their effectiveness and cost. Fisher and Greenberg summarised the practical evidence about psycho-analysis by concluding that "a patient suffering from chronic neurotic symptoms would do better with psychoanalysis than without".

"However," they went on to say:

> there is at present no justification for a patient to assume that he will achieve a greater degree of improvement in a therapy called psychoanalysis than in a therapy given another label such as analytically oriented, client-centered, or behavioral. There is virtually no evidence that psychoanalysis results in more long-lasting or profound patient change than other therapies. (S. Fisher and Greenberg, 1977, p.341)

There is no reason to choose psycho-analysis over other therapies, especially if its cost and duration are taken into account.

Whatever practical importance attaches to the question of the effectiveness of psycho-analysis as a therapy, the therapeutic arena is simply not the place on which to establish its truth as a theory.

CONCLUSION

Psycho-analysis as a theory of personality has little to recommend it. The formal deficiencies of much psycho-analytic explanation is fairly obvious but we ought to ask what needs explaining. Do processes like condensation and the summation of stimuli occur? Is there such a mechanism as repression? Is there a transformation of the primary process into the secondary? Is there an Oedipus complex out of which a super-ego forms and from which the ego is provided with defused energy? Is the development of adult sexuality, character traits, and object-choice as Freud described them? Is female sexuality as Freud pictured it? From these points of view, psycho-analysis is not so much a bad theory, but a theory in search of some facts.

Neither does psycho-analysis have any particular positive advantages as a psychological treatment. Nor does it possess any quality marking it as a unique therapy. What then of its value as a method of enquiry into human behaviour and mental processes? That is the question to be taken up in Chapter 15.

15 PSYCHO-ANALYSIS AS A METHOD

Cecily: That certainly seems a satisfactory explanation, does it not?
Gwendolen: Yes, dear, if you can believe him.
Cecily: I don't. But that does not affect the wonderful beauty of his answer.

Wilde: *The Importance of Being Ernest*, Act IV.

The question I attempt to answer in Chapter 15 is whether the psycho-analytic method is a reliable and valid one. That is, even if psycho-analysis has limitations as a theory of personality and a type of psychotherapy, does it nevertheless provide us with an objective means for establishing the facts about mental life?

Because the theoretical foundations of the basic method of psycho-analysis - free association - were examined in Chapter 4, I begin Chapter 15 by considering how it is used and evaluating the evidence for its objectivity. Although the assumptions on which the method is based are extremely plausible, its results are too variable for it to be considered a reliable and valid means for collecting data. Free association evidently *creates* its data rather than *recovers* it. In addition, there are not and can not be any guidelines to how these data should be interpreted or how they should be used in constructing or reconstructing the patient's past. Because there cannot be rules about how to use the data gathered by free association, the important differences between psycho-analysts about the basic characteristics of personality are, inevitably, irreconcilable. As a method, psycho-analysis is not able to give us data to help us decide between differing psycho-analytic conceptions of personality. Nor is it capable of providing us with real knowledge about the facts of human behaviour and mental life.

GATHERING THE DATA

Free association is *the* basic method of psycho-analysis. The patient focuses attention on an element of the dream, parapraxis, or symptom being analysed, suspends his or her critical attitude, and then reports all the ideas that force their way into consciousness. We have seen that Freud believed these ideas were not random. Gaps that did occur could be filled and broken causal connections restored. Trains of associations eventually led to the causes of the phenomenon being analysed. When he spelled out the procedure in detail, Freud emphasised the role of the idea with which the patient began. He asked his patient:

> to surrender himself to free association *while keeping an idea in mind as a starting point.* (Freud, 1916-1917, p.106)

Why was this? Freud believed that any thought chosen "quite freely" was determined and actually belonged to a connected whole. He argued that the same determinism was at work when a starting idea was given:

> we shall no doubt be justified in concluding that things that occur to one with a single link - namely their link with the idea that serves as a starting point - cannot be any less determined. (op. cit., p.108)

His investigations showed, he said, that apart from their connection with the starting point, the ideas so generated were also dependent on the patient's unconscious emotional thoughts and interests (ibid.).

For the train of associations to be influenced only by the patient's unconscious ideas, the psycho-analyst had also to adhere to a version of the fundamental rule. He or she had to maintain what Freud called "an evenly suspended attention". This meant attending to the patient's associations without paying attention to anything in particular and witholding their own conscious influences from them. Put purely in terms of technique, the psycho-analyst "should simply listen, and not bother about whether he is keeping anything in mind" (Freud, 1912d, pp.111-112, 115. Cf. 1909a, p.65, 1926b, p.219). By analogy, the psycho-analyst had to:

> turn his own unconscious like a receptive organ toward the transmitting unconscious of the patient. He must adjust himself to the patient as a telephone receiver is adjusted to the transmitting microphone .... the doctor's unconscious is able, from the derivatives of the unconscious which are communicated to him, to reconstruct that unconscious, which has determined the patient's free associations. (Freud, 1912d, pp.115-116)

As a receiver, the psycho-analyst was supposed to treat the patient with the coldness and detachment of a surgeon. He or she was not even to take notes, since this would already be to select from the material being produced (Freud, 1912d, p.113). In order to reduce the influence of the analyst's facial expressions, Freud even went so far as to promote the virtues of sitting behind the patient during therapy (Freud, 1913a, p.134). The method had long term applications. As long as analysts and patients gave up "their conscious purposive aims" and did not "dispute the guidance of the unconscious in establishing connecting links", a dream might be interpreted many days after it had been dreamed (Freud, 1911c, p.94).

Three things should be emphasised. The first is that Freud did not ask his patients just to talk about anything that came to mind. Most of the contributors to the 1970 Panel discussion of the American Psychological Association on "the Basic Rule", as well as most of the experienced psycho-analysts surveyed much later by Lichtenberg and Galler, seem not to have realised this (Seidenberg, 1971; Lichtenberg and Galler, 1987).

The theoretical underpinnings of the method, which I discussed in Chapter 4, required Freud's patients to free associate to a starting idea. It was that beginning which directed their thoughts toward the ideas that caused the dream, parapraxis, or symptom. The singularity of the method has also to be insisted on. It was never one of *three* techniques by which Freud believed the unconscious could be discovered, as Laplance and Pontalis mistakenly believe. They clearly confuse the *three phenomena* of symptoms, dreams, and faulty actions, which were all supposed to yield their secret causes to the method, with the method itself (Laplanche and Pontalis, 1967/1973, pp.178-179. Cf. Freud, 1910a, pp.33-37). Finally, free association is a technique for investigating thinking, not a type of thought content. However, there is now such a basic lack of understanding about free association among psycho-analysts that Gill has had to stress the obvious point that "free association should not be defined as chaotic 'deep' material - in fact not according to its content at all" (Reppen, 1982).

THE VALIDITY OF THE METHOD

Freud thought his method of analysing symptoms, based as it was on the way the fundamental rule was applied, was robust enough to guarantee "to a great extent ... that nothing will be introduced into it [the structure of the neurosis] by the expectations of the analyst" (Freud, 1925a, p.41). His claim was the development of an earlier one, first expressed in the *Studies on Hysteria* which I discussed in Chapters 4 and 8, that he had never been able to force memories of traumatic events on to his patients (Breuer and Freud, 1895, p.295). What this view reflected was Freud's belief that psychological phenomena had internal determinants.

Does Freud's belief in psychic determinism hold for free association? Is it as objective a method as he thought? Not only is this issue not discussed in the psycho-analytic literature, most psycho-analytic authors show such slight comprehension of what Freud meant by psychic determinism that they couple it wholly or partly with what are, in this context, the quite irrelevant issues of free will, pre-determinism, or questions of will, ethics and morality (e.g. Oberndorf, 1943 and Galdston's discussion; Knight, 1946; Lipton, 1955; Wheelis, 1956; Angel, 1959; Hoffman, 1964; L. Friedman, 1965; Waelder, 1963; Kanzer, 1968; Macklin, 1976; Basch, 1978; Phillips, 1981). In the psycho-analytic literature to 1952 there were, according to Zilboorg, only the two works by H. Hartmann and Anna Freud, which dealt with "the whole problem of free associations and of the fundamental rule" at more than the practical and utilitarian level (Zilboorg, 1952a, 1952b). Neither Hartmann nor A. Freud took up the issue of objectivity. About ten years later, Bellak (1961) could still describe the literature dealing with the rule as "scanty" and that "systematic metapsychological consideration is nonexistent.

Freud's concept has as little to do with free-will and predetermination as with content. What it does have to do with is *causality* (Waelder, 1963). Brill (1938-1939) stressed how "the psychoanalyst uses free association in order to find the origin of symptoms" and it was "equivalent to a search for the determinants" of traumatic episodes. In outlining how to use free association, Freud was being consistent with Meynert. Recall the significance given the sound of the lamb by Meynert and Breuer-Freud: as one element of a previously experienced association the sound allowed the logical inferences that a lamb was present and that it was the cause of the sound. Associations evoked by a starting idea were causes or links in a chain of causal associations which terminated in the causal idea.

The evidence from the clinic

Almost from the beginning there were those who questioned both the rule and the claims based on it. Determinism *per se* was not at issue - most critics accepted a psychological form of determinism (e.g. Wells, 1912). They asserted that there was no doubt parapraxes were caused but, like Roback, asked why it was necessary "to *create* a cause when the direct antecedent is in most cases apparent" (Roback, 1919). Generally the critics argued that free associations were too affected by suggestion to be reliable sources of facts and that psycho-analysts arbitrarily interpreted such facts as the method did provide.

Some of these doubters had never been especially sympathetic to psycho-analysis (Ormerod, 1910-1911; Woodworth, 1917) but others had once had a degree of fellow-feeling (e.g. Sidis, 1906-1907, 1912, 1918; B. Hart, 1916, 1929), some even to the extent of practicing analysis and defending the assumption before changing their view (e.g. Tannenbaum, 1917, 1922, 1923a, 1923b; Petersen cited by Cioffi, 1973). Others began with doubt but were won over (e.g. Putnam, 1906, 1910. Cf. Vasile, 1977, pp.x, 60, 74-75). I believe a major reason for these various positions, especially for the changes in them, was the paucity of facts about free association. From the earliest years most of what was said either for or against it was based solely on general logical considerations or clinical opinion (e.g. Schroeder, 1919; B. Hart, 1929, pp.67-77).

Studies of free association

When questions about the validity of the method of free association were raised, Freud sometimes referred to Jung's experiments using the word-association test (e.g. Freud, 1900, pp.531-532 and footnote of 1909; Freud, 1906b; Freud, 1910a, pp.29-30. Cf. E. Jones, 1911b). Jung's work illustrated that associations given to stimulus words may have unconscious determinants but is not relevant to the objectivity of free association as a method for gathering data. Jung nowhere discussed in any detail the associationist basis of his technique or the relation between his and Freud's methods (Jung, 1906/1973a, 1906/1973b, 1910). Consequently, his studies tell us nothing about the degree to which free associations given by the

patient *in the therapeutic situation* are determined *solely* by the patient's unconscious processes.

If Sears' reviews of studies of psycho-analytic concepts and phenomena can be taken as a guide, no investigation of any kind into the objectivity of the primary method of psycho-analytic data collection had been conducted by the end of the 1930's. Neither of his reviews even mentions the technique (Sears, 1942, 1944). That situation seems not to have changed over the last fifty years. Not a single study of free association is reported by Hilgard (1952, 1968), Eysenck and Wilson (1973), or P. Kline (1981) and little of what Fisher and Greenberg have to say about it touches on the validity of the method (S. Fisher and Greenberg, 1977, pp.386-388).

I have found only two studies of free association *per se*. Both provide clear evidence that the content and number of associations is influenced by situational factors as simple as whether another person is present or whether the verbal intervention of the experimenter is a question or a statement suggesting causality (Colby, 1960, 1961). There are no studies at all of how the method is actually used in therapy, only some as yet still unfulfilled methodological preliminaries to that topic (e.g. Bordin, 1966a, 1966b).

The Luria technique

The only other claim of "experimental evidence for the validity of the technique of free association" seems to be that made in 1950 by Pumpian-Mindlin for Luria's work on hypnotically produced conflict (Pumpian-Mindlin, 1952, p.141). What Luria did was to gather chains of associations from his subjects before and after they had been hypnotized. During hypnosis the suggestion was put to them that they had been involved in some traumatic incident, usually a crime. Words related to the suggested trauma were common among the free associations given after the suggestion whereas those given before were all entirely normal. For example, Luria suggested to one subject that she had produced an abortion in a room at the top of a staircase. The post-hypnosis associations included words like wound, forceps, scalpel, operation, instruments, nurse, and staircase. The motor reactions accompanying the associations were also abnormal (Luria, 1932, pp.149-161). Free association seemed capable of leading back to the 'trauma'.

Modern hypnosis research gives three reasons for thinking that findings like Luria's are not strong enough to bear the implication Pumpian-Mindlin and others want them to carry (e.g. Brickner and Kubie, 1936; Huston, Shakow, and Erickson, 1934). First, given that Luria's experiments were conducted in the middle twenties, it is not surprising that they fail to meet present-day methodological criteria, particularly in lacking simulation controls. Luria's subjects, like those in most later experiments, could have 'unconsciously' discerned the demand characteristics embedded in the experimental situation and behaved in accord with

them (Huston, Shakow, and Erickson, 1934; Erickson, M. H. 1935, 1944; J. Eisenbud, 1937; Young, 1941; Wolberg, 1947; Bobbitt, 1947/1958; Counts and Mensh, 1950; W. F. Moore, 1964). In fact, appropriate controls in later experiments on hypnotically suggested trauma do show that most of the associated phenomena result from demand characteristics. The different positions on this topic are covered in the papers by Reyher (1962, 1967), Deckert and West (1963), Gordon (1967), and Sheehan (1969).

Second, hypnotic amnesia is not as absolute as was once thought. Consequently, the parallel between amnesia for a suggested 'trauma' and that supposed to be brought about by repression is not as close as Luria supposed and as Pumpian-Mindlin's inference requires (Sheehan and McConkey, 1982, Ch. 7; Kihlstrom, 1985). Third, what would have happened had the subjects' expectations been systematically varied? Could subjects have been led to disregard the 'real' trauma and produce evidence of a 'false' one instead? This is the most important of the questions and there are no investigations of it at all.

Hypnotic investigations of dreams and parapraxes

The methodological criticisms that can be made of the work on hypnotically 'implanted' trauma also apply to most of the related work on hypnotically produced parapraxes and to dreams suggested or interpreted under hypnosis (Schroetter, 1911/1951; Roffenstein, 1924/1951; Nachmansohn, 1925/1951; Erickson, M. H. 1939; Farber and Fisher, 1943; Mazer, 1951; M. V. Kline, 1963). Most authors, including psychoanalysts, now doubt that hypnotically produced dreams are equivalent to their nocturnal counter-parts (Brenman, 1949; Gill and Brenman, 1959; Domhoff, 1964; Tart, 1965; Moss, 1967; Witkin and Lewis, 1967; Hilgard and Nowlis, 1972; Levitt and Chapman, 1979; Sheehan and Dolby, 1979). Where the dream experiments have been repeated, it has not proved easy or even possible to duplicate the results (Kaywin, Hilger, and Finzer, 1948, cited in Rapaport, 1951, p.252, n.11; Gill and Brenman, 1959, pp.348-351; Barber, 1962; Schneck, 1963; Moss and Stachowiak, 1963; C. Fisher, 1966). The positive results were very probably due to a combination of the enthusiasm of the experimenter and the knowledge of the subject of the particular dream theory being investigated (Reis, 1951, cited in Barber, 1962; Rapaport, 1951, pp.240-241 and n.13, p.252 and n.11, p.268 and n.29; Barber, 1962; Tart, 1965; C. Fisher, 1966; Moss, 1967; Evans, 1979).

Determinants or associations?

Since at least 1912 the objection has been made that the relation between associations and the unconscious ideas to which they seem to lead actually lack the strict determination Freud hypothesised (e.g. Wells, 1912, 1913). Associations to anything, even ideas selected at random, will lead, it was said, to a pressing problem whether the initial idea or the trains of thought are connected with it causally or not. Hence, Woodworth (1917) questioned how seriously it could be maintained that a thought 'B', to

which associations from 'A' led, was the cause of 'A' rather than that it was simply one of the subject's ideas. In rebuttal, Tannenbaum could call only on Freud's "Years of experience, corroborated by similar experiences of hundreds of other investigators" (Tannenbaum, 1917). E. Jones (1911b) gave the same response along with some other equally irrelevant comments.

In a 1920 footnote to the *Psychopathology of Everyday Life* Freud seemed to grant that more than a call to experience was needed. He had claimed that even associations to numbers chosen at random led back to determining ideas in the same way as those which were supposed to cause parapraxes and dreams. Schneider tested whether this might be only an apparent determinism by asking for associations to numbers chosen *by others*. Freud accepted that these associations "provided determinants just as abundant and full of meaning" as when subjects chose the numbers themselves. However, he asserted that Schneider had gone "too far" in concluding that numbers chosen spontaneously had not originated "from the thoughts discovered in the 'analysis' of them". He evaded Schneider's point by arguing that associations to numbers so *presented* said "nothing more" about the origin of *spontaneously chosen* numbers than was known before Schneider's experiment. Freud did allow that "a critical examination of the problem and with it a justification of the psycho-analytic technique" was needed but said that topic lay "outside the scope" of his book (Freud, 1901b, p.250, n.2).

Freud's concession, if concession it was, was much less generous than Flugel's about Wohlgemuth's similar investigation. Wohlgemuth generated his own associations to numbers chosen *by Freud* and said he was able to analyse them "with the same ease and elegance" as Freud (Wohlgemuth, 1923, pp.214-216. Cf. Freud, 1901b, pp.242-243, 246-248). Flugel saw that if this were true Freud's theory had to be "regarded as unproved", at least in regard to the evidence from associations. Because they "might prove very useful and illuminating", Flugel urged that experiments on number determination be undertaken and extended to dream analysis. He mentioned Bleuler as "the only investigator to have realised the value of work along these lines" but dismissed Bleuler's findings as "too few and unsystematic" to be of value (Flugel, 1923). But, as with the Schneider-type investigations Freud had intimated, nothing was ever published. No counter-attack was ever made to regain the position Flugel had so quietly surrendered and, until Sand's recent paper, the matter seems not even to have been raised in psycho-analytic circles (Sand, 1982).

The tendency not to reply to the criticism that free association generates associations and not determinants is well established in the psycho-analytic literature. Illustrating this point is the fate of Tannenbaum's criticism of Freud's use of free association to analyse one of his most commented-on parapraxes. After he had moved away from psycho-analysis, Tannenbaum (1922) re-examined the 'aliquis' slip which

Freud had described in *Psychopathology of Everyday Life* (Freud, 1901b, pp.8-14). The word had escaped the memory of a young man who was trying to recall Virgil's line *Exoriar(e) aliquis nostris ex ossibus ultor* so that what he produced was *Exoriar(e) ex nostris ossibus ultor*. From the association of 'liquid' to 'liquis' an indirect train of thought led to the young man's concern that he might have made a lady friend pregnant. As Stephen (1918-1919) had done before him, Tannenbaum pointed out that had any other word in the line been forgotten, the worry over the possible pregnancy would have been reached just as certainly. He then asked the further question why the anxiety did not cause "the forgetting of the word 'exoriare' (exorcism, expulsion, abortion) which can be so much more directly linked up with the presumably apprehended gestation". Because there had been no answer after some fifty years, Timpanaro seems to have felt impelled to ask the same question. On the basis of a much deeper and detailed philological re-analysis of the slip, he showed that almost any of the elements in the line can be made to have links with the supposed causal idea, and not just 'exoriare'. Like Tannenbaum, he pointed to the contradiction of Freud's explanation requiring an unconscious that was simultaneously primitive and linguistically sophisticated. Timpanaro was also able to show 'aliquis' to be part of an almost unique grammatical construction which would be strange and ambiguous, even to a Latin speaker, and therefore the word in the line most likely not to be recalled. The error bears the hallmark of a 'banalization', that is, an error of the kind made in the use of a language by one not completely familiar with it. 'Aliquis' is also the one word least necessary to the meaning of the whole. (Timpanaro, 1974/1976, pp.28-61, 219-220).

USING FREE ASSOCIATIONS

I now turn to four of the uses to which the data gained from free association is put: translating dream elements, interpreting dreams, symptoms and parapraxes, constructing or reconstructing significant events in the patient's history, and building up a complete narrative of the patient. The topics are considered sequentially.

Psycho-analysis as translation

Several times Freud drew an analogy between dreams and languages in which the differences between the manifest dream and the latent dream-thoughts were supposed to parallel those between one language and another. Interpreting the dream was therefore like translating. We need to consider what Freud meant by translation and ask whether it is possible.

Well before Freud's systematic interest in interpreting dreams had developed, he and Breuer had used the language analogy to explain hysterical symptoms. His 1895 discussion of the hysterical vomiting of Katharina, whom Swales (1988) has shown was seen by Freud in August

1893, indicates that they had used it generally:

> We had often compared the symptomatology of hysteria with a pictographic script which has become intelligible after the discovery of a few bilingual inscriptions. In that alphabet being sick means disgust. (Breuer and Freud, 1895, p.129)

When Freud applied the analogy to dreams he pointed out that they occupied "a far more unfavourable position than any of these ancient languages and scripts" (Freud, 1916-1917, p.231) and implied elsewhere this was also true of symptoms and parapraxes.

Problems with the analogy There are three reasons why the analogy between translation and the understanding of dreams, symptoms and parapraxes breaks down.

First, as Freud pointed out, even what he called primitive languages were:

> intended for communication: that is to say, they are always, by whatever method and with whatever assistance, meant to be understood. But precisely this characteristic is absent in dreams. A dream does not want to say anything to anyone. It is not a vehicle for communication; on the contrary, it is meant to remain ununderstood. (Freud, 1916-1917, p.231. Cf. Freud, 1900, p.341)

Although the patient's telling a dream was a communication, even then it was:

> a communication made by inappropriate means, for dreams are not in themselves social utterances, not a means of giving information. (Freud, 1933b, p.9)

Translation was supposed to transform the dream into a communication (ibid.). But if the manifest 'language' is not one which communicates, is this really possible? (Cf. Freud, 1900, pp.277-278; 1901b, p.5; 1916-1917, pp.175-177, 229-231; 1933b, p.20).

Second, the would-be interpreter of dreams has no rules for semantic substitution. Freud described dream interpretation as resembling the decoding method of the ancient dream-books in which the fixed meanings set out in the dream-book were substituted one at a time for each of the separate manifest elements. But, except for the act of substitution, what the psycho-analyst really does is not at all comparable.

To begin with, simple replacement of the elements produced by the dream-work is not possible because, with the exception of a limited group of symbols, the elements do not have fixed meanings. It is precisely that which made it necessary for the individual to provide associations to the elements of his or her dream (Freud, 1901a, p.684; 1916-1917, pp.150-151). Not even the dream-thoughts of the individual patient were

expressed through a lexicon of fixed meanings, and even the common symbols Freud claimed to have discovered, and which in dreams were used "almost exclusively" to express sexual objects and relations, sometimes functioned as symbols and sometimes as themselves (Freud, 1900, pp.345, 352; 1901a, p.684).

Dream-thoughts were so likely to be represented directly as by their opposites that there was an "interpretative rule":

> every element in a dream can ... stand for its opposite just as easily as for itself. We can never tell beforehand whether it stands for the one or for the other; only the context can decide. (Freud, 1900, p.471. Cf. pp.318)

Freud summed up these difficulties by saying that it was doubtful if any dream-element should be taken in a positive or negative sense or interpreted as a symbol, an historical recollection, or according to its wording (Freud, 1900, p.341).

There is also what linguists term the many-to-many mapping problem, caused in this instance by condensation. Condensation prevented "a faithful translation or a point-for-point projection of the dream-thoughts" simply by omitting elements (op. cit., p.281). Manifest elements were determined "many times over" by the latent thoughts which were themselves represented in the dream several times over. That is:

> Associative paths lead from one element of the dream to several dream-thoughts, and from one dream-thought to several elements of the dream. (op. cit., p.284)

Thus, a "manifest element may correspond simultaneously to several latent ones, and, contrariwise, a latent element may play a part in several manifest ones" (Freud, 1916-1917, p.173). No simple disentangling of the dream-thoughts from the condensed manifest elements was possible.

Third is the absence of syntactic rules. The production of the manifest elements by the dream-work is not governed by anything having the character of the grammatical rules of a language. True, there are rules but they lack even the definiteness of those enabling ambiguous expressions to be deciphered. For example, because of the requirement of pictorial representability, none of the conjunctions needed for understanding speech or sentences could be represented at all. Thus, when the dreamer said that something in the dream was '*either* one thing *or* another', the analyst had to take the expression to mean 'one thing *and* the other'. Deciphering causal propositions presented a particular problem. The dream might be divided into two parts, one for the principal and the other for the dependent clause, or the dreamer might report having seen one image being transformed into another. But it did not follow that what came first was the cause rather than the effect and, in the case of the two-part dream, the division did not always represent a causal relation (Freud, 1900, pp.312-318; 1901a, pp.650, 661; 1913e, pp.176-178; 1916-1917, pp.178-180;

1933b, pp.19-27).

Deciphering ancient scripts Despite Freud's suggestions to the contrary, the interpretation of dreams was by no means "completely analogous to the decipherment of an ancient pictographic script such as Egyptian hieroglyphics" (Freud, 1913e, p.177. Cf. Freud, 1916-1917, pp.229-232). Even with my minimal knowledge of how ancient language records have been deciphered, I know that in every case there was at least a partial parallel record in another already known script. For example, the beginning of our understanding of Egyptian hieroglyphs came from Young's alignment of eighty-six groups of demotic signs on the Rosetta stone with words of known meaning in the parallel Greek section. Young then demonstrated that the name of King Ptolemy, which occurred six times in the Greek and demotic scripts, was enclosed in each of the six otherwise incomprehensible cartouches in the hieroglyphic section. His study of this and other scripts led to his more-or-less correct deciphering the hieroglyphic equivalents of some eighty demotic words. The cuneiform alphabet similarly began to yield its secrets once it was conjectured that the names of known kings and known ritual phrases could be substituted at the beginnings and ends of the messages inscribed on the tablets. Similarly, Linear B was understood once Ventris had correctly surmised that it was a form of ancient Greek. That so many ancient languages remain unknown to us is largely because dual scripts have still not been found.

Freud's comparison of the reliability of dream interpretation with that of cuneiform decipherment was also very wide of the mark (Freud, 1916-1917, p.232). He cited the experts who had been tested by the Royal Asiatic Society in 1857 and who were able to produce a good deal of agreement about the meaning of a cuneiform text given them as a test. What agreement they did reach was possible precisely because by then there were two scripts and some of the semantic and syntactic rules were known. Similarly, when Freud had earlier drawn his famous parallel between archaeology and hysteria, even he allowed his hypothetical explorer the luxury of obtaining his information about "the events of the remote past" through a successful deciphering of *bilingual* inscriptions (Freud, 1896c, p.192. Cf. Breuer and Freud, 1895, p.129).

Solving a rebus Freud's much-commented-on comparison of the dream with a rebus is similarly misleading (or perhaps is construed in a misleading way). Both the manifest dream and the rebus are nonsensical of course, but Freud does not say that a dream *is* a rebus but that it is "a picture puzzle *of this sort*" (Freud, 1900, p.278. My emphasis, MBM). He could not have gone further. A rebus is solved by replacing its peculiar pictorial elements with other elements (syllables and words) from a language already known to the reader. It is not a script in another language so much as a variant and peculiar 'orthography' of the one language. So, when the puzzle-solver makes the substitutions, he or she at least knows

the rules governing the language into which the ''translation'' is to be made. The rules or conventions followed by the rebus maker can be discovered and may be partly known beforehand, as they often are to inveterate puzzle and cryptic crossword enthusiasts (Cf. Wohlgemuth, 1923, pp.65-67). [1]

Little of what is done in solving a rebus can really be applied to dream translation. According to Freud, the dream translator has to compare ''the original and the translation'' in order to discover the ''characters and syntactic laws'' of the manifest content and so make the dream thoughts ''immediately comprehensible'' (Freud, 1900, p.277). But, it is precisely that comparison which cannot be made. There is no original. The second script of the dream-thoughts does not exist in advance of the 'translation'.

A second script? Fifty years after Freud's death the meaning of the second script, if it exists, remains elusive. Of the popularity of Lacan's formulation of the unconscious as a language, the Italian philologist Timpanaro remarked:

> It is a matter for wonder that, for all the talk so little - if any - progress has been made in the formulation of the rules which must then govern the logic, grammar and lexis of the unconscious. (Timpanaro, 1974/1976, p.221)

It would obviously be absurd, he went on:

> to expect a kind of standardized grammar of the unconscious ... in which everything was rational and relied on a one-to-one correspondence between signifier and signified .... But however unencumbered and free-ranging, this language must still have its code. (ibid.)

One cannot but agree. Again it follows that it would be ludicrous to try and test psychoanalytic 'translators' in the same way as the Royal Asiatic Society tested the early translators of cuneiform. There is no other language to translate into.

Psycho-analysis as interpretation

Under 'interpretation' I evaluate the use to which psycho-analysts put the *sets* of elements already deciphered or translated after having been retrieved by free association.

Generally it is the case that the psycho-analyst regards interpreted data as more relevant to the testing of psycho-analytic propositions than observational data. Two problems have to be considered. The first is the

1. I owe this apt clarification of the nature of a rebus to Dr. D. C. Bradley. Recognising that the rebus is not a second language at all but only a variant form of writing has much deeper consequences than Jones' criticism that the rebus is consciously designed to dissemble. That criticism points only to the inconsistency of a dream work that is logical (R. M. Jones, 1965, 1970/1978, pp.7-9).

extent to which psycho-analysts can agree on the interpretation of a given behaviour, a dream, or a symptom. That is, how reliably can interpretations be made? The second is whether the interpretation is valid. Is it true or correct? Except for this second question, some of what I have considered as 'translation' could just as easily have been discussed here.

The reliability of interpretations We saw in Chapter 9 there were no rules for interpreting dreams. From that fact alone we would not expect psycho-analysts to be very reliable in their other interpretations, an expectation confirmed in the variety of re-interpretations of Freud's and Dora's dreams reported in Chapter 9 and in the varying interpretations of masochistic behaviour noted in Chapter 12. More formal studies of reliability are also consistent with these expectations. Most of the studies to about 1969 are examined by Wallerstein and Sampson in the wider context of the difficulties of basing psycho-analytic research on the clinical situation and Fonagy deals more briefly with subsequent work (Wallerstein and Sampson, 1971; Fonagy, 1982). Whether the groups of analysts who were studied considered dreams (Zane, 1971), transference (Luborsky, Graff, Pulver, and Curtis, 1973; Lower, Escoll, Little, and Otenberg, 1973), or more general aspects of behaviour in psychotherapy (Sargent, 1961; Seitz, 1966; Strupp, Chassan, and Ewing, 1966) none was able to arrive at a system that ensured even minimal agreement between different interpreters. So recalcitrant is the problem that at least one research group, that from the Chicago Institute for Psychoanalysis, gave up hope after three years and disbanded (Seitz, 1966).

The validity of interpretations Many analysts have argued or implied that the most appropriate place for testing the validity of psycho-analytic interpretations is the treatment setting itself (e.g. Isaacs, 1939; Kubie, 1952; Brenner, 1955; Schmidl, 1955). After an interpretation of the patient's behaviour is formulated, the subsequent behaviour of the patient is examined for confirmation or disconfirmation of the interpretation. The interpretation does not have to be put to the patient: the analyst may simply examine other aspects of the patient's more general behaviour. When the interpretation is made to the patient, however, it is generally suggested that the confirming or disconfirming signs be sought in some aspect of the patient's more-or-less immediate reaction. Psycho-analysts have discussed various problems of so using interpretation, sometimes suggesting refinements or additions, but the problems have not been resolved (e.g. Wisdom, 1956, 1967; Ezriel, 1951; Arlow, 1959; H. Hartmann, 1959; Lustman, 1963; Sargent, 1961; Wallerstein and Sampson, 1971; Bowlby, 1979, 1980; Rubinstein, 1980; Edelson, 1983, 1984).

