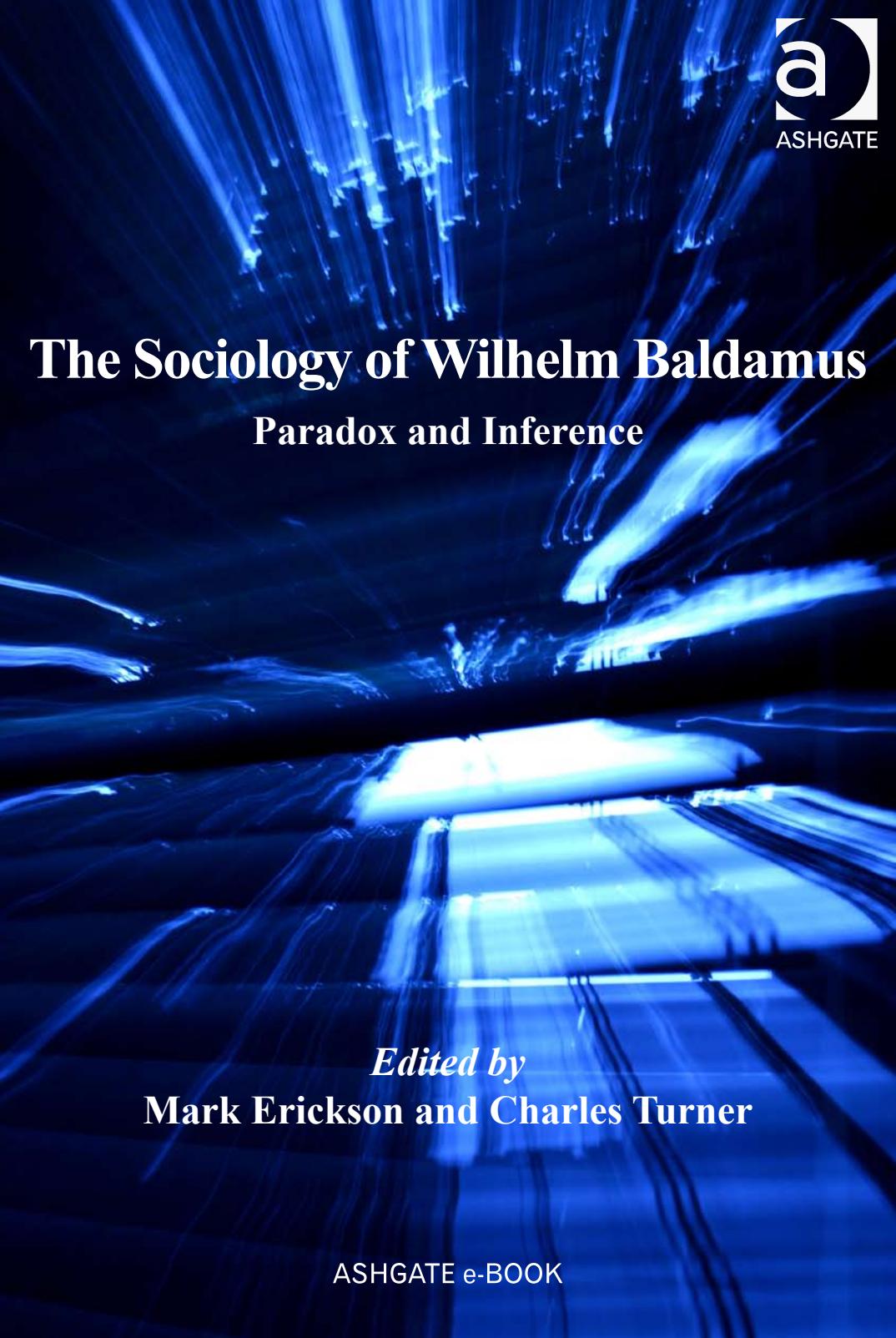
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Paradox and Inference
Edited by
Mark Erickson and Charles Turner
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THE SOCIOLOGY OF WILHELM BALDAMUS

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The Sociology of Wilhelm Baldamus

Paradox and Inference

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ASHGATE

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Notes on Contributors

Wilhelm 'Gi' Baldamus (1908–1991) joined the Department of Sociology at the University of Birmingham in 1961. He became chair and head of department in 1970 and retired from the university in 1976. His early work centred on industrial sociology (exemplified by his *Efficiency and Effort* (1961a)) but from the mid 1960s he focused on methodological and theoretical problems facing sociology which culminated in his second book *The Structure of Sociological Inference* (1976a).

John Eldridge is Emeritus Professor of Sociology at the University of Glasgow. He has held academic appointments at the Universities of Durham, York, Bradford and Glasgow. He was, until recently, Honorary Professor of Sociology at the University of Strathclyde. He has long-standing research interests and has published extensively in the fields of industrial sociology and the sociology of media. He has also published work on Max Weber, C. Wright Mills and Raymond Williams. He is a founder-member of the Glasgow University Media Group.

Mark Erickson is principal lecturer in sociology at the University of Brighton. His research is in the areas of sociology of work and sociology of science. His most recent books are *Science, Culture and Society: Understanding Science in the 21st Century* (Polity 2005) and *Business in Society* (Polity 2009 with Harriet Bradley, Carol Stephenson and Steve Williams).

Peter Lassman teaches political and social theory at the University of Birmingham and is currently working on some aspects of the history of political thought in the twentieth century. He has written on Max Weber, Isaiah Berlin, Stuart Hampshire, John Rawls, and the problem of value pluralism in modern political philosophy. He is a founder editor of the *European Journal of Political Theory*. From 1971 he was a member of the Department of Sociology at the University of Birmingham headed by Gi Baldamus.

John Rex is Emeritus Professor of Sociology at the University of Warwick. He has taught and carried out research in sociological theory and ethnic relations since his arrival from South Africa in 1949. He founded the departments of Sociology at Durham in 1964 and Warwick in 1970. He taught at Birmingham University with Wilhelm Baldamus from 1962 to 1964. His best known books are *Key Problems of Sociological Theory* (1964), *Race Relations in Sociological Theory* (1970) and *Race and Ethnicity* (1986).

Charles Turner is Associate Professor of Sociology at the University of Warwick. His research interests include social and political thought, the history of sociology and the politics of commemoration. He is the author of *Modernity and Politics in the Work of Max Weber* (Routledge 1992) and *Investigating Sociological Theory* (Sage 2010) and co-editor (with Robert Fine) of *Social Theory After the Holocaust* (Liverpool University Press 2000) and (with Ralf Rogowski) of *The Shape of the New Europe* (Cambridge University Press 2006).

Preface and Acknowledgements

The idea for this book arose out of a project that had been started and nearly brought to completion by Robin Cohen. Robin had been a colleague of Baldamus at Birmingham University in the late 1960s, and in 1981 began putting together a collection of his most important writings under the provisional title *The Exoteric Paradox*. With the exception of that title chapter, which had appeared only in German, and Baldamus's inaugural lecture at Birmingham, 'The Consumption Imperative', all of the material had already appeared in English; but because it had appeared in fairly disparate settings it was thought worth collecting under a single cover. Publishers, however, were less enthusiastic and after a few years the project was held in abeyance. Baldamus died in 1991.

Since then the internet has made his published writings more readily available, and so we thought that a different volume would be appropriate today. Nevertheless, it could not have seen the light of day had Robin not made available to us his correspondence with Baldamus from 25 years ago, along with a draft introduction that he had prepared for the volume. Besides the difference in tone and emphasis, ours now contains considerably more biographical detail than would have been possible or desirable while Baldamus was still alive.

Additional thanks are due to Christian von Ferber, David Perman, Ian Procter, Jennifer Platt, John Wrench, Peter Lassman, Michael Green and the late Richard Brown for providing a number of sources, and to Baldamus's late daughter Eva Pritchatt and his grandchildren Jeremy and Susie who gave us access to his personal papers.

M.E., C.T.
2009

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Introduction

Mark Erickson and Charles Turner

In Germany: 1908–1937

Throughout the Weimar Republic the University of Frankfurt held an annual student essay competition. This rotated between faculties, and in 1932 it fell to Karl Mannheim, Professor of Sociology, to set the title. His choice, the history of early German liberalism, was not an especially popular subject in Germany at the time, and the competition attracted just two entrants. One of these was Mannheim's assistant, the 24 year old doctoral student Hans Gerth, who just happened to be working on that very topic (Gerth 1976). In 1937 Gerth would flee Germany, and with the help of Mannheim and then Edward Shils, find his way via Harvard to Madison. By 1940 he was giving lectures there, entering a long and often difficult collaboration with C. Wright Mills and translating Weber's essays in the sociology of religion (Oakes and Vidich 1999). Although he never fulfilled the potential that Mannheim saw in him, if you open any book about German academic émigrés, Gerth's name is likely to be mentioned somewhere, as an example of the sort of broken life and fractured career that was one of Nazism's more enduring legacies.

Although it was his specialist subject, in a spirit of generosity or charity the university decided that Gerth would share the essay prize with the other entrant, another graduate student and fellow member of Mannheim's seminar. His name was Wilhelm Baldamus, and the story of his flight from Germany, his struggle to adapt to England and subsequent academic career will be sketched here. It is less well known than that of Gerth, or for that matter of Norbert Elias, another of Mannheim's assistants in Frankfurt; this is partly because Baldamus was for years reluctant to tell it himself at any length, rarely responding to the questionnaires that were often sent to German academic exiles after World War II, and remaining to the end something of a maverick, enormously respected by those who knew him but never a man of the mainstream. That Perry Anderson could find a place for him neither in his famous 1968 account of the role played by European émigré scholars in British intellectual life, nor in his reconsideration of it 25 years later, is perhaps not surprising: the first article had lamented the absence of a British theory of society with the scope and power of its continental European counterparts, the second celebrated the achievements of a new generation of Brits in closing the gap (Anderson 1968; Anderson, 1992). Baldamus had no interest in a 'theory of society', though he was more deeply rooted in that continental

European tradition than any of them. Towards the end of his life he did begin to dictate an autobiography of sorts, but it is telling that he thought of it as a 'political autobiography addressed to posterity', and imagined it covering the years 1930 to 1951, with '*no* connections with my *academic* life'.[1] Its provisional title, though, 'Blind Alleys', expresses much, not only about the fate that history dealt him but also about his attitude to the life of the mind: he was never inhibited by the thought that a project, be it intellectual or not, might lead nowhere.

Wilhelm Otto Hermann Baldamus was born on 2 May 1908 to lower middle class parents in Berlin. There he attended an *Oberrealschule*, a school whose nine years of study enabled its pupils to go to university but which, in contrast to the *Gymnasium*, was based not on classics but on modern languages and the natural sciences. Like many of their generation and background his parents were loyal enough to the Kaiser to name their son after him (and, it seems, after Otto von Bismarck), and Baldamus would later remark that as a teenager in the 1920s he had continued to admire the now-exiled monarch even when most Germans had turned against him. However, by the late twenties – from 1927 to 1930 he was a student of economics, philosophy and statistics at the University of Berlin – he had become a left liberal and supporter of the free democrats, and in the summer of 1930 would move, as he put it, to 'the extreme left of the SPD', under the influence of his wife and her parents, Russian Jews and Menshevik exiles;[2] by 1931 the name Wilhelm had become enough of a burden for him to have it changed to the French Guillaume, shortened to 'Gi', the name by which he would eventually be known by English friends and colleagues.

As an undergraduate in Berlin his teachers included Heinrich Herkner,[3] Ladislaus von Bortkiewicz,[4] Hans Reichenbach,[5] and Werner Sombart.[6] In 1930

1 Letter to Robin Cohen, 20 June 1986.

2 His father-in-law, Abraham Mutnik (born Abram Mutnikovich (1868–1930)), was a Bundist who had been on the central committee of the *Allgemeine Arbeiterbund* in Vilnius, escaped from a Tsarist prison, and arrived in Berlin via Sweden and France in 1926. There is a memorial to Mutnik and five other social democrats at the crematorium in Wedding, Berlin (Komander, 2006).

3 Heinrich Herkner (1863–1932) was an economist and founder member of the German Sociological Association.

4 Ladislaus von Bortkiewicz (1868–1931) taught statistics and political economy at the University of Berlin from 1920. Noted for his 'law of small numbers', according to which even events of very low frequency follow a Poisson distribution, and for important critiques of Marx's theories of profit and prices.

5 Hans Reichenbach (1891–1953) was an empiricist philosopher, member of the so-called Berlin Circle and co-founder (with Rudolf Carnap) of the journal *Erkenntnis*.

6 Werner Sombart (1863–1941) was an economist and sociologist who taught first at the University of Breslau (1890–1906) and later at the University of Berlin. His major books include *Der moderne Kapitalismus* (Sombart, 1987) and *The Jews and Modern Capitalism* (Sombart, 1913).

he moved to Frankfurt, wanting to study economics under Franz Oppenheimer.[7] But Oppenheimer had just retired, and this may have had an influence on his decision to join Karl Mannheim's sociology seminar, which also boasted the likes of economist Adolf Löwe (1893–1995) as well as Elias (1897–1990) and Gerth (1908–1978). Although this took place in the same building as the seminars of Adorno and Horkheimer, there was deep hostility between the two groups of students, which was maintained even for a few months after the Nazi seizure of power in January 1933, when everyone believed that the new regime would last less than a year. When the Department of Sociology was closed in Spring 1933, Mannheim and Löwe emigrated to England, Mannheim becoming Professor of Sociology at the LSE, Löwe joining the University of Manchester. Elias, having been due to submit his *Habilitation* – entitled 'Der höfische Mensch' – left for Paris and then in 1935 for London, where he plunged straight into the British Library and the research that would lead to *The Civilising Process* (Elias 2000).