The simplest way of establishing the validity of an interpretation might seem to be to take the subject's accepting it as confirmation. For example, a dream might suggest the hypothesis that the patient harbours unconscious sexual wishes for the mother. If the subject accepts the interpretation it might be claimed she really has such feelings, that is, the interpretation is a

valid one. The problem with this test is that it is indeterminate. Whether the hypothesis is true or false, it is not possible to state in advance, either from the hypothesis alone or in conjunction with other facts, whether the subject will accept or reject a given interpretation.

Consider the two bases for the subject rejecting an interpretation. Non-acceptance could mean *either* that it was incorrect *or* that it was correct but resisted because of, say, unconscious resistance. If these two reactions are difficult to distinguish, and there is reason to suppose they are, rejection is not clear evidence. On the other hand, if the subject accepts the interpretation it does not necessarily mean it is valid. It is not only possible to envisage a patient accepting an incorrect interpretation, but we have many notorious instances of agreement with false interpretations to show it is an actuality. For example, Freud's own patients agreed they had been 'seduced', Rank's that the trauma of their births caused their present anxiety, and so on.

Wisdom (1967) pointed to the major problem of finding a way of ruling 'suggestion' out from the confirmation. He formulated criteria which he thought would enable the analyst to do just that but I believe their use would not have detected what happened in Mendel's 'experiment'. Mendel examined the effects of *false* interpretations - in what appears to be the only investigation relating to this kind of suggestion. On each of six different days Mendel offered four patients one of six different and spurious interpretations of their behaviour. On any particular day the interpretation was the same for each patient and was made exactly ten minutes into the therapeutic session, irrespective of the content of the discussion at that time. On twenty of the twenty-four occasions, the patients experienced "enhancement of understanding ... and a forward movement in the therapeutic transaction" (Mendel, 1964).

Mendel's 'experiment' matches psycho-analytic *practice*. By the mid-thirties it had become apparent that psycho-analysts varied considerably in the ways they used interpretation, including what and how they interpreted, but that outcomes depended only broadly on this and other technical procedures (Glover, 1937). Psycho-analysts now agree that interpretations may have positive effects whether they correspond to what is actually the case or not. Nor is 'suggestion' excluded when interpretations are true: "truth is not even *necessary* for therapeutic efficiency" (Sherwood, 1969, p.250. Cf. Glover, 1931b, 1954, 1968; Schmideberg, 1939; Ezriel, 1951; Barratt, 1976; Spence, 1976, 1982a; Eagle, 1980a; Reppen, 1982; Wetzler, 1985; Sass and Woolfolk, 1988).

Reactions more complex than acceptance and rejection have also been proposed but the logical problems remain. True or false, an interpretation may be rejected or accepted, or have no agreed on effect, or produce a positive effect. Interpretation therefore provides an uninformative test of psycho-analytic hypotheses.

Pseudo-confirmation In 1952 Glover drew attention to the fact that because analysts did not apply to their observational data such controls as were appropriate:

> a great deal of what passes as attested theory is little more than idle speculation, varying widely in plausibility. (Glover, 1952)

He went on to stress how "in any given case interpretation is an essential part of the process of psycho-analytical investigation" before adding:

> that nevertheless there is as yet no effective control of conclusions based on interpretation, is the Achilles heel of psycho-analytical research. (op. cit.)

Glover pointed particularly to the way the uncertainties of the interpretive situation encouraged acceptance of the views of senior and prestigious psycho-analysts rather than the adoption of scientifically established propositions. Nearly twenty years later, Wallerstein and Sampson (1971) said that appeal to authority was still common.

Adding to these problems, Rapaport noted how the absence of rules led to pseudo-confirmations:

> We must be wary lest we smuggle in the confirmation (of the prediction) through the interpretation. Axiomatization and/or a canon of investigation protect other sciences from such circularity ... as things stand, there is no canon whereby valid interpretation can be distinguished from speculation, though *ex post facto* the experienced clinician can distinguish them well. (Rapaport, 1960)

Not everyone would share Rapaport's faith in the virtue of experience.

Among the factors Glover discussed in 1952 that militated against research in psycho-analysis was the undue neglect of criteria "to control the validity of interpretation" (Glover, 1952). Such canons as have since been suggested, for example, the system Sargent, Horwitz, Wallerstein and Sampson (1968) outlined for avoiding circularity, V. H. Rosen's (1969) ten criteria for helping to judge the validity of interpretations, or Rubinstein's (1980) more recent description of the kinds of behaviour that could confirm an interpretation, have not only not been adopted, they have been widely criticised or resisted (Fonagy, 1982; Kaplan, A. H., 1981). Consequently, we do not know if any of those of the 'findings' about personality which are based on interpreted data could meet any of them. Even when Freud's material is used, it is possible to provide a detailed re-analysis of a case like that of Little Hans - the first Oedipus - as not verifying "the classical theory of the Oedipus complex" (Garrison, 1978. Cf. Fromm, 1970).

The double construction What hardly anyone notices in discussions about rules for interpreting dreams and symptoms is that the latent content is actually *constructed* during interpretation, rather than *discovered* by it.

There can be no rules for arriving at a correct interpretation because the absence of a second script prevents any rules from ever being formulated.

What Freud did when he interpreted a dream was to use *the same material* - the patient's (and his own) associations - to construct *both* the dream thoughts *and* the rules for transforming them. The latent dream is "not a dream at all but an interpretive construction" (Foulkes, 1978, p.45). Put another way, Freud's method of interpreting a dream is equivalent to attempting to solve "a single equation with two unknowns" (Spence, 1986).

Weiss pointed out that when Freud made this dual use of associations he was "having his cake and eating it too" (Weiss, 1974). The question to ask is, is it possible to develop rules for revealing the meaning of a yet-to-exist second script when the meaning of the first also depends upon them? In believing it can be done for dreams, Foulkes has probably become a Micawber (Foulkes, 1978, pp.15-17, 45-46, 114-119). No one has suggested how it might be done for symptoms or parapraxes.

It is therefore inevitable - not just surprising - that psycho-analysts do so poorly on tests where they compare their interpretations of the same phenomenon. As simple in principle as that test might seem, success requires something that no one can have: knowledge of a second language that does not exist apart from the interpretation. The failures to agree on interpretations are therefore not matters to be explained away because of the complexities of human behaviour, or the obscurities of psycho-analytic propositions, or the poor quality of the analytic training in the interpretive arts. No matter how skilled the analyst, how simple the behaviour, and how clear the theoretical proposition, without a second script existing independently of the interpretation there can never be agreement. Timpanaro's stark implication cannot be avoided: "not even the analyst" is able to interpret the 'messages' conveyed from the unconscious by symptoms, dreams, and parapraxes (Timpanaro, 1974/1976, p.221).

Perhaps it is as well that psycho-analysts were, and have remained, "entirely ignorant of the attitude and knowledge with which a philologist would approach such a problem as that presented by dreams" (Freud, 1913e, p.177). Only by doing so have they been able to maintain their pose as guides along the royal road to the unconscious. For as long as there is no second script, not only can there be no rules for interpretation but pseudo-confirmations and appeals to authority will prevent secure findings about personality functioning and development ever being established. [2]

2. Arlow and Brenner (1988) have recently voiced the heresy that "Experience has by now convinced the majority of analysts that dreams and their interpretations are not the high road to the unconscious mental life that Freud first thought them to be". However, their assertion seems not to be based on evidence or on recognition of the failure of the linguistic parallel.

Psycho-analysis as construction

Freud used the term 'construction' to refer to the picture he built up of some incident he thought important in the patient's life but which the patient did not recollect at all or recollected only in part. Construction differs from translation and interpretation in that it necessarily draws on several sets of interpretations, each of which is based in turn on a large number of translated elements obtained by free association. [3]

Freud claimed that when a suppositional construction was put to a patient, the previously forgotten real event was sometimes recalled. This use of a construction can be illustrated from his own case of the Rat Man:

> I ventured to put forward a construction to the effect that when he was a child of under six he had been guilty of some sexual misdemeanour connected with masturbation and had been soundly castigated for it by his father. This punishment, according to my hypothesis, had, it was true, put an end to his masturbating, but on the other hand it had left behind it an ineradicable grudge against his father and had established him for all time in his role of an interferer with the patient's sexual enjoyment. To my great astonishment the patient then informed me that his mother had repeatedly described to him an occurrence of this kind which dated from his earliest childhood and had evidently escaped being forgotten by her on account of its remarkable consequences. He himself, however, had no recollection of it whatever. (Freud, 1909b, p.205)

The actual event the patient described was similar to the suppositional one Freud had put to him: for the only time in his life, his father had beaten him. The similarity was taken as confirming those parts of psycho-analytic theory from which the construction derived, for example, that the Rat Man had unconscious feelings of hostility toward his father. Other analysts have observed similar effects from putting constructions to their patients and those effects have been considered as going some way towards establishing the validity of free association.

Constructions, truth, and recall The significance of the reaction to constructions is only one of the issues involved in their use. There are two others: the accuracy of what the patient recalls and the likely truth of the construction itself. Consider first whether the incident could really have happened in the way it is recalled, either wholly or partly. Psycho-analysts who do discuss constructions are generally as sceptical of their veridicality as of the reality of the 'recollections' they engender.

As early as 1956, Kris said it was hopeless to expect a construction to recreate exactly what had happened in the patient's remote past. Memories were so modified by later experience that one could not say what the original incident had actually been (E. Kris, 1956). Kris' opinion is shared

3. The term 'reconstruction' came into the psycho-analytic literature some time after Freud's term but is usually regarded as a synonym for it (McGuire, 1971). According to Greenacre neither term had much currency ten years ago (Greenacre, 1980, 1981).

by a large number of psycho-analysts, for example, by Klauber (1968), Arlow (1981), and Spence and Geha (both cited in Messer, 1986). According to Gill, reconstructions change during the relatively short period of an analysis and analysts usually made insufficient distinction between the recovery of the event and the patient's experience of it. He then observed, "The past cannot be revived as such but only in terms of the present" (Reppen, 1982). These views of contemporary psycho-analysts are at one with the findings of experimental psychologists from Bartlett (1932) to Loftus and Loftus (1980). Memories are not like photographs which fade but do not really change. A personality theory cannot be made from them.

As to the likely truth of the constructions themselves, there is a good deal of psycho-analytic opinion against even the ones Freud put to his patients being veridical. Schimek (1987) found most of the seduction memories and all of the important early events in the cases of Little Hans and the Wolf Man to be constructions and that all of the 'memories' were in fact false. Most analysts would take a position on veridicality somewhere between Klauber and Spence or Skura. Klauber (1968) judged all constructions inevitably partial but pointed only to "gaps" in the constructions Freud made about the Wolf Man. Decidedly more sceptical, Spence called Freud's constructions about the Wolf Man "invention" (Spence, 1980. Cf. Ahlskog, 1980) and Skura seems to have dismissed them all as "delusive" (cited in Kermode, 1985). Others vacillate. For example, Blum, who regards construction as "fundamental" in analysis, confidently constructed the pre-Oedipal events of *Freud's* own childhood at the same time as suggesting that important "corrections" were needed to Freud's picture of the Wolf Man's childhood (Blum, 1977, 1980, 1982).

The main issues in the use of constructions are summarised by Laufer. A reconstruction is not the same as an infantile experience, transference experiences are not the same experiences as with the patient's parents, and fantasies obtained during treatment do not have the original content. To believe otherwise were pitfalls "which have more recently bedevilled and continued to weaken the use of our clinical data when formulating the nature and history of a specific psychopathology" (Laufer, 1982).

What is really fundamental is a consequence of the "paradox" M. T. McGuire (1971, p.3) outlines: the unconscious takes "once conscious perceptions, thoughts, and feelings" and changes them, often beyond recognition. That is, once again we have the absence of an independent second script which, in this case, means there cannot be a true memory to be reconstructed.

Psycho-analysis as narration

Sherwood (1969) argued that the core psycho-analytic explanation was a narrative account of the behaviour of the individual throughout an extended period of his or her life. He saw the narrative explanation as being very different from explanations which were deductions from a

covering law. A narrative explanation differs in its scope from a construction. A narrative is not restricted to a single incident, usually covering a substantial part of the patient's life, and it may draw on more than one construction.

Criteria for narratives Because there were then no criteria for assessing narrative explanations, Sherwood (1969) examined Freud's account of the case of Paul Lorenz ("The Rat Man") to see whether, and in what sense, its narrative characterisation of causal factors constituted an explanation of the patient's behaviour. In essence his conclusion was that the case narrative did constitute an explanation even though the deductive model of testing could not be applied to it.

Sherwood found it difficult to specify the positive explanatory properties of Freud's narrative but he did arrive at some criteria. He found Freud's account of the Rat Man to be adequate in the sense that it was self-consistent, coherent and comprehensive and he judged it to be accurate in the sense that Freud's suppositions about what must have happened were in some instances confirmed by the patient's recollection. The three criteria Sherwood therefore proposed for acceptable narrative explanations were:

(1) appropriateness - whether the explanation occurred within a relevant framework, whether the answers it gave to specific explanatory questions were apt, and whether it was at the proper level of complexity;

(2) adequacy - whether it was self-consistent, coherent, and comprehensive;

(3) accuracy - whether the explanation was true.

In one of the few psycho-analytic comments on Sherwood's criteria, Eagle judged that there was usually no problem with appropriateness and adequacy but that there was a real difficulty over accuracy or truth. His conclusions are at one with those I drew independently at about the same time (Eagle, 1973, 1980a; Macmillan, 1974, Chapter 17).

As Eagle (1980a) says, it is precisely over accuracy that Sherwood and other psycho-analytic writers are "ambiguous and unconvincing". Accuracy must be at the core of a genuine narrative because it is only accuracy that is concerned with the truth of what happened. To say this is not to demand an absolute truth. Even with a level of truth less than absolute, the problem remains of how that level of accuracy can be assessed and how real can be differentiated from spurious accuracy.

The strength of causal factors One reason why it is all too easy for the psycho-analytic narrative to be given spurious accuracy was pointed out by Freud himself, although not in those terms. In any given set of causal factors it is not possible to specify which are the weaker or the stronger.

Only a *post hoc* inference can be made: the strongest factors are those which bring about the outcome. No inference can be made until that outcome is known.

The dependence of the measure on the result gives the reverse reading of a case history a quite different quality from the forward reading as Freud saw:

> So long as we trace the development from its final outcome backwards, the chain of events appears continuous, and we feel we have gained an insight which is completely satisfactory or even exhaustive. But if we proceed the reverse way, if we start from the premises inferred from the analysis and try to follow these up to the final result, then we no longer get the impression of an inevitable sequence of events which could not have been otherwise determined. We notice at once that there might have been another result, and that we might have been just as well able to understand and explain the latter. The synthesis is thus not so satisfactory as the analysis; in other words, from a knowledge of the premises we could not have foretold the nature of the result.
>
> It is very easy to account for this disturbing state of affairs. Even supposing that we have a complete knowledge of the aetiological factors that decide a given result, nevertheless what we know about them is only their quality, and not their relative strength. Some of them are suppressed by others because they are too weak, and they therefore do not affect the final result. But we never know beforehand which of the determining factors will prove the weaker or the stronger. We only say at the end that those which succeeded must have been the stronger. Hence the chain of causation can always be recognized with certainty if we follow the line of analysis, whereas to predict it along the line of synthesis is impossible. (Freud, 1920b, pp.167-168)

It has been said by Wallerstein and Sampson (1971) that clinical retrospective studies have this kind of circularity built in to them and the strength of the factors at work cannot be assessed adequately. The problem is, however, a more general one, not limited to the clinic. Even in the simplest of situations, where one wants to say that a particular outcome is dependent only on one of the two factors being stronger, one must have a measure of strength independent of outcome. Whether the situation involves the past, the present, or the future makes absolutely no difference. In psycho-analytic studies - current, prospective, or retrospective - it is unusual to find that even crude measures of the strengths of causal factors have been attempted.

The flexibility of narratives More important than the inability to measure the strength of the factors are the points made by Cioffi and Spence about what Spence calls the "flexibility" of narratives. Cioffi, in his well-known seminar *Why we are still arguing about Freud*, delivered at Monash University in 1981, demonstrated that not only are there no criteria for judging the correctness of psycho-analytic explanations or interpretations but that none can be formulated. As Cioffi had done, Spence also showed

that narratives are almost infinitely flexible and anything can be fitted to them. Farrell's comments on Freud's study of Leonardo da Vinci aptly illustrate the point. According to Farrell, despite Freud's massive errors of fact about Leonardo's childhood, about the bird of his fantasy, and about the content of some of Leonardo's paintings and drawings "we must be grateful to him for giving us a simplified case history of Leonardo". The errors do not, therefore, basically undermine the force of Freud's interpretive narrative (Farrell, 1963; Cf. Freud, 1910b).

Narratives can also be designed to fit or illustrate any principle (Spence, 1980, 1982a). Blum's very plausible reconstruction of *Freud's* own pre-Oedipal development provides a good example. Without too much difficulty Blum fitted the data about Freud to Mahler's developmental theses, rather than to Freud's own, but he could have fitted it to any one of a number of similar theories (Blum, 1977). [4]

Cioffi's and Spence's points are also well-illustrated by comparing Freud's analysis of Michelangelo's statue of Moses with the later one by Bremer, even though it is properly neither a narrative nor a psycho-analytic interpretation (Freud, 1914a, 1927b; Bremer, 1976). [5] After spending hours looking at the statue, it came to Freud that Michelangelo had represented Moses full of anger at the very moment when, hearing his backsliding flock worshipping the Golden Calf, he was about to turn against them. Having just come down from the mountain with the tablets the Lord had given him with the Commandments already inscribed on them he proceeded to break them.

Bremer argues that Freud was completely wrong. According to him, Freud's interpretation was based "on several fundamental misconceptions" the most important of which was his use of the biblical account of Moses' *first* ascent of Mount Sinai rather than the *second*. Bremer concludes that the statue really depicts Moses in the presence of the Lord after this second ascent. All of the details of the statue are consistent with his interpretation, which is not the case with Freud's. For example, the tablets under Moses' arm are blank because this time Moses is to inscribe the Commandments at the Lord's dictation, rather than receiving them already written upon. The complex of emotions with which Moses' face is suffused is also appropriate to his being the only human being to have been allowed a glimpse of the glory of God's backside.

4. The data about Freud is not very secure and it says something about the standards of psycho-analytic judgement that Blum's paper was taken seriously enough to appear in a leading psycho-analytical journal.

5. As Ricoeur so stongly emphasises, the method by which Freud analyses the statue is exactly the same as the way he analyses dreams. In view of Bremer's demonstration, it is perhaps unfortunate that he also argues it is appropriately and genuinely analytic and that the psycho-analytic method is verified by it (Ricouer, 1970, pp.167-169, 1969/1974, 138-139, 201-202).

My point is not that I find Bremer's interpretation more convincing than Freud's (which I do) but, following Cioffi and Spence, that without external referents there can be no basis for judging any interpretation as better than another, let alone which is true. Consequently, the question of whether a psycho-analytic narrative gives a true account of what happened cannot usually be answered. We all have a sense of what constitutes an improbable or ridiculous interpretation or narrative explanation but this does not help us decide which, if any, of a number of non-ridiculous and more-or-less plausible interpretations or narratives is likely to be true.

There is a number of contemporary psycho-analysts who think asking about the truth of a narrative is as irrelevant as asking about the truth of a construction or interpretation. Kermode (1985) sums up this trend by saying an interpretation is never true or false but only something that contributes to "narrative intelligibility". One of the reasons why psycho-analysis has proved so impervious to criticism is precisely because it is impossible to make choices between differing degrees of this intelligibility.

The question of the truth of a narrative does matter, of course. Take, for example, Freud's response to what the Rat Man told him about being punished by his father. His father had indeed punished him, but not for masturbating nor other sexual misdemeanour. What Freud then did was to invoke a phylogenetically inherited fear of castration to account for the Rat Man's current fear. Now, while "narrative intelligibility" was undoubtedly maintained, it was not only at the expense of replacing an hypothesis that might have been true with one about which it was impossible to know anything, it also considerably altered the psycho-analytic theory of child development - surely a matter of some real importance. [6] There are also the therapeutic consequences that Fisher and Greenberg warn against. According to them, the analyst's shift from establishing 'what really happened' to the persuasive 'this must have happened' reconstruction used by Freud is "an open acknowledgement of suggestion occurring in the treatment" (S. Fisher and Greenberg, 1977, p.366).

FREE ASSOCIATION AND THE CREATION OF DATA

In the early part of this century there was no evidence that data generated by patients in therapy matched the theoretical expectations of their analysts. Opinions that that might be so could then only be matched by a contrary opinion. There were no facts about whether and to what extent the method of free-association generated its data. Thus, when Woodworth proposed that it might be the case, Tannenbaum had "no hesitation" in

6. Glymour's argument that this incident demonstrates how oriented Freud was to empirical test has been endorsed by a number of analysts. What Freud did seems to me, however, a little different from that to be expected from someone genuinely interested in formulating testable hypotheses (Glymour, 1974, 1980. Cf. Edelson, 1984; Schlessinger, 1986).

saying Woodworth would not be able to verify his supposition (Woodworth, 1917; Tannenbaum, 1917). The exchange could only be of opinion. Now, of course, Woodworth's suspicion is a commonplace.

However, in the sparse later research literature, the few who questioned the objectivity of free association were not much better off for facts than Woodworth and Tannenbaum. Zubin, for example, could make only the general observation that Freudian patients dreamed Freudian dreams, Jungian Jungian - wittily adding that Rogerian patients had no dreams at all (Zubin, 1964). Similarly, Marmor could call only on ''clinical experience'' to back his claim that patients' free associations were ''strongly influenced by the values and expectations of the therapist''. He did repeat his earlier observation that patients produced data which confirmed the interpretations and theories of their analysts but the only hard evidence he cited was some of the marginally relevant non-analytic literature on so-called verbal conditioning (Marmor, 1970. Cf. Marmor, 1962; Crichton-Miller, 1945, p.117). The situation has not changed despite the discussions of the objectivity of free association subsequent to Grünbaum's attack on psycho-analytic data as hopelessly contaminated by suggestion. All that has happened, *after nearly one hundred years*, is that psycho-analysts have finally put the need to study the method on the agenda (Grünbaum, 1980, 1984. Cf. Edelson, 1984; Wallerstein, 1986a, 1988; Arlow and Brenner, 1988).

Selecting from data

The implications drawn by observers like Zubin and Marmor are usually limited to discussions or warnings about selectively attending to and using data. For example, Zubin made his observation only to illustrate the selective effects of verbal reinforcement on affective statements in psychotherapy (Zubin, 1964). Despite holding free association responsible for the self-validating data produced by patients, Marmor went only as far as recommending it not be relied on as the sole technique (Marmor, 1970).

That psycho-analysts use their data selectively is now widely recognised. For example, A. Kris almost casually granted that analysts impose their ''personal proclivities and education'' in selecting, interpreting, and using the data obtained by free association. He also seemed to have no doubts that personal factors were responsible for the variant forms of psycho-analysis (A. Kris, 1983. Cf. A. Kris, 1982). It has actually been argued that this kind of selection is inevitable and may be inherent in the therapeutic situation itself. According to Ahlskog (1980), the analyst cannot fully understand associations unless they are fitted into a context. There is therefore a contradiction between the associations being generated freely by the patient and the analyst finding a context for them while maintaining an evenly suspended attention. This way of 'fitting' seems to me to involve selection.

The source of variant theories

More important than whether psycho-analysts selectively attend to and use the data gathered by means of free-association for the foundations of their particular theories, is the question of whether free-association *per se* creates the data from which variant theories are built. Two questions about Rank's birth trauma theory provide an illustration of my point: How did Rank's patients come to recollect the traumas of their own births and how was it that Ferenczi was able to confirm Rank's 'observations' but Freud was not? (Freud, 1926a, pp.135-136, 151-153, 161-162; E. Jones, 1957, *III*, pp.58-77; H. C. Abraham and E. L. Freud, 1965. Letter of 4 March, 1924, Freud to Abraham).

One explanation of the Rank debacle is to say Rank and Ferenczi selectively retrieved particular ideas from an overall store. Another is that all the ideas were retrieved but that Rank based his theory on only a sub-set of them and that Ferenczi simply followed him. Mixtures of these explanations are possible. But note the basic assumption all of them share: all the material was there, waiting to be recovered or used. Consequently, if faulty theories were constructed, it must have been because the predilections of the analysts selectively influenced the material retrieved or that they made idiosyncratic use of it. On either view, free-association could not have *created* the data on which Rank's theory was based. That method was as objective a device for collecting data as the microscope or telescope.

Constructions based on free-association are central to many of the important modern variant theories of psycho-analysis and it could also be argued that only personal influence in the retrieval or use of data was responsible for them. An implication of the opinions of a number of psycho-analysts must be counterpoised. In an extensive review of the main psycho-analytic theories of child development, Brody noted a result of the "primary technique" of psycho-analysis being "verbal and associative" was that too great a reliance was placed on reconstructions. She argued they should be drawn on sparingly and preferably only when direct observation was not possible. Brody concluded that the observational basis of all the developmental theories was questionable. She believed her conclusion was especially important for evaluating the imaginative constructive speculations of Balint, Kohut, and Kernberg (S. Brody, 1982). Holder similarly dismissed Melanie Klein's view of super-ego functioning: it was "unfounded in the light of general developmental principles as well as observational and clinical data" (Holder, 1982). Others have similarly pointed to the questionable bases of Melanie Klein's theories as well as those of Margaret Mahler and the ego-psychologists (e.g. T. Shapiro, 1981; Warme, 1982; Wallerstein, 1986b). Bowlby drew the more general conclusion that "Although psycho-analysis is avowedly a developmental discipline, it is nowhere weaker ... than in its concepts of development", a

weakness he put down to the "pride of place" given reconstructions over what developmental psychology had established (Bowlby, 1981).

Reconstruction had, of course, a central place in Freud's own developmental theory. Although Freud (1914b, p.90) admitted he had no direct evidence of primary narcissism, Steele and Jacobsen note how he:

> rests easy when everything has been traced back to the original objectless state of primary narcissism and the primary process, since for him this is the ultimate foundation. For the reader, though, it is the ultimate in speculation. (Steele and Jacobsen, 1977)

Steele and Jacobsen call Freud's inferentially constructed narcissistic state "a purely theoretical [concept] ... without any direct link to observation".

What these criticisms amount to is that the basic data, that is, the basic 'facts' of psycho-analysis are at variance with reality. For the most part, what these psycho-analysts are proposing is not a check with some standard method for using free-association or the use of an agreed on system of interpretation or construction, but a comparison with what actually takes place during development, with what exists. What is being questioned is the objectivity of the method itself.

In recently placing the question of investigating psycho-analytic methods on the agenda, Arlow and Brenner did revive the possibility that free-association may create its data. After asking how the same observations could give rise to different theories, they comment:

> It has been suggested more than once that the differences ... so apparent to every observer, may stem from the fact that the data of observation are not, in truth, the same. In fact, they are often very different. (Arlow and Brenner, 1988)

However, rather than biting this bullet, what they do is to advance what are in this context specious arguments about differences within disciplines being dependent on technique (e.g. astronomical theories based on the telescope versus those not, theories of infection using the microscope or not). All they can then plead for, but without being unduly optimistic about its realisation, is the standardisation of the psycho-analytic method of investigation. In one sense they have advanced little beyond Brenner's earlier critical remarks that the source of Melanie Klein's theory was the ease with which the analyst can find in a patient's associations anything that has been postulated in advance. Nor is it much different from Scott's reply to similar methodological criticism: he proposed "more detailed observations of *memories* of infantile states" as a source of evidence (!) about primary narcissism (Scott, 1952)

So considerable is the investment in the verbal and associative technique of free association, I would not expect Arlow and Brenner to do much more than they have, namely, to confuse reliability with validity.

Agreement on how the method is to be used or on how the material retrieved is to be translated and interpreted or fitted into a construction, narrative, or causal explanation does nothing for establishing its objectivity. The issue remains of whether the observations made with it are true.

Choosing between variant theories

Were only a personal selection of data involved in the construction of variant theories, that factor could be put taken into account when evaluating them. To judge from the famous closed debate which took place in London during the years 1940 and 1950 between Melanie Klein and Anna Freud and their respective followers, the matter is more complex. In Steiner's otherwise comprehensive account of this debate, which must rank with the longest and most concentrated within any field, there is not a single reference to a psycho-analytical fact or observation which might have helped resolve the considerable differences between the two positions (Steiner, 1985).

This failure to cite facts was probably because they were not thought important. In those of the discussions Home (1966) attended he observed that cases were presented "to confirm hypotheses rather than to test them." When there were disagreements, the appeal was "almost invariably to 'the literature' and not to the fact", a procedure having "no parallel ... in any other science". With this went the declaration that different views arose simply because different psycho-analysts spoke "different languages". To Home, that attitude seemed "a monstrous abdication of intellectual responsibility".

The failure may also have been connected with an inability to use such facts as were available. Describing an address he gave to the British Psycho-Analytic Society during that time Masserman recalled how:

> some members of opposing factions of the Society, neatly marshalled on opposite sides of an aisle that divided the room, insisted on indulging in polemics about various Freudian *versus* Kleinian dogmas by using my data almost completely out of context. (Masserman, 1959b)

Opinion was very polarised and the discussion period was, Masserman said, "far more memorable than the lecture".

Does this mean that controversy about differences between analytical theories is fundamentally different from scientific controversy? Holt (1982) seems to imply that Freud himself provided a positive answer. He says of Balmary's arguments in her *Freud and the Hidden Fault of the Father*: "Like Freud himself, she seems more concerned with rhetoric than science, aiming at convincing with persuasive words rather than refuting conjectures or testing hypotheses".

The present situation does not seem to be much different. There are, for example, no facts which can be adduced to allow a choice between Kohut

and the traditional Freudians or between either of them and Kernberg. [7] When Simon expressed his pessimism about the possibility of clinical data conclusively settling debates about the validity of psycho-analytic theory, he added he preferred it that way and his preference was endorsed by Schafer (both in Sacks, 1985). In the same discussion Sacks went so far as to praise the method of free-association precisely because it was "a prime guarantor of freedom from the tyranny of any one theory" (Sacks, 1985). Similarly, Breger denied Freud's claim of objectivity for the psycho-analytic method ("The process is not analogous to looking at cells under a microscope") and granted that only "in very general ways" would analytic observers agree about their observations (Breger, 1981).

Some analysts would resist the appeal to facts even were they available. Others would re-interpret them as Burgner's group did. Burgner and his colleagues found evidence of what seemed to be Oedipal fears and desires as well as post-Oedipal latency in children raised in the absence of their fathers. They did not regard their findings as confirming Freud's view of the centrality of the Oedipus complex. They called what they saw observations of "pseudo-Oedipal" and "pseudo-latency" behaviours (Burgner, 1985). What also do we make of those who, like Wallerstein and Reiser, endorse the psycho-analytic method as providing "an enormously powerful method for the study of human memory" at the same time as having the resource (or effrontery) to cite Grünbaum's criticism of it in support? Both Wallerstein and Reiser seem to believe the method has "enormous heuristic value" for generating testable hypotheses despite the centrality of Grünbaum's charge that the observations made with it are massively contaminated by suggestion! (Reiser, summarised in Wallerstein, 1985).

CONCLUSION

For Freud to have faced the Rank affair squarely would have required him to question the laws which he thought determined psychological phenomena in the treatment situation and for him to develop a more sophisticated understanding of how unconscious influences operated. The same consideration holds for the authors of the variant theories of psycho-analysis.