It is significant for an understanding of his life and work that Baldamus did not belong to the cohort of German exiles that left Germany in 1933. He was not Jewish, though his wife was; he came from more humble origins than most of his fellow students and teachers, and when he was increasingly harassed by the Gestapo it was because of his connections with social democracy. Like Hans Gerth, he remained in Germany until 1937, experiencing the pressures and dangers that were typical of the time. Although his first published article appeared in the only economics journal – *Ständisches Leben* – that was not yet Nazified (Baldamus 1933), its editor, Othmar Spann,[8] was a Catholic who wanted Germany and Austria to unite in the style of a medieval Ständestaat, and as Baldamus recalled 50 years later, the Gothic script that had been reintroduced after the collapse of the Weimar Republic 'gives the paper a curiously sinister flavour'. *Ständisches Leben* was finally closed in 1936. A more spectacular example was the episode in which, if his own recollections are to be believed, he wrote a PhD for someone else, Ernst Faehndrich, SS-Standartenführer and president of the Nazi Studentenbund at Frankfurt from 1934 to 1937. Having been paid for his services, Baldamus then sought to blackmail the Rector of Frankfurt University, Walter Platzhoff (a liberal who had agreed to join the Nazi party and would remain in post until 1945), threatening that if anything happened to him he would reveal all in a letter to members of the university staff. Nothing seems to have come of this, and after twice being arrested by the Gestapo, Baldamus resolved to leave Germany.[9]

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7 Franz Oppenheimer (1864–1943) was a sociologist and political economist, who in 1919 in Frankfurt became the first chair of sociology in Germany. His most well-known work was *The State* (Oppenheimer, 1922).

8 Othmar Spann (1878–1950) was an economist and political scientist who taught at the University of Vienna from 1919 until his dismissal in 1938.

9 Gerth's doctoral thesis was accepted, in 1935, at what had become a Nazi university, and as a journalist and writer of sorts he found himself composing articles with titles like 'Are the Japanese a Race?' and publishing an obituary of Elisabeth Förster-Nietzsche,

In Exile: 1937–1951

In order to be able do this he had to obtain a visa to enter another country, which was duly obtained when Mannheim provided him with a formal invitation to give a talk at the LSE. Apart from this Baldamus had nothing in the way of connections in England, so that when, in September 1937, he arrived at Victoria station, he knew virtually nobody, his efforts to communicate in English with his fellow passengers on the train from Dover meeting with laughter on account of his reliance on Shakespearean phrases. When he did arrive he was given financial support by the Society for the Protection of Science and Learning, and advised to Anglicise his name.[10] He thus became William Bould, and spent the two years before World War II in Birmingham, at first living in the sort of dreary bed-sits for single men that feature in Orwell's *Road to Wigan Pier*. His wife Lydia and four year old daughter Eva had already been housed near Swindon and it was there in 1938 that he gave what he later called 'a disastrous lecture to a working-man's club or trade union section … about the rearmament in Germany. This must have been the main reason that I decided to discard an academic career for good'. The opportunity to be true to his word would come with the war, but before that happened he did meet Löwe again, by chance, and four decades later his recollection of the encounter gave a flavour of his attitude to life at the time: 'We discussed particularly the possibility of me joining a "Christian-protestant-socialist group", the leader of which was Paul Tillich. However, I politely refused to have anything to do with this movement.'[11]

Indeed, while Gerth and others who ended up in America were receiving a measure of support and encouragement, between September1937 and June 1940 Baldamus was cast adrift academically and politically, though not entirely against his will. He seems not to have been especially active in cultivating German or other non-English contacts, and would devote the evenings following his arrival in Birmingham to copying out English words and phrases in an exercise book. In later life he would repeatedly claim that his English was poor, but slogging away for hours on lonely evenings stood him in good stead; certainly he managed to be wittier in English than most of his English academic colleagues were. He did of course have some German-speaking contacts, becoming friendly with the exiled nuclear physicist Rudolf Peierls,[12] and in 1938 forming a refugee club with the

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something that did not go down well in émigré circles in the United States (Oakes and Vidich 1999: 5).

10 The Society for the Protection of Science and Learning had been founded in 1933 as the Academic Assistance Council by the Hungarian-born Leo Szilard, who had fled Berlin in January 1933, and William Beveridge of the LSE (Seabrook 2009).

11 Letter to Klemens Wittebur, 17 February 1982

12 Rudolf Peierls (1905–1995) was a German-born physicist who was exiled in Britain following Hitler's seizure of power in 1933. He worked at Birmingham University from 1937. In 1940 he co-authored the Frisch-Peierls memorandum, which established

help of Arthur Wensley, a German-speaking lecturer in economics who he had met by chance in a cinema queue; Baldamus had bumped into Wensley and, without thinking, said 'Verzeihung'. The idea of the club was to help refugees to assimilate more rapidly, through language teaching and so on. But it seemed that the weekly meetings were devoted mainly to singing Austrian folk songs; Baldamus and two others took these onto Birmingham's Corporation Street at lunchtimes and were able to collect money for the club.

One of the group was Gretel, and Baldamus's description of what Gretel did for him is typical of the way in which, in later life, he would recall events and people with a mixture of humour, analytic detachment and the foreigner's sideways glance.

> It was through Gretel that I got to know a number of upper middle class ladies in the Edgbaston district … Gretel had been trained in Vienna in a special skill which was at that time as yet unknown or non-existent in Britain: to design and to produce tailor-made bras for private individual clients. And she had quickly established herself in this district where there was an ample supply of suitable clients who had a need for such garments and could afford them.

June 1940 found Baldamus working without a work permit as a dishwasher in a hotel in Chichester; as fears mounted of a German invasion of the south coast, he was arrested along with all other Germans living in that region, regardless of whether they were Nazi or Jew or social democrat. As a talented middle-distance runner he did attempt to escape but, as he later recalled with typical pedantry, after 'three quarters of a mile' ran into the arms of a policeman. Eventually, after a spell at the notorious internment camp at Huyton near Liverpool, he was sent to the Isle of Man, and there found himself part of the sort of concentrated group of German academic and intellectual talent to which he had decided that he no longer belonged. There was Kurt Schwitters, John Heartfield, Andre Deutsch, Martin Esslin, Hans Keller, future members of the Amadeus Quartet … and Norbert Elias. Reunited with his fellow student, Baldamus seems to have committed a *faux pas* by asking Elias where Mannheim was, not knowing that Elias had 'broken' with Mannheim in the time since they were together in Mannheim's seminar. Elias responded as if to say: 'how should I know?' (Kilminster 2007: 167). Later Baldamus would write:

> We had our own 'university'; Elias gave lectures on the nature of sociology and psycho-analysis. I remember one seminar was called 'shame, disgust, anxiety'. I took no part in any of these activities. Almost from the start, in view of the

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that an atomic bomb could be built using just 1kg of Uranium-235. In 1943 he joined the Manhattan project, and after World War II worked at Birmingham again until 1963, when he moved to the University of Oxford.

> inefficiency of the guards, I had worked out an escape plan together with a young chap who was an experienced dinghy sailor.[13]

Despite the detailed preparations, the escape never materialised, and in May 1941, eleven months after his arrest, Baldamus was informed that he was to be released. He gave his last address not as Chichester but as Birmingham, because his wife was now there, working for the BBC translating German radio broadcasts. On his first morning in Birmingham he reported to the Cadbury's chocolate factory, where he was given the choice between two forms of 'forced labour': working in the pioneer corps[14] or in a munitions factory; he chose the latter, became a nightshift worker and also applied to become an apprentice tool maker; later he worked in a brass factory that had been converted to make parts for Sterling bombers. This work continued until the war's end, after which he worked for another two years as a production manager in a small glass factory.

It seems that Baldamus had more or less abandoned hope of or desire for an academic career when, in 1948, he met and received encouragement from the economist Philip Sargant Florence (1891–1982). An American by birth, Sargant Florence had been to school in Rugby and had studied at Gonville and Caius, Cambridge; he was as English as T.S. Eliot. Head of the Faculty of Commerce at Birmingham University, he had written his doctoral thesis in 1917 on the effects of fatigue on industrial productivity, and he would become a mentor to Baldamus in the early 1950s before Baldamus began to go his own way.[15] Baldamus was made Honorary Research Fellow, and the result of his collaboration with Sargant Florence was *Investment, location, and size of plant: a realistic inquiry into the structure of British and American industries,* by 'P. Sargant Florence assisted by

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13 Letter to Wittebur. Baldamus would finally meet Mannheim again in 1946 at the Athenaeum Club in London, but remembers him being 'a sick man, defeated and isolated'. Mannheim died the following year.

14 The pioneer corps was a wartime labour unit that had been set up at the start of World War II; they were formed initially from infantry reservists and performed a wide variety of tasks including warehousing, stretcher-bearing and bridge-construction, in many theatres of war. They also included several thousand German and Austrian Jews or simply opponents of the Nazi Regime who had fled to Britain when it was still possible. Had he joined, Baldamus would have risked torture and execution on being captured by the enemy, but might also have had the chance (or been required) to serve administratively in the British occupation army in Germany after the war.

15 Curiously enough, Baldamus was not the first German refugee to benefit from Sargant Florence's support. A young art historian called Nicolaus Pevsner had arrived in Britain in 1933, and with his family's fur-trading firm in ruins was able to secure a two-year fellowship (1934–5) from the Academic Assistance Council, which he held not only at Birmingham University but in its Faculty of Commerce, where Sargant Florence, a man of many and varied interests, persuaded him to write *An Inquiry into Industrial Art in England* (Pevsner 1937; Madge 1988). Three years later Pevsner would find himself interned in Huyton along with Baldamus.

W. Baldamus', published in 1948 (Sargant Florence 1948). As we will see, the word 'realistic' is significant here. It was now that he began to ease himself more into the mainstream of British society, taking the oath of allegiance that same year and obtaining British nationality. The 40-year-old Baldamus may not have been well-known within British academia, but he was already respected enough in German-speaking circles for the young Ralf Dahrendorf to have come to visit him in search of doctoral supervision. Years later Baldamus would recall this encounter along with his more general impressions of Dahrendorf (who always regarded Baldamus as his intellectual superior):

> I have listened last week to three BBC2 lectures by Dahrendorf … All the time while watching him, at the back of my mind, something about him was stereotypically 'German'. Although of course his German accent is almost imperceptible. Then I remembered 1948/49 when he was visiting me, from London, searching for a PhD supervisor. We had a long discussion and of course I could not have accepted because I was only a lecturer at that time … And suddenly it came to me; the outstandingly German attribute was his *unchanging seriousness*, a feature which in the TV came out more sharply, on account of the deep fold between his eyebrows, and perhaps also because he is now older. Of course, Germans do laugh just as explosively or as loud as other nationalities, but what they seem to be lacking, genetically as it were, is the capacity to smile, I mean the gentle, soft, superficial smile. And it must have been this difference more than anything else, which I had noticed, intuitively, already during the train journey from Dover to London in September 1937.[16]

The work with Sargant Florence was his first step as an industrial sociologist rather than an economist, and he published his first article in English in the *British Journal of Sociology* in 1951 (Baldamus 1951a), on the strength of which he gained a Rockefeller Foundation grant for a year at Cornell's School of Human and Industrial Relations, where he worked with Maurice Stein[17] and William Foote Whyte.[18] Thereafter he remained at Birmingham until his retirement, as Lecturer in Industrial Relations until 1960 and then as Lecturer and later Professor of Sociology (1970–76).

If 1951 marks the beginning of one phase of his life it also marks the end of another. As we said, Baldamus himself imagined writing a political autobiography covering the years 1930 to 1951, one that would have nothing to do with his academic life. Or as he put it in 1982, 'the sole purpose of these memoirs is that I am attacking, and in fact hoping to destroy, the foundations of the academic system. By this I mean the institution of higher learning throughout the world,

16 Private Diaries, 3 January 1983.

17 Maurice Stein (b.1926) was later Professor of Sociology at Brandeis University.

18 William Foote Whyte (1914–2000) was author of the ethnographic classic *Street Corner Society* (Whyte 1943).

including Japan, Russia, America, Germany and of course Britain etc.'[19] That he should have put it like this was doubtless the product of 30 years' experience of British and German academic life, in particular his experiences at the hands of university administrators and the Social Science Research Council, which had a habit of rejecting his research grant proposals. But the years 1930 to 1951 also frame a period that began with the death of his father-in-law and ended with the death of his wife, killed tragically in a traffic accident outside the gates of the University of Birmingham. He never remarried, and it is tempting to see his real life, the life of the supporter of the left of the SPD in Hitler's Germany, of the passions and hostilities of Mannheim and Adorno's seminars, of the marriage to a Russian Menshevik's daughter, of the adventures and scrapes of the newly arrived exile in Britain, as having come to some sort of end in 1951, or at least being separated by a clear break from the 40-year life of scholarship and inquiry that followed it.

In Birmingham: 1951–1976

On the other hand, the work that that second life produced is marked by an intellectual sensibility and choice of topic that owe much to the formative experience of the Weimar Republic. The two books that seem to be so different from one another – *Efficiency and Effort* of 1961, *The Structure of Sociological Inference* of 1976 – were written by the same man who, though he was always on the look out for something new and fond of using words like 'obsolete', especially about his own previous writings, held fast to certain intellectual and political principles. His training in economics and statistics led him to what might be called a moderate empiricism, while from philosophy and sociology he took an interest in the informal or unacknowledged methods that all thinkers have resort to in order to carry out the task of thinking, specifically theorising. Here it is noteworthy that he managed to combine this interest in informal methods of theorising with a hostility towards phenomenology, and towards nominalism generally. This meant that he was able to call himself a realist, but that he did so without any of the dogmatism that marks either the neo-Durkheimian or neo-Marxist variants of it that can still be encountered today. His was the realism that appears in the subtitle of the book by Sargant Florence, a tentative realism arrived at from experience. Much later he would look back on this:

> I violently disagree with the … Weber-Rickert-Windelband neo-Kantian position. I know this position very well too because I grew up in it (during my first year at Berlin where Sombart was my teacher). Today I no longer believe that the real world is infinitely complex etc. etc. … Why have I come to disagree with this position? I don't really know. It happened gradually in the course of

19 Letter to Robin Cohen, 5 May 1982.

> my empirical work. I found, occasionally, the facts (the real world) do speak for themselves![20]

In fact, this attitude to reality is evident in his first English-language article, 'Type of Work and Motivation' (1951a). 'Motivation' sounds like a subjective concept, and one might imagine a sociological study of it involving a series of more or less elaborate surveys of workers' attitudes. But what Baldamus sought to show was that there was a correlation between motivation and type of work, so much so that much could be explained about motivation – in particular absenteeism and turnover – by looking at types of work themselves. The last sentence of that article reads: 'Motivational processes are largely a function of strain, which, in turn, is directly related to objective characteristics of the work situation. Different situations, as our analysis has shown, produce different processes of motivation which thus become accessible both to empirical research and to practical policy.' (Baldamus 1951a: 58).