What experimental data we possess on verbal influence, what we know about the fabrication of memories under hypnosis, what we know about pseudo-sciences like scientology, and what we glean from comparisons between different psychotherapies leads to the conclusion that each psychotherapy *creates* its own data. In this respect, psycho-analytic

7. Glassman's questionnaire-based attempt to provide just such evidence fails, in my view, because the questionnaires contain too many items of unknown validity. The differences he predicts may also not be specific enough (Glassman, 1988).

therapy seems no different from others. There is no evidence to show that *only* the patient's unconscious ideas guide the trains of associations in the psycho-analytic treatment situation. There is also no evidence that the analyst's conscious purposive ideas, supposedly abandoned, play no role. Everything is consistent with the method of free association creating its own data and nothing is consistent with those data being obtained by means of an objective method and then interpreted in a partial or biased way.

16 PSYCHO-ANALYSIS AS SCIENCE

When a truth becomes a fact it loses all its intellectual value.
Wilde: *A Few Maxims for the Instruction of the Over-Educated.*

It is easy to predict that three of the responses to my criticism will begin in the following way:

> "What Macmillan says about psycho-analysis is old hat. Everyone, or at least those who matter, has known for a long time that Freud was no theorist. Since his time, psycho-analysis has developed into a theory very different from Freud's. Macmillan's criticisms are as irrelevant as those made by others before him because they miss this point".

To this response I would rejoin: What is this new theory and where is it set out? Where is the evidence that it has superseded Freud's? Where is the demonstration of its superiority over the original?

The simple facts are that no one has set out an agreed-on comprehensive alternative to Freud's formulations. None of the many partial reformulations I considered earlier *en passant* command much assent. They are as flawed as Freud's and the clinical and observational evidence relevant to them is just as weak.

Left as stated, this first response appeals to the traditionalist who wants only to develop or refine Freud's theory in the light of modern knowledge. Those who want psycho-analysis to be a different kind of science from Freud's expand it into the second of the probable responses:

> "Macmillan's criticisms miss the point because psycho-analysis is certainly not and probably was not even intended as a scientific discipline in the mould of the natural sciences. Rather than causal explanations of behaviour, what psycho-analysis provides is an understanding of how reasons or motives explain people's behaviour. This kind of understanding gives an explanation, perhaps even a scientific one, but not a natural-science one".

Expanding on my first rejoinder, the fact is that Freud treats motives and reasons as causes in his explanations and those explanations are not different from natural science ones.

A third likely response is to be found in a development of the first two. It is one which denies scientific status to psycho-analysis altogether:

> "To treat psycho-analysis in the way Macmillan does misses the point that it is not a science at all. What psycho-analysis gives is an understanding of

> behaviour and feelings, possibly an empathic understanding at that, rather than an explanation of them. Psycho-analysis is properly to be compared with literature, history, or the hermeneutic enterprises rather than with any of the sciences. Like them, it seeks to build up a coherent picture through which the individual may understand his or her uniqueness. It is not at all concerned with general laws. Macmillan is irrelevant because he misunderstands its true nature".

Those who make this response do so from positions that are among the most modern of the contemporary versions of psycho-analysis. They also misunderstand the roles of reasons and motives in Freud's explanations and they overlook the similarity between historical narratives and scientific explanations. Their representation of psycho-analysis as an historical or hermeneutic enterprise is based on restricting what Freud meant by 'meaning', on glossing over problems in constructing coherent accounts of the patient's past, and on preventing psycho-analysis from having developmental significance.

What I want to do in Chapter 16 is to develop these rejoinders and offer a more detailed rebuttal of the arguments of irrelevance and misconstrual. I begin by considering what Freud himself had to say about the status of psycho-analysis as a natural science before exploring some of the senses in which psycho-analysis is said not to be a science. I conclude with some brief speculations about the bases for the continuing appeal of a theory and a practice about which one can say so very little that is positive.

PSYCHO-ANALYSIS A SCIENCE?

Against the claim that psycho-analysis is not a natural science what are we to make of Freud's belief that it was. Could Freud have been massively self-deluded or had he simply misunderstood the nature of his life's work? These seem to me to be such important questions that I shall first examine Freud's own characterisation of psycho-analysis. I do this in some detail because those taking the view that psycho-analysis is other than a natural science tend to cite one or two of Freud's remarks in isolation rather than to examine his position as a whole.

Freud's view of psycho-analysis

Detailed documentation of Freud's insistence that psycho-analysis was a natural science hardly seems necessary. His earliest work on the neuroses applied Pasteur's germ theory and Koch's postulates, albeit defectively, to a search for causes. When he did turn to hysteria, he used the same methodological precepts for the same causal purpose. One of his arguments for the direct causal status of the memories of the hysteric came from the effects of abreaction supposedly reversing the Latin dictum *Cessante causa cessat effectus* (Breuer and Freud, 1893, p.7; Freud, 1893a, p.35). And what could put Freud's early allegiance to science more clearly

than the opening sentence of his 1895 *Project* where he announced his intention of founding "a psychology that shall be a natural science" (Freud, 1950/1954, p.295). Later on, after psycho-analysis proper had developed, Freud spoke over and over again, without any qualification or doubt, of psycho-analysis as a science. Repeatedly he referred to psycho-analysis as our young science, our new science, or eventually simply as our science and the brief definition Freud most often gave of psycho-analysis was that it was the science of unconscious mental processes.

Science and psycho-analysis at work The details of Freud's descriptions of psycho-analytic work all match exactly what he said about the other natural sciences. Thus, psycho-analysis shared the empirical outlook of science, "like every other natural science, it is based on a patient and tireless elaboration of facts from the world of perception" (Freud, 1925c, p.217). In particular, there were no sources of knowledge "other than the intellectual working-over of carefully scrutinized observations ... and ... no knowledge derived from revelation, intuition or divination" (Freud, 1933b, p.159). Psycho-analysis did not have a *Weltanschauung* of its own nor need to construct one - psycho-analysis was part of science and accepted its *Weltanschauung* (Freud, 1933b, pp.158-159, 181).

Psycho-analysis had the same basic aim of physics in wanting to go behind the world of perceptual appearance and build up a picture "which approximates more closely to what may be supposed to be the real state of affairs" (Freud, 1940a, p.196). Psycho-analysis commended itself because of the truths it had established about human nature (Freud, 1933b, pp.156-157). There are many other places where Freud put a similar stress on the empirical character of psycho-analysis and its natural-science realist aims (e.g. Freud, 1914b, p.77; 1915a p.117; 1926c, p.266; 1940a, pp.158, 196).

The uncertain way in which any science developed was also responsible for the uncertainties in the theoretical ideas of psycho-analysis (Freud, 1915a, p.117). Freud met the charge of uncertainty by comparing psycho-analysis with "all sciences", at various places specifically mentioning physics, chemistry, zoology, botany, and biology (Freud, 1916-1917, p.102; 1925a, pp.57-58; 1940a, pp.158-159, 196). Where basic psycho-analytic concepts were "nebulous, scarcely imaginable" Freud defended them by saying that they were not:

> the foundation of science, upon which everything rests: that foundation is observation alone. They are not the bottom but the top of the whole structure, and they can be replaced and discarded without damaging it. The same thing is happening in our day in the science of physics (Freud, 1914b, p.77. Cf. Freud, 1925a, pp.32-33, 58; 1926c, p.266)

Freud complained that he had "always felt it as a gross injustice that people have refused to treat psycho-analysis like any other science" by demanding greater sufficiency and completeness from it than was possible (Freud, 1925a, p.58. Cf. 1940a, pp.158-159).

A science of mental processes Freud made a special point of arguing that psycho-analysis was not different from other sciences because it dealt with mental processes:

> the intellect and the mind are objects for scientific research in exactly the same way as any non-human things. (Freud, 1933b, p.159)

In studying the mind, psycho-analysts filled the gaps in their observations in the same way as physicists did with their experiments: they inferred "a number of processes which are in themselves 'unknowable' and interpolate them in those that are conscious to us" (Freud, 1940a, p.196-197. Cf. Freud, 1940b, p.286). It was exactly those interpolations which gave psycho-analysis its understanding of symptoms, dreams, and parapraxes and which constituted it a science (Freud, 1925a, p.47; 1940a, p.158). Psycho-analysis had made a special contribution to science just because it approached unconscious mentation like any other scientific phenomenon: "Its contribution to science lies precisely in having extended research to the mental field" (Freud, 1933b, p.159).

Only in one respect was the analogy with other sciences incomplete. Psycho-analytic observations on "the psychical apparatus" were made through "the medium of the same perceptual apparatus". Although important, the difference was not fundamental. It was precisely the breaks in the sequence of 'psychical' events that enabled psycho-analysts to fill them in:

> by making plausible inferences and translating it into conscious material .... The relative certainty of our psychical science is based on the binding force of these inferences. (Freud, 1940a, p.159)

The only difference was trivial, "everyone ... has his opinion on psychological questions" (Freud, 1940b, pp.282-283). [1] There was therefore no need to find a special place for psycho-analysis: "Psychology, too, is a natural science. What else can it be?" (Freud, 1940b, pp.282).

Psycho-analysis and scientific achievement When Freud publicly claimed a place for himself in the pantheon of the gods of science it was not merely because of vanity or because he wished to lay the foundation for a myth, although both of those motives undoubtedly contributed. Science, said Freud, had delivered three blows to mankind's narcissism. Copernicus had given the *cosmological* blow when he removed the earth on which man stood from the centre of the solar system. Darwin struck the *biological* blow when he took man from his position of domination over the other animals. Psycho-analysis had delivered what was "probably the most wounding" blow of all, the *psychological*, when it discovered the ego was not master of the sexual instinctual drives and the unconscious processes in its own house (Freud, 1917b, pp.139-144. Cf. Freud, 1925c, p.221). Of

1. For reasons that I do not understand, Forrester (1986) thinks this is an important difference.

course Freud had not been made the equal of Copernicus and Darwin by some impersonal 'other'. He had conferred the honour himself. But, whatever else Freud may have been doing, he was making what was probably his strongest claim for psycho-analysis being a natural science.

Freud matched his publicly declared beliefs with his private. We see this most strikingly in his reply to the wishes Albert Einstein extended to him on his ninetieth birthday. Einstein remarked that he had not been able to form a definite opinion about the amount of truth in Freud's ideas until he had heard of some instances which he thought could be explained only by repression. He had been delighted, he told Freud, "since it is always delightful when a great and beautiful conception proves to be consonant with reality". Freud concurred with this realist interpretation of his theories in his reply: "I have often asked myself what indeed there is to be admired in them if they are not true, i.e. if they do not contain a large measure of truth" (Letters of Einstein to Freud of 21.4.1936 and Freud to Einstein of 3.5.1936 cited in E. Jones, *III*, 203-204).

At the time of his ninetieth birthday Freud was proposed for Corresponding Membership of the Royal Society and some time after his election, the Secretaries brought the official Charter Book of the Society to Freud for him to sign (E. Jones, *III*, pp.206 and 234). In describing the visit to Arnold Zweig, Freud added:

> They left a facsimile of the book with me and if you were here I could show you the signatures, from I. Newton to Charles Darwin. Good company! (Letter of Freud to A. Zweig, 28.6.1938, in E. L. Freud, 1970)

Could Freud have been as wrong about psycho-analysis as his modern self-appointed interpreters would have us believe?

ALTERNATIVES TO A NATURAL SCIENCE

Some psycho-analytic critics of Freud want to give up one or more of Freud's metapsychological viewpoints. Although a few want to maintain psycho-analysis as a natural science, many do not. In its place some offer science, but of a different kind, while others willingly abandon its scientific pretensions altogether. What opposes Freud's view of psycho-analysis as a natural science, then, is not a unitary argument but rather a number of separate arguments, made up of different components. Most of the arguments question the analyst's primary task. For some, that role is to understand mental processes rather than explain them in the ordinary way of science. Others wish to give meaning to the individual's thoughts, feelings, and behaviours by interpreting them or explicating the reasons, motives, or intentions underpinning them. Others again wish to generate narrative accounts of the individual more akin to those of the historian.

More than a rejection of natural science standards is involved in most of these positions. All place an exceptional importance on subjectivity in one or more of the ways in which they define the area to be studied, the choice of methods by which it is to be investigated, or the standards by which their end-product is to be evaluated. The emphasis is not accidental. We shall see in our seriatim examination of the central components of these arguments that most feed off an avowed or disguised Cartesian dualism.

UNDERSTANDING VERSUS EXPLAINING

Contrasting what we now term understanding with explaining goes back to at least renaissance times (Klauber, 1968; Grolnick, 1982). The contrast was introduced into psychology by Dilthey (1894/1977) and to psychiatry by Jaspers (1959/1962) and it was with Jaspers that the two were first represented as opposites.

According Jaspers, in the natural sciences one found rules, higher level laws, and the general causal connections which it was the aim of the sciences to grasp. In psychology, on the other hand, there were only rules and particular causal connections. Partly for that reason, psychology had to have a different aim which, Jaspers argued, was one of understanding empathically how one psychological event emerged out of another. He called this type of empathic understanding 'genetic'.

Jaspers believed the evidence for genetic understanding was of an ultimate kind. It carried:

> its own power of conviction and it is a pre-condition of the psychology of meaningful phenomena that we accept this kind of evidence just as acceptance of the reality and of causality is the pre-condition of the natural sciences. (Jaspers, 1959/1962, p.303)

The sense of conviction was not acquired inductively, through repeated experience, but, Jaspers stressed, "*on the occasion* of confronting human personality".

There are three parts to Jaspers' argument. First he represents science as a completely inductive enterprise which arrives at its explanations purely through the study of particular instances. He then asserts that psychology is unable to move beyond the particular and has to use a different method. He then jumps to the conclusion that that method has to be understanding. Each part of his argument can be disputed.

Hypotheses and facts Jaspers' version of scientific endeavour is a misleading high-school one. Not even at the level of gathering data for the formulation of rules and laws has any science ever been purely empirical and based solely on inductive enquiry. Whether scientific or not, one's enquiry is always guided by concepts and hypotheses.

What really seems to happen in scientific enquiry is that the scientist begins with a more-or-less clearly posed question and formulates some more-or-less clear and tentative hypotheses which might lead to answers. The hypotheses then guide a more-or-less clearly structured programme for gathering and interpreting data. In the programme, judgments are continuously made about the relevance of the data. *Relevance* here means that the fact or relation is *logically related* to the tentative hypothesis, that is, its occurrence or non-occurrence may be *deduced* from the hypothesis. Consequently, in real scientific enquiry there is a continuous interplay between fact and hypothesis, between induction and deduction.

The inductive method also stops precisely at the point of most interest, that of the formulation of explanatory theoretical concepts. Consider Freud's mechanism of repression, for example, and the non-factual notions of 'unconscious wish', 'disposition', and 'libido' with which it was associated. Theoretical concepts like these cannot be generated by induction. Their sources have to lie outside the realm of the factual because they refer to things and processes that cannot be observed directly.

Nor is the process by which theoretical ideas and hypotheses are formulated always completely logical and rational. Sometimes semi-rational processes play a part. For example, Kekulé's hypotheses about the arrangement of atoms in the molecules, which his structural theory of organic chemistry required, came to him during a reverie. Travelling across London on an omnibus late one evening he 'saw' the atoms join together in chains while "whirling in a giddy dance". In a second reverie on another occasion the idea of a possible structure for the benzene molecule came to him in the form of a dance of snakes (cited in Japp, 1898).

After the hypothesis is arrived at, its logical implications have to be worked out and tested against the data of observation. As a consequence of his first reverie, Kekulé spent part of the rest of the night attempting to sketch the "dream forms"; once woken from the dream of the snakes "as if by a flash of lightning", he spent "the rest of the night working out the consequences of the hypothesis". Then, to confirm the reality of the hypothesised structure, he had to work for months at experiments testing it (cited in Japp, 1898. Cf. Russell, 1971). Nowhere are hypotheses generated purely by induction although, as Gruber (1981) brings out so well in his discussion of Kekulé's and similar experiences, many facts have usually been collected and mulled over before the insight occurs.

The process by which hypotheses are arrived at is also socially and historically conditioned. At the time Charcot wrote, for example, few people considered expectations relevant and it is not surprising that he did not collect data on them. Charcot studied only those facts like reflex action and muscle contraction which were relevant to the physiological basis he supposed hypnosis to have. By itself, the collection of what appear to be

relevant facts will not necessarily draw attention to incorrect hypotheses.

Empathic comprehension Jaspers represents the non-inductive, direct, empathic comprehension of psychological phenomena as if it were the only road for psychology to follow and has no doubt about its validity:

> When Nietzsche shows how an awareness of one's weakness, wretchedness, and suffering gives rise to moral demands and religions of redemption, because in this roundabout way the psyche can gratify its will to power in spite of its weakness, we experience the force of his argument and are convinced. It strikes us as something self-evident which cannot be broken down any further. (Jaspers, 1959/1962, p.303)

Although it is clear that repeated experiences of the will to power mediating the connection between the human condition and redemptive religious experience cannot be responsible for any conviction Nietzsche's demonstration conveys, it is also clear that something is needed. It may not be an inductive inference, but experience of *some kind* is necessary.

To begin with, how much conviction does the argument produce in the present-day reader, especially one to whom the whole set of Nietzschian concepts are unfamiliar? To illustrate a point about the relation between consciousness and being, an English Marxist once wrote:

> Suppose someone had performed the regrettable experiment of turning Bertrand Russell, at the age of nine months, over to a goat foster-mother, and leaving him to her care, in some remote spot, unvisited by human beings, to grow to manhood. When, say forty years later, men first visited Bertrand Russell would they find him with the manuscripts of the *Analysis of Mind* and the *Analysis of Matter* in his hands? Would they even find him in possession of his definition of number, as the class of all classes? (Caudwell, 1938, p.214)

Of course, Caudwell's answer was "No". Russell and his thought were, Caudwell argued essentially, if perhaps a little too simply, social products: "Society made him, just as it makes a hat" (ibid.).

Analogously, would we expect even a Jaspers raised by peasants in a remote village in the Black Forest, reading only books by the German equivalent of Enid Blyton, to be swayed immediately by Nietzsche's argument? Of course not. Without some minimal experience of wretchedness, moral demands, redemption, and some understanding of the causal connection between them and the will to power, Nietzsche's arguments are incomprehensible, even faintly absurd.

Empathic conviction We should nevertheless ask about the sources of the sense of conviction that empathy produces and about the status of the knowledge the feeling gives.

Although he was speaking of the basis for understanding the reasons for an action, M. S. Moore gives us a good indication of the conditions

under which empathic understanding occurs. Moore says we understand an action because we understand that a rational agent would act that way and that had we a similar set of desires and beliefs we would act similarly:

> As long as the object of the agent's desire is intelligible to us as something a person in our culture could conceivably want, and so long as the factual beliefs are not themselves irrational, we can empathize with the action ... because, knowing the belief/desire set, we perceive the activity to be the rational thing to do for an agent with such beliefs and desires. (M. S. Moore, 1980)

Even those philosophers who maintain that reasons are distinct from causes grant that reasons explain behaviour to the extent that the observer comprehends the cultural context in which the reasons were acquired (e.g. Peters, 1958/1960, pp.5-7; Toulmin, 1970a). Similarly, empathic understanding seems to derive from a set of beliefs and values shared with the person with whom we are empathising although we may not be consciously aware of its basis at the time.

Perhaps a lead to how empathic understanding comes about is found in the common "Eureka!" experience. We have all had the thought, often during a dream or on wakening, that we have found the solution to some problem. Although the experience is most frequently accompanied by an intense conviction that our solution is unassailably correct, as often as not we find it to be hopelessly wrong. I believe "Eureka!" solutions are the conclusions of deductive arguments of which we are partly or completely unaware. When we have the experience, we are not aware of how the steps have led from the initial premisses to the conclusion. Only after scrutinising the argument, sometimes also semi-consciously, are its deficiencies made out. When this happens, we realise we have shouted "Eureka!" too soon. From this everyday experience we learn that no matter how certain this kind of knowledge seems to be or how much we would like to believe it, it cannot be taken on trust. Similarly, without further scrutiny, empathically based understanding has no status as knowledge. When it does survive our examination, what it gives us is an ordinary causal pattern or context for understanding and explaining which, as Davidson has commented, "explains the effect, in a sense of 'explain' that we understand as well as any" (Davidson, 1963).

The issues involved in the debate over understanding are more complex than I have represented them. But there is no escaping the main point that the case for a sharp distinction between explanation and understanding is very much weaker than at first appears (Rangell, 1979; Grünbaum, 1984; Holzman, 1985), a view consistent with the way the split developed and with earlier psycho-analytic opinion (H. Hartmann, 1927; Eissler, 1968).

Further, the argument to separate psycho-analysis from the natural sciences rests, as will be seen, on two paradoxes and a bad analogy. First,

most of those who make it, base their criticisms of psycho-analysis on the narrowest positivism and so join Jaspers in supping with the devil (e.g. Kaplan, D. M. 1977; Ellman and Moskowitz, 1980; Blight, 1981). Second, the meaning of 'meaning' is not at all clear (Shope, 1973; Rubinstein, 1975). Quite apart from the failure of the analogy between deciphering a text and psycho-analytic work (the centrality of which was discussed in Chapter 15) there is the further problem of patients actively resisting attempts to understand their discourse in ways that texts never do (Holzman, 1985). Freud was himself opposed to psycho-analysis being classed as an understanding discipline (Bird, 1981) and in what follows, I shall attempt to show that some of the efforts to class it otherwise are quite misleading.

WHAT DO MOTIVES DO?

Suppose we could obtain an understanding of another's motives or reasons, empathically or otherwise. What would that tell us about dreams, symptoms, faulty actions, or parapraxes like slips of the tongue?

Freud refers many times to desire, intention, motive, wish, purpose, and reason in connection with these phenomena (e.g. Freud, 1900, pp.570; 1901b, pp.69, 80, 142, 143, 153, 239; 1916-1917, pp.54, 56-57, 65). He has been read as if it is these very phenomena which are desired, intended, motivated, wished for, or which result from purposes or reasons. It is argued that showing they are so intended, desired, etc. explains them or leads to our understanding of them (e.g. Balmuth cited in Shope, 1967; Kolenda, 1964; Flew, 1956; Siegler, 1967). Is the matter as simple as this? Are motives to be so directly understood or explained? There are two reasons for supposing the answer is 'No'.

First, Freud's basic position on symptoms and dreams was, as we saw in Chapter 9, that they were compromise formations expressing two mutually conflicting wishes. After her case had been reported, Anna O.'s symptoms were also said to have arisen:

> in situations involving an impulse to an action which, however, had not been carried out but had for other reasons been suppressed. The symptoms had, in fact, appeared *in place of* the actions that were not performed. Thus, to explain the aetiology of hysterical symptoms, we were led to ... the interplay of mental forces (to dynamics). (Freud, 1924c, p.193)

Similarly, in order to appear in the manifest dream, the wishful impulse had first:

> to submit to a distortion, which is the work of restrictive, censoring forces in the dreamer's ego. In this way the manifest dream ... comes about .... It is a compromise between two conflicting groups of mental trends, just as ... with hysterical symptoms. (op. cit., pp.199-200)

Parapraxes also served definite purposes which:

> owing to the prevailing psychological situation, cannot be expressed in any other way. These situations as a rule involve a psychical conflict which prevents the underlying intention from finding direct expression and diverts it along indirect paths. (Freud, 1913d, p.167)

Thus, Freud did not describe the phenomena as intended, desired, or wished for. True, intentions and wishes did underlie them but it was precisely because the phenomena were compromises that they themselves could not have been intended or wished for.

Second, when Freud wrote of the meaning, the purpose, or the intention of the symptom, dream, or faulty act he was clearly referring only to one of the two intentions: the underlying, suppressed or repressed one. For example, "the secret meaning" of the dream was in the latent dream thoughts. Its latent meaning:

> is always a wishful impulse which is represented as fulfilled at the moment of the dream. But, except in young children and under the pressure of imperative physical needs, this secret wish can never be expressed recognizably. (Freud, 1924c, p.199)

Consequently, it was not the intention (if it can so be put) of the repressed wish or latent meaning of the dream to produce a distorted fulfilment. Left to itself that intention would express itself directly, without distortion. The forces opposed to it acted with it to produce the distorted compromise.

Similarly, when Freud said that suppressed intentions were "responsible" for parapraxes, he clearly stated that those intentions had been prevented "from finding direct expression" and that the meanings and intentions served by the parapraxes could not "be expressed in any other way" (Freud, 1913d, p.167). Freud's claim that "our blunders often turn out to be *a cover* for our secret intentions" clearly means, as my emphasis shows, that the blunders themselves are not intended (op. cit., p.169). Boudreaux (1977) has cited some of Freud's examples in favour of the opposite view. To that extent he has challenged Shope's rejection of the point that some slips are intended, at least in the form Shope put it (1967, 1970). I believe Boudreaux is wrong and that his error is clear in Freud's discussion of what Boudreaux supposes to be counter-examples. Take, for example, the slip made by a man who Freud described as having "intended his slip to express [a] view". Clear enough, one might think. But Boudreaux reads this literally and out of context; Freud actually discusses the slip in the context of "a thought-content which is at pains to remain concealed but which cannot nevertheless avoid *unintentionally* betraying its existence" (Freud, 1901b, p.80. My emphasis, MBM).

The same points hold for symptoms. Hysterical symptoms had "the meaning of fulfilments of secret and repressed wishes" but their "tormenting character" was due to the internal mental conflict occasioned by the

need to combat the wishes. Obsessional symptoms had a meaning given by "the proscribed wish", but the symptoms themselves reflected the conflict between the wish and "the punishment and atonement which that wish incurs". Even when Freud seemed to imply that "the ruling wish" could sometimes be expressed more-or-less directly, as in the stereotypies of the severe schizophrenias, those symptoms were the remains of an original compromise action or verbal symptom (Freud, 1913d, pp.173).

Consequently, whether psycho-analysts wish to understand or explain compromises, they must have a theory of mental dynamics showing how compromises are formed from the interaction between intention and counter-intention. Understanding only what the intention and counter-intention are is to know less than a half of what needs to be known. Without a theory of compromise formation, understanding motives, empathically or otherwise, gives no understanding of mental life.

CAUSES, INTERPRETATIONS, AND MEANING

What did Freud think he was finding when he interpreted dreams, symptoms and parapraxes? Was it an empathic or similar understanding of the motives of his patients? When he said the behaviours were not 'senseless' but 'had meaning' was he fitting them into a hermeneutic context rather than placing them in a scientifically causal framework? Both questions must be answered in the negative. As I showed in Chapters 4 and 8, for Freud mental processes were continuous and governed by a psychic determinism in which associations, causal connections, and logical relations were identical. He did not need to differentiate between meanings and causes or between explanation and understanding.

Meaning

One of Freud's main criticisms of the scientific work on dreams which had preceded his own was that it treated the dream as the product of a physiologically impaired condition in which associations were followed randomly. It could therefore have no meaning. What, then, did he mean by 'meaning'? The most essential sense he gave to the word was that of replacing the unintelligible account of the symptom or the senseless and incoherent manifest dream with something that made as much sense as normal mental life. For Freud:

> 'interpreting' a dream implies assigning a 'meaning' to it - that is, replacing it by something which fits into the chain of our mental acts as a link having a validity and importance equal to the rest. (Freud, 1900, p.96)

Freud assumed dreams were:

> designed to take the place of some other process of thought, and that we have only to undo the substitution correctly in order to arrive at this hidden

meaning. (ibid.)

Free association to the dream elements caused the "substitutive structures" to emerge. The "intermediate links" so brought out could be inserted between the manifest and latent dream, the latent content reinstated and the dream interpreted (Freud, 1916-1917, p.113; 1940a, p.169).

Similarly it was precisely the gaps in the patient's account of his or her symptoms that indicated the presence of "secret motives"; it was there that the psycho-analyst had to look for the connecting threads. Toward the end of the treatment, the facts given by the patient enabled the construction of an "intelligible, consistent, and unbroken case history" (Breuer and Freud, 1895, p.293; Freud, 1905a, pp.17-18).

Meaning and context Giving meaning by placing some psychological phenomenon into its context in this way requires the assumption that psychological processes are continuous. In Chapter 4 we saw how Freud made precisely that assumption. The essence of his view was that gaps in a psychological process marked the points at which the process had become unconscious. After translating the material hidden in the gap into conscious material a sequence of conscious events complementary to the unconscious psychical processes resulted (Freud, 1940a, p.159).

What Freud claimed to find at the end of the chain of events was the memory of some causal event. He took this view because of Meynert's associationism. To give meaning to some psychological phenomenon was to establish its causes. Symptoms, dreams and parapraxes were given meaning by being placed in the context of the processes that produced them. This meaning of 'meaning' is the one Freud most frequently drew on. It combines what Shope (1973) has discerned as two of the connotations of meaning: the one just discussed, that for which a mental phenomenon substitutes, and its intention, purpose, and position in a causal sequence. [2]

Placing the symptom in the context of the circumstances in which it originates also allows for a somewhat different kind of explanation or understanding, one of why the symptom has the particular content it has. Take as an example Jackson's comments about recurrent utterances:

> By considering (1) the external circumstances at the time of being taken ill; (2) the intensity of the emotional state under which the last attempt at speech was made; and (3) the gravity of the lesion, we may perhaps be able to show why this or that kind of recurring utterance remains in particular cases of speechlessness. (Jackson, 1879-1880b)

Jackson here explains two different things. The first is the utterance itself.

2. For Freud (and others) giving meaning by placing phenomena in a causal context was not restricted to mental events: "Darwin taught us" that the innervations and motor activity underlying emotions and emotional responses "originally had a meaning and a purpose" (Breuer and Freud, 1895, p.171).

In the sense that without the lesion there would be no recurrent utterance at all, the cause of the utterance is the lesion. The second thing being explained is the content of the utterance. The emotional state jumbles the elements of the words and the external circumstances in which the victim was trying to articulate the particular proposition conspire to make the content "committymy" out of "Come, pity me".

Meaning as content Meaning as *content* is something like the third of the connotations of 'meaning' Shope (1973) differentiates - the symptom as a sign of something else.

It is pretty clear that those who would turn psycho-analysis into an interpretative discipline restrict Freud's meanings to just this one. Forrester (1980) has gone so far as to propose that Freud's explanation of symptoms was essentially the same as Jackson's. Both, he asserts, attempted to make sense of symptoms by putting them into context. Forrester uses this proposal as part of his argument against the ordinary view of Freud as a scientist. Giving meaning in this way is somehow supposed to run counter to scientific endeavour. Forrester seems not to be aware that Stengel (1954) had drawn the parallel before without feeling impelled to draw the same conclusion. Perhaps Stengel saw the obvious breakdown of the parallel in that the context explains only the content of the utterance and not that it is a symptom. [3]

Meaning and development Making psycho-analysis an interpretive search restricts its scope drastically. Freud did not confine the analyst to listening and interpreting. For him the purpose of the interaction was to place the verbal exchanges within a context which had to do with the origins of the symptoms.

The business of psycho-analysis, said Freud, was "to explain the striking symptoms by revealing their genesis" (Freud, 1918, p.105). As a therapy, psycho-analysis had been unable to eliminate symptoms until their origins *and* development had been traced. "From the very first" psycho-analysis was "directed towards tracing developmental processes"(Freud, 1913d, p.183). And, in addition "to discovering the genesis of symptoms", psycho-analysis had been led:

> to turn its attention to other psychical structures and to construct a genetic psychology which would apply to them too. Psycho-analysis has been obliged to derive the mental life of adults from that of children. (ibid.)

The genetic psychology provided answers to "The many riddles of the sexual life of the adult" (ibid.) as well as demonstrating how infantile mental formations persisted and gave rise to the dreams of adults and to their dispositions to later illness (op. cit., p.184).

3. One also wonders what Jackson would have made of the insinuation that he was not a scientist.

Psycho-analytic theoretical concepts about development still come mostly from working over the data gathered during the analytic hour and are very different from the verbal exchanges themselves. Without the extra-therapeutic context provided by the developmental processes even the 'understanding' of the patient would be very limited (Cf. Eagle, 1980; Holzman, 1985).

FREUD'S CAUSAL MOTIVES

Freud leaves us in no doubt of his view that interpreting dreams, parapraxes, and symptoms leads to their causes, that those causes are motives, intentions, and wishes, and that psychical forces like them do more than just speak through their mental products.

The search for causes

Throughout his life Freud maintained the position he adopted from the very beginning. He began his work on the actual neuroses and the psychoneuroses by placing the search for causes at the head of his endeavours and brought Pasteur's germ theory and the related causal postulates of Koch into the study of the neuroses. Those same considerations dominated *The Interpretation of Dreams*, *The Psychopathology of Everyday Life* and the later works, including the very last ones. Throughout his quest, Freud saw motives as a species of cause. Without that identity, his picture of a mental life caused by the interplay of psychical forces makes almost no sense.

As early as the *Preliminary Communication* we find the search for causes was central. Breuer and Freud spoke of "the causal connection" or "the causal relation" between the determining or precipitating psychical trauma and the hysterical or other pathological phenomenon (Breuer and Freud, 1893, pp.3, 6). Breuer clearly saw that his first analyses of symptoms led to the experiences which had caused the symptoms and Freud himself spoke repeatedly of ideas, memories, recollections, impressions, psychical groups, and trains of thought as pathogenic. When Freud elaborated these expressions, words like 'causation', 'determined', 'explanation', and 'understanding' were used as synonyms (Breuer and Freud, 1895, pp.209, 269, 282, 283, 287-288, 290; Freud, 1896c, pp.193-196).

An identical mode of thought characterises Freud's later work. For example, wherever there was a symptom there was a gap in the memory and filling the gap implied removing the condition which had produced the symptom (Freud, 1910a, p.20). Again, Freud claimed that in so far as analytic therapy did not make its first task the removal of symptoms it was like a causal therapy (Freud, 1916-1917, p.436).

Formulations similar to these general ones are found in Freud's discussions of specific patients. Of Elisabeth von R.'s treatment Freud said, "I

would carefully note the points at which some train of thought remained obscure or some link in the causal chain seemed to be missing''. He described the whole work as ''based on the expectation that it would be possible to establish a completely adequate set of determinants for the events concerned''. As we know, he failed to trace ''any psychical cause'' for her first leg pains (Breuer and Freud, 1895, pp.139, 147). Of the Wolf Man, Freud wrote that a particular recollection provided ''an important link between the primal scene and the later compulsive love which came to be of such decisive significance in his subsequent career'' (Freud, 1918, p.92). Tracing the memories of childhood events and impressions which were supposed to cause perversions led to such ''commonplace and unexciting'' impressions, ''without any traumatic force'' that Freud had only been prepared ''to come to a provisional end ... in tracing back the train of causal connection'' (Freud, 1919a, p.182).

Causes and motives

Many times Freud uses motives and causes as synonyms. One of the earliest occurs when he discusses his own dreams:

> since I am an excellent sleeper and obstinately refuse to allow anything to disturb my sleep, it very rarely happens that *external causes* of excitation find their way into my dreams; whereas *psychical motives* obviously *cause* me to dream very easily. (Freud, 1900, p.229. My emphasis, MBM)

Here Freud explicitly equates psychical motives with causes and implicitly grants them the same status as external, physical causes of excitation (Cf. p.224).

Among other examples of Freud's explicit identification of causes with motives, intentions, and wishes, we note the following:

> A personal complex causes a name to be forgotten, an unconscious desire plays a part in causing a slip of the pen, the cause of a slip of the tongue is a motive other than the conscious intention, a rejected intention plays a part in causing a slip of the tongue and (in one of Freud's last works) it was when unconscious thoughts, wishes, and intentions became effective that they accounted for slips (Freud, 1901b, pp.22, 128, 272; 1916-1917, pp.65; 1940b, p.284).
>
> A neurotic has a cause or motive for falling ill, the frustration of a wish or motive leads to the outbreak of a neurosis, the cause of psychical impotence is a masochistic attitude (Freud, 1909b, p.199; 1917d, p.129; 1919a, p.197).
>
> Evil intentions cause a dream, unconscious wishful impulses combine with day residues to create or construct the latent dream-thought, and opposition to hidden impulses causes the basic feature of the dream - its distortion (Freud, 1916-1917, pp.218, 226; 1923d, pp.262-263).

For all the phenomena he wanted to explain, Freud regarded motives as synonymous with causes.

Motives and forces

Shope (1973) has also pointed out that Freud treated causes and motives as the same partly because behind his use of the word 'motive' lay the concept of a 'motive force'. For example, consider Freud's endorsement of Delage's opinion about the contribution made to the dream by thoughts interrupted or suppressed during the day:

> The psychical energy which has been stored up during the daytime by being inhibited and suppressed becomes the motive force for dreams at night. (Freud, 1900, pp.81-82)

In his own conceptualisation Freud had the unconscious dream-wish supplying or providing 'the motive force' for producing or making the dream (Freud, 1900, pp.541-542, 560-561).

A complex interplay of similarly conceived causal motive forces was the very basis of mental life. Not only did Freud speak of "the force ascribed by the patient to his motives" (Breuer and Freud, 1895, p.293), but he also conceptualised resistance and repression as motivated by a force having causal efficacy. What was the "kind of force", he asked, that caused resistance and "what motive could have put it into operation?". It was the same "repelling force of which the purpose was defence" that had originally "forced" the idea "out of consciousness and out of memory". He had, he said:

> attempted to sketch out the psychological hypotheses by the help of which this causal connection ... can be demonstrated. (Breuer and Freud, 1895, p.269)

The motive for this repression was to be found in feelings of unpleasure (Freud, 1904, p.251. Cf. Freud, 1913d, pp.167, 171).

The most important consequence of giving meaning to a dream or symptom was that it identified the motive forces that had caused it and allowed the repressing force to be modified. Hypnosis, the pressure method, and free association could all be described as aiming to fill the gaps in memory. Dynamically speaking, however, the aim was "to overcome resistances due to repression" (Freud, 1914c, p.148). The 'translation' which replaced what had been unconscious with what was conscious had causal consequences:

> we lift the repressions, we remove the preconditions for the formation of symptoms, we transform the pathogenic conflict into a normal one. (Freud, 1916-1917, p.435)

A striking example of the use of the identity between causes and motives occurs in Freud's interpretation of the meaning of the symptoms of two of

his obsessional patients. He had translated the ritual actions of both as having the meaning of wish-fulfilling re-enactments of earlier experiences. Each had a memory of the event but neither connected it with her ritual. Of the first patient Freud said her memory "did not occur to her when she was asked directly to look for the *motives* of her obsessional action" and of the second he said she failed to connect her "ceremonial and its *causes*" with her memory. Here Freud not only explicitly equated motive with cause but argued that the ritual stood for the cause (Freud, 1916-1917, p.283). *Wishes* similarly provided "the motive power" or "the motive force" for the construction of dreams (Freud, 1900. p.598, 1901a, p.682)

Motives and causal conditions

In concluding this section I would stress that Freud does not accord psychic forces the status of causes just because they are instinctual or just because they draw on a somatic source of energy. He infers their causal status from the effects he supposes they bring about. How they achieve their ends through compromise formations and what energy they possess is only part of the issue of whether they are causes. All Freud's causal trappings are to be found in his discussions of parapraxes. For example, in *The Psychopathology of Everyday Life*, Freud placed the famous Signorelli error in the context of "conditions" which had to be fulfilled and in which "a motive" was added to the "factors" that brought about the forgetting. It was also a context in which Freud discussed his "explanations" in terms of "necessary" and "sufficient" conditions (Freud, 1901b, pp.5-6). Similarly, Freud reached an "explanation" of the distortion in recalling a line from 'The Bride of Corinth' which was "sufficient" by tracing it to a thought-content which was "its cause" or the "source of the effect" (Freud, 1901b, pp.18, 21). Even without the notion of forces as ideas cathected or invested with psychic energy Freud would have insisted on the causal status of motives and wishes (Cf. Edelson, 1977).

Consequently, Freud does not identify the ideas recovered by free association as the causes of dreams, parapraxes, and symptoms, simply because he made a *post hoc ergo propter hoc* confusion and neglected to consider suggestion, as Grünbaum has argued (Grünbaum, 1980, 1984, pp.58-59, 198-199). Freud was not quite so simple minded. Certainly, criticism of psycho-analysis must cover, as Grünbaum's does, Freud's misplaced reliance on the therapeutic touchstone and the contamination of free association by suggestion. But what seems to have escaped Grünbaum is that free association produced only a putative cause which Freud then evaluated with his (deficient) adaptation of Koch's postulates. It was Freud's concept of determinism which had it that the ideas to which associations led were causes. A really penetrating excavation of the logical foundations of psycho-analysis has to get to the bedrock of Freud's view of psychic determinism and its relation to the fundamental rule of psycho-analysis. This Grünbaum fails to do.

Freud's belief that psychical forces had causal effects must be seen in the context of his general deterministic framework. The most general conclusion he had drawn from his analysis of parapraxes was that:

> certain shortcomings in our psychical functioning ... and certain seemingly unintentional performances prove ... to have valid motives and to be determined by motives unknown to consciousness. (Freud, 1901b, p.239)

He went on:

> If we give way to the view that a part of our psychical functioning cannot be explained by purposive ideas, we are failing to appreciate the extent of determination in mental life. (op. cit., p.240)

Here as elsewhere, 'determine' and its derivatives are synonyms for 'cause' and its derivatives (Cf. Freud, 1910a, p.14). Consequently, when Freud later said it was "quite unscientific" to think that there were "undetermined psychical events" and that that idea "must yield to the demand of a determinism whose rule extends over mental life" he was demanding recognition of motives and the like as causes (Freud, 1916-1917, p.106).

CAN MOTIVES BE CAUSES?

What about the arguments that motives are different from causes? Are they strong enough for us to reject Freud's equating them?

The motive-cause distinction

Home is usually credited with having first raised the motive-cause distinction within psycho-analysis. He argued that Freud's "totally new principle of explanation" was that "the symptom could have meaning" and meaning was not the product of causes but "the creation of a subject" (Home, 1966). The comprehension of meaning was a humanistic enterprise, not a scientific one. Home believed the distinction he was making was the same as that between interpretation and explanation or between reasons and causes. He thought psycho-analysts should ask the question of why things happen and look for answers "in terms of a subject's motives". Home proposed a kind of Jaspers-like empathy, what he called cognition through identification, as the main way of understanding the motives and meaning of behaviour.

By the time Home wrote, Davidson (1963) had already examined most of the following commonly held distinctions between reasons and causes:

causes were events but reasons were not

unlike reasons, causes were logically distinct from their effects

cause-effect relations were law governed but reason-action connections were not

causes were known inductively but one's reasons were known directly

causes resulted in movements and not actions

Davidson found the differences were not as absolute as was usually claimed and there were more similarities than differences (Cf. Pears, 1973; Davidson, 1973; Dennett, 1973). Most of the subsequent philosophical and psycho-analytic discussion has endorsed Davidson's conclusions.

Motives and dualism

Actually, in the late 1940's, well before Home, the distinction between motives and causes began to be made (or was revived) in relation to psycho-analysis by Toulmin (1948). At about the same time, in his *The Concept of Mind*, Gilbert Ryle (1949) also made an attempt to deal with the causal contribution of motives. Even at this point, the conflict was never substantial enough to give comfort to those who believe psycho-analytic explanations apply to a domain separate from that of science.

Toulmin (1948) based his distinction between motives and causes on an explicit dualism, as Flew who developed it made clear. For Flew, motives were "quite intangible and insubstantial while efficient causes notoriously have to be substantial". Therefore the causes of "real and palpable" actions could only be the "substantial" physiological processes sub-served by "solid, visible, tangible" neurones (Flew, 1949).

Toulmin had differentiated between three types of factor used in explanations of behaviour. Two were as intangible as they were motivational: the individual's *stated* reason for his or her behaviour and the reason *reported* for that behaviour by someone else. Only the third factor was tangibly causal: the events that explained how the reasons had been acquired. Psycho-analytic explanations constituted a fourth type. They gave patients reasons for their behaviour, the reasons were one with which others familiar with the case would agree, and they necessarily drew on a causal history of facts from the patient's early life. Psycho-analytic explanations therefore combined causal and motivational factors (Toulmin, 1948).

Ryle was not primarily interested in the motive-cause distinction as much as with overcoming the consequences of Descartes' rigid separation of the mind from the body. When he tried to bring motives under a non-dualist but causal rubric it was within his larger argument (Ryle, 1949). For Ryle the word 'explain' had two meanings. The first was the causal one used when explaining, for example, that a glass broke 'because' a stone hit it. The second sense explained the breakage 'because' the glass was brittle, that is, because it had a disposition to break. After showing the logical connection between the two kinds of cause (Ryle, 1949, p.89), Ryle then defined 'motives' as the dispositions or propensities for the individual

to behave in certain ways. Consequently, his argument was that explanations by motive were explanations of the dispositional type.

Just as with the brittle glass, a specific causal event had to be added to the motive if one wanted to know why a particular behaviour occurred at a particular time. Politeness might be the motive from which a person, say, passed the salt when asked but it was merely an inclination to be polite generally. What made the person pass the salt at that moment was hearing someone ask for it, a cause with which, Ryle said, "we are perfectly familiar" (Ryle, 1949, pp.113-114). Showing actions had causes in this way did not conflict with their having motives; a cause was already prescribed in the initial logical conditions which stated the motive (ibid.). Hence, for Ryle the question why a person acted in a certain way asked either for an explanation by requesting the specific cause of the action, or it asked about the motive, that is, the character or disposition. We knew quite well, said Ryle, what caused someone to scowl and slam the door - he had been insulted (Ryle, 1949, op. cit. p.325). In this "everyday sense", "we can all give 'causal explanations' for many of our actions and reactions" (ibid.).

In the comments which were made on the Ryle and Toulmin arguments two things stand out. The first is the varied meaning of the term motive and the extremely limited scope of Ryle's concept of motive-as-disposition (Peters, 1952, 1954, 1958/1960, pp.32-33. Cf. 1952; Urmson, 1952; Pap, 1959, pp.288-289). Second, causal status of some type was granted directly to some or all of the terms to be considered as motives or reasons (Pap, 1959. Cf. Peters, 1958/1960, pp.12-16). Toulmin himself granted that dispositional motive-explanations did not differ "in any striking respect" from ordinary causal ones (Toulmin, 1954) and Flew also softened his stance. Originally he had maintained that though a given behaviour could be both motivated and caused it was not possible to translate the language of causes into that of motives. Now he apologised for representing the difference "as an unbridged and unbridgeable gulf" (Flew, 1954. Cf. Flew 1949). From his frankly dualist position even Toulmin accepted that a *compelling* reason had causal efficacy and that reasons could be assimilated to causes (Toulmin, 1970b).

Reconciling motives and causes

As early as the late 1950's, therefore, it was clear that some reconciliation of reasons and causes was possible. The most important step then seems to have been taken when Davidson (1963) defended what he called the ancient and common-sense position that reasons which explained actions by giving the agents' reasons for them - what he termed *rationalizations* - were a species of ordinary causal explanation. For a reason to rationalise an action, the doer had to have some kind of positive attitude towards certain kinds of actions together with a belief that their own action was of that kind. Davidson also observed that if causal explanations were wholly irrelevant to the understanding of actions, the connection between

reasons and actions indicated by the word 'because' in sentences like "He did it because ..." could not be understood at all. Aristotle tried to close the gap via the causal concept of 'wanting' but that concept had been found too narrow (Cf. Brandt and Kim, 1963). 'Rationalisation' was also wider in scope than 'disposition': the positive attitude included desires, wants, promptings and all sorts of views, principles, prejudices, and the like. On Davidson's analysis, reasons were causes.

Moore developed Davidson's notion of rationalisation by showing that beliefs and desires could function as a species of cause in Aristotle's "practical syllogism" (M. S. Moore, 1980). To simplify, if X gave their desire to cool a room as their motive for opening a window, the motive was causal provided that it really was their desire to cool the room (major premise) and that it was X's belief that opening the window would in fact cool it (minor premise). The particular set of desires and beliefs *causally* explained X's action because, although X may have had any number of belief/desire sets, just that one "caused" them to act as they did. X's action was also *rationalized* by those same beliefs and desires because they portrayed the action as "the rational thing to do". A true motivational explanation, Moore went on, had to provide both rationalisation *and* cause.

Subsequent to these discussions, Grünbaum made the very cogent point:

> the causal relevance of an antecedent state X to an occurrence Y ... is a matter of whether X - be it physical, mental, or psycho-physical - *makes a difference to the occurrence of Y, or affects the incidence of Y.* (Grünbaum, 1984, p.72. Emphasis altered, MBM)

Grünbaum gives the example of someone who desires to read a book and believes it is available at a library:

> If that combination of desire and belief actually prompts him/her to go there to borrow the book, then his reason (motive) M for doing so qualifies as explanatory *just because M makes a difference to going*: when the agent neither needs a book nor has any other business at the library, i.e., when he has no motive (reason) for going there, then he indeed refrains from going. (op. cit., p.73. My emphasis, MBM)

Grünbaum correctly concludes that his analysis vindicates Holt's assumption that "a reason is one kind of cause, a *psychological* cause" (Holt, 1981). Of the many others who also made Holt's 'assumption', the most notable was Freud himself.

Freud's treatment of motives as causes poses a problem only to those who are not prepared to abandon Descartes' dualism and/or step outside the exceptionally restricted field within which Hume allowed causes to act. A rigid opposition between cause and reason is no more necessary than one between mind and body.

THE ALTERNATIVES PROPOSED BY THE CRITICS

Having considered the components of the different kinds of criticism, I now try to relate what has been found to the critical positions themselves.

The natural-science critics

The natural-science critics like Pumpian-Mindlin (1958-1959), Peterfreund (1975), Rosenblatt and Thickstun (1977), and Reiser (cited in Wallerstein, 1985) all welcome and foster attacks on Freud's metapsychology. They are not motivated by an opposition to psycho-analysis as a natural science, however. Nor are they against abstract or higher-level theories *per se* - they may even emphasise, like Reiser, the importance of staying close to the clinical level. Their wish is to develop psycho-analysis on a different basis from Freud. They derive their explanations of what is *meta* to consciousness from physical field theory, information processing theories, systems theory, and modern neurophysiology respectively. In brief, the aim they share is to substitute a new metapsychology for Freud's.

These new natural-science proposals have been little discussed in the psycho-analytic literature and have won practically no support. Actually, as Warme observes, the majority of psycho-analysts are "troubled, if not shocked" that metapsychology is even under attack (Warme, 1981. Cf. Ross, 1980). Ellman and Moskowitz (1980), Meissner (1979c, 1981b), Ornstein (in Chattah, 1983), and Erlich and Blatt (1985), believe that these analysts typically refer to one or other of Freud's theoretical perspectives, especially the economic and structural, as having immense value. Some, like Lotto, add the fear that giving up the concept of instinctual drive would deprive psycho-analysis of the one concept which unified its basic concepts "into a coherent, interrelated whole" (Lotto, 1982). Even though we saw in Chapter 14 how the use of the metapsychological viewpoints was often wrong-headed, in that metaphorical description was confused with explanation, it is equally clear that disquieted analysts have not been won over to any of the supposedly more modern positions.

A quite different kind of criticism, one of the kinds of bases chosen for the new natural sciences, has been made by Gill. According to him, psycho-analytic theories should be developed in the language used in the "common discourse of psycho-analysts" rather than, as he sees it, in other inappropriate and forced translations (Gill, 1977). At best, Gill's criticism is superficial and at worst it is irrelevant: what is important in a theory is its explanatory power, not its language.

The different-science critics

Those seeking to establish psycho-analysis as a different kind of science usually make a sharp distinction between scientific-causal and

personal-motive or-reason theories. What most frequently results are the prototypal formulations of Schafer and G. S. Klein.

Schafer focuses on the purposively acting person and derives his explanations from reasons rather than from drives or motives. His alternative requires Freud's metapsychology to be jettisoned completely and a fresh start made by describing the behaviours to be explained in a new and, what he takes to be, more theoretically neutral language (Schafer, 1976). G. S. Klein, on the other hand, wants to explain behaviour precisely in terms of motives although, it is true, he also focuses on aims, reasons, and meanings. He wishes to save what he calls the clinical theory of psycho-analysis from the metapsychology. Klein's central concept is that of a self-structure, rather than an ego. He places great importance on the role of the self in resolving incompatible motives, attributing this to a need for the self to maintain itself as a unified structure (G. S. Klein, 1976).

The main point to be made about Schafer's and Klein's proposals is that the basic distinction on which their criticism rests does not hold. Motives and reasons both cause behaviour, a point generally not brought out sufficiently strongly in the otherwise extensive discussions of Schafer's proposals by Meissner (1979a, 1979c), Rawn (1979), Calogeras and Alston (1980), Ellman and Moskowitz (1980), Anscombe (1981), Modell (1981), and Spence (1982b), and of Klein's by Ross (1980), Eagle (1980), and L. Friedman (1980), or of both by Frank (1979).

Many psycho-analysts have accepted a good deal of Schafer's and Klein's criticism along with other criticism from the difference-science position. However, almost all commentators have turned away from the particular theoretical directions to which they point or have expressed considerable doubts about them. Among other things, by dismissing the concepts of drive and motive Schafer places restrictions on what psycho-analysts are able to consider as clinically important. Klein has a similar difficulty with sexual motives and, in addition, has to adopt a nebulous Rogers-like self and revive an almost Janet-like dissociation as a substitute for repression. There are also major logical inadequacies in Schafer's concept of person and in Klein's of self-structure. Perhaps of greatest significance is the inability of either to provide a coherent account of unconscious motivation. [4]

The narrative-science and hermeneutic critics

What we found about psycho-analytic narratives can be briefly summarised. Narrative explanations do not differ in their logical structure from explanations generally. Apart from the groundless distinction between

4. Klein and his supporters also imply that the clinical theory he foreshadowed is not metapsychology. The implication is quite misleading. Invoking the interplay of mental forces to explain symptoms was to turn, as Freud said, to "dynamics", that is, to an explanation deriving from the dynamic metapsychological viewpoint (Freud, 1924c, p.193).

cause and motive or reason, the main problem is the absence of standards for judging the validity of a narrative. For those not interested in truth, the problem of choosing between equally plausible narratives remains. These points are well brought out in the main published discussion of narratives - the symposium on Sherwood's analysis of narrative explanations (Rubinstein, 1973; Eagle, 1973. Cf. Sherwood, 1969, 1973).

A hermeneutic 'reading' of the patient's discourse is supposed to lead the patient to a better understanding of the reasons and motives governing his or her life rather than to an explanation of it. Some of the difficulties with the narrative position overlap with those of the hermeneutic, even though the two may be essentially different in aim (Sherwood, 1973). Both share the problem of choosing between alternative readings. Obviously if one is interested only in the transformation of understanding, rather than in what it becomes or causes, the problem of different readings goes away .

The indeterminacy of hermeneutic readings has the same source as that of narratives: double construction in interpretation. The logical difficulties of hermeneutic readings are much more profound than those of narratives, however. Sherwood thought it made sense to ask about the truth ('accuracy') of narratives but the constructivist view of reality taken by hermeneuticists disallows that question. According to them there is no uninterpreted reality; reality comes into being through the act of interpretation. Now, as Petocz put it in her examination, any coherent theory of symbolism requires three independently characterisable terms: signifier, signified, and subject. The constructivist assumptions of hermeneutics violate this requirement by collapsing signifier and signified into the one term (Petocz, 1988a). She brings out the consequence in her contemporaneous critique of the place of hermeneutics in psychology. Any reading or interpretation requires:

> some *thing* to be interpreted; and to a hermeneuticist who claims - "there is no reality, only an 'interpretation' of reality", the reply is: 'an interpretation of ... *what*?' (Petocz, 1988b)

Bringing a hermeneutic reading to a conclusion, if only temporarily, by what hermeneuticists term 'closing the hermeneutic circle' is no solution. Closure is reached when the to and fro movement between the whole and the parts of the patient's story results in the emergence of some consistent pattern which allows all the details to be understood (e.g. Ricoeur, 1970, 1974, 1977; Steele, 1979). Emergence of pattern is little more than the notoriously faulty jig-saw puzzle test Freud used to test the childhood seduction memories and which underlies his equally doubtful analyses of Dora, Leonardo, the Rat Man, and Michelangelo's Moses, to name only the ones I have discussed.

A number of commentators has vigorously defended the scientific aspirations of psychology and psycho-analysis against hermeneutic criticism (e.g. Brenner, 1980; Ellman and Moskowitz, 1980; Modell, 1981).

They especially dispute the analogy between interpreting passive and unresponsive text and interpreting the free associations of the living patient with whom the analyst-reader interacts. Some of them do grant the meaning-cause distinction to be valid but do not see the hermeneutic and scientific enterprises as being incompatible. The dilemma for most of these theorists is pithily summed up in Meissner's contrast:

> Hermeneutics without metapsychology gives us meaning without structure; metapsychology without hermeneutics gives us structure without meaning. (Meissner, 1981b)

The contrast may not signal an absolute opposition. Both Muslin and Holt (in Chattah, 1983) have asserted that Freud's empathic listening to his patients was influenced by his metapsychological propositions. Muslin says "the data of the clinical encounters" with Dora, the Rat Man, and the Wolf Man are "recognized in a fashion that is reminiscent of the line of theorizing that has its roots in the 'Project'". To this Holt adds that metapsychology "actually directed Freud's observations" and is "intertwined with the clinical theory".

While this may be true, what Freud meant by meaning and the way he used his data was rather different from contemporary hermeneuticists. Reducing the opposition in this way does not answer the most telling of the criticisms, those by Grünbaum, in relation to psycho-analysis, and Petocz in relation it and to psychology more generally. Both Grünbaum and Petocz rebut the by-now-familiar hermeneutic claims that there cannot be such a thing as a scientific psychology because science is ahistorical, unable to encompass human purposes, or give causal explanations. Both also attack the test of narrative coherence, Grünbaum in breadth and detail, Petocz with philosophical depth. Petocz brings out particularly clearly the logical and conceptual confusions in the consequences of the hermeneutic choice of a constructivist over a realist epistemology: the failure to grant ontological priority to existence over expression, particularly linguistic expression, the hermeneutic rejection of language as a referential system, and the promotion of what hermeneuticists term "values" above what they term "facts". Although much of Grünbaum's attack has been discussed, the limited response to these particular rebuttals is inadequate (Grünbaum, 1984; Petocz, 1988b. Cf. The Commentary in *Behavioral and Brain Sciences*, 1986, *9*, 217-284 as well as Ferguson, 1985; Evans, 1986; Forrester, 1986; Sharpe, 1986; Spruiell, 1986; Strenger, 1986).

We have seen that choosing between different hermeneutic readings is as impossible as choosing between traditional psycho-analytic interpretations, reconstructions, or narratives. It may be this which brings about the downfall of the hermeneutic position. How can one judge the emotional, cognitive, or therapeutic value of a hermeneutic reading? The main issue of interest to psycho-analysts which hermeneuticists exclude is that of development. Not only does one have to forgo an understanding of how

the patient comes to be as he or she is, but all theories of child development become impossible.

Being able to restrict psycho-analysis to a science of meaning, as Frank, Rosen, and Ricoeur have done, or to a kind of religious exegeses as do such hermeneuticists as Lacan and Chabot, is also only possible, Brenner argues, by overlooking the similarities of the methods and data of psycho-analysis to those of the other sciences. These critics almost never refer to the psycho-analytic method and prefer to concentrate on textual analyses rather than on the interactions which Freud believed led to an understanding of causes (Brenner, 1980). As I understand Brenner's point it is that were they to accept Freud's view of the nature of the method, they would have no choice but to class psycho-analysis with the other natural sciences.

THE APPEAL OF PSYCHO-ANALYSIS

Criticism of psycho-analysis is not new and we should ask why Freud's theory continues to appeal. I believe there are five main reasons. First, most lay people, as well as a large number of non-analyst professionals, think of psycho-analysis as beyond substantial criticism and as not much changed from the ideas put forward by Freud. Second, the understanding which psycho-analysis gives of the determinants of behaviour and personality seems to be especially extensive. Third, there is the attraction of the irrational which appeals in and of itself. The psycho-analytic irrational also appeals because many aspects of it are like processes familiar from everyday life and not at all difficult to understand or apply. Fourth, psycho-analysis concentrates upon precisely those things in which people have the greatest interest and about which no other discipline says anything very much. Fifth, most people take it for granted that the effectiveness of psycho-analysis as a therapy for a wide range of disorders and problems is well established and certainly not a matter of dispute. Several aspects of these reasons are worth discussing even though there is little hard data bearing on the contribution they and others make to the continuing appeal of psycho-analysis. I have, therefore, to apologise for abandoning my so-far dull position of fact to take up the more exciting stance of speculation.

Criticism is not known

First, that none of the ideas Freud thought to be central to his theoretical perspectives has escaped attack, for the most part by psycho-analysts, is simply not known outside of psycho-analysis. It is not recognised how much of Freud's theory has been criticised, nor how profound that criticism has been, nor how little of his theory would remain if all the criticism was accepted, nor how much of it has in fact been accepted within psycho-analysis. Who outside of psycho-analysis or related circles knows, for example, of the devastating criticisms by Holt,

Bowlby, and S. Brody, of the instinctual drives, the mental structures, or the developmental theses? Who has read Pumpian-Mindlin or Peterfreund or Rosenblatt and Thickstun or Reiser, for example, all of whom seek to explain what lies behind conscious mental processes with more powerful weapons than those in Freud's poorly equipped armoury? Who is familiar with the proposals by G. S. Klein and others to do away with almost all of Freud's central theoretical concepts and found a purely clinical theory? Who is aware of the force of the case Schafer has made for completely discarding Freud's concepts and starting afresh at the most basic level - that of uncluttered observation - as if psycho-analytic theory had never existed? Were these things known and their significance properly appreciated I suggest psycho-analysis would lose a lot of its appeal. If to this were added knowledge of the inadequacy of the psycho-analytic method for inquiring into the human condition, the almost total disagreement among psycho-analysts on how to interpret the data gathered by it, and the lack of evidence for the effectiveness of psycho-analysis as a psychotherapy, the appeal, to rational beings at least, would be even less.

Pseudo-explanatory power

Second, on their first contact with psycho-analysis few people escape the feeling that they have been introduced to an extremely powerful explanatory system. But they do not appreciate the paradox on which that conviction rests. Psycho-analytic explanations and interpretations are basically indeterminate, even though they have the appearance of being comprehensive and rigidly deterministic. Nothing seems to fall outside the explanatory net, but one cannot be too sure what has been caught.

The paradox partly comes about because there are no agreed on rules for interpreting so-called products of the unconscious. The interpretations of Dora's symptoms and the dreams of Freud and Dora, which we examined in Chapter 9, of supposedly perverse sexual behaviour (Chapter 10), and of aggressive behaviour (Chapter 12) show how phenomena like these can be interpreted in almost any way at all. Because each presumed cause is also thought of as connected with every other, causes can be combined to make any number of plausible explanations. Consequently, any one psycho-analytic explanation of say, Schreber's psychosis (Chapter 11) or the development of masochism (Chapter 12) is, within broad limits, as good as any other.

The absence of rules for arriving at interpretations and evaluating explanations is only part of the problem. Much more basic is the use of the double-construction in translating from the language of consciousness into the supposed language of the unconscious. Nothing is known or can possibly be known about this 'language' which is independent of the act of interpretation. For the hermeneutic interpretation of psycho-analysis this indeterminacy poses an especially acute problem.

Nothing of this matters to those not concerned with how patients come to be the ways they are or what might be done to promote healthy development. To judge from the welcome Kermode (1985) gave recent interpretive revisions of Freud, psycho-analytic explanations appeal to those interested in history, philosophy, and the arts, and especially in literature and literary criticism, precisely because they already have a high degree of tolerance for other equally indeterminate endeavours (Cf. Timpanaro, 1974/1976, pp.223-224; Chomsky in Anonymous, 1989).