The publication of *Efficiency and Effort* in 1961 (published in German in 1960 as *Der gerechte Lohn* [The Fair Wage]) coincided with Baldamus's formal shift away from commerce and economics and into sociology. This was, perhaps, inevitable as Baldamus's view of economics was that it was underpinned by sociological concerns and themes, a view not shared by many in that discipline. *Efficiency and Effort* is groundbreaking work because up to this point the primary concern for academics investigating the operation of work organisations was with efficiency, and how it could be measured and promoted. In contrast, Baldamus starts from a different, social point of view, examining what constitutes the effort bargain, the arrangement between an employer and a worker whereby the latter will sell their labour power in return for money. Baldamus's focus on the asymmetric nature of this relationship is significant as the contrasting needs and desires of employers and workers became the abiding concern from the 1960s to the present day in British sociology of work.

But where subsequent sociologists of work, notably labour process theorists of the 1970s and onwards, drew on Marx, Baldamus highlights the disparities and inequalities of the effort bargain from a Weberian perspective that takes seriously both macro and micro aspects of work and employment. Adapting Weber's ideas on bureaucracy and organisational analysis, he focuses on the way that modern industry is organised through an administrative process that regulates the employee's actions and efforts to achieve employers' goals. But he also examines the world of everyday individual social action in workplaces, firstly to produce a typology of deprivations and satisfactions experienced in work and, secondly to connect these to the operation of industrial administration, so that a complete picture of the relationship between efficiency and effort is constructed. En route he trenchantly criticises the loose or woolly use of core concepts: he is particularly scathing about economists who apply the term 'efficiency' without having first

20 Letter to Ian Procter, 6 February 1972.

defined it. After all, the efficiency sought by the employer may not be the same as the efficiency sought by the employee; and if both has his or her own, rational definition of what efficiency means, adopting one side's definition at the expense of the other's will undermine the objectivity of the inquiry.

As Erickson's chapter in this volume shows, the significance of *Efficiency and Effort* was noted by a number of commentators, and it was this sort of empirical work that established Baldamus's place in British sociology of work through the 1960s and into the 1970s. Following *Efficiency and Effort* his main empirical work in the 1960s was a study of the relationship between absenteeism and accident rates in UK industry, a topic that was a matter of some concern at the Home Office. Baldamus's contribution was to show, analysing statistical returns from a number of regional case study sites, that in workplaces there are different types of accident and that the rate of occurrence of one type – 'absenteeism accidents', or minor accidents – shows a clear decrease as the working week progresses, while severe accidents show no such pattern. Baldamus's method of what might be called 'discovery' here was simple and elegant: he introduced the criterion of severity of accident into the statistical breakdown of the data. But such simplicity produced two quite dramatic conclusions: firstly, there is now a clear connection – one that can be examined using sociological methods – between social factors and accident rates in workplaces; secondly, and more profoundly, one can ask: what constitutes an 'accident'? As Baldamus puts it: 'If some accidents are "truly accidental", there must be others which are not quite so accidental; which are they? … Sooner or later the question of *not* truly accidental accidents will have to be answered' (Baldamus 1969c).

This concern with empirical research and practical policy remained at the heart of his work until at least his inaugural lecture of 1970, the last sentence of which repeats the message at the end of his first English article. Superficially it seems to have been an attitude shared by many of his colleagues at Birmingham; the difference was that while theirs was inflected with a distinctly English and markedly non-philosophical reformism, his was grounded both in the rigours of continental European philosophy and sociology and in the experience of German Marxism. In university life differences between oneself and one's immediate colleagues can sometimes go hand-in-hand with a closer relationship with those in other departments; with Baldamus this seems not to have been the case, and the inaugural lecture was something of a failure within the faculty. Few in the audience understood what he was talking about.

Almost immediately after he had taken over as head of department, the distance between Baldamus and many of the rest of the academic community at Birmingham came to head in a major crisis that came to be known as 'the Atkinson affair'. Dick Atkinson had been a temporary assistant lecturer in the department in 1968–9 and was, in 1970, selected to take up a permanent post as lecturer in sociology. He was, however, already known for his participation in and support for a number of student protests around the country in the late 1960s, and specifically for his involvement with a student sit-in at the University of Birmingham in 1968. The

appointment committee had been deeply divided (and not entirely along left-right lines) but the majority, and notably most of the sociologists on the panel, including Baldamus himself, had supported him. However, his appointment was blocked by the senior management of the university, on the grounds that Atkinson would be a disruptive influence on the student body (Platt 2003: 113). The affair quickly escalated, receiving national media attention and leading the BSA to blacklist the department, effectively barring BSA members from taking on permanent teaching posts there, a situation that persisted until 1973 when Atkinson, who was being employed in a private capacity by members of the University of Birmingham through a support fund, finally left to become a full time community activist in Balsall Heath, Birmingham (Platt 2003). Emotions surrounding the Atkinson affair still run high, an indication of how fraught and fractious events were at Birmingham in the 1970s.

If running the Birmingham department was made difficult by the Atkinson affair, it was perhaps not made any easier by Baldamus himself. Like many of his generation and background, he could be extraordinarily generous with his time, exchanging long and detailed letters on intellectual matters with those junior colleagues or graduate students who sought advice from him, and reading and commenting on their stumbling first drafts; at the same time, if he decided that he disapproved of someone he would make it clear. In addition, he became head of department at a time when he was intensely preoccupied with problems of sociological theory. His classic essay 'The Role of Discoveries in Social Science' had been written in 1966, though it would not appear until 1972 when it prompted an unctuous piece of fan mail from Robert Merton, and 'The Category of Pragmatic Knowledge in Sociological Analysis' had appeared in 1967. It was during this period that he wrote the series of working papers that, as John Eldridge recalls, would keep appearing on the desks of colleagues, and which were the basis for *The Structure of Sociological Inference*. As the chapters by Lassman and Turner make clear, this book, which is both a technical examination of recent developments in theoretical and empirical research methods and a study of long-term sociological trends, was also rooted in Baldamus's earliest experiences as a student. Like the sociological classics at some of whose feet he studied, it raises some basic and still unanswered questions about what sociology is and should be.

In Retirement: 1976–1991

The Structure of Sociological Inference appeared in 1976, and thus coincided with what the biographical note for this volume calls his retirement from the University of Birmingham. As John Rex points out in his anecdote about visiting Baldamus soon afterwards in his houseboat on the River Severn, retirement was not, initially, an especially happy experience. Before too long, however, he had moved to Leeds, where he lived until his death in 1991. Once there, he began to do what many German professors have done in retirement, carry on as before, reading voraciously

or dictating letters and drafts in the mornings, pottering in the afternoons. He also turned increasingly to the dictation of his memoirs, and to somewhat Pooterish diaries recording the day-to-day life of a retired professor in Leeds; these would include everything from his thoughts on a new book to the price of posting a parcel, from a list of phone calls made and received to an extended reflection on the embarrassment potential of being treated by a female physiotherapist in his curtainless front room. Curtainless his suburban bungalow may have been; it nevertheless concealed from most of the outside world an unconventional lifestyle, organised, or rather disorganised, around an arrangement of makeshift bookcases, camping equipment and furniture that he had modified to accommodate both his arthritis and his chain-smoking; he built extensions to the arms of his chair for his lamp, book rest, coffee cup and of course his ash tray. He rarely went out and, other than his immediate family, his only visitors were old colleagues.

Baldamus once said that he had changed direction four times in his career, and it was during this final phase of his life that he turned increasingly to the history, philosophy and sociology of science. The two pieces included in this volume come from this phase; 'The Exoteric Paradox' was first published in German in 1979 (Baldamus, 1979a), and 'Networks' was written in 1982. Both are marked by a characteristic combination of attention to detail, mordant wit, and remarkable prescience.

Baldamus had alerted the Anglophone academic world to Fleck's *Genesis and Development of a Scientific Fact* (Fleck [1935] 1979) as early as 1966, four years after the publication of Kuhn's *Structure of Scientific Revolutions* (1962).

> It was first only idle curiosity that prompted me (in 1963) to search for a copy of Fleck's unknown monograph. My immediate reaction was that his analysis was thoroughly sociological, indeed much more distinctively so than Kuhn's work. (Baldamus 1977a: 138)

Fleck's 'unknown monograph' was, as Baldamus pointed out in his 1977 contribution to the Norbert Elias Festschrift, quite readily available in American libraries, and the publishers were still sending out review copies as late as 1959 to academics – ıncluding Michael Polanyi – who Fleck thought would be interested in his work (Baldamus 1977a: 139).

From 1963 Baldamus had begun to collect as many of Fleck's scientific papers as he could, and by the 1970s he was adopting some of Fleck's ideas for his own work. He compiled a rudimentary biography of Fleck for *The Structure of Sociological Inference*, and extended this in other papers written at this time, many of which present extensive translations from Fleck's work. Baldamus clearly thought he was onto something, and it was typical of him that he should pursue such an apparently obscure subject with such tenacity. It was in this period that he began to collaborate with Thomas Schnelle, who would go on to edit the most important collection of Fleck's other writings available in English (Cohen and Schnelle 1986).

While he was sometimes tempted to accuse Kuhn of having borrowed more from Fleck than he admitted, Baldamus was always keen to emphasise a different point, namely that Fleck's was a *sociological* not a historical or philosophical account of science, and all the better for that. Ironically, given the impact that Kuhn's work had on sociology of science, it is this aspect of Fleck's 1935 work that caused it to be ignored for so long: at the time of its publication it simply had no points of contact with the sociology of science, and it avoided any confrontation with the then dominant philosophy of logical positivism (Baldamus 1977a).

With each study in which he deploys Fleck's work Baldamus's opinion of Kuhn seems to drop lower and lower. Fleck's sociological account, which Baldamus describes as both remarkable (as it was produced in difficult circumstances by a practicing scientist) and sophisticated, examines the relationship between the esoteric thought community formed by a small group of scientists working on a single problem, and the exoteric thought communities (such as families, neighbourhoods, political parties) of which they are all members. In 'The exoteric paradox' he puts this insight to telling use, turning Fleck's distinction between esoteric and exoteric membership circles into one between esoteric and exoteric vocabularies, and then applying this distinction to the work of Kuhn himself. The exoteric paradox is this: the more the history of science seeks to make use of exoteric terms, that is, those having general comprehensibility rather than being restricted to an esoteric circle of specialists, the more it finds itself having to define those terms; and the more it tries to define them, the more it becomes involved in a struggle against ambiguity that is doomed to failure: witness the history of the term 'paradigm'. Baldamus's exploration of this theme in the work of Kuhn is not without its shortcomings; it is never entirely clear, for instance, why he calls some terms esoteric and others exoteric! Nevertheless, the essay can be read not only as a technical exercise for those interested in the relationship between the history, philosophy and sociology of science, or as an important commentary on Kuhn, but also as one that raises questions about more topical matters, such as the public understanding of science and the genre that has come to be known as 'popular science', which is forced by the nature of its audience to use terms that are primarily exoteric while attempting to convey something informative about science.

The second piece, 'Networks', demonstrates Baldamus's nose not only for trends that are long-term but for those that are about to begin. The essay is directed at the network analysis that developed in the United States among mathematical sociologists such as Harrison White, but its account of the way in which metaphors, or analogies, or even words, can become objects of theoretical commitment, often to the detriment of the theory, applies to a broad range of sociological theory in the 20th century, before and after 1982. Today, the most well-known invocation of the imagery of networks is found in the work of proponents of Actor Network Theory. ANT, especially in its Latourian form (Latour 1987; 1996), is certainly more user-friendly than the mathematical and technical rigour of Harrison White; on the other hand, precisely because of this, not only may it share some of the

shortcomings of the earlier network analysis: it may also be subject to the exoteric paradox.

In a Lost World

There is a long and venerable tradition in European universities according to which, at the start of their studies, students receive a small book, a little like a pension or ration book. As his or studies progress, the student fills in the names of courses taken and at the end of each semester goes to the professor and receives a mark along with the professor's signature. In 1929, at the end of the summer semester at the University of Berlin, Baldamus knocked on the door of one Professor's office in search of the all-important signature. He entered a long and gloomy room, far larger than the average university teacher's office, and lined from floor to ceiling on both sides with books; at the far end near the window was a desk, and seated at it was a bearded professor, writing. The 20-year-old student approached the desk; without looking up, the professor extended his hand, took the little book, signed it, handed it back, and continued writing. Baldamus turned and walked out, the two of them having exchanged neither a word nor a glance. The professor was Werner Sombart.

However much we read the German sociological classics and try to connect ourselves with the tradition they represent, the academic and political world from which Baldamus came – the world of Sombart and Mannheim and Löwe, of Adorno and Horkheimer – was not ours, nor was it that of his Birmingham colleagues in the 1950s, especially not theirs. Nor was it the world of the German universities after World War II, which he thought irreparably damaged by the Third Reich. Baldamus once remarked that he had assimilated into English life even less than Norbert Elias had, and that therefore the problems and questions that were important to him must have seemed somewhat boring to his English colleagues. With some exceptions, doubtless they did. Three or four decades on, many who work in today's universities may be hardly aware that those problems exist. But that is their loss.

Chapter 1
Wilhelm 'Gi' Baldamus: The Man, his Principles and his Academic Work

John Rex

The following is a deeply personal record of my relations with Gi Baldamus, and unlike other contributions to this volume it focuses on him as a colleague. Although I was only in the same department as Baldamus for a few years, whilst at Birmingham University in the 1960s, we remained in touch until his death in 1991.