For the amateur, the most important consequence of the absence of criteria for evaluating the adequacy of translations, interpretations, and explanations made by psycho-analysts is that he or she is at no disadvantage. Amateurs can arrive at explanations of their lives, their ideas, their feelings, or their behaviours which are just as plausible as those of the professionals.

The appeal of the irrational

Third, there is the appeal coming from the importance psycho-analysis attributes to irrational processes. At some time or another nearly every one has believed themselves to be at the mercy of forces they do not understand. Many are therefore ready to believe in the irrational. There can be few psychologists who have not had at least one of their prosaic explanations of an unusual experience, say, a premonitory dream, rejected in favour of an irrational one. Similarly, a large part of an audience witnessing a magician demonstrating 'extrasensory' powers will prefer an irrational explanation for what they have seen even after the magician assures them that only routine stage magic was involved. It is easy to accept the possibility that the unconscious and irrational processes underlying psycho-analytic explanations do exist because they seem to reflect so much ordinary experience.

A further aspect of the appeal of the irrational is what Wittgenstein called the charm of psycho-analysis, a charm coming from the resemblance which Freud's unconscious motivational explanations have to ordinary ones. For all Freud's talk of a chaotic and irrational primary process, the unconscious wishes and motives with which he explains dreams or slips of the tongue seem just like ordinary ones, acting in exactly the same way as their conscious counterparts. Although the components from which psycho-analytic explanations are constructed may be irrational, they have the charm of a familiarity which makes it relatively easy to grasp how they cause their effects. Nowadays, when so many of the concepts and technical terms of psycho-analysis are in common use, psycho-analytic ideas and the practise they engender are very easy to accept. There are few films or television series dealing with psychological matters that do not include a psychological miracle worker, often a psycho-analyst or a psychiatrist or psychologist modelled on one. Quite young children may even become familiar with psycho-analytic theoretical concepts as when the id of the

main character (a kind of evil destructive double) was featured in an episode of *Dr. Who*, the BBC children's television programme.

It is probably not an accident that the feeling of conviction generated by psycho-analytic explanations is greatest for dreams and parapraxes. There is no doubt these phenomena belong with the more irrational and puzzling aspects of our mental lives and it is for them that the analyst's explanations seem most complete, the unconscious processes most like conscious ones, and the motives uncovered the most reprehensible even when they are not sexual. Coming to Freud for the first time, we find we already understand the purely conscious instances of motivated forgetting and have little difficulty with the preconscious ones. It is then but a short step to accepting Freud's examples of unconscious motives along with the rest of the theory. When, in turn, we come to supposed unconscious lusts and hatreds, we have been readied to find they, too, resemble our conscious drives. Our self-applications, now easily made, produce a high level of conviction.

Freud may have been aware of the charm and power of his conceptualisation of unconscious processes. Certainly he frequently capitalised on the postulated resemblance between them and conscious processes. Thus, he introduced his major work on parapraxes, *The Psychopathology of Everyday Life*, with what Shope (1970) shows to be a demonstration of a continuum of errors. As Freud set it out, slips of the tongue varied between those supposedly produced by counter-intentions of which we are aware at the time to those produced by repressed unconscious impulses. In between are those caused by the preconscious motives or counter-intentions we can fairly readily bring back to consciousness. But, whatever their type and wherever they are located, these counter-intentions act on the primary intention in exactly the same way. We also find Freud playing on the conscious-unconscious parallel extensively in the *Introductory Lectures*. The first four lectures are on the parapraxes and the next eleven on dreams. Only then does he begin the thirteen on the neuroses (Freud, 1916-1917). Certainly others have recognised the power of the parallel. Timpanaro, for example, cites and endorses Musatti's view that after 'understanding' the simple parapraxes, the reader of Freud's *The Psychopathology of Everyday Life* "is destined to become an adherent of analytic theory" (Timpanaro, 1974/1976, pp.15-17. Cf. p.105).

The appeal of Freud's parallel is inversely related to the strength of its logical foundations. Tannenbaum and Timpanaro have pointed out that many of Freud's interpretations of parapraxes depend on a verbally competent unconscious and so contradict his basic postulate that unconscious processes are irrational and non-verbal. The inconsistency is especially marked when the interpretation requires the supposition that there has been a complex translation between languages in which the speaker is not fully proficient (Tannenbaum, 1922; Timpanaro, 1974/1976, pp.78-81). The point also applies to dreams. Progress from parapraxes and dreams to the

further reaches of Freud's theory is made easier if one does not know of these conceptual inconsistencies. But, when Freud first proposes the conscious motive as model, there seems to be no difficulty about understanding the unconscious ones.

Sexuality

Fourth, there is the emphasis on sexuality. There is no doubt this was part of the attraction which psycho-analysis exercised in its early days. While it is alleged that a sexual revolution has occurred, that same emphasis on sexuality seems to me to appeal to young people today (i.e. the undergraduate students I know) as strongly as it did to those two or three generations ago. Even among those who have some knowledge of the revolt of the object-relations theorists like Fairbairn or Winnicott, or perhaps just a dim understanding that Freud's sexual theories are out-of-date, psycho-analysis continues to licence public interest in a matter in which almost everyone is still curious. Today when people derive explanations for their behaviour from their own sexual unconscious motives, what they arrive at seems as valid as it did years ago.

The appeal of therapy

What of the appeal of psycho-analysis as therapy? Why are patients and psycho-analysts so convinced that what they do brings about change? We must start by reiterating that psycho-analysis is actually no more effective than other verbally based psychotherapies and marginally less effective than non-talking treatments. Equivalence in outcomes necessarily means, as Lakoff (1982) has argued, that nothing in *the content* of the verbal interaction can be responsible for such effects as are produced.

I have argued elsewhere that we cannot go much beyond this confession of ignorance (Macmillan, 1986). Some factors can be ruled out completely, however, particularly the personality characteristics of therapists and patients or the relation between them. What, if anything, about the relation that promotes change is unknown. It is certainly not the qualities determined by the therapist's genuineness, accurate empathy, and non-possessive warmth - the conditions Rogers once proposed as necessary and sufficient for therapeutic change (Rogers, 1957. Cf. Mitchell, Bozarth, and Krauft, 1977). Even were they or the relationship with the patient or client critical we lack concepts, psycho-analytic or otherwise, to bridge the gap between them and the consequences they are supposed to produce.

The Columbia Study and the other studies of psycho-analytic therapy cited by its authors show psycho-analysts to be as ignorant as anyone else about the basis of therapeutic effects. First, analysts are unable to predict at much better than a chance level who will benefit from their therapy. Even though the factors held by their ''accepted clinical wisdom'' to predict outcome can themselves be predicted to some degree, those factors have almost no relation to outcome. Observations made while the analysis

is in progress do not help: "One must wait until an analysis is over before one knows what the final result will be" (Bachrach, Weber,and Solomon, 1985). Therefore:

> once a patient has been carefully selected as a suitable candidate for psychoanalysis ... the eventual fate of the treatment is only marginally predictable. (Weber, Bachrach, and Solomon, 1985b)

As with the psychotherapies generally, neither is there any connection between outcome and the qualities of the analyst.

Second, those patients in whom an "analytic process" will develop and who will be "analysable" cannot be identified before treatment begins. That is, it cannot be foretold who will manifest transference, use their self-observations and dreams, etc. during therapy, or use insights gained from therapy in everyday life. Even were the analytic process predictable, there is the damaging fact that when it does develop there is little or no relation between it and whether the patient benefits from treatment. The Columbia investigators summarise a number of earlier studies as showing that in only about 50% of the improved patients were signs of an analytic process reported. Their own investigation also found only a "modest" relation (correlations of 0.3 to 0.4) between its development and outcome. If I am interpreting this finding correctly, it means that whether patients responded to therapy or not was largely independent of whether there were signs that a *psycho-analysis* had taken place! Now, while the highly selected nature of the patients and psycho-analysts did make prediction difficult, these findings are consistent with the data on the other verbal therapies. In my view they are also consistent with the Menninger Foundation Psychotherapy Research Project finding that most of the change which did take place in their patients was due to the "supportive" elements in treatment rather than to the factors postulated in the classical psycho-analytical theory of therapy: insight through conflict resolution (Wallerstein, 1986c, pp.718-730).

What does happen in therapy The fact of the matter is that no one knows what brings psychotherapeutic effects about. Two important things have to be taken into consideration in trying to find out why therapists and patients think it is what they do that causes such change as does occur. First, there is no evidence that psycho-analytic therapy produces changes over and above those brought about by non-systematic treatment. Second, even the most effective of the psychological therapies, usually the behaviourally based ones, do not do that much better. A success rate of 85% is not, after all, a great deal higher than the 65% base-rate. We ought therefore to be asking why the base-rate as high as it is rather than simply using it as a standard for evaluating systematic treatments. The question would still be worth asking even were the rate as low as the 40% some authors accept. In other words, what causes so-called spontaneous remission? Finding out why no therapy is needed in so many cases requires us to turn away

completely from current therapies. If this were done, I believe we would discover what factors really are at work.

My own belief is that patients or clients seek help precisely at the point when they have decided to try to regain control over some part of their life. For most patients, what happens in the psychological and behavioural therapies is that they learn to take responsibility for themselves. It may be that the content of the therapy does not matter (Macmillan, 1986).

Some psycho-analytic opinion is consistent with my view. Of interpretation in psycho-analytic therapy Glover has said:

> Should the analyst's interpretations be consistently inaccurate then quite clearly he is practising a form of suggestion ... It follows then that when analysts differ radically as to the etiology or structure of a case ... one side or the other must be practising suggestion. (Glover, 1954)

My point is not that it is as logical to conclude that *both* sides are practising suggestion, which it is of course, but rather that the outcome of a psycho-analysis is independent of interpretation. Glover had also shown earlier that it was independent of technique (Glover, 1937) and the Columbia study shows it may be independent of whether a 'psycho-analysis' even takes place or not.

What therapy provides is a framework within which the individual can come to understand and explain his or her behaviour. Several analysts have said exactly that about psycho-analysis. According to Marmor, for example:

> what we call insight is essentially the conceptual framework by means of which a therapist establishes ... a logical relationship between events, feelings, or experiences that seem unrelated in the mind of the patient. (Marmor, 1962)

And, even though he grants that what happens in therapy "is still mysterious", Klauber makes much the same point:

> when the analyst gets a clear idea of what unifies the patient's associations and communicates it, something therapeutic happens in the patient. (Klauber, 1980)

Auerhahn's discussion of the research of Bieber and the observations of Anthony on the transcript of Dewald's sessions with a single patient bring out very clearly that patients come to take over the style of speaking, the mode of thinking, and methods of analysis and interpretation used by their analysts. After an analysis has been completed, Auerhahn suggests that:

> if the analysed patient is successfully to meet future trials and tribulations, then he must take away with him not only (static) content but a structure and mode of interpretation. Analysis is terminable *when the patient has*

> *internalized the process* and learned to self-reflect constructively. (Auerhahn, 1979. My emphasis, MBM)

Similarly, Weiss concluded that dreams are used in therapy to build up meaning *for* the dreamer. According to him, the inherent meaning *of* a dream and the meaning it might have *for* the dreamer are not the same. There are no criteria by which meaning-of can be evaluated or its congruity with meaning-for established but meaning-for can be created even when the themes developed through the dreamer's associations are related "only very loosely or perfunctorily" to the dream report. Weiss implies that the real use Freud made of dream associations was to build up meaning-*for* (Weiss, 1974). One might presume from the Columbia study finding of a "modest relation" between outcome and length of treatment that the longer a psycho-analysis lasts the more time patients have to build up 'a logical relationship' between the different parts of their lives and to construct meanings for themselves.

In that many non-analysts have *speculated* that the effective ingredient in therapy is the construction of meaning for the patient, none of this is all that new. What is new is that some analysts now give credence to and extend what was originally an heretical opinion of Marmor's. For example, Basch, has recently outlined a theory explaining how interpretation could give rise to the cognitive transformations underlying changes in therapy (Basch, 1981). Should any significant numbers follow in these new directions, psycho-analysis may cease to be. Psycho-analysts will then come to grips with the fact that transformations need not involve truth. A widespread recognition that the truth of psycho-analytic theory is independent of its plausibility and that its truth has no bearing at all on what little special effectiveness it does have as therapy would completely undermine any claim of psycho-analysis to uniqueness. Apart from its roles as a belief system and a social movement, its status would revert to that of any of the other poorly understood psycho-therapies.

CONCLUSION

When Brenner reiterated his characterisation of psycho-analysis he did so in essentially the terms we found Freud to have used: psycho-analysis is different from other sciences only in the trivial sense of its different subject matter or field of enquiry making it so. For Brenner, as for Freud, science is defined by its empirical approach and pragmatic and empirical attitudes. There was 'no science other than natural science' and nothing to prevent psycho-analysis from being, or becoming, a branch of science. While its data had meaning, what psycho-analysts did with their data was 'no different in principle from what any other scientists do' (Brenner, 1980. Cf. Brenner, 1968; Edelson, 1977; Holt, 1981; Wallerstein, 1986a).

Should we therefore conclude that psycho-analysis is a science? My evaluation shows that at none of the different stages through which it

evolved was Freud's theory one from which adequate explanations could be generated. From the very beginning, much of what passed as theory was found to be description, and poor description at that. The concepts of the *Project*, for example, were pseudo-physiological and based on little more than a direct translation of Freud's ideas about condensation and the summation of stimuli into neurophysiological concepts which were inadequate and out-of-date in their own day. Freud's key psychological concept of repression was nothing but an objectification of his own sense of effort in overcoming resistance and had no greater explanatory power than realisation, hypnoid isolation, or dissociation. In every one of the later key developmental theses, Freud assumed what had to be explained: in the transformation of the primary process into the secondary, in the resolution of the Oedipus complex and the formation of a superego having the right sexual qualities, in the fusion and defusion of instinctual drives providing the different kinds of energy to it and the ego, and in the development of adult libidinal sexuality, character traits, and object-choice.

What then of the potentiality of psycho-analysis to become a science? There are two related issues. First, no discipline exists apart from its practitioners. It is their attitudes which determine whether they use their methods to gather data and develop and test theories in an objective way. From the differences between chemistry and alchemy or between psychology and phrenology we see how much attitudes matter. In the history of psycho-analysis there have been many occasions when scientific attitudes were not noticeably manifest. They were almost completely absent in the evaluation of the Rank birth-trauma theory and completely so in the attempt to resolve the Melanie Klein-Anna Freud differences. It is entirely in keeping with this tradition that discussions of contemporary variant theories of psycho-analysis are similarly deficient in those attitudes and that the kinds of programs for methodological reform outlined by Wallerstein and his colleagues have still not been adopted (Wallerstein and Sampson, 1971; Wallerstein, 1975).

Second, at least until very recently, neither the defenders of Freud's psycho-analysis nor its psycho-analytic critics doubted the objectivity of the psycho-analytic method. For both, the method is supposed to have established certain basic facts reasonably securely. All that is or ever has been questioned are the sources of the theoretical constructs (e.g. in biological or cultural factors), the kinds of constructs (e.g. economic, structural, or dynamic), or the appropriateness of the type of theorising (e.g. at the clinical or other level). The few who do have doubts about the method do not question it in any depth and never ask whether it creates its data (e.g. Brenner, 1968; Arlow and Brenner, 1988). Questions like that demand a radical change in attitude and the explicit adoption of scientific scepticism. Until those questions are raised and answered, any potential which psycho-analysis might have to be a science must remain unrealised.

Over a hundred years ago the lesson psycho-analysts needed to learn was given by the hysterical patient in Brouillet's painting. According to Charcot, her progression into the arc-de-cercle was as inevitable as it was physiologically lawful. As I pointed out in Chapter 3, she could use the drawing of another patient who had already completed her arc as a model for her own. Charcot never accepted that the law-like regularities in the behaviour of patients at the Salpêtrière could result from unconscious influences of that and more subtle kinds. Nor did Freud after him.

When Collins (1980) examined Freud's defence of psycho-analysis against the charge that it was a version of suggestion therapy, he found it had three components. First, Freud dismissed the term 'suggestion' as having no definite meaning; second, he said it could be kept under control by various technical stratagems including the 'emotional coldness' of the analyst; it had never occurred in his own practice. Collins finds the first '-disengenuous', the second 'scarcely satisfactory', and the third 'astonishing' from someone whom he describes (incorrectly, of course) as 'the first to describe the unawareness of behaviour'. Freud did not believe in the importance of unconscious influences from expectations and demand characteristics and he did not guard against them. In fact, his particular beliefs about the internal determinants of psychological phenomena caused him to develop and use a method for gathering data which in its nature could not exclude them. None of his followers, including his revisionist critics who are themselves psycho-analysts, have probed any deeper than did Freud into the assumptions underlying their practise, particularly the assumptions underlying 'the basic method' - free association. None question whether those assumptions hold in the therapeutic situation; none has attempted to break out of the circle.

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INDEX

Abend, 361
Abend & Porder, 479, 484, 485, 486, 488
Ablon & Mack, 273 & n.2
Abraham H. C. & Freud, E. L., 354, 572
Abraham K., 335, 354, 537, 572; Schreber's megalomania, 336; oral & anal stages, 530-531
Abrams, 197, 268, 500
Abreaction, 11, 29-30, 161; Freud's expectations &, 219-220; mastery by, 373-374; physical 'explanations' &, 529; trauma & fractional discharge by, 409-410
Absence, 12, 23, 69; secondary consciousness &, 12, 23, 69
Absence of movement & absence of idea (Charcot), 153
Accumulation-transfer model of nerve impulse transmission, 182-185
Acting out: confusion over, 416
Actions & causes, 596
Active excitatory processes, 524
Actual neuroses, (Freud), 121-126, 131-143, 201-208: abnormal responses to excitation, 187, 189; counterinstances excluded, 203; etiological equation 137-138, 140-143, 202-203; explanation of symptoms, 162-165, 166, 203-207; Koch's postulates, 124-126, 137-140; precedence over hysteria, 123-124, 142-143; specific causes, 201-203; theory abandoned, 228
Acquired characteristics, inheritance of, 470
Adrian, 406, 525
Affect: delayed conversion, 118; & Jackson, 98; reversal of, 322; stripped from idea, 322; transformation of, 271
Affection: non-sexual feelings, 484; determinants, 517
Affective interpretation of Breuer's therapy, 11; Freud 99
Affective & cognitive experience, 30
Affects: discharge of excitation, 189, 458; psycho-analytic theory of, 463
Affects unconscious, 458-459
After-pressure, 331; and primal repression, 455-457; caused by signal anxiety, 455
Aggression & libidinal frustration, 326
Aggressive drive: does not resemble sexual & hunger, 428; no aim, 523; primary & unrelated to Freud's death instinct, 425; removal of biological foundations, 425; source unspecified, 523
Aggressive pleasure: mechanism unclear, 389
Aggressiveness: as mode of instinctual operation, 424; non-death instinct explanations of, 423-427; non-destructive aggression, 424; secondary destructive aim, 424; primary drive, 425
Ahlskog, 566, 571
Alexander, 405, 409, 417, 418, 421, 426, 427, 481, 543
'Aliquis' slip: alternative analyses, 555-556
Alleviating discharge & catharsis, 22, 25
Almansi, 541
Alternatives to Freud's psycho-analysis, 599-603; natural science, 599 ; different science, 599-600; hermeneutic-narrative science, 600-603
Amacher, 171, 172, 180, 182, 198, 287, 463, 525
Ambivalence, 342-343
Amnesia, hypnotic, 31; neurotic & infantile, 292
Ampère, 149 n.2,
Anal character: term of abuse (Fonagy), 540
Anal erotism, & sadism, 531
Analogies: instincts & anabolism & catabolism, 381; analyses of texts & associations, 602; sexual development & bean & apple tree, 316; deciphering texts & interpreting dreams, 586; dream & political censorships, 238; dreams & languages, 556; ego & water tap, 464; neural transmission & liquid flow, 182-184; germ-plasm & soma with life & death instincts, 380; germ theory & causes of neurosis, 126 n.2; ideas as actors, 24; ideas as parasites, 64; hysterical symptoms & languages, 556; mind & reflex, 264; telephone & mental structure, 503
Andersson, 1, 20, 84, 99
Angel, 551
Animal gravitation, 33
Animal magnetism, Mesmer's theory, 33-35, 541; evaluation of, 36-38
Anna O., 10-17, 75
Anna O.'s diagnoses: 16-18
Anna O., magnetic-illness features, 19, 67; 'cure' as foundation for psychotherapy 11, 20; prevision, 25
Anna O., mental states: absences, 10, 12, 13, 14, 69; abstraction, 10; altered consciousness, 16; aphasia, 18; autohypnosis, 11, 15, 20; caprices, 14, 159; auto hypnosis, 15; multiple personality, 17; second consciousness, 12, 13, 14, 17, 27
Anna O., origins of symptoms, 15: caprices, 14, 24, hallucinations 10, 11, 14; memories, 27; paralyses, 11; suppressed remark, 21, 25; secret listening, 25; snake hallucination, 11; stockingwearing 'caprice', 14
Anna O., phases of illness: first, 12; second, 12; third, 13; fourth, 15; evolution of illness, 25

Anna O. preconditions for illness: habitual day dreaming, 68; private theatre, 23
Anna O., root of illness: hallucination of snake & arm paralysis, 10, 16, 25, 68
Anna O., results of treatment: four relapses, 16; Inzersdorf Sanatorium, 16; retreatments, 16; Sanatorium Bellevue, 16; temporary remission, 25, 67
Anna O. symptoms: mental, 10, 12, 14, 16-18, 25, 27; physical: 10, 12, 15, 16, 18, 27, 111, 157, 461
Anna O. symptoms explained via hypnoid state, 158
Anna O., talking cure: 14, 15, 17-19, 20, 24; alleviating discharge, 25; Andersen-like stories, 21, 23, 27; calming of mood, 21, 24; extended to origins, 14; no effect on mental symptoms, 14; secret over come, 20-21; stories as freelycreated compositions, 27
Anna O., treatment: invention & direction, 19, 20, 25; hypnosis in, 15, 18, 19, 27; stages of: first, 20; second, 21; third, 25; fourth, 25
Anna O. & beliefs shared with Breuer 21
Anscombe, 600
Anthropomorphism, inadequately defined, 498
Anticathexis, in barrier-based theory of trauma, 516; cathectic energy & barriers, 376
Anxiety: characteristics, 454; emotion, 163; repression as cause, 450, 471; preparedness for trauma, 444; reproduction of, 459-461; signal, 443
Anxiety attack: & weak resistance, 193
Anxiety experience: prototype of, 466-470
Anxiety, libido, & repression: 204, 450-457, 470-474
Anxiety neuroses: conclusions about cause, 122, 201; excitation &, 187; explanation of modified by Freud, 164; inadequacy of theory, 163; inference from sexual anaesthesia, 164; incomplete gratification, 162; inhibition of sexual function, 136; source of anxiety, 136
Anxiety & toxins: 205-206, 451-453
Anxiety: traumatic & signal, 454-457, 457-474, 505
Anxiety theory revisions (Freud's): 204-207
Apfelbaum, B., 52
Apfelbaum, E. & McGuire, 526, 528
Appeal of psycho-analysis: speculation on, 603
Appeal of psycho-analysis: 603-610; attraction of the irrational, 605-607; conscious processes as model, 605-606; criticism not known, 603-604; double construction, 604; paradoxical feeling of conviction, 606; indeterminism, 605; lack of rules for explanation & interpretation, 604; pseudo-explanatory power, 604-605; sexuality, 607; therapy, 607-610
Applegarth, 350, 351, 463, 477, 524, 525, 527, 528, 529
Archaic heritage, 469, 541
Aristophanes, in Plato's *Symposium*, 383
Aristotle, 21, 22, 25
Aristotle on causation: practical syllogism, 598; 'wanting', 598
Aristotle's catharsis: 21-23; Bernays' medical interpretation, 21; Milton's interpretation, 22; *Poetics & Politics*, 21, 23
Arlow, 126 n.2, 271, 272, 315, 362, 370, 389, 425, 427, 457, 475, 476, 479, 481, 492, 497, 498, 500, 521, 561, 566
Arlow & Brenner, 264, 266, 267, 268, 271, 497, 564 n.2, 571, 573, 611
Arndt, 127, 133, 135, 174
Arnoult, 167
Associations: determination of trains of, 113, 549; disposal of excitation, 177; facilitation, 242; magic, 337
Associations & causes: causal connections (Meynert), 116; as causes, 552; Cäcilie forehead pains, 111; determined by past experience, 113; Elisabeth von R.'s leg pains, 111; lead to causes, 549; Lucy R.'s olfactory symptom, 111; mental & physical symptoms, 112, 177
Associations & excessive affect, 97, 160
Associations identical with causal connections & logical relations, 112, 172, 212, 588
Associationist psychology, physiological basis (Meynert), 111-112, 171-173
Associative inaccessibility, 93-100, 118, 159-160, 180; & Jackson, 98; & Janet, 93, 96
Associative reaction & conversion, 193
Assumptions of determinism (Freud's), 2
Attitude produces appropriate idea, 66
Attitudes to hypnosis, credulous & sceptical, 38
Attitudes passionelles (Charcot), 461
Auerhahn, 609
Augmentation solution of difference between child & adult sexuality, 320, 321
Automatic organic repression, 204, 284-286, 322-324, 328, 330-332, 341
Autonomic reactions & anxiety, 193
Auto-erotic & object-directedness distinction difficult, 517
Auto-erotism: & loss of breast, 298; & narcissism, 335, 358, 516; & movement to object-love, 517; & stimulus instinct difference, 344
Auto-hypnoses, true & spontaneous, 20, 27
Avery, 419
Azam, 45, 66, 68

Babcock, 276
Babinski, 72
Bachrach, Weber & Solomon, 545, 608

Bailly, 36
Bair, 152, 153
Bak, 419
Baldwin, phylogeny & ontogeny, 285
Balint, 312-313, 315-316, 482, 489, 517, 572
Balmary, 226, 227, 574
Balmuth, 586
Banalization & 'aliquis' slip, 556
Barber, 554
Barnett, 519
Barratt, 562
Barrucand, 51 n.3
Bartlett, 566
Basch, 489, 490, 551, 610
Basedow's disease, 206, 207
Basic phenomenon of hysteria, 101
Basis for evaluating Freud's personality theory, 508
Baudry, 540
Beard, 129, 131
Beard: analysis of sexual problems, 129; as Barnum, 130; enumeration of causes, 129-130, 140; evidence of excess, 129; explanatory theory, 129; functional view, 127; Freud & sexual factors, 133, 134; influence on Freud, 132; nationalistic explanation, 131; social theorising, 130
Beard & Rockwell, 129
Begel, 261, 262
Beliefs as causal explanations of actions, 598
Bellak, 362, 551
Bellamy's *Dr. Heidenhoff's Process*, 80
Belmonte Lara, 488
Bemporad, 312
Benedikt, 20, Benedikt's *Elektrotherapie*, 173
Benjamin, 465, 466, 467, 515
Beres, 498, 499 n.4, 500, 501, 504, 521
Bergin, 543, 545
Bergin & Garfield, 542
Bérillon, 53
Berliner, 419, 422, 423, 427
Bernard, 300
Bernays' medical interpretation of catharsis, 21, 22-23, 25, 30
Bernfeld, 170, 171, 174, 198, 408, 423, 424, 476
Bernfeld & Feitelberg, 406, 424, 427
Bernheim, 32, 42, 44, 48, 49, 52, 72, 76, 81, 90
Bernheim: approach to hypnosis, 47-49; attention, 173; Charcot's observations &, 48; Delboeuf's visit, 81; Freud's method, 74, 77, 76; Freud's *Preface*, 168, 170; Freud's translations, 76, 110; Freud's only therapy, 88; Freud's visit, 52; methods of induction, 48; *Preface*, 170; *Suggestion*, 94, 95; therapy & Breuer's method, 87
Bernstein's theory of nerve impulse transmission, 172
Bernstein, I., 260
Bernstein, D., 518, 519
Bernstein, M., 217
Berrien, 502
Bertrand, 40, 49
Bibring, E., 347, 348, 349, 363, 370, 389, 390, 391, 409, 410, 413, 424, 425, 427
Bibring, G. L., 482
Bieber, 309, 417, 419, 498
Binding, 375-377, 384; act preparatory to repetition, 384; damming of excitation, 393-397; independent of pleasure principle, 376; repetitive discharge, 395; instinctual process, 384; primary process discharge, 523
Binding & mastery: meanings not clear, 393-394, 527
Binet, 24, 46, 51, 69, 86, 90, 101, 151; associationist psychology, 158; 'fanciful' basis of hypnosis, 51; states of consciousness, 158; surrenders to Delboeuf, 51
Binet & Féré, 33, 34, 42, 46, 51, 66, 67, 69, 284
Bing, McLaughlin & Marburg, 482, 483
Biophysics movement, 171
Birth anxiety, 451, 466-467; basis for grandiose recapitulation, 467; brings anxiety components together, 455; not prototype of anxiety, 466; primal repression, 467; prototype propositions untestable, 467; psycho-analytic data on, 466; toxins reaction to, 451
Bird, 586
Birth trauma: as pathogenic nucleus, 455; & Freud's search for uniform cause, 469; recollections (Rank), 279; resemblance to other trauma, 469; & three components of anxiety, 469; toxins, 454
Bisexuality, 441, 478; & identification, 441, 478-479
Blanck & Blanck, 315, 426
Blau, 201
Blight, 586
Bliss, 17
Bloch, 303, 312
Blos, 262, 482
Blum, 277, 413, 473, 488, 566, 569 & n.4
Blunders & secret intentions, 587
Blyton, E., 584
Bobbitt, 554
Boesky, 361, 362, 415, 416
Bolkosky, 24
Boor, 17
Booth, 203
Botanical monograph dream (Freud), 237
Bordin, 553
Boudreaux, 587
Bourru & Burot, 68
Bowlby, 489, 561, 572, 604
Bradley, 560 n.1
Braid, 41, 42, 43, 50, 66, 76
Braithwaite, on theory & model, 199
Bram, 17, 23

Bramwell, 39, 40, 42, 43, 45, 48
Brandt & Kim, 598
Breast as object, 298, 318
Breger, 355
Bremer, 569
Brenman, 554
Brenner, 201, 271, 389, 406, 411, 423, 426, 427, 458, 459, 464, 466, 475, 487, 489, 490, 500, 512, 513, 514, 521, 528, 540, 561, 601, 603, 610, 611
Bressler, 482
Breuer, 10, 19, 23, 24, 25, 26, 27, 30, 31, 81, 101, 146, 152, 242, 376
Breuer & affective interpretation of Anna O.: alleviating discharge of emotion, 30; analytic therapy, 26; emphasis on talking or verbal expression, 27-30; etiological role of affective ideas, 27; expression of fear & pity, 30; memories of emotionally charged events, 27; original notes & retrospective affective interpretation, 1-2, 27
Breuer & hypnosis: 15-16, 19-20
Breuer & hypnoid states, 26-31; as precondition for symptoms, 27; 'caprice' of stocking-wearing &, 27; ideas in hypnoid states, 31; memory of hypnoid state, 27; views similar to Charcot, 68-70
Breuer & Freud differences, 194-198: repression & retention in hysteria 26, 160; sexual causes & building analogy, 197-198; sexual cause as single nexus, 198 concept of unconscious, 197; styles of theorising, 196
Breuer's method, components of, 75
Breuer: as the father in Anna. O.'s pseudo pregnancy, 16; cultural background, 23; interest in Greek drama, 23; private practice, 26; scientific research, 25-26
Breuer & Freud, 12, 13, 14, 20, 27, 28, 29, 78, 79, 112, 115, 116, 142, 157, 158, 159, 185, 223, 578, 589 n.2
Breuer's & Freud's theory of neuroses, 188-198: analogical & reductionist, 168; central assumption, 168; logical structure, 191-191; outline of, 188-191
Breznitz, 269
Brickman, 482, 492
Brickner & Kubie, 553
Bridey Murphy, 218; & pseudo-memories, 217, 18
Brierley, 476, 501
Brill, 202, 552
Broca, 45
Brody, M. W. & Mahoney, 480, 486
Brody, S., 572, 604
Brody, S., & Axelrad, 466
Brouardel, 225
Broughton, 285
Brouillet, *Une leçon clinique à la Salpêtrière*, 70, 612
Brown, 29
Brücke, 170, 171-172, 180, 182-183, 241, 263; summation of stimuli & previous state, 175
Brücke & Meynert: physiological schema, 175; & reduction of surplus excitation, 175; & reflexes & associations, 171
Brun, 183, 392
Brunswick, 405, 424, 465
Bum's *Lexicon*, 76, 77, 87
Bunker, 127
Burgner, 490, 491, 575
Burnham, 302, 303
Burq, metallotherapy, 45
Bush, 466, 525
Butcher, 21, 22
Bywater, 22

Cain, 427
Cajal, 183
Calder, 527
Calmness: during hypnotic reliving, 30; Freud's reference to, 88
Calogeras & Alston, 600
Camis, 26
Camuset, 68
Capacity for conversion, 162; basic predisposition, 101; logical status, 165
'Caprice', origins of, 24
Carlson, 68, 131
Carneiro Leao, 485, 488
Carrel, 393
Carter 126 n.2
Cartesian dualism: in understanding &. explaining, 582, 598
Cartwright, 216
Casanova, amatorial skills of, 323
Case material not congruent with sexual causal scheme, 203
Cases:
Anna O., 105
Anna von Lieben (see Cäcilie), 79
Cäcilie, 79, 87, 178, 461
Dora, 204, 205, 249-256, 271, 287, 316, 385, 604
Elisabeth von R., 77, 91, 103, 105, 117-118, 221, 288, 434
Emma Eckstein, 225, 276
Emmy von N., 77, 78, 84, 91, 93, 105, 156
Felida X., 68
Katharina, 91, 107, 116, 221, 556
Le Log__, 61-64, 71, 154, 372
Little Hans, 470, 473, 563, 566
Lucie, 83-84
Lucy R., 91, 103, 106, 108, 116, 221
Ly__, 59-61, 59
Marie, 82, 86
Pin__, 61 n.1, 61
Porcz__, 94
Rat Man, 567
Rika van B., 19
Schreber, 336, 604
Wolf Man, 469, 470, 473, 566, 592

Castration anxiety, 421; repression, 535; universal, 469
Castration anxiety & structural theory, 456
Castration memory inherited, 469-470, 570
Castration threat, veiled, 542
Catharsis, 21, 24; abreaction, 29; infallibility of method, 224; method & limitations, 24, 29-30; homeopathic action, 22; inherently & indefeasibly therapeutic, 23
Catharsis & Aristotle, 21; *Poetics & Politics*, 22-23; difficulty of translation, 21; drama, 21; passive spectator &, 23; purifies pity & fear, 22; removes fear & pity, 22; role in tragedy, 21
Cathartic cure first instance of, 83
Cathection of an idea by energy, 350; & instinctual drive, 349-350
Cathexes sample external world, 444
Cathexis of object: associations, 361; identification, projection & incorporation, 362
Cathexis, side & inhibition, 394
Cathexis & unpreparedness, 376
Caudwell, 584
Causation: assumption of uniform, 219, 283; explanation & understanding, 591; interpretation & meaning, 588; motives & reasons as causes, 577, 591-599; no universal definition of cause, 124
Cause: lesion & content as causes of recurrent utterance, 589-590; reproduced in symptom, 286
Causes: analysing, 124; establishing, 124; framework for, 2; guidance in recovery of causal memories, 220; *post hoc ergo propter hoc*, 594
Cautions: logical & methodological, 43, 44, 49
Cayce, Edgar, 19
Cäcilie, 77, treatment, 79; components of treatment, 80; head pain, 461
Censorship, 237; function, 246; primary process, 267
Censorship, problems in: condensation & displacement, 269; different locations, 268; dream work, 268; limited explanations, 272; location, 268; mode of operation, 268; placement between *Pcpt & Cs*, 268, 280
centres érogènes, 284
Central drive state, 307
Central traumatic event as prior focus, 455
Cerebration: unconscious (Charcot), 62
Cereijido, 518
Cerminara, 19
Cessante causa cessat effectus: in identifying causes, 141
Chabot, 603
Chambard, 284
Changed role solution of sexuality, 320-321
Character traits, 611
Characterisation & reality of theoretical terms, 149
Charcot, 26, 31, 32, 42, 44, 47, 49, 50, 52, 53, 67, 72, 74, 89, 93, 94, 131, 146, 152, 160, 165, 168, 170, 174, 180, 284, 372
Charcot: background, 44-46; 'electrodiagnosis', 174; interest in mesmerism, 45; errors & doubts, 50; neurasthenia, 131; physiological determinism, 211; reserve about Anna O., 67-68; suggestion at Salpêtrière, 612
Charcot on hypnosis, 45-46, 51; physiological approach, 46, 49, 583; realisation in, 63, 148; three states of, 46, 52; induces hypnosis 50 n.2; victim of deception & errors of, 49, 51
Charcot on hysteria, 56-59: characterisation of, 94; dynamic lesions, 58; production of symptoms experimentally, 63, 158; external events as cause, 60; major hysteria (*attitudes passionelles, clownism, decline terminal, epileptoid*), 58-61; hysterics not malingerers, 55-56; ideas & symptoms, 63, 72; concepts similar to Breuer's, 68; lawful changes in, 70; mechanism of realisation, 154, 156; theoretical contribution neglected, 66 & n.1; traumatic hysteria, 61-67; physiological method of study, 57; unconscious cerebration, 62
Charcot & de la Tourette, 76
Charcot & Marie, 66 n.1, 71, 76
Charcot & Richer, 66, 154
Charpentier, 52
Chasseguet-Smirgel, 481, 518, 519
Chatel, 131
Chattah, 513, 526, 599, 602
Chehrazi, 519
Chemistry & alchemy, 611
Chertok, 45
Chertok & Saussure, 27, 45, 48, 61 n.1, 76, 121
Childhood sexual impulse, 284-285, 292-296, 309; adult sexual drive &, 287-289, 288; analysis of symptoms, 316, 317; objects & auto-erotism, 314; repression & hysteria, 292
Childhood memory as focus, 222
Childhood seduction & psychoneuroses, 5, 208-213; Freud's expectations as explanation of, 218-220; objections to theory, 227; alternative accounts of, 225-229
Childrens' dreams, doubtfully transparent 273 n.2
Child's view of reality, 338
Child's penile manipulation, 311
Chodoff, 317, 318
Choice of object, adult & inversion, 299
Choice of neurosis, 354
Chomsky, 605
Chrzanowski, 131
Cioffi, 210, 224, 227, 552, 568, 569, 570
Civilization as male creation, 497; woman's limited contribution, 496
Claus, 181

Clinical bases of repetition, 371-377, 385-391
Clinical data: claim of privileged status, 317; ability to confirm, 518; clinical theory, 272, 510; clinical theory still metapsychological, 600 n.4 600
Clitoris, excitability of, 319
Cognitive functions emphasised in structural theory, 441, 464
Cohen, 389
Cohen & Kinston, 468
Coherence as proof of reality, 222
Collins, 612
Colby, 213, 477, 553
Colquhoun, 25, 39
Columbia Center Study of psychoanalytic therapy, 545-547, 607, 610
Commissions into Mesmerism, 36-41
'Compelling reason' as cause, 597
Component drives, 303, 308; & objects, 314; & continuations, prolongations, reaction-formations, sublimation, 532-33
Compromise as displaced excitation, 245
Compton, 164, 201, 202, 286, 303, 313, 314, 315, 322, 326, 331, 349, 361, 448 n.2, 462, 463, 465, 467, 473, 474, 483, 488, 517, 518
Compulsion to repeat: explanation based on pleasure principle, 386; highly instinctual, 377; in normal lives, 372; sexual instinctual drive &, 382
Concept of ego-instinctual drive & first detour, 347
Concept of instinctual drive, 289
Concept of psychic determinism & suggestion, 229
Condensation, 236, 254; as accumulated excitation, 245; energy not needed to explain, 529; & pseudo-physiology, 611
Condition seconde, 68, 69, 91, 92, 101, 158, 160, 162, 461
Conflict between sexuality & standards of the ego, 370
Conjugation & senescence, 380, 382
Connecting assumptions, 106, 119
Conscience: 342-343, 439, 479-480; embodies parental & social criticism, 343; scrutinises ego, 343; standards of ego & repression, 342
Conscious-unconscious terminological confusion, unimportance of, 432
Conscious ideas as word-representations, 340, 447
Consciousness: alterations of, 24, 101; double/second, 24, 29, 69, 151, 155
Consciousness as theatrical metaphor, 24
Consciousness: in topographic theory, 265-266, 280; attached to ego, 448; displacements of energy, 447
Consciousness: restriction of field, 155, 165
Consciousness, logical status of split in, 165
Constancy: empirical basis, 191; principle of, 176, 191; tautological explanations, 192; theorem, theory or principle of, 180
Consumatory end-point in component & organic drives, 311
Context & plausible reconstruction, 224
Continuity of mental processes: as determining factor, 110; Freud's assumption, 109
Conversion of affect & discharge, 187
Cooper, 423
Counter-intentions unconscious, 93
Counter-will, 92-93, 118, 159-160, 165-166
Counter-will as hysterical *sine qua non*, 93
Counts & Mensch, 554
Craig, 302
Cranefield, 26, 170, 171
Credulous position of Charcot on hypnosis, 32
Crichton-Miller, 571
Crum-Brown, 25

Daemonic, 412, 414; character of repetitions, 370, 377; equivalent to 'uncanny', 388
Dahl, 389, 405
Dalma, 21
Dams of disgust, shame & pity constructed from sexual impulses, 293
Danger situation & signal anxiety, 454
Damton, 35
Darwin, 183, 300
Darwin: explanation of emotion, 184, principle of antithesis, 92; principle of overflow of excitation, 93; *The Expression of Emotions*, 181; quantitative view of nerve force, 182; use of Spencer's principles, 182
Dating Delboeuf's, Janet's & Freud's treatments, 84-86
Davidson, 585, 596, 597, 598
Day residue, a point of entry for unconscious wishes, 245; Dora's concern of fire, 252; neutrality denied, 275; unconscious, 246
De Faria, 40, 41, 49
De Sennevoy, 39
Death, internal reasons, 384-381, 392-393
Death instinct: acquisition & conservative, 391; acts only to remove tensions, 390; aim equated with those of pleasure principle, 399; aim of tension reduction, 389; beyond the pleasure principle, 384; changed concept of instinctual drive, 390; ego's sexual drive, 437; lack of characterisation, 389, 426, 505; mental structures, 6, 437; mode as elimination of excitation, 403; no manifestations of, 418, 427; repetition compulsion, 370-374; sense of 'beyond' pleasure principle, 398; source & phenomena, 390; source 'the entire body or musculature', 389; source unspecified, 523
Death instinct alternatives, 408: aggression, 423-427; repetition, 409; removal of

biological foundations, 425; sadism, 416; none has clear advantage, 428; partial & fragmentary, 428; second law of thermodynamics, 406, 405
Death instinct, arguments for, 384: application of concept, 389; biological evidence defective, 427; clinical data capable of better interpretations, 427; conclusions do not follow or not entailed uniquely, 392-393; contradictory, 370, 505; observations on protozoa irrelevant, 393; riddled with faults, 427; unconscious operation in ego, 434; unconscious self-punishment, 438; theoretical needs &, 436
Death instinct, theoretical consequences; assumes instinctual fusion & defusion, 438; breaks isomorphism of structural with topographic theory, 441; 'positive force in psychological, 425; precondition for 'death motivation', 429; rules out narcissistic state, 484
Death instinct, other considerations: consistency with biology, 381; First World War &, 367-368; fills explanatory hole, 427; latent/manifest distinction, 393; outward direction of, 369
Death instinct & pleasure principle, 397: pleasure of orgasm, 404
Deciphering & second script, 556-560
Decker, 66, 162, 203, 262, 263, 275, 278
Deckert & West, 554
Defence, 104
Deferred action: castration threat, Little Hans & Wolf Man, 471-473; & deferred revision, 492
Defusion of instinctual energies, 438-439
Deigh, 436, 479, 480, 488, 491
Delage, 593
Delboeuf, 53, 74, 80-82, 83; acknowledgment of by Breuer & Freud, 86; dates of paper & book, 84-85; hypnotic experiments, 51-52; forces admission of Charcot's errors, 52; suggests symptom away *in statu nascendi*, 81-82
Deleuze, 39, 40
Delusions of persecution as regression to fixation, 336
Demand characteristics, 48
Dementia praecox & paranoia: separate disposition, 357
Dennett, 596
Dependent concomitant, 110
Deri, 476
Descartes & reason-cause separation, 598
Description rather than explanation, 611
Desexualised libido: difficulties of concept, 536
Desires: causal relevance of (Grünbaum), 598
Determining quality of memories, 212
Determinism: biological & sensory content, 324; ideas & associative structure, 325; & lawfulness of ideas, 72; & starting idea, 550
Deutsch, 482, 497 n.1
Developing ego: possessing strength, 332
Development models, three distinct & mutually exclusive, 516
Developmental schemata, two of, 518
Deviant sexual behaviour as strong fixation, 535
Differentiation of restriction from realisation, 157
Different-science alternatives: reception of, 600; & Janet-like dissociation, 600; & Rogers-like self, 600
Dilthey, 582
Dingwall, 19, 25, 39, 40, 50
Disappointment-substitution hypothesis of homosexuality, 352
Discharge along abnormal pathways, 99
Discharge mechanism, absence in child, 312
Disease: germ theory of, 125-126, 126 n.2
Disgust, 254, 326, 336; & excretory functions, 255; as mental force, 290; evidence for repressed sexuality, 256; observations, 323; organically determined, 322; restricts sexual aim, 290
Displacement, 236, 254; energy not needed for explanation, 529
Dissassociation of words & actions (Jackson), 98
Dissociation, 68, 329, 611; tendency to undefined, 159
Distinction between outer & inner, 445
Domhoff, 554
Donkin, 173
Donne, 429
Dora, 249-256; childhood sexuality & symptoms, 287-288, 290, 316
Dora 'acts out' her recollections, 385
Dora & Freud: her denials & correct explanation, 251, 260; her disgust, 254-256; his countertransference, negative & positive, 262; his associations, 256-258, 259-261; his sexualisation of case, 261-262; his theoretical needs, 261
Dora's dream, 252-253, 276; re-interpretations, 262; wish & drive-discharge, 277
Dora's sexuality: heterosexual, 259, 324; homosexual, 249, 251, 258; reliable facts about, 261
Dorey, 412
Double consciousness, 24, 101, 158; & associationism, 151
Downey, 389, 411, 423, 425
Down's syndrome & emotional shock, 539
Dream: censorship & repression, 237-238, 246-248; compromises, 238-240; dream work, 236-237; latent content, 235; manifest content, 235; deduction of features from reflex model, 263; latent & manifest content, 235; manifest content
..... continued

as compromise, 586; motives in, 233-235; structure & content, 232-233, 235; symbols, 233, 237; wishes, 232, 238-240, 245
Dream interpretation: absence of rules & indeterminacy, 275-276, 278-279, 557-560; use of manifest content, 276-277; alternative approaches, 277; discrepant, 279; double construction in, 563-564; many-to-many mapping problem, 558
Dreams: compulsion to link ideas, 233, 237; infantile wishes, 243; modification of excitation, 243; wish-fulfilment, 272
Dreams & analogy with languages: 556-559
Dreams & 'clinical' theory, 272-275
Dreams & intellectual activity, 266-267
Dreams & symptoms: 233, 250; explanatory deficiencies, 264, 272; resemblance incomplete, 249
Dream-censorship & political censorship, 238; necessity of concept, 276
Drever, 300, 302
Drive: & connection to object, 289; instinctual, 6
Drive discharge: no longer necessary, 276
Drives: component, 295, 296; directed toward object, 296; instinctual & separate components, 303; place in theory, 271
Du Bois-Reymond, 170, 171, 172, 174
Dual consciousness & dissociation & affect, 99
Dual instinct theory account of mastery, 412
Ducey & Galinsky, 398
Duchenne de Boulogne, 174
Dufay, 68
Dupau, 41, 49
Duruz, 483
D'Eslon, 34, 36, 37

Eagle, 562, 567, 591, 601
Eckardt, 276
Edelson, 561, 570 n.6, 571, 594, 610
Edes, 202
Edgcumbe, Lundberg, Markowitz & Salo, 519
Ego annihilated in hypnotism (Charcot), 65
Ego, 430
Ego: anthropomorphism, 498; empty construct, 477
Ego development: anxiety, 467; early functions, 477-478; helplessness, 456; secondary process, 522
Ego functions: action, 340; anxiety, 443 446, 447, 453; attention, 340; censorship, 330; cognition, 443, 446; conflict with sexuality, 341, 347, 359; consciousness, 340; critical function, 479; judgement, 340; memory, 340; motor control, 446; passive, 441, 442-443; reality testing, 339-342; repression, 329, 351, 367, 443; sampling stimulii, 244, 444; thinking, 340, 446; unconscious portion, 433, 434
Ego & ego-instinctual drives, unexpected introduction to theory, 363-364; characterisation, 347-352; energy & agency, 334; hatred &, 343-346; inappropriate model for, 347-349, 364; obscurity of relation between ego-function & ego-drive, 350, 363
Ego, id & super-ego functions: lack of agreement on, 501, 505; do not fit observational data, 522; vain searches for functions, 521
Ego as object of instinctual drive: energy, objects & masochism in the ego, 474-475; energy & sublimation, 447, 474-484; instinctual drives, 6; object of sexual drive, 367; sexualisation v. sublimation, 475-476; narcissism present from start, 357; source unspecified, 523; relation with *Pcs.* & anticathexes not specified, 364
Ego as well-cathected neurones, 504; attracts & binds excitation, 394-396; masters by binding & abreaction, 409
Ego & id, free communication between, 439, 449, 450
Ego control (Charcot), absence of uncharacterised, 154
Ego-ideal: cathexes of ego-libido, 343; as critical agency, 480; difference from super-ego, 480-482; Klein &, 491; relation to narcissism, 480, 482-484; modelled on parents, 480; role of parental criticism, 480; standards for repression, 343; super-ego &, 437-438, 481
Ego-instinctual drives & repression, 364; brutality & wartime destructiveness &, 368-369
Ego-instincts & reality, 339-342
Ego-instinctual drive v. sexuality, 326, 328-329, 347-363
Ego-libido, toxic, 452
Ego-psychology & Ego-psychologists, 315, 502, 572
Eidelberg, 405, 421, 427
Einfall, difficulties of translating, 114 n.4
Einstein & Freud's ideas, 581
Eisenbud J., 554
Eisenbud R.-J., 420, 427
Eissler, 319, 419, 420, 427, 458, 467, 498, 502, 585
Electricity in treatment & diagnosis, 174
Elisabeth von R: no psychical cause of leg pains, 591
Ellenberger, 1, 16, 17, 19, 21, 23, 25, 39, 77, 83, 85, 90, 104, 175, 183, 248, 406; & Anna O., 67
Elliotson, 37, 38; credulity, 40; introduced mesmerism to English medicine, 39
Ellis, 303, 312
Ellman & Moskowitz, 150, 199, 498, 528, 586, 599, 600, 601

Elms, 277
Else, 21, 22
Embryology & recapitulation, 377
Emma Eckstein, 225; as Irma, 276
Emmy von N: arm & leg pain, 461; components of treatment, 78; de la Tourette's syndrome 78 n.1; expectations, 86; Freud's summary of treatment, 81; resemblance to Anna O., 86
Emotional ambivalence & taboos, 342
Emotional circumstances relived, 11
Emotional discharge, 23; of fear & pity, 21
Emotional expression, 29, 30; abnormal, 181-182, 186-188; overt, 31; posture, 66; symptom formation, 181
Emotional ties & instinctual drives, 484
Empathic comprehension, 584; Jaspers-like, 595; & 'Eureka!' experiences, 585; & shared knowledge, 585
Empathic conviction, 584
End-pleasure, 297, 320; Freud concedes difficulty, 321; genital, 311
Engels, 42, 124; hypnotic &, 42-43
Enuretics, personality characteristics of, 539
Ephemeral cures of hysteria, 26
Equilibrium of the nervous system, 98
Erb, 44, 135
Erect posture & disgust, 204
Erickson, 554
Erikson, 258, 263, 276, 489, 554
Erlich & Blatt, 517, 599
Erotogenic zones, 291, 293, 294; contribution to sexual excitation, 297; innervation of, 306; orgasmic potential, 321; sensual sucking, 293; sexual pleasure, 294
Erwin, 545
Esdaile, 40
Esman, 515, 516
Etchegoyen, 479, 480, 482, 485, 486, 487, 488, 491, 492, 516
Ethological concepts & Freud's 304 n.1
Etiological equation, 137-138, 140-143; guarantees dissension, 203; indeterminacy of, 202
Eulenberg, 303, 312
'Eureka!' experiences, 585
Evans, 554, 602
Excessive stimulation & preparedness, 444
Excessive valuation of thinking, 337
Excitation: activates functions, 263; accumulation & transfer, 182; affect, 169; discharge regulation, 193; modes of disposal (levelling out, speech, motor reaction) 186; hysteria, 173-174; ideas & redistribution, 170; reduction, 168-170, 175-180; regressive, 263, 264; sexual, 306; sums of, 99; tendency to minimize, 168; transmission of unidirectional, 172, 280; & affect, 169
Excitation & reduction, 168, 188
Excitatory energy: limitations, 524
Exhibitionism & sexual satisfaction, 295
Exner, 183, 242, 263, 524
Expectations: Breuer & Anna O., 20, 30; Freud & actual neuroses, 201-203; Freud & hysteria, 142-143, 218-225
Experience sexual, in childhood, 283; of satisfaction, 244
Experiments on hypnosis, caution about conclusions, 44, 48
Explanations: reasons & motives, 578; of inversion, 333; repetition & mastery, 387
Explanatory mechanisms: reality of, 147-151; of symptom formation, 152-165
Expressing emotions, 23
External world, differentiation from internal, 344
Eysenck, 17, 539, 542, 543
Eysenck & Wilson, 553
Ezriel, 561, 562

Facial expression modifications & bodily attitudes, 66
Facts, as opposite of theories, 167; deductive explanation of, 168
Facts obtained by free association, 227, 279
Factual consequences & testability, 199
Fahmy, 214
Fairbairn, 302, 315, 370, 489, 607
False memories in psychotherapy, 217
Fancher, 273
Fantasies: link to perverse impulses, 282; neurotic, 281; sexual wishes, 230; structured, 270-271; substitute for seduction, 229
Farber & Fisher, 554
Farrell, 569
Faure, 85
Fayek, 389, 415, 426, 457, 498
Fear of object, 421
Fechner, 175, 263
Fechner's principle of stability, 175, 406
Federn, 202, 417, 418, 426
Feelings, voluptuous, 306
Feigl, 4
Feldman, 321
Female object choice, 318, 494
Female contribution to cultural development, 495: moral standards acquired from males, 497; super-ego rigidity, 495, 519
Female sexual development: effect of goal chosen by Freud, 520; Freud's explanation rejected & unnecessary, 520; oedipal situation, 493
Feminine characteristics acquired from male, 495; character, 298; characteristics questioned, 519
Female sexuality, 493: male libido, 319; masculine sexuality, 493, 518; masturbation, 519; repression at puberty, 318
Freud's five works on, 493-494
Fenichel, 201, 202, 312, 315, 389, 408, 411,
..... continued

417, 421, 424, 426, 427, 464, 465, 479, 486, 540, 542
Ferenczi, 19, 133, 163, 202, 203, 204, 229, 312, 315, 345, 365, 436, 486, 489, 517
Ferguson, 602
Ferreira, 414
Feyerabend, 4
Féré, 284
First censorship, 246
Fischer, N., 423
Fischer, R. E., 540
Fisher, C., 554
Fisher, S. & Greenberg, 261, 538, 544, 548, 553, 570
Fixation: as failure to pass a stage, 334; between auto-erotism, narcissism & homosexuality, 356; sexual gratification, 312; fixation-disposition-regression hypothesis, 335-337, 353-356, 367
Flescher, 464
Fletcher, 300, 301
Flew, 586, 596, 597
Fliegel, 519
Fliess, 204, 205, 324, 540
Fliess suggests Emma Eckstein's haemorrhaging is hysterical, 226 & n.1
Flowerman, 257
Flugel, 370, 393, 406, 427, 481, 492, 555
Fluid (Mesmeric) 36,40
Fodor, 501
Fonagy, 538, 545, 561, 563
Force & ego uncharacterised, 330
Forel's *Hypnotism*, 77, 87
Fore-pleasure, 297, 311, 320, 321
Forrester, 178, 580 n. 1, 590, 602; & isolation of aphasic & hysterical symptoms, 179
Fosshage & Loew, 279
Foulkes, 273 n.2, 274, 276, 564
Foxe, 406
Frank, 600, 603
Frank & Muslin, 467, 468, 469
Frankley, 202
Free association: 2, 7, 115-116, 114 n.4, 549-551
Free association & causes, 551-552; clinical evidence, 552; hypnosis experiments &, 554; Luria's experiments, 553-554; studies of, 552-553; reply to criticism, 555; verbal conditioning &, 571
Free association & creation of data: 570-575; data contaminated by suggestion, 552, 570, 571, 594; determinants of, 554; evenly suspended attention, 571; source of variant theories, 572-574; facts & choice between variants, 574-575
Freeman, L., 17
Freeman, T., 355
French, 410
Freud, Anna, 390, 425, 467, 516, 551
Freud & causality, 551: etiological equation, 138; causal analyses, 2, 6, 11, 122-123, 124-126, 137, 142, 143, 588; causes of hysteria, 31; confusion of necessary & sufficient conditions, 203; examination of normal subjects, 142; identifying & measuring causal factors, 140; Koch's postulates, 125
Freud & determinism: beliefs in internal determinants, 73, 74, 551; extended determinism 118-119; free-association, 11; hypnotic & hysterical phenomena, 31; ideas & concept of childhood, 325; psychic determinism, 32; psychological phenomena, 11, 32; tests of assumptions, 518; views, 2, 6, 211, 231
Freud & suggestion: Collins on Freud's defences, 612; considers only direct suggestion, 211; defends Charcot, 72-73; denies unconscious transmission, 53, 73; dominates treatment, 260; foists his reconstructions on patients, 224; influences recollections, 109; rejects possibility of false information, 208; uses his associations, 256
Freud's biological & neurophysiological concepts: accumulator-transfer model, 183, 184, 185; model of need satisfaction, 312; *On Aphasia*, 176; outdated neurophysiology, 6, 184, 196, 477, 524-525; pseudo-biology, 325; purpose for reflex, 176, 180; translation of psychological terms into biological, 406; two motive framework, 370
Freud's cathartic method: affective basis, 75, 89, 118; & therapy 1889-1892, 87
Freud's collaboration with Breuer: 10, dissociates himself from, 195
Freud's dreams: botanical monograph, 233; Irma's injection, 326, 276; structural interest, 233
Freud's style of theorising, 199, 370: ad hoc explanatory principles, 256, 258; assumes what has to be proved, 361, 387; claims method objective, 119, 261, 575; dismisses 'familiar motive forces' for repetition, 385; fails to exclude external basis for fantasy, 209; gives information to patients, 222; ingenuity & plausibility of explanations, 278; inconsistencies in formulations, 398, 516; 'interpretive artistry', 278; redescribes infantile sexual impulses as unsatisfied, 386; relation between observations & conclusions, 353; pseudo-logical explanations, 194, 523; puts pressure on neurasthenics to admit to masturbation, 208; standards of adequacy, 199; subjective judgement & of daemonic quality & instinctual basis of repetition, 388; use of evidence 203, 208, 209, 293, 318, 473; vagueness of concepts, 141, 298, 349
Freud's theoretical debts: Beard & sexual factors, 132; Charcot's physiological theses, 55; Darwin on emotional expression, 181, 187; Exner's view of facilit-

ation, 185; French school, 100; Jackson's excitation reduction, 177; Janet, 70-72, 82-84, 95, 120-121; Koch's postulates, 125, 138; Meynert, 171
Freud's theoretical realism, 499, 503, 578
Freud's therapy: hypnotic, 75-77; hypnosis & Bernheim's technique, 77-80, 86-88; pressure method, 88-91; with Cäcilie, 79-80, 87; with Emmy von N., 78-79, 86-87; in years 1889-1892, 87, 99
Freud's three assumptions about connection of memories (repression, summation, incubation): 106-109, 116-118
Freud's three expectations: effect of abreaction, sexual etiology, uniform causation, 142-143, 218-225; childhood seduction &, 6; sexual etiology, 133; causes, 200; retained after seduction theory collapse, 231
Freud's waking state conceptualisations: 91-91, 100-102; repression, 102-104
Friedenburg, 418
Friedman L., 99, 551, 600
Fromm, 563
Frosch, denial of role of homosexual tendencies in paranoia, 355
Fuchs, 479, 485, 486, 487
Functions in functional version of structural theory, 497-504
Fundamental rule of psycho-analysis, 114-116; implied in the *Studies*, 115
Furer, 481, 483, 492
Furst, 466, 515, 516
Fusion of instinctual drives & narcissistic cathection, 483

Galaty, 171
Galdston, 551
Galenson, 519
Galileo, Lakatos on his telescope, 119
Gall, phrenology of, 42
Gamgee, 50 n.2
Garma, 264, 279, 426, 527
Garrison, 563
Garza-Guerrero, 463, 492, 521
Gattel, 140; data on seduction, 227; Freud's attitude to, 228
Gediman, 201
Gedo & Goldberg, 264
Gedo, Sabshin, Sadow & Schlessinger 1964, 196
Geha, 566
Generation of signal & inhibition of discharge, 464
Genital primacy, 296-298; & augmentation & changed role solutions, 320-321; Freud on difficulty, 321-322
Genitals, pleasurable feeling, 294; substitutes of anus & mouth, 291
Germ theory analogy, 126 & n 2
Gero, 419
Gifford, 405, 411, 413, 428
Gill, 262, 267, 268, 269, 271, 457, 498, 500, 503, 511, 512, 513, 522, 523, 529, 566, 599
Gill & Brenman, 554
Gill & Holzman, 513
Gillespie, 313, 315, 316, 370, 389, 424, 427
Gillman, 491, 492
Gilman, 295
Glassman 575 n.7
Glenn, 260, 261, 423
Glick, 462
Glover, 279, 463, 467, 476, 504, 511, 521, 539, 562, 563, 609
Glymour, 570 n.6
Goldberg, 361, 362
Goldsmith, 33
Gomperz, 277
Goodman, 501
Gordon, 554
Goshen, 17
Gould, 492
Gowers, 44
Grand, 420
Greaves, 17
Green, 169, 361, 459, 462, 463
Greenacre, 413, 466, 467, 515, 516, 565 n.3
Greenberg & Pearlman, 276, 277
Greenson, 485, 486, 487, 527
Greenspoon, 214
Grenell, 196 n.1
Grinker & Spiegel, 415
Grinstein, 125, 234, 275
Groddeck, 388, 448, 540
Groos, 300, 301
Grossman, 417, 423
Grossman & Simon, 490, 498, 503, 575
Grotstein, 467
Gruber, 583
Grunberger, 481
Grünbaum, 141, 571, 575, 585, 594, 598, 602
Guillain, 45, 46, 49, 50
Guntrip, 489