I first came into contact with Baldamus when I was appointed to a lectureship in the Department of Sociology at the University of Birmingham in 1962. The head of department at that time was Charles Madge who, whilst lacking a degree in sociology, was a co-founder of Mass Observation, the movement that recorded on a day-to-day basis the everyday lives of people (Jennings and Madge 1937; Madge et al. 1937), and a noted poet. Madge held the chair of sociology until his retirement in 1970, when he was succeeded by Baldamus. Another colleague in the 1960s was Norman Dennis, who was devoted to survey research, and this orientation set the tenor of the department. In these circumstances Baldamus and I saw ourselves as theorists (on my appointment he wrote to me to this effect and commented on my recently published *Key Problems in Sociological Theory* (Rex 1961)) although at the time of my arrival I was unaware of his interest in theory as at this point all his publications were in the area of industrial sociology, notably *Efficiency and Effort* (Baldamus 1961a).

The study of industrial sociology in Birmingham had been pioneered by Philip Sargant Florence (1890–1982), introducing sociology to a social science faculty dominated by economists. Sargant Florence employed Baldamus as a research assistant in the late 1940s and Baldamus co-authored the book that reported on their field work research in 1948 (Florence and Baldamus 1948). By the time Sargant Florence's updated version of his 1953 book was published in 1961 (Florence 1953, 1961) Baldamus had moved in a significant new direction. Rather than concentrating on the study of efficiency, as industrial sociology had to this point, he argued that the separate question of labour's effort was also worth pursuing, indeed was more significant due to the intangible nature of 'efficiency'. This, along with his criticism of Sargant Florence's selective use of evidence, led to a parting of their ways.

Baldamus was deeply schooled in German sociological theory in the tradition of the Frankfurt School. I did not at any time have access to his doctoral thesis and he told me that he had received his Habilitation in an unusual way: as part of post-war reparations those who had been unable to complete their studies due to Nazi rule were given Habilitation where appropriate. Baldamus was familiar with Kantian and Hegelian traditions in the philosophy of social science, which led him to the work of Lakatos, Kuhn and Feyerabend. It was Baldamus who first explored the foreshadowing of Kuhn's work by the Polish scientist Ludwik Fleck, and from the late 1960s onwards Baldamus became increasingly interested in sociology and philosophy of science.

Baldamus was regarded by some colleagues as wilful and egocentric, partly due to his obstinacy concerning syllabus. He was not inclined to negotiate in discussions on this topic and disliked mushy compromises. He insisted on the syllabus including the main points of his understanding of sociological theory and industrial sociology. In doing so he ensured that students' papers reflected an understanding of his ideas and teaching. As a member of examining boards deciding on degree classifications he showed himself as committed to his own ideas and voted for students who adopted them. This brought him enemies amongst his colleagues.

I had never sought promotion to a senior lectureship whilst at Birmingham believing that it would be wrong of me to do so until such a promotion was offered to Baldamus whom I regarded as my intellectual superior. I left Birmingham in 1964 to take the newly created chair of social theory and institutions at Durham University. While there I learnt that Baldamus had succeeded Madge as chair of sociology. As chair, Baldamus was able to have far more control over shaping the syllabus, but he also had to take difficult decisions, most notably that relating to his offer of an appointment to Dick Atkinson.

Atkinson had been one of my students at Leeds where he completed his first degree before moving on to the London School of Economics. As he was a political activist, involved in the student revolt of 1968, the University of Birmingham attempted to block Atkinson's appointment, and there was opposition in the department too. Baldamus offered him the appointment despite this. As the battle continued between the department and the university, Atkinson left to set up a school for children who had been excluded from mainstream schools, working with Anita Halliday in Balsall Heath. Atkinson remains a community activist in Balsall Heath to this day, and has written a number of publications on community regeneration, most notably for the think tank Demos (Atkinson 1994).

What was clear in all of this was that Baldamus was capable of taking principled stands in what he thought to be right even when they were politically unpopular. The Atkinson affair had significant consequences for sociology throughout the UK, particularly inside formal institutions that organised sociological research such as the SSRC and the BSA. However, Baldamus had no interest in the BSA and had an antipathetic relationship with the Social Science Research Council (SSRC).

From time to time Baldamus submitted applications for grants to the SSRC. These were either rejected or returned to him with suggestions for resubmission. He was never able to accept these suggestions which he saw as being entirely at odds with his ideas as to what sociological theory and empirical research should be. In these matters he had his own standards and he saw himself as offering a rival definition of social science research to that which was regarded by nearly all grant awarding bodies as standard.

If Baldamus is to be considered as a theorist then *The Structure of Sociological Inference* (1976) can be taken as a statement of his position. However, it is a strange book in many ways. What it does is to draw the attention of the sociologist to how we can see sociological theories themselves as data in their own right. To do this we must record the history of sociological theories, or of sociological *inferences*. Thus the title of the book immediately alerts one to the fact that inference is something which has a structure which deserves to be an object of study.

When I joined Baldamus at Birmingham he had been a widower for a number of years and his daughter no longer lived with him. To me he seemed lonely and all his attention was directed towards academic matters, teaching and writing. When he eventually retired he moved to a narrow boat on the River Severn a good distance from Birmingham University. When I visited him together with a colleague's wife I noticed that he had not taken any of his sociology books with him; the only items on his bookshelf were some industrial psychology journals. This was not a particularly successful visit; Baldamus resented our intrusion into his now private world and he told us so.

Clearly he was a unique individual. He did not go along with the changing trends of sociological thought and practice and he welded his substantial background in traditional German sociology into the most serious aspects of sociological thought in Birmingham. Finally, he was someone who could be said to have the courage of his own convictions and who stood by them in his dealings with friends and colleagues.

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Chapter 2
Baldamus's Adventures with Cross-classification

John Eldridge[1]

Introduction: Serious Playfulness

I will begin with three quotations:

> Graham's memory stick contained a lot of the humdrum of commerce – *feasibility studies, projected figures, tight margins.* The world seemed full of so many vague concepts but you had to wonder – were these actually important? (Were they even real?) (Atkinson 2006)

> When in the course of our work we are uncertain, we sometimes become more concerned with our methods than with the content of our problems. We then try to clarify our conceptions and tighten our procedures. (Mills 1963: 533)

> [T]he intrinsic vagueness of concepts (and connections between concepts) is the most characteristic attribute of sociological inferences, inasmuch as they have something to do with 'society'. If the importance of conceptual vagueness is borne in mind, the practical usefulness of cross-classification will be less surprising. It is then simply a technique of reducing the uncertainty or unclarity of sociological inferences in *relative* terms. As the use of a single concept is relatively vaguer than that of a dichotomized concept, the combination of *two* dichotomies will be proportionally even less vague. In other words, cross-classification is a method of *definition.* Its aim is description not explanation. (Baldamus 1976a: 98–9)

Gi Baldamus's approach to sociology was one of serious playfulness. Schooled as he was in the German tradition of sociology, economics and philosophy and committed to the practice of empirical work he wrestled with epistemological questions unremittingly. Can we advance in our knowledge of the social world

1 My thanks to Professor Bridget Fowler (University of Glasgow) and to Professor Lee Parker (University of South Australia) for their very helpful comments on an earlier draft of this chapter. My thanks also to the editors of this book for their invitation to write this piece and their continuing encouragement.

and, if so, how? What is the method of theory? Can the dichotomy between theory and method be dissolved? In engaging with such questions he was both inventive and imaginative. Those of us of a certain age will remember the pamphlets with blue covers from Birmingham University's Faculty of Commerce and Social Science. He was a noted contributor to that series and I suspect it may have been his favourite form of academic communication for it was there that he disclosed his latest thoughts. Indeed, his Inaugural Lecture at Birmingham, in November 1971, was published in that format (Baldamus 1971c). The pamphlets arrived on our desks, without fuss or publicity. They were always a challenge to one's thinking and sometimes a provocation.

My purpose in this chapter is to explore why he became fascinated with the phenomenon of cross-classification as an aid to sociological analysis. Perhaps I should say that my own interest in cross-classification was first stimulated by reading C. Wright Mills's *The Sociological Imagination* (Wright Mills 1959), especially what he had to say about it in the appendix on Intellectual Craftsmanship. There Mills had argued that 'for a working sociologist, cross-classification is what diagramming a sentence is for a diligent grammarian. In many ways cross-classification is the very grammar of the sociological imagination. Like all grammar, it must be controlled and not allowed to run away from its purpose' (Wright Mills 1959: 213). Mills saw this as a way of dealing with quantitative and qualitative materials. It was a tool which enabled the sociologist to clarify and critique one's own and other people's work and possibly to make innovating moves as a result. This does indeed tell us something about the method of theory, not only in terms of how specific categories like age and gender may be cross-classified to yield statistical results from empirical surveys, but also how new concepts may be developed, which inform and refine existing theories or even re-orient them:

> Charts, tables, and diagrams of a qualitative sort are not the only ways to display work already done; they are often genuine tools of production. They clarify the 'dimensions' of the types, which they also help you to imagine and build. …When they work they help you to think more clearly and to write more explicitly. They enable you to discover the range and full relationships of the very terms with which you are thinking and of the facts with which you are dealing. (Mills, 1959: 213)

Baldamus was well aware of Mills's work and refers to his 'undeservedly neglected quasi-autobiographical Appendix'. He thought that Mills's treatment was remarkably insightful. The fact that Mills referred to his own extensive use of cross-classification, when sometimes it worked and sometimes it did not, was, for Baldamus, an important illustration of the trial-and-error nature of theorising. Baldamus, however, thought that Mills somewhat obscured the significance of what he was pointing to because the actual examples he gave were quantitative – the cross-tabulation of statistical data – rather than theoretical. This is true for the most part, although I can draw attention to an example Mills used as long ago as 1948 (Mills, 1949).

Table 2.1 Cross-tabulation in Mills (1949)

		Objective structure of power	
		Participates	**Does not participate**
Subjective condition of individual	Cheerful and willing	1. Unalienated worker	2. Manipulated pseudo-morale
	Not cheerful and unwilling	3. Malcontent	4. Alienated worker

What Mills was attempting to do was to throw fresh light on the interpretation of studies of job satisfaction, of which then, as now, there were plenty. He wanted to clarify the relationship between subjective feelings of work satisfaction and dissatisfaction and cross-classify it against the structure of power within which work is actually carried out. By bringing into play the possibility of participating in the exercise of power Mills essentially invents two new categories. This not only challenged some then conventional thinking in industrial sociology but also showed the ways in which empirical findings could be shaped and re-shaped by the invention of new concepts. Of course, it is only a beginning so far as the analysis of power relationships is concerned but it does illustrate Baldamus's point about newly emerging concepts which, he argued, are not 'solely an ad hoc creation of the sociologist's imagination, but also relate to properties of the real world in the sense of *not-previously-known* entities.' For him, this underlines a realist view of knowledge and the possibility of the growth of knowledge about the world through sociological work.

Baldamus made a similar point about Mills in *The Structure of Sociological Inference* (1976a), arguing that what Mills said about cross-classification is more relevant to theoretical concerns than it may at first seem. He then adds: 'If, furthermore, we translate his introspective experience of imagination, skill, inventiveness, craftsmanship etc., into objective patterns of observable pragmatic behaviour, the category 'qualitative' takes on a more specific and therefore stronger meaning: it describes the *genesis* of qualitatively*, new* concepts, ideas, frameworks, models' (Baldamus 1976a: 101). The use of the term 'objective' here should be noted and does reflect Baldamus's own realist ontology.

This movement towards a realist ontology was strongly influenced by the work of Norbert Elias on the sociology of knowledge. He was quite explicit about it (Baldamus 1976a: 49; Baldamus, 1971b: 77). Apart from the published work, to which Baldamus refers, Elias also did some teaching at Birmingham during his time. Elias stressed the importance of understanding how knowledge could both progress and regress and that the process needed to be understood in terms of its long-term development: 'Not only knowledge of objects, but also knowledge of how to gain and how to advance knowledge of objects, how to catch them in one's

net, and how to make better nets, for catching them, develops over the generations' (Elias 1971: 166).

Baldamus shares this robust, even optimistic, view of the development of knowledge, including the sociology knowledge, which of course was his main preoccupation. In his paper 'On Testing Hypotheses' (1969b) he makes what might be described as a bed-rock statement in this regard: 'It is possible, after all, that even in the realm of methods, facts may be sufficiently stubborn to destroy an obsolete theory of knowledge' (Baldamus 1969b: 38). Yet it was a hard-won position, as he admits. We can see something of his journey if we examine his paper on 'Types of Trivialisation' (1971a).

Trivial Pursuits

Trivialisation sounds like a nasty sociological disease. In fact, because of the connotations of the word Baldamus sometimes uses 'complexification' or 'attenuation' as synonyms for it. Most generally, trivialisation represents a sociology that has no relevance to social and political problems. What starts off as a piece of work dealing with a major issue ends up looking rather thin when compared to its stated intentions or aspirations. But he also, almost comically, suggests that studies which are quite trivial to begin with can end up being even more trivial than when they began. What is it, he wondered, that caused this process? Following on from a comment of Tom Bottomore's, Baldamus suggests that there are two forms of trivialisation: empirical and theoretical. Whereas Bottomore had expressed some confidence that in the future sociology would become less trivial, Baldamus was less confident and, indeed, thought that the question of what really was non-trivial sociology and how it could be accomplished had not properly been explored. Bottomore's own statement was fairly programmatic: 'we can expect to see, in the next few years, a concentration of attention upon issues of major social and political importance, more vigorous theoretical and ideological controversies, and a still larger incursion of sociologists into the public debate on long-term social aims and policies' (Bottomore, 1970: xi–xiii).

It is worth noting that the immediate stimulus for Baldamus's paper came from the collection of essays *Race and Racism*, which was the product of a B.S.A. conference. This included an essay by John Rex. Whereas the notion of empirical trivialisation was more familiar to grasp, with its critique of social survey work, the matter of theoretical trivialisation was disregarded. But not by Rex who, in typically robust fashion, rejected the practice of merely testing particular hypotheses as part of a programme of piecemeal social engineering (*pace* Karl Popper) and argued that:

> This response has necessarily led to trivialisation of sociological concepts in the race-relations field. But sometimes trivialisation seems to have been chosen for its own sake. Thus although I believe that there is a great deal of scope

> within an overall framework such as I have outlined for micro-sociological studies, too often the cart has been put before the horse and potentially useful concepts referring to immigrant-host relations, to the stranger and colour-class hypotheses, to role theory and to status-crystallisation, have been used as though they by themselves provide a sufficient theoretical foundation for the study of race relations. I find, it difficult, myself, to regard work such as this as professionally serious. (Rex 1970: 54)

Baldamus said that this was the only explicit and general exposition of the theoretical aspects of trivialisation , with its concerns for theory and concept formation, that he had come across. Further, he commended the paper as a notable attempt to integrate theory and research in the field of race relations.

Under the heading of trivialisation, Baldamus is trying to disentangle a number of considerations. In fact, as he sees it, the empirical and the theoretical can affect one another, resulting in various processes of trivialisation. First, there is the realisation that empirical research, especially survey work, almost irresistibly leads to trivialisation through the use of independent and then intervening variables. Again, he illustrates this from the edited collection, *Race and Racialism*, this time with reference to a research study by Sheila Allen, 'Immigrants or Workers?' (Allen 1970). He commends the study for its serious attempt to connect a study of immigrant workers to macro-social theory and to sustain a focus on macro-structures. Yet despite that, as it were, the refinement of the empirical variable from union membership, to industry, to type of skill, leads Baldamus to comment:

> [I]f we look at the conclusions of the paper … we find a highly complex enumeration of numerous minute causes and conditions to account for the failure of trade unions to involve coloured workers in union activity. The contrast between the diversification of the complex empirical findings and the uniform largeness of the initial theoretical goal of the study could hardly be greater. In view of the extreme degree of relative trivialisation in this case I want to make quite clear that my intention is not to criticise. On the contrary, I am trying to get hold of such elements of progressive complexification which irrepressibly assert themselves independently of the theoretical perceptiveness, the ingenuity and methodological competence of the researcher.' (Baldamus 1971a: 12)

Secondly, there is a form of trivialisation that arises when what starts off as sociological theory crosses into another discipline. This can prove very problematical if, as Baldamus believed, the central theories of different disciplines are incommensurable. He himself had considerable contact with economists and had worked with them on research projects. An example of what he had in mind was the first volume of the *Affluent Worker* study (Goldthorpe et al. 1968). He pointed out that the central sociological problem was concerned with the theory of social stratification and the changing class structure in modern society. However,

he claimed, the focus of the study shifted. The Parsonian concept of instrumental orientation became the main independent variable:

> This is broken up in the course of various empirical refinements into specific attitudes of industrial workers towards employment and wages. Unnoticed by the authors, the whole approach transforms itself gradually into a fully fledged *economic* theory of wage determination, curiously akin to Alfred Marshall's theory of balanced 'net advantages' of pecuniary and non-pecuniary satisfactions or dissatisfactions. (Baldamus 1971a: 20)

Thirdly, theoretical trivialisation can occur even within a sociological framework. This can have several forms. There is the shift from a macro to micro sociological focus. This Baldamus termed 'scope transmutation' and gave examples of macro sociological concepts such as alienation, anomie and class being translated into operational definitions with empirical referents. One of the things that can happen here is that the central macro concepts are attenuated into concrete but undefined criteria taken from everyday language. This, Baldamus thought, can have insidious effects since we do not know how to return from the micro to the macro: 'As a result the growth of social knowledge contains an ever-expanding proportion of micro-sociological phenomena endowed with an inexplicable tenacity and recalcitrance' (Baldamus 1971a: 24). There is another form of theoretical trivialisation which, with some misgivings, Baldamus terms 'subjectivistic'. What he has in mind are that concepts which give the appearance of being objective, such as power structure or social class, are transmitted into subjective entities such as perceived injustices or self-rated positions and reference group classifications. Or again, structural conflict is transmuted into emotional dissatisfactions, frustrations, grievances, and deprivations. Moreover "on an empirical level, the transmutation from solid objective entities to subjectively diluted epiphenomena covers everything that goes under the name of 'intervening variables'" (1971a: 24). Finally, Baldamus refers to a form of theoretical trivialisation whereby what is intrinsically a dynamic problem, such as how to explain differences in power, wealth, income and status, is transmuted into a static theoretical framework. Parsonian functionalism is what he had in mind at the time. With this in mind it is no surprise to find in another paper, 'Notes on Stratification Theory', he comments: 'Underlying the whole development of modern functional theory has been a continuous process of methodological purification: step by step the structural dynamic properties of society have been eradicated, eliminated or devalued in favour of functional-static aspects. The most dramatic manifestation of this development is the repeated re-definition and re-interpretation of the concept of stratification' (Baldamus 1967b: 17)

These forms of attenuation – empirical and theoretical – could feed upon each other, as it were. For Baldamus, this represented a challenge for him to think afresh about the problems associated with linking theory and method in sociology. It is only at the end of the paper that Baldamus himself uses the technique of

cross-classification. In what he admits is a slightly compressed way (to avoid a three-dimensional figure, he tells us) he distinguished dynamic-objective theory from static-subjective theory and then cross-classified it against macro-theory and micro-theory. It looks like this:

Table 2.2 Cross-classification of types of theory (Baldamus 1971a: 27)[2]

	Macroconcepts	**Microconcepts**
Dynamic – Objective	A Social Stratification	a Interaction Processes
Static – Subjective	B Social Order	b Interpersonal Relations

He then takes as an example the *Affluent Worker* study previously referred to (Goldthorpe at al. 1969) and suggests that it is a good example of cumulative attenuation since it involves the transition from macro to micro, from objective to subjective and from dynamic to static. As a result, in his view, the general trajectory of the study is from A to a, to b and then to B. All of this, we may note, in addition to his earlier claim that the study also represented a form of extra-disciplinary attenuation, from sociology to economics. I think it is worth quoting his conclusion as to why sociological inquiry, even of the rigorous type that the *Affluent Worker* study represents, is subject to attenuation:

> Our concepts and methods are particularly suitable to take up, as points of departure, the sort of large and important issues which are commonly associated with non-trivial 'socio-political' problems. They meet us in the first place with the richness and simplicity or real-life situations, aligned to pragmatic knowledge and common sense terms. Trivialisation is thus to some extent the unintended product of disciplined sociological enquiry. (Baldamus, 1971a: 33)[3]

2 Editors' note: Baldamus rarely appended titles to tables, diagrams and figures he included in his working papers series. The titles included in this chapter have been added by the editors at the request of the publisher.

3 One sociologist who consciously sought to integrate macroscopic and microscopic work as a way of looking at the relationship between theory and method was C. Wright Mills. This is done in a way that paralleled some of Baldamus's concerns. In *Two Styles of Social Research* he pointed out, for example, that as matters now stand 'the propositional meaning of many macroscopic statements is ambiguous and unclear; the conceptual meaning of many molecular statements is often barren' (Mills 1963: 566). His emphasis on the idea that the intellectual craftsman needs to shuttle between the macroscopic and molecular levels both in formulating the problem and in explaining it has much in common with Baldamus's argument. It challenges the conventional hypothetical-deductive logic that

And there the paper ends with the problem of how real sociological knowledge can be obtained unsolved. Fortunately, it was not the last word.

Little Boxes

Actually, when we turn to the paper on cross-classification (Baldamus 1971b), it starts on page 37! This I take to be an indication that it is a direct follow-on to the paper on trivialisation. There he states, right at the outset, that despite the pessimism of the earlier paper, he has a strong confidence that there will be improvements in the empirical and theoretical procedures of sociology. Part of his reasoning is that, despite what passes for the formal rules of what sociologists are supposed to do, their actual praxis in discovering facts and theorising about them is different. Not only does this relate to the practice of cross-classification – explicit or implicit – but also to the use of *post factum* interpretation. This is supposed to be 'illegitimate' as a practice but in reality it is done a great deal, often in conjunction with cross-classification. It was Robert Merton who kicked the procedure of *post factum* interpretation into touch in a very influential essay (Merton 1957). Baldamus argued that 'Merton did not realise that post-factum theorising, far from being an occasional, illegitimate aberration, is in fact an *indispensable* element of all dominantly empirical techniques in modern sociology' (Baldamus 1971b: 41)

So where did Baldamus get his confidence from, given his troubles and tribulations with the problem of trivialisation? I think the answer lies with his growing conviction that the methods of cross-classification and *post factum* interpretation provide a means of discovering something about the nature of social reality. If, in other words, we start with the assumption (as Baldamus urges) that social reality 'includes objects and connections between objects which are in some sense and to some degree discoverable as real things' (Baldamus 1971b: 59), then the production of theory 'is therefore not wholly a reality constructing enterprise, it is also a *reality-describing* process' (Baldamus 1971b: 60). This process of discovering unexpected, even surprising, connections as a result of this method can be seen as an exemplification of serendipity. In retrospect, given Merton's great interest in the role of serendipity in sociological research, it is fascinating to note that by making a break with Merton's position on post-factum theorising, the possibility of serendipitous findings becomes more likely. Merton, it may be recalled, described serendipity in the scientific context as 'the discovery through chance by a theoretically prepared mind of valid findings which were not sought for'. Despite the significant difference between Merton and Baldamus, identified here, Merton nonetheless believed that it was important to go beyond formal codifications of what scientists say they do, or think they should do, to what they

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Baldamus found so unacceptable. In his paper, Mills explicitly employs cross-classificatory techniques to illustrate his position.

actually do and think about the research process (Merton and Barber 2004; Dalton 1964).

By way of illustration Baldamus analysed a situation, in which he had been involved, concerning the appointment to a lectureship in his department, which had led to a good deal of conflict. The analysis of a situation in which one was centrally involved has intrinsic difficulties. When Pierre Bourdieu wrote *Homo Academicus,* on a much larger scale, he admitted that it contained a considerable amount of 'self-analysis by proxy' (Bourdieu 1988: xii–xiii). The whole Preface to the English edition is of relevance to my present concerns. Note particularly his comment:

> Social science may expect to derive its most decisive progress from a constant effort to undertake a critique of sociological reasoning – that is, to establish the social derivation not only of the categories of thought which it consciously or unconsciously deploys, such as those pairs of antithetical terms which so often inform the scientific construction of the social world, but also of the concepts which it uses, and which are often no more than commonsense notions introduced uncritically into scholarly discourse … or of the problems which it elects to study, which not infrequently are nothing more or less skilfully designed versions of the latest 'social problems'. (Bourdieu 1988: xii)

In such a way Bourdieu sought to distance himself from what he termed nihilistic attacks on science promoted by 'certain so-called 'postmodern' analyses' (Bourdieu 1988: xii). Yet if one can do this without a self-indulgent narcissism and look at the familiar world with a detached scrutiny, there are, Bourdieu argued, real gains by such epistemological vigilance. Baldamus, through the application of the technique of cross-classification, eventually formulated the following rather complex formulation, which he noticed had something of the character of Weber's ideal types. Baldamus decided to follow Mills's advice to experiment with cross-classification as a way of analysing 'an administrative conflict over the appointment of a controversial candidate for a lectureship' (1971b: 64) [Baldamus is referring here to the Atkinson affair of 1970–3 – Eds.]. He made rough sketches that started with the contradiction between two clusters of complex dynamic factors (the procedure of the lower selection committee and the procedure of the higher selection committee). 'Very soon an incessant process of redefining, discarding, readjusting all sorts of labels sets in' (ibid 65) but the original axis of opposition between the two clusters (in the diagram below, between upper left and lower right) remains.

Table 2.3 Cross-classification to highlight decision making processes in academic appointments (Baldamus 1971b: 67)

		Latent Criteria	
		Competitive	Non-competitive
Manifest Criteria	Academic	academic merit suitable for merit rating by a committee of peers	nepotistic orientations related to prerogatives inherent in academic status hierarchy
	Political	democratic principles suitable for voting procedures	authoritative orientations connected with unilateral decisions based upon the administrative power structure

Now here is Baldamus's comment on the experience of using this method of analysis:

> The most interesting aspect for anyone who has tried this technique of progressive conceptual articulation seems to me the certainty – amidst the overwhelming confusion – which one experiences in respect of discrepancies and disharmonies. It is only the *negative* experience of 'bad fits', the frustration of recalcitrant discordances in the overall system, which keeps the never-ending process of outer-inner double-fitting going, rather than the satisfactions derived from occasional 'good fits'. (Baldamus 1971b: 67–8)

This notion of 'double-fitting' was for Baldamus nothing less than a distinctive method for social enquiry. In the very process of trial and error activity the investigator 'simultaneously manipulates the thing he wants to explain as well as the explanatory framework' (Baldamus 1972a: 295).

So it was that, while he started with the supposition that the top left-hand box would be the most significant, he pointed out that the very technique of cross-classification allows for the possibility of developing a new conceptual focus which can transcend the state of knowledge or insight reached at a given moment. When, therefore, he shifted his attention to the top right hand box – nepotism – he came to the conclusion that the conflict between the academic-merit institution and the administrative power structure was not the crucial problem. Rather it was what he described as the inherent antagonism between the nepotistic-paternalistic basis of the traditional academic status system as against the values and expectations attached to democratic voting procedures. It is here, I think, that *post factum* interpretation comes for he tells us, as though looking at things with fresh eyes:

> Every detail in the day-to-day affairs of administration took on a new meaning. Wherever I looked I now saw all kinds of more or less submerged and complex

> elements of paternalistic nepotism and it was this attribute of the academic system more than anything else that appeared to provoke the main impetus to conflicts over university reforms. (Baldamus, 1971b: 70)

This, surely, is a good example of serendipity in social research. Needless to say, it does not always work precisely because of the trial and error aspect of the activity of the researcher.