Hacker, 476
Hägglund, 481, 482, 483
Hall, 171, 172
Hallucinations: content, 60; *condition seconde*, 86; & excitation, 265
Hammerman, 482, 500
Hanly, 389, 389
Hardison, 22
Harrington, 45, 50 n.2
Hart, B., 552
Hart, H. H., 476, 486
Hart, H.L. & Honoré, 124
Hartman, 277
Hartmann, E. von, 116
Hartmann, H., 272, 332, 349, 351, 405, 413, 425, 435, 463, 465, 476, 477, 483, 498, 500, 502, 512, 551, 561, 585
Hartmann, H., & Kris, 542

Hartmann, H., Kris & Loewenstein, 389, 425, 426, 476, 483, 485, 498, 500, 502
Hartmann, H., & Loewenstein, 481, 482, 485, 486, 500, 502
Hate development through association with ego-instinctual drives, 345-346, 359, 361, 366
Havens, 66 n.1
Hayman, 498, 500, 501, 502, 521
Heidenhoff's thought extirpation, 80
Heimann, 426
Heine, 216
Hell, Father, 33
Helmholtz, 170, 477; misnomer of 'school of Helmholtz', 171
Hendrick, 410, 411, 482, 486, 489, 540
Hereditary factors in hysteria, 154, 165, 325, 541
Heresies: Jungian & Adlerian, 370
Hering, 24, 109, 381
Hermeneutic position: criticisms of, 600-603; choosing between, 602; collapse of signifier & signified 601; double construction & indeterminacy of, 601; neglect development & clinical interaction, 602-603; therapeutic value, 602
Hermeneutic readings & closure: Dora, 601; jigsaw puzzle comparison, 601; *Moses* of Michelangelo, 601; Rat Man, 601
Heynick, 270
Hilgard, 158, 553
Hilgard & Nowlis, 554
Hillman, 52
Hirschmüller, 11, 12, 13, 14, 16, 17, 18, 26, 27, 28
Historical evaluation, 3-5
Hobson & McCarley, 241
Hoch, 422, 423
Hoek, 19
Hoenig, 330
Hoffman, 551
Hojer-Pedersen, 202
Holbrook, 407
Holder, 480, 487, 491, 492, 542, 572
Hollender, 22 n.1, 29
Holt, 202, 226, 267, 270, 370, 389, 393, 395, 405, 408, 426, 477, 510, 513, 522, 523, 525, 526, 527, 528, 574, 598, 602, 603, 610
Holzman, 585, 586, 591
Home, 574, 595, 596
Homosexuality & narcissism, 333-334, 352-353
Horney, 420, 518
Hubbard, 218
Hull, 148
Hurst, 72
Huston, Shakow, & Erickson, 553
Hypercathexis of receptive systems, 444
Hyper-excitability & lethargic state, 50
Hypnoid isolation (Breuer), 157-159, 165-166, 611: explanatory power, 157; generality, 159; tests of, 158; & repression, 101
Hypnoid state, 26, 27, 31, 118; as focus, 68; & hypnotic, 157; reappearance, 31; revived, 157; similar to traumatic, 65; true, 31; varieties of, 27
Hypnosis: credulous v. sceptical positions (Sutcliffe), 32; Mesmerism, 33-38; post-Mesmer, 38-44; Bernheim, 47-49; Charcot, 44-47, 49-52; Delboeuf, 51-52; Freud & Charcot-Bernheim controversy, 52-53
Hypnosis & hysteria, excitability, 168, 170
Hypnosis & therapy: Charcot's negative evaluation, 75; concentration, 90; direct suggestion (Bernheim), 78; fabrication of memories in, 575; questioning under, 88; recall & pressure method, 89; combined with Breuer's method, 77
Hypnosis & access to memories of the *condition seconde*, 90
Hypnosis & annihilation of ego (Charcot), 65
Hypotheses, & expectations, 231; incorrect, 2; facts &, 582; rejection, 3
Hypothetical: constructs, 147-148; relations, 149; terms, 149-150
Hysteria, 19, 31, 55-56, 68, 200
Hysteria (Charcot): hereditary basis, 60; incubation of unconscious process, 62-63; isolated processes, 64-65, 73; major v. traumatic, 59-61; realisation, 65-66, 70; physiological basis, 47; produces symptoms experimentally, 65-66
Hysteria (Freud, early): conversion & redistribution of excitation, 184; counter-will, 133; essential characteristics & affect, 186; hysterical attack & discharge of excitation, 460-461; nature of 'lesion', 95; physiological modifications, 169; suggestion ruled out as cause, 70; surplus of stimuli, 170; symptoms as discharge along abnormal pathways, 186; symptoms caused by memories, 122; symptom formation by symbolisation, 79; symptoms elaborated unconsciously, 72; symptoms of organic & hysterical paralyses, 94; proposes traumatic hysteria as model 69-70
Hysteria (Freud, late): analogous to language, 556-557; structured & unconscious fantasies, 280; symptoms caused by opposing wishes, 240, 587
Hysteria (Janet): symptom & idea of organ or organ dysfunction, 70-72, 93, 99; unconsciously imitate organic disorders, 70; uniformity of symptoms, 71
Hysteria & childhood seduction (Freud): causes of & Freud's expectations, 142-143, 200, 218; childhood sexual trauma, 143; parental seduction, 200; paternal etiology, 209
Hysterical sexuality (Freud): craving & aversion, 291; dominated by perverse

impulses, 281; negative of perversion, 284; repudiated perversion, 284
Hysterogenic zones, pressure on starts & stops attacks, 58-59
Hysteria-as-ignorance thesis, 95-96; 120

Id, 430, 448-450, 501; as repository of death & sexual drives, 449; & relation to unconscious, 521; no barrier between it & the repressed, 449
Id structure & functions: absence of meta-psychological description, 521; ambiguities, 501; anthropomorphism inherent in, 498; empty theoretical structure, 505; functions impossible to list or account for, 502, 505; lack of consensus, 521
Id, *Ucs.*, & death instinct, 448-450
Ideas activated by instincts: descendant of notion of 'linkage', 329
Ideas, analogies: as actors, 24; as parasites, 64
Ideas as determinants of hysterical symptoms, 70-71, 120
Ideas (Charcot): connected to posture, 67; isolated & realised, 64; lodged in the mind like parasites, 64; realised, 67; & movement, 67; unconsciously created from sensations, 66; unconsciously transformed, 67
Ideas (Freud): aesthetic & moral, 292; affect of as determinant of symptoms, 6; as structures, 458; consciousness & verbal linkages, 458; disgust, shame & pity, 292; opposition between as expression of instinctual struggle, 329; distribution of surplus excitation, 170
Identification, as becoming like another, 439, 484; as mechanism, 485; five meanings, 487; Freud's flexible use of the term, 487; no agreed on definitions, 485
Identification & formation of super-ego from Oedipus complex, 439-441, 484-489; inadequate theory, 489; incompatible with narcissism, 487; & sublimation of Eros, 447
Identification, primary: 'being like', 484, 487-489
Identification, secondary: 'having' 484; incorporation, 479
Ideo-motor action, theory of, 152, 156, 165
Ikonen & Rechardt, 201, 427, 428, 516
Imagination as cause of hypnotic effects, 40
Immutability of unconscious wishes, 248
Impulses, auto-erotic, 287; component instinctual, 322
Inadequacy of ego-instinctual drive, 351
Incest: barrier, 299; object-choice, 324; pubertal fantasies, 324
Incorporation as functional association between memory traces, 360
Independent ego energy, no unambiguous criteria for, 526
Individual development & recapitulation, 540
Inductive method: historically conditioned, 583; generation of hypotheses, 583; logical relevance of facts, 583
Infant's hallucination of satisfying object, 244, 270
Infant's craving for stimulus, 408; tri-phasic post-natal development, 515
Infantile: amnesia, 292; fixations, 335; mental life & primary process, 243; sexual drives & non-production of pleasure, 386; repressed sexual wishes indestructible, 246, 248, 271, 280; Freud's contradictory descriptions, 314; observations of infants, 326
Infantile wishes: dreams &, 245-246, 275; immutability of, 248, 271; thinking &, 243-244
Inhibitory process, contradiction, 524; lacuna, 196 n.1
Initial premise for death instinct argument, equivocal support for, 383
Innate sexual constitution, 296
Insistence of therapist as effect in psychotherapy, 109
Instinct of mastery, 412, 413, 531
Instinctual anxiety as signal, 450, 465
Instinctual basis of compulsion to repeat, 384
Instinctual drive concept & genesis: 300-305; ambiguity, 349; internal v. external basis, 300-302, 307; early definition v. later, 408; energy aphysiological, 477, 525-526; pattern of satisfaction, 348; revised conceptualisation, 368; philosophical force, 424; sources inferred from aims, 348; true motive forces, 344, 345
Instinctual drives & formal argument for death instinct, 378-380
Instinctual drives: infantile fixations, 335; personality theory, 328, 523-524
Instinctual drives, psychological: disputes over, 426; problems of definition, 428
Instinctual drive transformations & socially valued aims, 476
Instinctual forces outside pleasure principle, 370
Instinctual drive: representational & cathectional meanings, 349
Insufficiency (Janet) evidence for, 156
Integration of pathogenic ideas with primary consciousness, 31
Intentions: responsible for symptoms & parapraxes, 587; provide partial understanding, 588; & wishes in dreams, 592
Interconnection, principles of, 470
Interconnections as causal connections, 211
Internal stimulus, 307
Internal world, differentiation from external, 344
Interpretation, indeterminacy of, 276
Interpretations & validity, 561

Interpreted dream: & latent dream, 276; replaces other thought processes, 588
Intervening variable & empirical law, 147
Interviews: & influence on childhood recall, 215; & verbal conditioning, 214
Intracerebral excitation: & affect, 192; independent evidence, 193; indexing behaviours, 192
Introjection, 345-346, 360
Introjection & projection: arbitrary assumptions, 365, 366; of object, associations, 361
Intrusion of mental content, 31
Irma dream (Freud), 236: Irma as Emma, 276; manifest content & Freud's concerns, 277; multiple interpretations, 277; preconscious wish, 276; reinterpretations, 276
Isaacs & Haggard, 214
Isay, 473
Izzard, 466

Jackson, Hughlings, 44, 74, 98, 110, 175, 176, 178, 179, 182, 528, 529, 589; as scientist 590 & n. 3; recurrent utterance & speech ejaculations, 176
Jackson, S. W., 526
Jacobsen & Steele, 213, 325, 472, 473
Jacobson, 405, 479, 480, 482, 483, 484, 487, 493
Jaffe & Naiman, 315, 316, 459
James, 67, 84, 152, 153, 175, 300
Janet, J, 158
Janet, P., 24, 50, 69, 71, 74, 75, 79, 80, 81, 84, 85, 99, 121, 146, 152, 155, 158, 160, 165, 180, 329
Janet's theoretical views: determinism, 53; criticism of Charcot, 50 & n.3; emphasis on heredity, 156; Freud's use of, 96, 99; ideas as detemninants of symptoms, 70-72; priority in relation to Freud, 120-121; revival of, 329, 600; uncharacterised mechanisms, 156
Janet's therapy: combats reborn symptom & modifies memories, 83; suggestion to second personality, 84; influence on Freud, 85; personal perception, 155; publication of thesis, 85; treatment of Marie & Lucie by hypnotic suggestion, 82-84
Japp, 583
Jaspers, 582, 584, 595
Jeliffe, 183
Jennings, 262, 300, 301
Jensen, 17
Jigsaw puzzle analogy: childhood seduction memories, 222; Dora, 257; hermeneutic closure, 601
Joffe & Sandler, 459
Jones, E., 1, 17, 67, 120, 183, 201, 203, 232, 279, 370, 392, 404, 405, 423, 424, 427, 437, 465, 475, 476, 478, 518, 537, 543, 552, 555, 572, 581
Jones, R. M., 274, 276, 560 n.1
Joseph, 414
Juda, 412
Jung, 339, 354, 356; word-association experiments, 552
Juni, 355, 540

Kampman & Hirvenoja, 217
Kanfer, 214
Kanzer, 260, 261, 262, 265, 314, 357, 411, 492, 524, 525, 551
Kaplan, A. H., 563
Kaplan, D. M., 207, 586
Kanzer & Eidelberg, 405
Kapp, 406
Karpe, 17, 23
Kasanin, 411
Katan, 356, 404, 405, 427
Kaywin, 476, 477
Kaywin, Hilger & Fitzner, 554
Keiser, 420
Kekulé & structure of molecules, 583
Kepecs, 416
Kermode, 570, 605
Kernberg, 420, 463, 481, 483, 484, 493, 498, 522, 572, 575
Kessel & Hyman, 543
Kestenberg, 519
Key neurones, 462-464
Khantzian & Mack, 349, 427
Kiell, 275
Kihlstrom, 554
Kinston, 323
Kinston & Cohen, 468
Klauber, 5, 566, 609
Kleeman, 311, 519
Klein, G. S., 273, 405, 458, 498, 510, 600, 600 n.4, 604
Klein, M., 315, 465, 490, 491, 519, 572, 573; little use of identification, 491
Klein, M. I., 209, 226, 315, 423, 425
Klein M. I. & Tribich, 225, 227, 229, 250 n.1, 258, 355; seduction memories not repressed, 225
Kline M. V., 554
Kline P., 538, 539, 544, 545, 553
Knight, 485, 486, 543, 551
Koch's postulates, 125, 578; Freud adapts, 123, 133-140, 143; necessary & sufficient conditions &, 125; parapraxes, 594
Koff, 479, 486
Kohon, 261
Kohut, 412, 428, 483, 489-490, 521, 572, 575
Kolenda, 586
Koppe, 183, 185, 196
Kraepelin, 303, 312
Krafft-Ebing, 203, 285, 295, 302, 303, 305, 312; Anna O. &, 13
Kramer, 482, 488, 492
Krasner, 214
Kris, A., 571
Kris, E., 140, 272, 499, 565

Krohn & Krohn, 259, 260, 261, 262
Krystal, 411, 516
Kubie, 411, 414, 427, 465, 476, 526, 561
Kuhn, 4, 5
Kuper & Stone, 277

Lacan, 519, 603
Lafontaine, 42, 50
Lagache, 411
Lain Entralgo, 21
Lakatos, 4, 119
Lakoff, 607
Lamb, Meynert's, 112, 552
Lambert, 543
Lampl-de Groot, 202, 425, 427, 481, 519
Langs, 260, 261, 262, 277
Lantos, 424, 425, 467, 476
Laplanche, 278, 393, 406, 465, 475
Laplanche & Pontalis, 156, 325, 356, 357, 394, 406, 438 n.1, 482, 498, 551
Lashley & Colby, 526
Latency period, 292
Latent content constructed not discovered, 563; latent dream as interpretive construction, 564
Latent incubation (Charcot), 12
Laufer, 490, 491, 492, 566
Laws & determination of psychological phenomena, 575
Laws of primary process & Breuer's mobile energy, 377
Laycock, 172
Lebovici, 490
Lebovici & Diatkine, 389, 427
Lennard & Bernstein, 213
Leonardo, 363, 569; sublimated homosexuality, 333
'Lesion' in hysteria as inaccessibility, 97
Lester, 463, 466
Levey, 476
Levin, A. J., 407
Levin, K., 45, 87, 120, 131, 134, 135, 169, 173, 175, 176, 203, 206, 209, 463
Levitt, 542
Levitt & Chapman, 554
Levy, 311
Lewin, 258, 262
Lewinsky, 419, 421, 427
Lewis, H. B., 545
Lewis, W. C., 503, 526
Libidinal cathexis as source of *Pcs.* anticathexes, 351
Libidinal development: recapitulation, 338; stages, 518
Libido, 308; as a stream, 291; as object seeking, 315; sexual, 300; sexuality of adult, 611; sexuality of little girls, 298; redirection on to ego, 337
Libido & associative connection with disgust, 204
Libido-narcissism & ego-id-superego conceptualisation, 483
Lichtenberg, 515
Lichtenberg & Galler, 550
Liébeault, 47, 76
Life: creation of & Newton's law, 391-392
Limentani, 315, 459
Lindner's sensual sucking, 293-294, 309-310, 311, 326, 530, 531
Linkages or logical threads, 220
Lipin, 413
Lipps, 265
Lipton, 551
Literature & the fundamental rule, 551
Loeb & Carroll, 467
Loewald, 411, 490, 492
Loewenfeld, 203
Loewenstein, 347, 370, 389, 404, 412, 419, 421, 423, 424, 427, 481, 482, 501, 527
Loftus & Loftus, 161, 566
Logical & associative structure of neurosis, 211-213, 257, 325
Logophania (Ellenberger), 104
López Piñero, 127, 131, 133
Lorand, 276
Lorenz, K., 302
Lorenz, P., ('Rat Man'), 567
Loss of reality function & repression, 356
Lost object in melancholia, properties of, 440
Lotto, 599
Love, association with sexual drives, 346
Lovibond, 539
Low, 398
Lowenfeld, 411, 415
Lowental, 389, 428
Lower, Escoll, Little & Otenberg, 561
Lowery, 519
Luborsky, Graff, Pulver & Curtis, 561
Ludwig, 170
Luria's experiments on hypnotic conflict, 158, 553
Lussier, 389, 424, 425, 427
Lustman, 527, 561
Lyons, 355

Macalpine, 416
McCarley & Hobson, 241, 408, 524, 525
MacCorquodale & Meehl, 147, 148
McCulloch, 526
MacCurdy, 202, 415
McDougall, 29, 300, 301
McGuire G.R., 44, 45, 51, n.3, 52
McGuire M. T. 565 & n.3, 566
McGuire W., 339, 354, 356
Mach, 25
McIntosh, 268, 487, 502
McIntyre, 241
Macklin, 551
McLaughlin, 277
Macmillan, 126, 141, 567, 577, 607, 609
Maddi, 261
Madison, 468
Magic & association by contiguity, 337
Magnetism (animal), 32-35, 36-38, 39-41
Mahler, 483, 572

Malan, 544 n.4
Malcolm, 272
Male choice of female objects, 299
Maleson, 417, 422, 423, 475, 497 n.1
Malev, 411
Mancia, 176, 184, 196, 524
Mancia & Meltzer, 481, 482, 491
Manifest dream content: association with unconscious idea, 235; direct interpretation, 276; incorporation into dream, 266; waking experience, 277
Marcovitz, 427, 498, 500, 502, 521
Marcus, 262
Marmer, 17
Marmor, 30, 216-218, 416, 571, 609, 610
Martorano, 17
Masculine & feminine character, 298
'Masculine' & 'feminine' uncharacterised, 479
Masochism, 290; basic facts needed, 423; broad & narrow meanings, 423; desexualised, 423; erotogenic & female, 496-497, 497 n.3; Freud's original description, 422; Hoch's fundamental question, 422; Maleson's ten explanations, 421; sadism &, 422; Speilberg's four different kinds, 421; & unconscious need for punishment, 421-423; & observation, 417-421; with & without death instinct, 417-418
Masochistic behaviour: bound by formal contract, 421; different from fantasy, 421; disagreement about aim, 421; pain as release, 421
Masserman, 417, 419, 420, 422, 574
Masson, 76, 79, 91, 94, 99, 121, 123, 132, 137, 140, 163, 203, 204, 205, 208, 223, 225, 227, 228, 229, 258, 276, 282, 286, 295, 322, 324, 325, 388, 435, 447
Masson: edition of Freud-Fliess correspondence, 2; Freud's lack of courage, 225
Mastering stimuli by binding, 376
Masterson, 17
Mastery: Dorey's two distinct meanings, 412; instinctual formulations of, 413
Matarazzo, 215; determinants of interviews, 213
Maudsley, 160
Maxwell, 33
Maze, 148
Mazer, 554
Mead, 33
Meagher, 202
Meaning, definition of & Shope's three distinctions: 588; as substitution (first), 589; as placement in chain of mental acts (second), 589; as sign & content of symptoms (third), 590
Meaning & interpretation, 588-591, context 589-590; content, 590; development, 590-591; meaning of neural innervations, 589 n.2; not product of cause (Home), 595
Measuring causal factors independent of outcome, 568
Mechanisms of symptom formation: 146; reality of, 146-148
Medical interpretation of catharsis, 21-23
Megalomania, 334, 336-339
Meissner, 362, 480, 485, 486, 487, 488, 513, 517, 521, 599, 600, 602
Melancholia: self reproaches & object-incorporation, 439; unconscious guilt, 435-436
Melanie Klein-Anna Freud debate, 574; absence of facts, 611
Memories: childhood, 292; fabricated, 227; intrude into consciousness, 68; linked to fantasies, 282; & later experience, 566
Memory structure, pathogenic: 105, 106, 107, 112, 470
Memory traces stored in systems, 242
Menaker, 419, 422
Mendel's experiment on false interpretation, 562
Menninger Foundation Psychotherapy Research Project, 547, 608
Mental apparatus as reflex, 242-243, 264-265, 525
Mental events, explanation in physiological terms, 170
Mental forces of disgust, shame & pain, 290-293, 326-321
Mental life: conflict between sexuality & ego, 328
Mentalistic explanation, 164
Mesmer, 32-38; animal magnetism, 32; *baquet*, 34; criticisms of Commission, 37; psychological explanation of effects, 41; theory of fluid, 33; therapy, 33; therapeutic claims & validity of theory, 35-37
Mesnet, 68
Messer, 566
Metaphors: confused with explanation, 599; as mirrors of conscious experience, 528
Metapsychology, 511-514: as beyond the psychology of consciousness, 512-513; characterisation by level misleading, 513-514; three viewpoints v. five, 511-512
Method of free association, singularity of, 551
Meyer, 23, 24, 25
Meynert, 74, 111, 113, 171, 172, 173, 175, 181, 241, 242, 263, 552
Meynert's theoretical ideas: associationism, physiological, 112, 211; cortical reflex, 172; rejection of hydraulic analogy, 182; disagrees with Darwin, 181; Fechner's conservation law, 175
Meynert & Exner on facilitation, 376
Mill, 109
Miller, 323
Miller, Sabshin, Gedo, Pollock, Sadow & Schlessinger 6, 66

Millet, 420
Milton, 22
Mintz, 544 n.4
Mitchell, 519
Mitchell, Bozarth & Krauft, 607
Mitchill, 68
Mitscherlich-Nielsen, 527
Modell, 490, 501, 513, 521, 600, 601
Models & theories, 199
Modes of satisfaction, auto-erotic, 529
Moll, 284, 285, 295, 303, 309, 311, 312; & concept of component drives, 305
Mollinger, 417, 419
Montgrain, 320
Moore, B. E., 482, 483, 517
Moore, M. S., 584, 585, 598
Moore, W. F., 554
Moore & Fine, 500
Mora, 131
Moral feelings, 336
Morality, 323, 326, 336
Morgan, 300, 301
Morin, 50
Morton, 131
Moses, 411
Moses of Michelangelo, 569
Moss, 554
Moss & Stachowiak, 554
Motives: dispositions (Ryle), 596-597; intangibility of, 596; partial understanding provided by, 586-587; reconciled with causes, 597
Motives & causes: distinction between, 595-596, dualism &, 596-597; reconciliation of, 597-599
Motives & causes (Freud): 577, 591-595; motives & force, 593-594; motives & causal conditions, 594-595
Movement & kinaesthetic image, 153
Mozart, Leopold, 317
Möbius, 66
Müller, 170, 171, 172, 477
Multiple personality, 24; symptom dependent on state of consciousness, 68
Munthe, 49
Murray, E. J., 215
Murray, J. M., 481, 482
Musatti & parapraxes, 606
Muslin, 262, 602
Mutton, Breuer's & Freud's, 112, 113
M'Uzan de, 418, 421, 426, 527
Myers, C. S., 29
Myers, F. W. H., 84, 85

Nachmansohn, 554
Nacht, 500
Nagel, 199, 527, 528
Nagera, 499, 502, 519
Narcissism, 333, 334-337, 338, 532; character of super-ego &, 482; contradictions, 517; empirical problems, 482; in revised instinct theory, 369; love & hate, 343; omnipotent thought & reality, 337-339; psycho-analyses of Sadger & Freud, 355; stimulus-instinct difference &, 344; topographic theory &, 482
Narcissism: autoerotism &, 344, 356-358; as developmental stage, 334, 335, 334, 337, 353, 516, 523; reconstruction of an interpretation, 532
Narcissism as primal state, 335-336, 344; existence problematic 482-483, 517 primary identification, 487; theoretical problems, 483-484; three contradictory paths from, 334-336, 358-363
Narcissism & homosexuality, 335-336, 352-353
Narcissism, Freud's blow to, 580
Natural death & internal causes, 380
Necessary & sufficient conditions, 124
Need for punishment, 421
Needs: linked with external satisfying object, 360; unpleasant states projected, 366
Needles, 392, 405, 406, 408
Negative therapeutic reaction, 433, 435; seen only by Freud?, 436
Nemiah, 158
Nerve force, pseudo-physiological, 130
Nervous system: Breuer's self-regulating view, 399-400; Freud's out-dated concepts, 525; protected by sense organs, 443
Neubauer, 466, 515
Neural transmission: accumulation & transfer, 183; discontinuous propagation, 183; hydraulic & electrical models, 183
Neurasthenia: central symptom, 203; demise of, 131; history, 127; literal meaning, 126; loss of nerve force, 130; & masturbation, 135, 162; sexual, 136; sexual factors & general form, 135
Neurasthenia (Beard): 126-128; syndrome, 541
Neurasthenia (Freud) 131, 132; importance, 123; disagreement about symptom production, 203; & excitation, 187; exclusively sexual etiology, 123; failure to confirm causes, 202; false conclusion, 122; typical symptoms, 132; dominates Fliess correspondence, 123; spontaneous emission, 162; & weakness, 163
Neuromuscular excitability, 47
Neuronal flow, suggested by condensation etc., 529
Neuronal inertia, principle of, 394
Neuroses: & abnormal disposal excitation, 168; & Basedow's disease or thyrotoxicosis, 206; excess repression, 300; faulty causal assumptions, 470; Freud's theory of sexual basis, 123; logical & associative structure, 220, 230; similar to perversions, 313; similarity with autointoxication, 205, 206; as negative of
..... continued

perversions, 291, 313; as regression to earlier fixation, 535; specific causes, 138; toxicity essence of, 450
Neuroses, actual, 121-126, 131-143, 201-208
Neurotic anxiety: & normal, realistic or objective anxiety, 452-453; prototype in birth, 451; reactions to toxins, 451
Neurotics: amnesia, 292; perverse dispositions, 291; sexual life of, 290
Newton, 33
Nichols & Zax, 19, 29, 87
Niederland, 355
Nietzsche & moral demands, 584
Nirvana principle & neuronal inertia, 398
Non-repressed memories, 225
Normal recollective thought, 267
Normality & repression, 300
Noshpitz, 17
Novey, 349, 356, 481, 482
Noy, 522
Number choice & determinism, 555
Nunberg, 351, 410, 418, 426, 467, 481, 482, 486, 540
Nunberg & Federn, 363, 451, 516
Nuttin, 408

Oberndorf, 551
Object & object cathexes: 360-362; concept ambiguous, 361; instinctual, 300; investment of libido in representations, 362; overvaluation, 290
Object-choice, 611; adult, 299; difficulty in explaining female, 518; primary identification &, 485
Object-loss, 421
Object-love, 314; & loving/affectionate current, 517; narcissism, 335; sexual, 517
Object orientation: creation of hate &, 345-346; opposition between ego & sexuality, 346, 347-352, 358-361; self-preservation drives &, 346; stages of object-directedness &, 518
Observational tests, negative crucial, 4
Observations v. clinical analysis, 316
Obsessional neuroses, 435; defensive willing, 102; guilt recognised first, 435; repression, 122; sexualization of thought, 365
Ockham (William of), 151 & n.3,
Oedipus, 283, 611; a shibboleth, 489-490
Oedipus complex: negative form & bisexuality, 441, 519; primary & secondary identification, 440; super-ego, 438-441
Oedipus complex: revised estimates of importance, 489-492; theory 'rich & elastic', 493
Oedipus complex & clinic, 490; precursors of, 491-493; time of occurrence, 490-491
Oerlemans, 464, 465; Freud's cures produced by suggestion, 208
Ogden, 355
Omnipotence & sexualisation, 338
Oppenheim, 44
Opposition between ego & sexual drives, 329, 362
Oral incorporation, mechanism of, 440
Oral stage, sucking & sadistic, 530; reconstruction by K. Abraham, 531
Organic automatic repression, 204, 284-286, 322, 324, 330-332, 328, 330, 341; cause of primal repression, 363; theory of, 442
Organic reinforcement, 247; uncharacterised, 271-272
Orgasm substituted for in symptoms, 137
Original reality-ego & pleasure ego, 523
Ormerod, 552
Orne, 48
Ornstein, 599
Ornston, 362
Orr-Andrawes, 17
Ostow, 313, 389, 405, 419, 423, 424, 427
Owen, 66 n.1
Ozturk & Ozturk, 311
O'Neil, 148, 149, 199

Padel, 279, 484
Page, 72
Pain, third mental force, 290
Pain, excitation & counter-cathexis, 395, 462
Palombo, 275, 277
Pao, 355
Pap, 597
Pappenheim, 78 n.1
Paralyses diagnosed according to response to electrical stimulation, 174
Paralyses resulting from the idea of paralysis, 64
Paranoia, ego's relations with reality, 339; fixation-disposition-regression notion, 336; persecutory delusions, 334
Parapsychological powers in artificial somnambulism, 39, 40, 41
Parapraxes: *Bride of Corinth & Signorelli* parapraxes placed in causal framework, 594; Grünbaum & *post hoc ergo propter hoc* argument, 594; hypnotic investigations of, 554; intentions of, 587; purposes in relation to conflict, 586
Parens, 424, 492
Parens, Pollock, Stern & Kramer, 519
Parkin, 351, 420, 468, 488
Pasteur, 125, 578
Pathogenic memory structure, 104-109; free association &, 114-116; logical & associative structure of neuroses &, 211; use in reconstructing symptoms, 116-119
Pathogenic secret, 20
Patients' previsions in illness, 41
Pattie, 33
Patton & Sullivan, 522

Pears, 596
Pearson, 467
Peele, 131
Penis envy, 519
Penrose, 406
Perception as protective function, 444
Perkins, metallic tractors, 39
Perverse behaviours: & genital orgasm, 313; & strength of component instincts, 300; & tension discharge, 313; part of normal constitution, 292
Perverse impulses, linked to fantasies & memories, 282
Perversion: active & passive, 312; based on preliminaries to sexual act, 297; as deviations in aim or object, 289; & fixation, 321; & oedipal transformation, 315; & repression, 283-287, 315, 324; traced to drives, 291
Peterfreund, 510, 515, 516, 599, 604
Peters, 585, 597
Petersen, 552
Petocz, 601, 602
Peyer, 134
Phallic stage, Little Hans source of Freud's evidence, 532
Philips, 45
Phillips, 551
Phobia, no anxiety about castration, 472
Phylogenesis, 467, 540-542; phylogenetic determination, 322; phylogenetic explanations embarrass psycho-analysts 541, 542; repression &, 285-286
Physics, basic aim of compared with that of psycho-analysis, 579
Physiological associationism (Meynert), 172-173
Physiological theorising, 170-173
Piaget, 412
Pieper & Muslin, 393, 427
Piers & Singer, 481
Pitt, 539
Pity as a mental force, 336
Placebo effects, history, 141
Plausible inferences, 580
Plausibility: in account of Elisabeth von R., 117-118; of narratives & readings, 601; of reconstruction & ascertainable facts, 117
Plaut, 349, 351, 412 n.4, 428
Play: other interpretations of repetitive play, 373-374, 390, 414-415; Freud's grandson's play neurotic, 373, 414
Pleasure & unpleasure: distinction, 413; increases & decreases of excitation, 404; principles & Schreber case, 339-340
Pleasure principle, 246, 398; behaviour beyond, 384; & control over all drives, 383; equivalent to the principle of inertia, 401; Freud finds nothing beyond it, 397; no control over libido, 403; operates only on internal sensations, 401; & pleasure-ego, 341; responsible for all functions, 402; serves death instincts, 382; & traumatic dream repetition, 414; violation of, 371
Pleasure-ego: & pleasure principle, 341; & primary process, 342; from reality-ego via introjection, 345
Pleasure sucking, 309
Pleune, 389, 426, 427
Podmore, 38
Pollock, 17
Polymorphous perverse disposition, 295
Popper, 3
'Pore', Harvey's theoretical term, 150
Possick, 262
Postures connected to ideas, 67
Post-Eysenck critiques of psychotherapy outcome studies, 543-545
Post-hypnotic suggestion (Bernheim), 90
Pösténvi, 482
Pratt, 389, 392, 411, 427
Preconscious idea & hyper-cathexis, 448
Pregenital stages of libidinal organisation, 530
Preyer, 134, 300
Pre-Oedipal components & events: relative contribution, 491; importance, 492
Pribram, 512, 525
Pribram & Gill, 196 n.1, 512
Primal repression: as automatic response to danger, 455-457; current status, 467-469; demarcated from after pressure, 351, 456, 467; early passive & later active forms, 468; as fixation, 351, 455
Primary aggressive drive, rules out narcissistic state, 484
Primary identification, 487-489; as 'being like', 440, 485; earliest emotional tie, 440
Primary identification: narcissism &, 488; object-choice &, 440, 485; & reorganisation of ego-traces, 504; resolving ambiguities, 488; strengthening of, 489
Primary identification: a confused mechanism, 487-489; at variance with rest of theory, 489
Primary masochism, 366, 382
Primary process, 246, 341-342, 432, 523; relation to secondary, 270, 523
Primitive movements as indicators of breakdown in resistance, 193
Primitive organism: excitation & consciousness, 375; fiction of, 407-408, 443; intensity of stimuli &, 375
Prince, tests Freud's dream theory, 279
Principle of constancy, 185, 187, 243, 344, 398; a secondary acquisition, 401
Principle of neuronal inertia, 397, 398; an abiological concept, 407; controls primary process, 400; more fundamental or central, 401-402
Principles: behavioural evidence, 192
Principles of pleasure: & constancy, 384, 403; inertia, 402