When I entitled this chapter 'Baldamus's adventures with cross-classification', I perhaps did not fully realise how adventurous he was. For while he realises that there is a nominalistic element in cross-classification, for example in its ideal type aspect of accentuating reality, he also thought that cross-classification as a technique somehow revealed something about the in-built nature of social reality. For example, he thought that the existence of antagonistic complexes, which cross-classification techniques showed up, represented what he termed 'recalcitrant anomalies' (Baldamus 1971b: 73). They were there even against the expectations or biases of the observer. Further, he was fascinated by the fact, as he saw it, that in any one box it was possible to sub-divide and continue to do so, which could lead to a process, even an infinite sequence, of successive dichotomisation. With some boldness he concludes this part of his analysis by claiming that 'the strange tendency of cross-classification towards an ever-increasing complexity of telescopically interlocked 'boxes' reflects an in-built structure of social reality rather than an attribute of the sociological imagination' (Baldamus 1971b: 73). It was indeed this continual moving backwards and forwards between the projective theorising of the sociologist and the assimilation of recalcitrant facts that made the advancement of knowledge possible. Indeed, the impression is conveyed, that, in the course of this process, the ideal types with which one began, become in some instances real (empirical) types.

Word Games

I referred at the beginning of my chapter to Baldamus's approach to sociological analysis as one of serious playfulness. I think this is nowhere better illustrated than in his paper 'Implicit Observation: Further Notes on Cross-Classification' (Baldamus 1973a). Whereas in the earlier paper he had been concerned to show the pervasiveness of cross-classification across statistical empirical work and theoretical practice, here he is struck by the semantics of two kinds of unexpected connections that cross-classification techniques can throw up. These, he noticed, were of two kinds: pleonasms and paradoxes. This he nicely illustrated in the following simple example:

Table 2.4 Pleonasms and paradoxes (Baldamus 1973a: 1)

(Society)	(Personality)
repressive	intolerance
permissive	tolerance

If one takes the diagonal connection we can see that repressive tolerance serves as an example of a paradox. Although Baldamus does not mention it (probably because it was so well known at the time) this was a paradox which Herbert Marcuse had famously deployed in his critique of American society (Marcuse 1964). What looks like a contradiction can actually have some fruitful application. Sociology is replete with such dichotomous pairings: non-contractual order, ideal type, substantive rationality, unintended consequences, and so on. Baldamus saw such contradictions as a basis for making conceptual innovations: 'For reasons which are as yet obscure, the successful innovator selects intuitively, a connection between attributes that are contradictory. Furthermore, the attributes are so chosen that they express an unstated, intangible, puzzling, unexpected, anomalous, irreducible antimony' (Baldamus 1973a: 3)

If one looks horizontally at the pairings then we get, for example, repressive intolerance, which is indeed a pleonasm. Such pairings are probably not so fruitful, although conceptual examples can be found: contractual order, statistical average, formal rationality, intended aims and many others. Even so, Baldamus found the occasion to put such pairings to good use. Perhaps the most well known is 'perceived perceptions'. It is developed out of a critique of the *Affluent Worker* study, previously referred to, and Beynon and Blackburn's *Perceptions of Work: Variations within a Factory* (Beynon and Blackburn 1972; Baldamus 1973b). These studies he saw as radically subjectivistic, in which sociology becomes phenomenological, resulting in an endless chain of perceived perceptions that can only be appreciated from outside those conceptual boundaries. What the investigators end up with, to use another pleonasm, are defined definitions, which take one away from the economic realities of physical variables and real structures, or, if you like, the conditions of exploitation in the real world.

Some may feel that Baldamus pushed the point too strongly in his critique of the *Affluent Worker* study, as when he writes:

> There is a distinctive message in it, a message that is attuned to a large and growing body of opinion. What it says is very simply that 'wages' and 'labour' are merely subjectively-perceived phenomena. As this argument is linked with a general theory of 'conflict' and 'cooperation' between workers and employers, it follows by implication that profits and capital are also imaginary. (Baldamus 1976a: 65)

I suspect this conclusion would have been strongly contested by the authors of the study, but it does make Baldamus's own position very clear.

The second example is that of 'accidental accidents'. Baldamus was involved in a research study of industrial accidents that involved large-scale statistical analysis of official accident records and more intensive case studies of particular firms (Baldamus 1969a, 1969b, 1969c). It was primarily a British-based study although he also collected data from elsewhere. It was a way of highlighting their research conclusion that there were grounds for doubting that all the events categorised as accidents were caused by a truly accidental happening at work. Baldamus was exploring here what he termed absenteeism-accidents. Such accidents gave the employee a chance of withdrawing from work with a legitimate reason. The remaining ones were the truly accidental accidents. The 'non-accidental accidents' would more likely be minor in their effects since we can assume the individual would have some measure of control over the event.

In 'Implicit Observation: further notes on cross-classification' (1973a), Baldamus plays with the categories of pleonasm and paradox. His interest, as we have seen, is in teasing out the relationship between theory and method in sociology by looking at what he sometimes termed the method of theory. To do this he constructs two categories which are pleonasms – Explicit Theory and Explicit Observation – and two which are paradoxes – Implicit Theorising and Implicit Observation. These emerge from a particular form of cross-classification in which Theory and Research are set against concepts of Explicit and Implicit. This is how it looks:

Table 2.5 Cross-tabulation of theory and research (Baldamus 1973a: 4)

	Explicit vs. Implicit	
Theory vs. Research	a Explicit Theory	b (Implicit Theorising in Survey Research)
	c (Explicit Observation in terms of Interview Data, etc.)	d Implicit Observation?

Briefly, Baldamus argued that categories b and c could be set on one side since they represented conventional ideas which were unproblematic. It was the diagonal connection between a and d that interested him because both were problematical. In the case of explicit theory he argued that, despite the text book formulations and its necessary connection with research, the gap between theory and research remained. It is precisely this that leads him back to his point about trivialisation and the consequences it has had for sociological practice:

> Over the past ten years the substance of all major theoretical controversies has disappeared, usually through the form of theoretical trivialisation which results from increasingly differentiated concept connections within a given problem. As the substance evaporates, the sociological discourse changes imperceptibly into a *philosophical* one. But since this is rarely possible on a technical level, the most notable outcome is an increasing amount of philosophical platitudes. (Baldamus 1973a: 5–6)

At this point Baldamus performs something like an intellectual somersault. He takes the work of Talcott Parsons as a significant example of theoretical work which appears to be based on the assumption that an axiomatic system of categories can be constructed without reference to empirical experience. However, he then points out that reference is made to empirical studies, for example on the subject of primary socialisation, and that these are in effect forms of implicit observation. The result is, that for all their pretensions, this kind of approach to axiomatic theorising is actually coloured by implicit observations grounded in a particular culture at a specific period of development. All this leads him to an amusing, even ironic conclusion:

> An alternative way of reducing the inflated element of axiomatic construction is to interpret the role of prominent theoreticians as one of participant observation, in the sense of what may be called '*macro*-sociological participants'. The analogy with the familiar kind of face-to-face participant observation reinforces the crucial fact that there are no known procedural rules. This also helps to recall the importance of internalised value commitments as an invisible steering mechanism in the selection of 'relevant' problems, concepts and concept connections. (Baldamus, 1973a: 7)

Conclusion: Surprising Connections

The conclusion of this analysis is, therefore, that what appears to be a contradiction between Explicit Theory and Implicit Observation in fact manifests a surprising connection. It provides a new perspective on the relationship between theory and research. The sterile formulation that led to the theory-research gulf is side-lined. The alternative approach achieved through the use of cross-classification holds out the possibility for understanding how knowledge about the social world is actually obtained and how it may grow. Indeed, it is only at the end of his paper on Implicit Observation that he introduces the term *praxis*. It is by concentrating on the paradox of implicit observation that he is able to throw light on the question of what it is that sociologists actually do, whether in the guise of theory or empirical enquiry. Moreover, he argued, this offers a way in to understanding the long-term changes, innovations and developmental sequences that have occurred in sociological work.

The use of the method of cross-classification when applied to 'theory' and 'method' in sociology does then lead to some typically paradoxical conclusions. Although he is resolutely anti-phenomenological, he recognises that sociologists need to make intuitive inferences in their work. Again, although such work involves improvisation and has a trial and error character about it – and part of his interest was to try and find out how and why errors occurred – nevertheless, real advances in knowledge can be made. I think the clue to this is the connection he makes between invention and discovery in sociology. There is the intuitive skill we deploy in inventing concepts (not least in the formation of contrast conceptions).

> Primarily these inventions are a product of the human mind. This is what tools, machines, experiments, classificatory schemas, concepts, theories and words have in common; they are all "methods" in a wide sense. Discoveries, by contrast, refer to an objective and observable reality outside the mind. The connection between the two is (chiefly) that a discovery can only occur when a newly invented method is projected into a non-mental section of reality. In the social sciences, and above all in sociology, the invented mental constituent of discoveries is more intangible than in most of the physical sciences. For … it consists overwhelmingly of complex verbal operations, some of which, like the capacity of dividing unknown and unobservable entities, are conspicuously obscure. (Baldamus, 1976a: 24)

If, for Wright Mills, cross classification was the grammar of the sociological imagination, for Baldamus it was the distinctive method of sociological theory. His aim was to cut through the obscurities and mystifications to show how and why sociology could be a science of social reality. This hard-won position represents in the end a grounded confidence in the future of sociology. Whatever our overall judgement on the endeavour, I think we can safely say that Baldamus's adventures with cross-classification, as far as sociologists are concerned, are more fruitful than solving a cross-word puzzle and more satisfying than doing Sudoku.

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Chapter 3
Efficiency and Effort Revisited: Emotional Labour and Contemporary Sociology of Work

Mark Erickson

Wilhelm Baldamus's *Efficiency and Effort* (1961a) has become a classic of British sociology of work. Despite its age many of those involved in sociology of work in the UK will have read the book and noted its value as an attempt to systematise the examination of employees' efforts in work situations, and as an analysis of how workplace controls are ordered and implemented; Baldamus sums up this process as the 'effort bargain', and shows how it is a complex process of contest and co-operation between management and workers. Given that the book's central focus is on how the effort bargain is constructed and how this bargain is central to the organisation of industry, its longevity is understandable: the effort bargain is still at the core of industrial relations and the organisation of industry despite the many changes in modes and forms of work and employment that have arisen in the past four decades.

Perhaps surprisingly, the citation of *Efficiency and Effort* in journal articles has remained consistent across the years. In 1964 it was cited four times; 1972 twice; 1982 four times; 1992 three times; 2002 once and in 2009 three times (Social Sciences Citations Index search on 28 May 2009 for citations of Baldamus 1961: 122 hits in total). Many of those who cite the book note its signficance. For example, Richard Brown highlights the importance of Baldamus's insights into the way that work obligations are internalised (Brown 1992), John Eldridge describes Baldamus's work as challenging, provocative and original, noting that this is partly due to Baldamus's status as an outsider to British academic life (1998: 159), and Keith Grint uses Baldamus's work to illustrate the continuing centrality of the effort bargain to understanding industrial disputes (Grint 1998: 159). Yet this continued presence in British sociology of work suggests an impact that *Efficiency and Effort* did not actually achieve even during Baldamus's lifetime. The book never attracted a following in the way that, say, Braverman's *Labour and Monopoly Capital* (Braverman 1974) did. Baldamus largely turned aside from sociology of work in the 1970s to concentrate on sociological theory and methodology. And when he did revisit *Efficiency and Effort* in an essay he wrote as a BSA conference address (never delivered) he noted that it was an 'irreparable failure' that he had discarded a long time ago (Baldamus 1984a: 1). Baldamus's sociology of work

was gradually eclipsed from the early 1970s onwards, firstly by phenomenology and symbolic interactionist approaches, then Marxism and labour process theory and, more recently, feminism, poststructuralism and social constructionism. But despite this, Baldamus's work continues to be cited by eminent sociologists of work, and it is unlikely that this is simply for reasons of dogmatic adherence to a cherished book. Groundbreaking at the time, *Efficiency and Effort* is full of insights, and there are still some lessons we can learn from it even if we don't go so far as to resurrect the entire analytical framework (although this was proposed by Baldamus – 'a few repairs to the theoretical framework could probably make it last a few years more' (1984a: 1)).

In this chapter I propose to use Baldamus's sociology of work to examine some recent trends in understanding work and the organisation of work; I will focus in particular on a recent, and very popular topic in the sociology of work – emotional labour. It is not the intention here to examine how, or even whether, emotional labour is being deployed in workplaces. Instead I propose to use Baldamus's understanding of work, and his critical approach to concept formation to examine how a discourse of emotional labour is being deployed in contemporary sociology of work.

Baldamus's Sociology of Work and Employment

Baldamus's sociology of work differs from others of the time in a number of respects. The first was that it departed from the managerialist frame of reference that suffused British sociology of work. Rather than starting from the point where industrial conflict was seen as a deviation from a norm of quiescent and harmonious industrial relations, Baldamus began from the opposite point. Although he concedes that starting from the perspective that 'the organization of industry as a whole reveals a natural harmony of interests between employers and employees' (1961a: 6) has the advantage of projecting 'some order into the complex system of industrial affairs' (1961a: 7), it suffers from the problem that by assuming co-operation between employer and employee, such co-operation is never explained (1961a: 7). Baldamus does something quite different. He starts from the observation that the structure of employer-employee relations present: 'a structure of differentiated power that reflects unequally distributed advantages and disadvantages. Consequently the emphasis will be on the causes of conflict and disorganisation' (1961a: 7).

This radical, for the time, approach requires Baldamus to present an *ab initio* form of analysis: he needs to build up his theory from the ground up, as there are few other studies he can rely on for a similar frame of reference. Much of business studies and sociology of work is concerned with investigating why it is that work stops – through absenteeism, resistance, strikes or other disputes. Yet, as Baldamus notes, when we look in depth at what work actually involves for many, the meanings attached to work and the costs of work to the individual in terms of

stress, workplace conflict, alienation and ill-health, the real question we need to address isn't why people stop working, but why they work at all.

The second challenge is perhaps more subtle, but one that has equally far reaching consequences for sociology of work. In attempting to find the connections between efficiency and effort, Baldamus concludes that definitions of these terms are frequently confused and confusing, and will always be contentious and contestable. Again, the current orthodoxy at the time was to see efficiency as a cornerstone of any management enterprise, and to assume that all business organisations will attempt to maximise efficiency through controlling effort. On examination, Baldamus found that efficiency was usually left undefined by those advocating it (including academics) and that the use of the term was as a platitude or dogma that hid the reality of the situation. In addition some people really meant economising when they talked of efficiency, but this was, for Baldamus, wrong on two counts. Not only was it an undefinable term as it relies upon a neutral standpoint, one unavailable in the power-differentiated workplaces he examined, it was also the substitution of an economic concept by a sociological one. What people were often talking about when they spoke of efficiency was the application of effort (an aspect of occupation) and skill (an aspect of employment) (Baldamus 1961a: 9–10). The distinction is an important one: effort is a consequence of individual action where an individual has agency in deciding on how much they will exert themselves, whereas skill is a consequence of a social process where occupation is constructed through education and training. Baldamus separates these two 'closely interlocked' things that combine to form 'work': 'occupation', which can be identified from the deployment of occupational costs which compensate the worker for their skill, education, experience, and 'employment' whose costs compensate for the effort a worker expends.

These two starting points are central to Baldamus's sociology of work, and he constantly restates this frame of reference in the rest of the book. Throughout he attempts to analyse, evaluate and ultimately quantify the relationship between effort and wages, but finds the task almost impossible given non-rational disparities such as the distribution of earned income between management and wage earners. He does produce a schema which classifies different forms of control – stability and intensity controls; the first stabilising the relation of effort to earnings and the second raising the level of wages with respect to effort expended, but the possibilities of this producing an *absolute* measure of effort-wages relationship founders on the presence of a range of subjective perceptions brought to the relationship by both managers (with their 'intuitive' judgements (1961a: 124)) and workers. However, Baldamus does not stop at this point, and continues his analyses of effort. His next step – a meticulous examination of what effort actually is, is much more successful, at least in terms of producing an analytical schema that helps us to understand work and employment. This is constructed from a close examination of what the realities of work are, rather than from suppositions of what work involves, again a departure from the norm of the time.

Baldamus constructed his analyses from an examination of his own working life, but also from observations of manual workers that he carried out in the 1950s, mainly in Birmingham. Using his starting point of why work is done he looks at why workers will continue in jobs that many would consider to be unpalatable, tiring, degrading or boring. In looking at workplaces Baldamus noted that employees survived negative experiences of work – what he termed deprivations – by generating specific coping mechanisms – what he termed relative satisfactions. Understanding these, and observing them, could perhaps allow one to make workplaces better places, or make work tasks more palatable, but would also aid us in understanding how work was continued.

Using previous work from industrial psychology, Baldamus notes that effort cannot be measured by tests. However, by breaking down work into objective and observable realities and subjective feelings that the work and the work environment arouses, he is able to construct an ideal type of effort that reflects both the environment and the individual's perceptions. From this, a typology of deterrents and rewards for work emerges; a cross-classification of subjective and objective factors:

Table 3.1 Realities of work (Baldamus 1961a: 76)

Work realities	Physical conditions	Repetitiveness	Routines
Deprivations	Impairment	Tedium	Weariness
Relative satisfactions	Inurement	Traction	Contentment

Impairment arises due to the physical discomfort of work, particularly strenuous work. But this can result in a relative satisfaction of inurement – getting used to the hard work and coping with it. Similarly, boring and routine work produces tedium, but as time proceeds the worker copes with the routine and boredom and actually begins to enjoy being taken up and propelled along by the task-traction. Finally, weariness arises from the work realities of routine, the daily grind of work activities, the coerciveness of having to keep a job. This engenders a relative satisfaction of, as Baldamus put it, 'contentment, a state of mind that is often expressed by saying that one is in the mood for work. This is usually a kind of dull contentment, though it may be more positive in the case of marked susceptibility to weariness.' (Baldamus 1961a: 70) There is a strong connection and a mutual dependence between these pairs of deprivations and satisfactions: the worker cannot experience one without the other, cannot experience contentment without having first experienced weariness. From this perspective effort is thus defined as the total of work deprivations (hours of work, monotony, fatigue), which the

worker is compensated for by, principally, wages but also by relative satisfactions. Ideally a wages-effort parity results.

Do Baldamus's categories describe all aspects of work processes? Reducing deterrents and rewards to such a small number of options seems too restrictive to describe all forms of work, and it could be argued that Baldamus's typology of deprivations and satsifactions only applies to manual workers in routine occupations (the group that there was most interest in at the time Baldamus was writing). However, Baldamus's categories do two useful things for us. They remind us that work is often unpleasant for many people, and they show us the complexity of work, how it is understood by workers and how they are orientated towards and motivated in work. In addition, they are rooted in the real experiences of individuals – both Baldamus and you will have experienced similar feelings, particularly with respect to traction: the feeling of being caught up in a task and carried along by it, regardless of whether or not you really want to do it. Such feelings and experiences are not quantifiable, are not even comparable, for we all have different sensations of how we are expending our effort, but we can classify them and use them as a way of making better sense of the work that people do. Yet, and we will return to this point, there is a problem here with Baldamus's classificatory scheme: are these categories representative of 'real' things and do they have an objective status? If not, what status do they have? Baldamus alludes to their construction coming from his observations of work and the expenditure of effort, but he is unable to make the claim that these are definitive categories, or exhaustive ones. Not only that, the possibility also remains that even if these categories are based on empirical observation they have reduced the richness and complexity of experience to an attenuated concept.

In addition to these points we also must note the dramatic changes in the world of work and industry in the last few decades including the massive decline of manufacturing occupations and industry, changes in the labour process and control of effort through automation, Japanisation and other management regimes, a significant feminisation of the workforce and a decline in trade union power and membership. And as these socio-economic changes have occurred, the sociology of work has amassed a much larger methodological toolkit. Despite this, or perhaps because of this, Baldamus's sociology of work coupled to his methodological innovations can still have some analytical force in understanding sociology of work.

Central to this is a consideration of Baldamus's idea of how trivialisation as a persistent problem for sociology is still a significant issue and one, as I hope to show, that still besets sociology of work.

Types of Trivialisation

According to Baldamus sociological work is always beset by problems of trivialisation, a process in which major issues of sociological and social relevance degenerate into increasingly trivial sub-issues.

> [A] given study sets out with a 'major' or 'vital' or 'urgent' political problem, and then undertakes certain analytical or empirical operations, as a result of which, finally, certain conclusions emerge; these conclusions appear to be trivial *relative* to the point of departure. (Baldamus 1971a: 4)

> The word 'trivial' in everyday use is loaded with emotive connotations and should therefore be replaced, wherever possible, by more technical terms. When it is applied in dominantly theoretical contexts, its most obvious meaning suggests a process by which an important substantive issue is unintentionally attenuated, diluted or weakened in the course of certain methodological activities. (1976a: 58)

The idea of theoretical or conceptual trivialisation covers several kinds of attenuation.

1. Incorporating a theoretical concept (Baldamus uses the example of 'race') into the 'established repertoire' of traditional sociological concepts such as class, social order, etc. This is an elusive process, but one where conceptual deterioration takes place as the focus loses its initial sharpness and 'gives way to a series of less established concepts'.
2. Extra-disciplinary attenuation is a process where interdisciplinary research, which inevitably leads to conflicts over theory, results in a form of trivialisation, often by shifting from an explanatory mode to a purely descriptive mode.
3. Transmutation, where tightly defined technical concepts (e.g. those of economics) become transformed through being combined with other less tightly defined concepts, or with common sense concepts, or through the transition from being deployed at a macro-sociological to a micro-sociological level, or the switch from dynamic to static occurrences (or vice versa).
4. Cumulative attenuation occurs when different transformations reinforce one another.

All sociology is afflicted with these problems at least to some extent:

> At the back of all the trivialization effects is the intrinsic weakness of the sociological discourse in general: the difficulty of locating identifiable errors. The methodological lesson that has come out of the present chapter is as yet

> very simple. This is the extraordinary power attached to invented definitions. (1976a: 66)

This is, of course, not the *main* methodological lesson Baldamus is trying to get across which is that concepts must be tightly and rigorously defined and operate as ideal types, at least where possible. This is clear in, for example, his construction of a typology that classifies relative satisfactions and deprivations in work and employment. It is also clear in the original definition of emotional labour that has subsequently been operationalised in a great many sociology of work studies, studies that often exhibit precisely the types of trivialisation effects Baldamus alerts us to.

Emotional Labour

The term 'emotional labour' was first coined by American sociologist Arlie Russell Hochschild in her book *The Managed Heart: The Commercialization of Human Feeling* (1983). Hochschild notes that emotional labour is associated with an increasing number of occupations, and more and more people are now employed in positions that require them not only to deploy skill and/or effort, but also to present certain emotions. Increasingly, corporations require employees to show their commitment to customers or clients and to employers by overt displays of emotion or by concealing their actual feelings.

Hochschild's research investigated this emotional work, and found that a complex process of controlling emotions took place. Workers had to manage their emotions in precise ways, particularly to ensure that their real feelings did not come to the surface: surface acting, in which employees pretend to feel an emotion and subsequently express it is separated from deep acting through this process of managing emotions. Hochschild's core definition of emotional labour is: 'the management of feeling to create a publicly observable facial and bodily display' (Hochschild 1983: 7); we must note, at the outset, that the definition of emotional labour used by other writers on the topic diverges from this quite considerably (see Noon and Blyton 2007: 182 for a list of these varied definitions).

According to Hochschild jobs that deploy emotional labour have three distinct characteristics:

> First, they require face-to-face or voice-to-voice contact with the public. Second, they require the worker to produce an emotional state in another person – gratitude or fear for example. Third, they allow the employer, through training and supervision, to exercise a degree of control over the emotional activities of employees. (Hochschild 1983: 147)

Hochschild's paradigmatic case is that of air stewardesses, where workers are trained to manipulate the emotions of the public through controlling their own

emotions, and are trained to express the company's public image through their actions. But we can see similar processes in many forms of service sector work: call centre operatives who are obliged to follow closely worded scripts, fast food outlet staff who must greet customers in a specific way, indeed most front-line customer service workers will now be subject to some form of emotional labour and emotion management.

Emotional labour is something that has been identified in many forms of work, particularly in the service sector, and is increasingly seen by employers as an important resource. Bolton points out that there are many complexities hidden by the term, but argues that 'it has never before been so important to a capitalist economy.' (Bolton 2005: 157) However, the operationalisation and application of the concept is quite varied: the popularity of the term emotional labour has led to a proliferation of studies, so much so that Bolton argues that something of a bandwagon has formed (Bolton 2005: 53). This proliferation of studies has, perhaps inevitably, led to a proliferation of usages and to the sort of attenuation that Baldamus describes.

As an indication of just how much proliferation has occurred we can examine the Social Sciences Citation Index. Hochschild's book has been cited 1872 times between 1983 and 2008. From a modest start of a few citations per year we can see a very dramatic rise in citations:

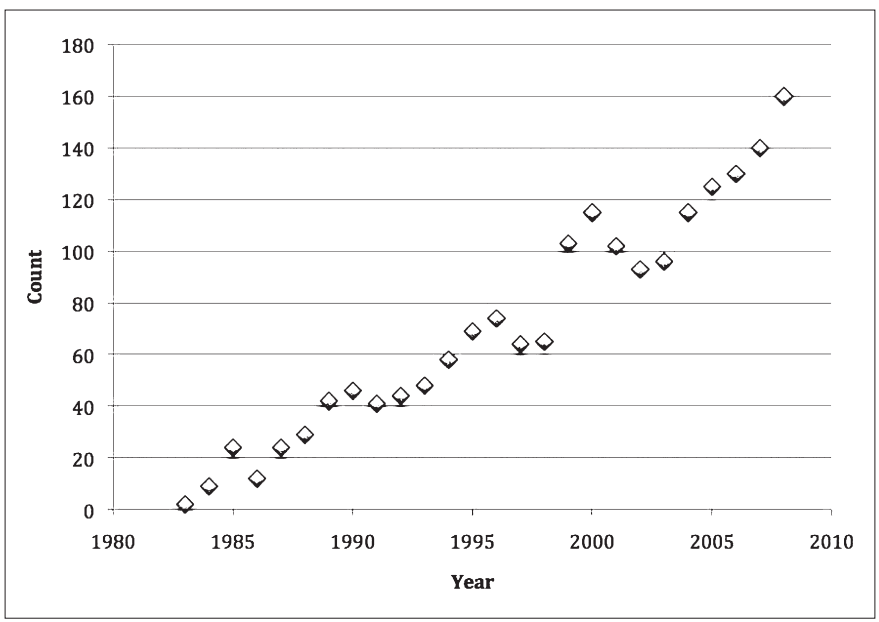

Figure 3.1 Citations of Hochschild (1983), Social Sciences Citations Index 1992 to 2008

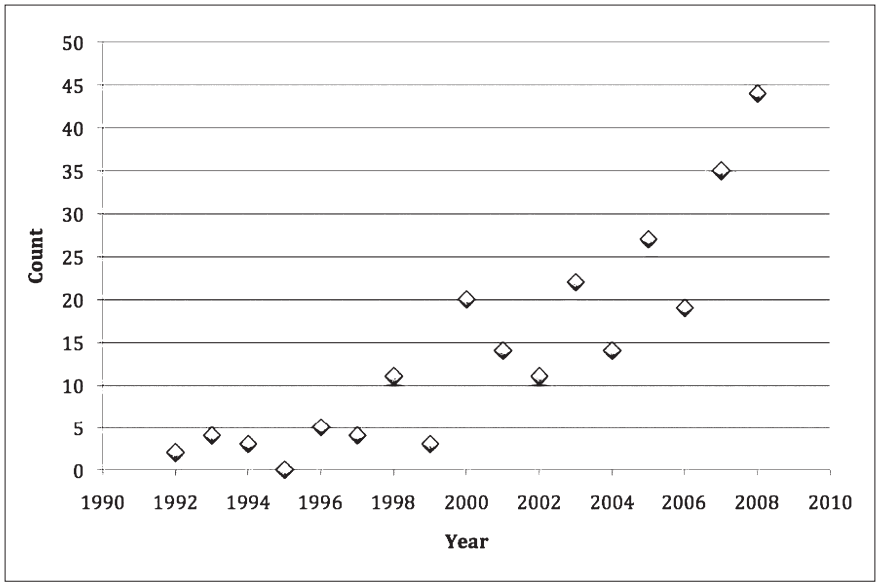

Figure 3.2 Journal articles on emotional labour, Social Sciences Citations Index 1992 to 2008

A similar rise, although one lagging behind by about ten years, can be seen in the number of studies that classify their main topic as 'emotional labour' (239 hits between 1992 and 2008).