Principles, relations between, 397-404; reformulations of, 404-407
Privileged status of clinical data, 317
Project, 114, 399-401, 525
Projection, 342, 360, 362; basis of, 376
Projective identification & 'being like' & 'having', 491
Proof of reality of effects in uniformity, 39
Protection from stimuli more important than reception, 375, 408
Protective shield or stimulus barrier: 412, 443-444, 515; confusion over, 515; energy & creation of, 375; internal drive stimuli, 375; own energy, 443-444; practical value impugned, 515; & traumatic anxiety, 456
Pseudo-physiological constructs of *Project*, 525
Psychic determinism, 588; as psychic causation, 115; & free association, 116, 552
Psychic energy, 524-529: alternatives to, 528; analogy with physical energy, 525; as metaphor, 527-528; capacity to impel, 528; in relation to displacement etc., 529; psychical energy as motive force, 593; pleas for retention, 528; theoretical nightmare of changes in, 526
Psychic reality, 230
Psychic structure, prototype of is a mental representation, 503
Psychical activity, associative, 98
Psychical excitation, 164
Psychical force & effort, 103
Psychical insufficiency (Janet), 155
Psychological continuity, Freud's assumption of, 109-111
Psychological description in physiological terms, 170
Psychological needs in structural theory, 263
Psychological phenomena, determinants of, 11
Psychopathology, Breuer's use of French concepts, 195
Psychosexual group of ideas, 306; instincts &, 329
Psychotherapies: creation of data, 575; differential responses of therapist, 213-216
Psychotherapy evaluation: 542-548; comparison of outcome, 543; Eysenck's critique, 542; inappropriate as test of theory, 547; what therapy provides, 609
Psycho-analysis as interpretation, 560-566: absence of criteria for interpretation, 563; double construction, 563-564; failure of linguistic parallel, 564; interpretation & ignorance of language, 564; patient's reaction to interpretation, 561-562; pseudo-confirmation, 563; reliability of interpretation, 561; suggestion &, 562; effectiveness of therapy &, 562; validity, 561-562
Psycho-analysis as narrative, 566-570, 600-603: Bremer & Michelangelo's *Moses*, 569; criteria for narratives, 566; flexibility, 568; Freud's errors about Leonardo da Vinci, 569; history &, 581; measuring causal factors in, 567; Sherwood's criteria, 567; truth of, 570
Psycho-analysis as method, 549-551; creation of data, 575-576; long term application, 550; objectivity, 227, 575; parallel with tests of translators, 560; reliability & validity, 549, 551-556; standardisation, 573
Psycho-analysis as personality theory, 7, 508-509, 529-536; basis for evaluation, 509-511; beginning point, 514-520; defence, 535; female development, 518-520; instinctual drives, 328; mental structures, 520-522, 536; narcissism, 516-517; objects & autoerotism, 517-518; primary & secondary processes, 522-523, psychic energy, 523-529; stimulus barrier, 514-516
Psycho-analysis as personality theory & traits: evidence for, 537-540; childhood behaviours related to, 538-539; continuations & 'normal' repression, 540; dispositions as cause, 536, 538, 541; findings dependent on defective methods, 505; Kline's summary, 538; prolongations uncharacterised, 540; role of retrospective, observations 539; sublimations & behavioural correlates, 540
Psycho-analysis as science, 578-598, 610-612; attitude of practitioners, 611; causes & motives, 586-588, 591-598; explanation v. understanding, 582-586; Freud on, 578-581; meaning & interpretation, 588-591; objectivity, 611; veridicality, 566
Psycho-analysis as science, alternatives to: 581-582, 599-603; new natural science, 599; different science, 599-600; hermeneutic-narrative, 600-603
Psycho-analysis as therapy: appeal of, 608-610; evaluation of, 542-548; Columbia Center study, 545-547; Eysenck's analysis, 542-543; post-Eysenck studies, 543-545; therapy an inappropriate test, 547-548
Psycho-analysis as translation: 556-560; analogy deficient, 557-559; deciphering languages, 559; rebus, 559-560, 560 n.1
Psycho-analysis as social movement, 610
Psycho-analysts: status of Freud's theory among, 3; need for scientific scepticism, 611; use of cases to confirm, 574; use of persuasive rhetoric, 574
Psycho-analytic criticisms: isolated, 3; little historical sense, 3; never probe facts, 3; three types, 510, 581, 599-600

Psycho-analytic explanations as metaphorical redescriptions, 527
Psycho-analytic facts & choice between variant theories, 575
Psycho-analytic reconstructions: at variance with realities of development, 573; Freud's reconstruction of narcissism 573; Little Hans & Wolf Man, 566; questionable basis of, 572; Rat Man's reaction, 570; weakness of developmental theory &, 572
Puberty & sex differences, 298
Pulver, 276, 458, 517
Pumpian-Mindlin, 465, 510, 526, 553, 599, 604
Punishment, unconscious need for, 433, 434, 435, 450
Purge, catharsis as, 22
Purpose, Freud adds to reflex, 177
Putnam, 552
Puységur: artificial somnambulism, 38; magnetic sleep, 38; Victor subject, 39

Quay, 215
Quota of affect, 97, 98, 169, 184

Rado, 418, 419, 465
Ramzy & Wallerstein, 466
Rangell, 201, 361, 362, 405, 465, 485, 491, 516, 585
Rank, 279, 575
Rank's birth trauma theory, 572, 611
Rapaport, 467, 500, 503, 511, 521, 554, 563
Rapaport & Gill, 500, 512, 513, 527
Rationalisations & causal explanation, 597
Rawn, 600
Reactions to sucking, orgasm-like, 311
Reaction-formations, 531, 533; how identified & produced, 533
Realistic view of entities, 499: Freud on ego, super-ego & id & *Pcpt., Cs.*, 499
Realisation (Charcot) 66, 152-155, 165-166, 611
Realistic thinking, based upon mnemic images, 244
Reality principle, 340, 398
Reality-ego: change into pleasure-ego, 345; knowledge of reality, 340; reality orientation, 346; secondary process &, 342
Reality-testing as ego function, 446: perceptual & motor apparatus basis of, 344, 445-447
Reasons: & causes compared (Davidson), 595; as a kind of psychological cause, 598; treated as causes, 577
Rebus: a variant orthography & solving not like translation, 559-560, 560 n.1
Recapitulation: biogenetic law of, 285; early refutations of, 392; libidinal development, 338; ontogenetic, 285
Reconstruction: connecting assumptions, 116-118; conflict with facts in Elisabeth von. R. (117-118), Rat Man (565, 567, 570 & n.1), & Wolf Man, (472, 566); suggestion in therapy, 570
Recovery of memories: Freud's role, 223; specificity of guidance, 223
Recurrent utterances, 176: role of emotion in, 179
Reduction of internal tension to zero, 407
Refinding & finding breast as object, 298
Reflex function, hypnosis & changes in, 42
Reflex basis for theory of mental apparatus, 242-243, 264-265, 525
Reformulations of the principles, 405; separation of instincts from principles, 404
Regression: as two sided process, 246; concept absent from *Three Essays*, 353; doubly determined, 246; & detachment of libido, 355; & resexualisation of social drives, 335
Reich, A., 202, 481
Reich W., 210, 409, 417, 418, 419, 426
Reichard, 17
Reification, definition inadequate, 498
Reik, 417, 427
Reis, 554
Reiser, 510, 575, 599, 604
Reliability of psycho-analytic method confused with validity, 573
Reliving of traumatic episodes, 14, 30
Repetition of remembered pleasure as a motive, 306; & erotogenic zone, 308
Repetition compulsion, alternative explanations: play, 414-415; Schur, 413-415; trauma & fixation & mastery, 409-413; transference, 415-416
Repetition compulsion, clinical aspects: transference, 371-372, 385-387, 415-416; trauma, 372-373, 375-377, 387, 390-391, 415; play 373-374; 390; 414-415
Repetition compulsion, theoretical aspects: binding & mastery, 393-396; dual drive framework, 370; instinctual characteristics, 374-384, 388-390, 392; internal death, 392-393; premises of formal argument, 378-381; pleasure principle, 397-398; single thesis, 383-384; trauma, 375-377, 390-391
Repetition compulsion & mastery: 373, 409-416
Repetition & uncanniness, 388
Reppen, 262, 513, 562, 566
Representability (visual), 237
Representational meaning of drive, 349
'Representation' v. 'presentation', 448 n.2
Repression, 101-104, 148, 160-162, 165-166: anxiety &, 450-457, 470-474; associationism &, 161, begins with act of will, 101-104, 102 & n.3, 108; ego-instincts &, 342-343; hypnotic trauma &, 554; mechanism of, 6, 160; pathogenic memory structure &, 106-108; reality of,
..... continued

160-162, 166; scope, 101, 162; subjective sense of effort, 104, 611; transformation of affect, 246-248, 330; uncharacterised term, 160, 166
Repression, basis of: automatic & organic, 204, 284-286, 322-323, 330-331; ego or psychological force, 103-104, 329, 331-332, 351; undefined, 322-324; standards of ego & conscience &, 342-346
Repression proper & primal repression, 331-332, 455-456 467-469, 505
Repression & psycho-sexual development, 284-285, 298, 300, 318-320, 322-324
Repression-identification hypothesis about homosexuality, 353
Resistance: force, & repulsion, 104; Freud's first use, 432-433; repression &, 104; unconscious ego process, 434-435
Restriction of consciousness (Janet), 155-157, 165-166
Retrospective studies, typical errors in studying causal factors, 539
Reyher, 158, 554
Reynolds, 66
Ribot, 24, 45
Ricoeur, 437, 476, 484, 485, 569 n.5, 601, 603
Rieff, 258, 260
Rika van B., 19
Ritvo, L., 92, 181
Ritvo, S., 482
Ritvo, S. & Solnit, 486
Rivers, 415
Roback, 552
Robertson, G. M., 76
Robertson, G. C., 85
Roffenstein, 554
Rogers, 215, 607; & influence in counselling, 215; & selective responding, 216
Rogow, 250 n.1, 287
Roiphe & Galenson, 519
Role of affect, 87
Romm, 419
Root cause: of hysteria, 10; of snake hallucination, 68
Root of illness, 25
Rosen, G., 39
Rosen, V. H., 501, 526, 563, 603
Rosenbaum, A. L., 272
Rosenbaum, M., 17
Rosenberg, 129, 131
Rosenblatt & Thickstun, 502, 510, 526, 599, 604; & accumulator-discharge, 183
Rosenfeld, 427
Ross, 599, 600
Rothstein, 411, 517, 522
Rubella & intellectual handicap, 539
Rubenstein & Levitt, 542, 563
Rubinfine, 527
Rubinstein, 561, 586, 601
Russell, Bertrand, 584
Ryle, 596, 597
Sacher-Masoch, 420, 422
Sachs, & Freud's perversion theses, 315
Sack & Miller, 422
Sacks, 489, 490, 575
Sadger, 312, 353, 354, 418, 516; observations on homosexuality, 352
Sadism, 290
Sadism & masochism, 290; alternative explanations, 416-423; facts needed, 423
Sadism as representing death instinct, 381
Salpêtrière, Freud's departure & Le Log__'s arrival, 61 n.6
Salzinger, 214
Salzman, 417, 420, 422
Sand, free association, 555; Freud's evidence from Dora case, 257
Sandler, 177, 389, 425, 427, 465, 484, 485, 486, 487, 501, 516, 526
Sandler & Joffe, 389, 477, 526
Sandler & Sandler, 268, 498
Sandler, Dare & Holder, 457, 498
Sandler, Holder & Meers, 481
Sargent, 561
Sargent, Horwitz, Wallerstein & Sampson, 563
Sarton, 33 n.1,
Sass & Woolfolk, 562
Satinover, 370, 470, 540
Satisfaction identical with sexual excitation, 296
Saul, 405, 427
Sceptical tradition, 40; & Mesmerism, 40-41
Schafer, 270, 273, 437, 459, 475, 476, 477, 482, 486, 498, 511, 515, 519, 520 n. 1, 575, 600, 604
Schafer's action language, implication that psycho-analysts must begin anew, 506
Scharfman, 258, 261
Schatzman, 355
Schilder, 411
Schiller on love & hunger, 364-365
Schmideberg, 562
Schimek, 210, 223, 224, 473, 566
Schizophrenia & paraphrenia, 357-358
Schizophrenic withdrawal & loss of social sublimation, 335
Schlessinger, 570 n.6
Schlessinger, Gedo, Miller, Pollock, Sabshin & Sadow, 26, 196
Schmidl, 561
Schneck, 75, 554
Schneider's associations to numbers chosen by others, 555
Schorske, 277
Schreber, 334-337, 339-340, 351, 353, 354-356, 363
Schreber, homosexuality & paranoia, 336-337, 353-356
Schroeder, 552
Schroetter, 554
Schur, 17, 234, 276, 405, 413, 464, 465, 501, 502, 521

Schur & Ritvo, 405
Schusdek's explanation of seduction theory, 225
Schwartz, 500, 503
Scientology & production of pseudo-memories, 217-218
Scopophilia, 308, 312
Scott, 573
Scriven, 547
Sears, 538, 553
Sechenov, 172
Second explanation of male homosexuality, 352
Second personality of Adrienne, 84
Secondary consciousness, 118, 197
Secondary identification as choosing or 'having', 485; as regression from object-choice, 440
Secondary process, 244-245, 248, 341-342, 432; cannot develop from primary, 270; deficiencies in Freud's theory, 522; description rather than explanation, 271, 522
Secondary revision, 237; problems of placement, 269
Secret as cause of illness, 91
Secret motives & gaps in patient's account, 110
Seduction prevalence in relation to masturbation, 228
Seduction memories, coherence & place in logical structure as proof of reality, 222; non-recall an unconscious memory, 223-224
Seduction theory, 208-209, 229-231: father as seducer, 208, 210; Freud's expectations &, 218-220; guidance of recollections, 220-225; reasons for giving up, 224; sexual instinctual drive &, 281-282
Seduction theory collapse, 209; alternative accounts of, 225-229; effect on Freud's expectations, 224, 230-231; sexual drive &, 280-287
Segel, 411, 425, 427
Seidenberg, 550
Seitz, 561
Self as sexual object, 333
Self-preservation drives, 363; creatures of the death instinct, 379
Self-preservation in relation to primal narcissism, 345
Self-regulation of nervous system, 399-400
Sensations, determining quality of: reproduced in adult, 283, 323-324; unconscious ideas & in Charcot, 66
Sensory hallucination (Dora), 254; as compromise, 255
Sensual sucking: 293-294, 309-310; flow of milk &, 294
Sexual development, 296-300, 318-324; data merely described, 281
Sexual excitation, 306-308: arises in different ways, 295; basis unspecified, 298; pleasure &, 310-311; discharge of, 190; general stimulation &, 295; obscurity of change into disgust, 205; physiological, 285; toxic basis, 164, 204-207
Sexual factors as causes of neuroses, 231: disagreements among Freud's followers about neurasthenia, 197; not singled out as important in the *Studies*, 122
Sexual & hunger drives: deficient as models for ego-instinctual drives, 281, 364; sexual tension & hunger drive, 311
Sexual instinctual drive, 281, 300-305, 306-308, 325: death instinct argument &, 379-380; schematic picture of, 303-304; seduction theory &, 281-287
Sexual instinctual drive, ego-functions &, 341; super-ego &, 478-479
Sexual objects: body as, 313-316; choice of, 298-300, 318-320, 322-324; pleasure-giving, 366
Sexual paradox in the masculine basis of the *Three Essays*, 320
Sexualisation, different effects of, 337-339, 365, 475-476
Sexuality: childhood, 284-285, 292-296, 305-308, 308-312, 313-316, 322: adult sexuality &, 287-289; clinical v. observational data, 316-318; germs present at birth, 292; precocious & sadism, 295, 310; satisfaction of component drives, 295, 311-312; theory implicit in Dora analysis, 288
Sexuality: feminine & masculine, 319, 493-495; psychoneurotic, 281; perverse, 284, 289-292, 312-313, 315-316
Shame, 326, 336: as mental force, 290; observations of, 323; organically determined, 322; reference ambiguous, 533
Shapiro, A. K., 141, 408
Shapiro, A. K. & Morris, 141
Shapiro, T., 572
Sharpe, 602
Sheehan, 158, 554
Sheehan & Dolby, 554
Sheehan & McConkey, 554
Sherrington, 525, 526
Sherwood, 562, 566, 567, 601
Shevrin, 526
Shield (protective): active or passive, 515; inconsistent with contemporary knowledge, 515-516; own energy, 443
Shope, 526, 586, 587, 589, 590, 593, 606
Shor, 527
Shulman, 521
Side-cathexis & inhibition of the transfer of excitation, 394
Sidis, 552
Siegel, 459
Siegler, 586
Signal anxiety, 441-443, 457-458, 459-464: attenuation, 462-464; mechanism, 457-458, 459-462; psycho-analytic views of, 464-466; reproduction, 457, 459-461

Silberer, 392
Silverberg, 421
Silverman, 491
Simenauer, 393, 485, 486, 488
Simmel, 389, 404, 405, 424, 427
Simon, 490, 575
Simulation in hysteria, Anna O., 18; Charcot's experiments, 56
Sjoback, 349
Skura, 566
Slap, 389, 521
Slap & Saykin, 521
Slipp, 263
Smirnoff, 420, 421
Smith D. L., 516, 517
Smith J. H., 465, 466
Social dissolution & interest in hypnosis & hysteria in France, 44
Social sublimations & schizophrenic withdrawal, 335
Sokal, 278
Solms & Saling, 525
Solnit & Kris, 516
Solomon, 184, 196
Somatic compliance, 288, 316
Somatic sexual excitation, 162; accumulator-transfer model &, 184; similarity with activation of ideas by instinctual drives, 329
Source (instinctual), 300: somatic & instinctual drives, 308; of component drives, 307-308; Freud's drive concept, 302; remembered satisfaction &, 306-307
Source of standards for repression, 351
Sources of anxiety, 204
'Sovereign remedy' for hysteria in talking cure, 26
Spanjaard, 275, 276
Speech ejaculations & emotional reactions, 98
Spehlmann, 183
Spence, 277, 562, 564, 566, 568, 569, 570, 600
Spencer, Herbert & flow of nerve force, 181
Spencer-Hall, 42
Spero, 323
Spiegel, 417, 419, 422, 475
Spielman, 515
Spitz, 465, 466, 492
Spitz & Wolf, 311
Spitzka, 130
Split in consciousness as basis of all hysterical symptoms, 101, 118
Spotnitz, 17
Spring, 406
Sprott, 499 n.4
Spruiell, 314, 482, 522, 602
Stainbrook, 176
Starke, 527
States of consciousness & variation in symptoms, 12, 14
State of rest, reestablishment, 182
Steele, 601
Steele & Jacobsen, 270, 276, 523, 573
Stein, 272, 415, 419, 420, 501
Steiner, 574
Steingart, 481
Stekel, 202
Stephen, 556
Stengel, 590
Sterba, 540
Stereotypies in schizophrenia, 588
Stern, A, 202, 203
Stern, D, 515
Stern, M. M., 411
Sternbach, 418, 422, 427
Stewart, 1, 199, 302, 465
Stiles, Shapiro & Elliott, 545
Stimulation, anal, 311; non-genital, 310
Stimulus & instinctual drive defined, 445
Stimulus barrier, 375, 412, 443-444, 456, 514-516
Stolorow, 420, 484, 498
Stolorow & Atwood, 273
Stone, 420, 426, 427
Story-telling (Anna O.), likely effects of, 21
Stott, 539
Strenger, 602
Strengthening of primary identification not defined, 158
Structural theory, 430-431; evaluation, 457-505; Freud's arguments for, 431-437; origin of term, 499; functional version of, 497-504; reception & present status, 497-498
Structures & functions, 497-504: functions & metaphors, 497-499; characterisation, 503; lack of agreement on functions, 500-502, 520; memory traces & structures, 504; realistic structures, 499-500; structures are not functions, 502-504
Structures & functions of ego-instinctual drive, 352
Strupp, 545
Strupp, Chassan & Ewing, 561
Subconscious association, 97
Subjective feeling of certainty, as standard, 222
Sublimation: & development of culture, 293; as an empty concept, 476; behaviour & underlying processes, 476-477, 537; ego energy &, 447-448; Freud's definition vague, 536; innately determined, 536; libido genital or pregenital, 476; mechanism of, 476-477; theoretical problems, 526, 537; usefulness of the concept, 476
Substitute function of acting out, 386
Substrate of intention & counter intention, 159
Sucking: associated with masturbation, 309; blissful end-state as indexing its sexual nature, 310; exemplar of auto-erotism, 293, 294; exultant (Lindner), 309; non-nutritive, 308; not secondary pleasure,
.....continued

293; rapture (Lindner), 309; sensual, 288, 293; sexual consequences, 288
Suggestion: incapable of producing uniform phenomena, 53; as influence in Freud's fact gathering & treatment results, 208; Freud's defence of charge against him, 612; in seduction theory, 210-211
Suggestion in hypnotic therapy, 77
Sulloway, 1, 117 n.5, 176, 183, 227, 232, 248, 275, 283, 284, 285, 305, 541
Sully, 300, 301
Summation of stimuli & reflex disposal of excitation, 175
Super-ego, 430, 437-441: absence of metapsychological description, 521; acquisition of destructive energy unexplained, 505; anthropomorphism inherent in, 498; character of & narcissism, 482; empty concept, 505; energy made available during identification, 438-439, 441
Super-ego formation, 438-441: development in first year, 490; difficulty explaining, 518; phylogenetic factors, 542; wrong objects in, 478-479, 505
Super-ego functions, criticisms by psychoanalysts 480-482, 505, 521
Super-ego, female, 493-497
Susemihl & Hicks, 22
Sutcliffe & sceptical & credulous positions on hypnosis, 32
Sutcliffe & Jones, 24
Swales, 18, 78, 79, 91, 123 n.1, 556
Swanson, 196 n.1, 525
Symons, 414, 415, 417, 421
Symptoms: appear in place of actions, 586; as compromise, 238; as conflict between wishes, 586; as punishment summating with wish, 239; as pair of wish fulfillments, 239; as representing wishes, 238; formation, 161; memory & pathway to, 114; obsessional & conflict between wishes, 587; origins of, 78; removal in waking state, 74; problem with symbol, 159; wish-compromise view, 239
Symptom & sensory content, 27, 212, 238, 286; from innervation, 238, 255; as associations, 111; associations between affect & innervation, 111; based on sensations, 27; basic hysterical, 283; connection with impulses & fantasies, 282; objectivity of symptom analysis, 279
Symptoms & dreams, Freud's two lines of thinking about, 232
Symptomatic act as symbol of masturbation, 256
Symptom-producing mechanisms as hypothetical constructs & intervening variables, 148
Systems of reflex mechanism, 240, 241: inadequately characterised, 267, 280
Szasz, 393, 406, 424
Taboos & conscience, 342
Taine, 24, 45, 68
Talking cure, 11, 20, 25, 26; emotional reinterpretation, 11, 180
Tannenbaum, 552, 555, 570, 606
Tart, 554
Tausk, 202
Taylor & Martin, 24, 68
Teicholz, 484, 516, 517
Tennes, Emde, Kisley & Metcalf, 515
Tension, 307: genital sexual excitation, 297; as unpleasure, 297
Tension reduction function, 399-400
Terminological problem & structural theory, 431
Thanatos fusion with Eros, 439
Theatrical metaphor: affected way of life, 23; human brain as a, 24
Theoretical entities, processes & terms, 37, 151, 503
Theorising, Breuer v. Freud, 195
Theories, 167-168: abstract, 167; analogical or metaphorical, 167; explanations & 167, 199; models &, 199; reductionist, 167; structural & economic, 272
Theory, as disposable scaffolding, 273; asymmetry of test, 547; historical evaluation, 3; importance of testability, 151; therapeutic arena & test, 548
Theory of neuroses (Breuer & Freud), 167-168, 188-194; differences between, 194-198
Therapeutic effects, establishing basis of, 141
Thompson, 199, 419, 420
Thornton, 17, 18
Thought: omnipotence of, 365; sexualisation of, 365; wish-fulfilling, 365
Three hypotheses of homosexuality based on same evidence, 353
Timpanaro, 556, 605, 606; philological analysis of slip, 556; rules of unconscious language, 560
Tinterow, 39, 42
Topographic theory of mind, 240-243: censorship in, 267-269 ; consciousness &, 265-266; infantile wishes, 271-272, limitations of reflex concept,263-265; ; secondary process in, 270-271; secondary revision &, 269-270; thoughts in dreams, 266-267; unable to accomodate Thanatos, 436
Toulmin, 596, 597
Toxicity: & birth, 450-451; & ego's signal function, 451; & essence of neuroses, 450; & normal anxiety, 452-453
Tragedy as purge, 21
Trains of thought not purposeless, Freud, 115
Transference repetition, 371-372, 385-386, 396, 412, 415-416; social-psychological basis, 416; under pleasure principle, 415
Translation, psychoanalysis as, 556

Translation & transformation of pathogenic conflict, 593
Transposition or displacement of affect, 102
Trauma: ambiguities of post trauma processes, 394; neglect of real, 229; summation of, 222; without emotion, 30
Traumatic amnesia, parallel with hypnosis, 554
Traumatic dreams, fading of, 396
Traumatic excitation: acts like instinctual source, 395; explanation extended to play, 396; pleasure principle & binding, 390; shield breached, 376
Traumatic force of an event, 212
Traumatic neuroses, 372; result of shield being breached, 376
Traumatic repetition, 375; & fixation, 387
Traumatic situation & automatic anxiety, 454
Treumiet, 527
Trieb, 301-302
Truax, 214, 215
Truax & Carkhuff, 214
Truax & Mitchell, 216
Truth of first premise of death instinct argument, 379
Tuke *Dictionary*, 173
Turiell, 481
Tyson & Tyson, 484, 517

Unconscious affect, 450, 458, 459, 463
Unconscious, 187; & structured repressed fantasies, 270; term used three ways, 431, 432
Unconscious & preconscious, inconsistencies in characterisation, 270
Unconscious associations, Meynert, 115
Unconscious death instinct & id, 448
Unconscious ego resistance, 432-435
Unconscious guilt, 421; separate from need for punishment, 418
Unconscious ideas: not only guidance of free associations, 576; as thing-representations, 447
Unconscious infantile sexual wishes, 248
Unconscious influences: & determinism, 72; & the process of recollection, 224
Unconscious need for punishment, 433-434, 435-436
Unconscious processes, access to higher structures, 449
Unconscious motive, von Hartmann, 115
Unconscious system, dreams & symptoms, 242
Understanding & explanation, 582-586; Jaspers' genetic, 582
Unexpressed affect, 27
Uniformity of causation assumption, 470
Unintentional influences in therapy, 213-218
Unpleasure: avoidance of & repression, 247; excitation &, 463; release of, 285
Urmson, 597

Vagina, excitability of, 319
Valenstein, 413, 423
van der Hart & Braun, 158
van der Hart & Horst, 156
van der Hart & van der Velden, 19, 30
van der Waals, 457, 501
Van Deusen, 127
VandenBos, 545
VanOphuijsen, 418
Vasile, 552
Veith, 55, 75
Velpeau, 45, 48
Verbal conditioning, subjects' unawareness of, 215
Verbal influence & experimental data, 575
Verbal utterance, 27
Vetter, 227
Viewpoints of Charcot & Bernheim, 32
Vigoroux, 174
Villaret, 88; Freud & *Handwörterbuch*, 168; *Handwörterbuch*, 76, 94, 114, 120
Vision, eyeless, 40
Voluntary movement, 173

Waelder, 126 n.2, 198, 201, 409, 411, 425, 427, 428, 465, 481, 486, 513, 516, 551, 552
Waite, 42
Waking state formulations (Freud's), 91-92, 100-104; pressure method &, 104
Wakley, 39
Waldeyer & neurone theory, 183
Wallace, 414
Wallerstein, 528, 547, 571, 572, 575, 599, 608, 610, 611
Wallerstein & Sampson, 561, 563, 568, 611
Walmsley, 33
Warme, 511, 528, 572, 599
Weber, Bachrach & Solomon, 545, 608
Weber, Solomon & Bachrach, 545
Weiner, 524, 525
Weininger, 295
Weismann, 380, 381
Weiss, 420, 564, 610
Weissman, 414
Wells, 552, 554
Well-characterised terms, essential features of, 150
Werman, 423, 424, 524, 541
Wetzler, 562
Wexler, 527
Wheelis, 551
White, R. B., 355
White, R. W., 428, 476, 477, 526
White, W., Hartzell, & Smith, 217
Whitman, 467
Whyte, 263
Widlöcher, 479, 484, 486, 488
Wiedeman, 498, 500, 502
Wiener, 127
Wilbur, 393, 406, 411
Williams, 149 n.2
Wilm, 301, 302

Wilson, 499 n.4, 500, 501
Winnicott, 424, 489, 607
Wisdom, 482, 561, 562
Wish as movement of excitation, 244
Wish-fulfilment in dreams & symptoms, 233-235, 238-240: consequence or cause, 274; as empirical proposition, 273; Freud's doubt if theory testable, 274; theory not needed, 277
Witkin & Lewis, 554
Wittgenstein's 'charm', 605
Wohlgemuth, 555, 560; association to numbers chosen by Freud, 555
Wolberg, 554
Wolf, 51, 428
Wolf Man: anxiety simultaneous with repression, 473; Freud adds elements to his recollection, 473; negative Oedipus complex, 472; recall of other castration threats, 473; sequence of events in, 472
Wolfenstein, 405
Wolff, 270, 317, 318, 490, 515
Wollheim, 75, 241, 264, 491
Woodworth, 152, 153, 552, 554, 570
Word-representation: consciousness &, 450; linkage, 458
Wundt, 172

Yankelovitch & Barrett, 428
Young, 553

Zane, 561
Zem, 523
Zetzel, 201, 315, 411, 465, 473, 474
Zilboorg, 486, 551
Zones érogènes, 384, 303
Zubin, 571
Zweig, 581

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