A systematic examination of all of these studies would be the ideal way to examine a putative process of trivialisation, but that is beyond the scope of this chapter. A more pragmatic method has been chosen: the selection of all the studies on emotional labour that have appeared in the BSA's journal *Sociology*. This has the advantage of producing a smaller number of studies, being at least in part representative of British sociology (to maintain some comparisons to Baldamus's working environment) and being easily available. Three studies have appeared:

Sharma, U. and Black, P., 2001: 'Look Good, Feel Better: Beauty Therapy as Emotional Labour', *Sociology*, 35 (4), pp. 913–931.

Wellington, C. A. and Bryson, J. R., 2001: 'At Face Value? Image Consultancy, Emotional Labour and Professional Work', *Sociology*, 35 (4), pp. 933–946.

Toerien, M. and Kitzinger, C., 2007: 'Emotional Labour in Action: Navigating Multiple Involvements in the Beauty Salon', *Sociology*, 41 (4), pp. 645–662.

Sharma and Black's study of beauty therapists provides an insight into the labour process and working conditions of this under-researched, and indeed under-regulated, industry. They interviewed 15 beauty therapists, all female, in a variety of occupations about their work, customers / clients and future prospects:

> All our interviewees defined beauty therapy in terms of work with *feelings* as well as with the *body*. We deal with this claim as an instance of occupational rhetoric, but also as a potential description of aspects of the labour process in which they are engaged. In this sense, beauty therapy can be understood as a form of what Hochschild (1983) calls emotional labour and we use this concept as a means of exploring the work which beauty therapists do. However, we shall argue that the explicit claim to emotional work is as likely to compromise as to strengthen their claims to a 'serious' professional status. (Sharma and Black 2001: 915)

Beauty therapists do 'emotion work' in their conversations with their clients, and manage this work closely. Yet the degree to which these skills are taught, learned or acquired through experience is debatable. Many of their respondents thought that they had just 'picked up' their ways of responding to a client's emotional needs. Sharma and Black concluded, with respect to this, that: 'Considered as occupational rhetoric, therapists' insistence on the emotional dimension of their work supported their claims to professional knowledge, but by describing such skills as largely intuitive they undermined these same claims.' (Ibid: 923)

This is a conclusion that, perhaps, Baldamus would have been at odds with. The intuitive skills and informal methods of professionals were of great interest to Baldamus, and he considered these things to be central to how professionals constructed their role. An appeal to 'intuition' would not be seen by him as undermining but rather constituting a professional role.

Sharma and Black, in looking at the labour process in the beauty industry, found it useful to analyse this work in terms of the concept of emotional labour (925). They start from Hochschild's initial definition that emotional labour is work that demands that the worker: 'induce or suppress feelings in order to sustain the outward countenance that produces the proper state of mind in others' (Hochschild 1983: 7).

However, unlike Hochschild's definition where emotional labour is carried out in response to an organisation's demands, beauty therapy emotion work is carried out because the worker *wants* to do it. This is not to deny the expenditure of effort that takes place in performing such work. But workers reported very positive feelings about their emotion work, and did not feel coerced into it (as Hochschild's airline crews, in contrast, did):

> most of our interviewees tended to see the emotional aspect of their work as a source of satisfaction rather than exploitative. Although the beauty therapists could often cite situations where, like the flight attendants, they were required

> to engage in deep acting to mask their own feelings, they were fairly free to perform this work in their own way. (Sharma and Black 2001: 927)

So – emotion work is an aspect of emotional labour which is a consequence of deploying, at least in the case of beauty therapists, intuitive and learned skills, skills that can also be used, freely, outside of the workplace.

Wellington and Bryson's study of emotional labour also looks at an under-researched area of work: image consultants. They argue that studies of service sector work, including studies of emotional labour, have tended to ignore well-paid professionals, but that this group are under just as much pressure to conform to societal norms, particularly of heterosexuality. This research investigates image consultants working for the Colour Me Beautiful (CMB) organisation, and used a mixture of postal questionnaires and face-to-face interviews to gather data.

They start from Hochschild's standard definition of emotional labour, but place this in the context of service sector work being 'a qualitatively different form of work to manufacturing employment' (934); it is a hybrid in which the economic and cultural are blurred. Much service sector work, and particularly management consultancy, is about impression management and performance. Image consultancy is deployed to transform the bodies and performance of employees, often being focused on women in workplaces so that they become further subject to the male gaze. For example, CMB consultants will re-write employees' dress codes, run style workshops and self-presentation classes for corporate clients.

This is an interesting piece of research that describes what image consultants do, and places these tasks in a wider context of the presentation of gender. However, the paper offers no discussion, or even description, of how emotional labour is being enacted, or even if the image consultants think they are performing emotional labour. The title of this paper is, for this reason, somewhat misleading: this is not research about emotional labour, despite deploying Hochschild's definition at the start and mentioning emotional labour in the abstract. It is, for this reason, difficult to assess as a presentation of research or theories of emotional labour, although it is interesting that the authors have chosen to use the term in the title of their paper.

Our third article by Toerian and Katzinger is also an investigation of beauty therapists' work, this time using conversation analysis of a single episode of a recorded beauty salon interaction. The paper attempts to make visible the *skills* (their emphasis) of emotional labour whilst at the same time showing how multiple involvements are managed. Once again we start from Hochschild's 'path-breaking analysis of service providers' 'emotional labour'', although Toerian and Kitzinger note that the distinction between emotional labour and emotion work made by Hochschild is 'simplistic' (Toerian and Kitzinger 2007: 646). They go on to offer an extensive classification of what emotional labour may entail. Paraphrasing:

> Emotional labour takes skill
> It may be effort intensive

> It is designed to generate profit, improve workplace functioning
> It includes being paid to 'look nice', smile, be caring, soothe tempers, be polite, boost confidence, fuel pride, prevent conflict, mend ego wounds
> It may involve suppressing or inducing one's own emotions
> It *always* involves following the prescription of the organization regarding emotions. (Ibid: 646 – my emphasis)

An extensive list, but perhaps somewhat vague, and certainly contradictory of the research of Sharma and Black who did not observe or identify the compulsive element that Toerian and Kitzinger describe as being 'crucial' in identifying emotional labour.

As feminist researchers Toerian and Kitzinger focus on how the skills of emotional labour, so often deployed by women in workplaces, are unrecognised and under-remunerated. Beauty salons are particularly poorly paid workplaces. Toerian and Kitzinger present a compelling case for their method of using conversation analysis as a means to reveal the deployment of the skills of emotional labour ('what does it take to *do* that which sociologists have theorized as emotional labour?' (Ibid: 648)) – we may want to question their decision to only use one interaction, however.

Their conversation analysis reveals fairly standard interaction tropes, but they extend this with a second analysis of 'emotional labour in action'. Here they focus on how the therapist's 'multiple involvement', which involves 'threading' the client's eyebrow while at the same time talking to the client (actually, chatting with the client is a more accurate description) and, secondly, navigating between different involvements, i.e. moving from one threading task to another while still chatting to the client in a way that maintains a smooth interaction, is the actual 'work' of emotional labour: 'The therapist's ability to be 'multiply involved' – to perform the threading while simultaneously 'chatting' with the client – is one of the many competences that enable her to treat the client as an individual. It should be understood, then, as an example of emotional labour in action.' (Ibid: 655)

Toerian and Kitzinger conclude that their identification of the two processes in beauty salon work that constitute emotional labour will help to construct a typology of verbal and non-verbal strategies that are instrumental in performing emotion work (Ibid: 655–6). Their identification of how emotion work is being done in the beauty salon means that:

> Within the highly gendered, low-paid world of beauty therapy, using our technical analysis to argue for the recognition of emotional labour *as skilled* is significant. While the skills we demonstrate are hardly 'spectacular', that does not diminish their importance. On the contrary, we would argue that their 'everydayness' is what makes them crucial: they are not merely required under special circumstances (e.g. dealing with an upset client), but are fundamental to the routine work of beauty therapy. Ironically, it is their 'everydayness' that

> makes these skills so difficult to recognize. Woven into the fabric of everyday interaction, they are even subtler – and hence more hidden – than the critical literature has suggested. (Ibid: 658)

Revealing the conditions under which people work and how they deploy skill is certainly a useful project. Yet there is something about this piece of research that calls into question the nature of 'emotional labour'. Here we have, as with Sharma and Black's research, workers performing tasks that they have not been 'trained' to do, but which they have brought with them into the workplace, or have developed through their experience of working. The management of emotions here is, arguably, not being forced on workers through the strictures of an organisation controlling the labour process, but is a feature of how we interact with each other as members of society. It is, perhaps, difficult to see how what we are identifying here is something *additional* to how we made sense of work before the concept of emotional labour was available to us as sociologists. Indeed, we may want to argue that what is happening when we use the term 'emotional labour' and treat it *as if* it were an ideal type that we can compare cases to is itself a form of trivialisation, in Baldamus's strict terms. This is not to denigrate the work being carried out; looking at gendered work, the under-researched and those often marginalised from sociological study, is important and significant. But it may be that the categorisation of this work as 'emotional labour' may serve to occlude and confuse rather than clarify and reveal.

The three papers published in *Sociology* all start from Hochschild's work and all deploy her definition of emotional labour. Yet two depart from it very radically, and one does not discuss or deploy the concept. What would Baldamus make of this?

He would probably start from an examination of the core concept, emotional labour as set out by Hochschild, and note that the juxtaposition of emotion and labour is paradoxical. What does emotional labour *really* mean and how has this concept been constructed? Hochschild's concept of emotional labour is extracted from observations of the work that people do – primarily airline crews. It therefore has a connection to the 'reality' of work that people experience. Yet if we look closely at the definition we see that it actually isn't a definition at all, rather it is a set of conditions that pertain for emotional labour to emerge. Nor is there much substance here in terms of 'objective' points to locate and measure, or even parameters for where emotional labour may be taking place. Hochschild and others have suggested that emotional labour is becoming a requirement of an increasing number of jobs in an increasing number of sectors. This is debatable: it may be, in contrast, that what we are doing is expanding our definition of the concept of labour to include more things and aspects which have always been a part of the labour process.

Hochschild and other proponents of emotional labour are certainly on to something; we recognise features of emotional labour in our own lives and, perhaps, our work. They also have empirical evidence to show that those failing to provide the 'correct' emotions in a workplace will face negative consequences.

But is this a new concept? Haven't emotions in a capitalist society always been commodified? And quite what is Hochschild basing her definition on? Is it really labour she is looking at? Perhaps what she is identifying is control and authority, and emotional labour is a misnomer from the outset? If we assume that the deployment of emotions, even when commodified and done to order, is a skill that can be learned or deployed at will, then why are we recategorising this skill as something else?

What Hochschild is doing is attenuating the concept of labour by focusing on only very specific aspects of, variously, the labour process, the affectual state of the individual and the context that labour is located within. Her aim is twofold: to delineate this 'new' form of work but also to make claims about all labour in a changing, Post-Fordist world of work. But if we were to simplify things by looking at the context of post-Fordism as being the time when more is asked of us – more of everything – then we would obviate the need for a new concept of emotional labour and could move straight to where the action is, in this case the deployment of emotional skills in the workplace. For Baldamus, the concept of emotional labour would represent the specific type of trivialisation he calls 'extra-disciplinary attenuation' as it is the result of an interdisciplinary strategy combining sociology, social psychology and economic analysis that results in analysis and explanation being replaced by description. Hochschild's work, and the subsequent research on emotional labour, does often take this form, providing description but little explanation other than through a critical appraisal of the changing wider context of work. So, for example, Hochschild's identification of a global 'care deficit' (2003) is a description of a state of affairs abroad in the world, but not an explanation of why this has happened other than through a very general description of globalisation effects.

Baldamus might also identify his first form of trivialisation, where a concept loses its focus as the complex concepts of emotion and labour become attenuated through being combined with other concepts and theories. Specifically, we might look at the way that emotional labour becomes attenuated when combined with gender and with labour process theory (specifically in terms of control). This is quite clearly happening in the two *Sociology* papers that do discuss emotional labour. Both shift attention from analysing just emotional labour to focusing on how this form of work is gendered, and how this itself is indicative of wider, societal, gender issues.

He might also want to focus on the process of transmutation that is taking place in these research studies. Emotion is being transmuted and attenuated by being diluted and, perhaps, misidentified. If emotion work is a skill that is learned and requires the disingenuous deployment of a 'real' emotion then surely it ceases to be an emotion. We may feel emotions *about* our emotion work, but our emotion work itself is not the deployment of emotion. Indeed, in the Toerian and Kitzinger study emotion is redefined as being implicit in everyday chat – something that most of us do most of the time. Emotion as an ideal type has been attenuated into everyday interaction through this process.

Almost certainly Baldamus would describe these research studies and perhaps even Hochschild's original work as examples of conspicuous trivialisation where a number of trivialisation processes produce an effect of cumulative attenuation. This does not mean that the work is poor, invalid or even 'trivial'. Far from it, the research reveals vital aspects of how work is gendered, how some aspects of women's work, in particular, are unrewarded and unnoticed, and how contemporary work requires more of us in many ways. But the point remains that we must be wary of trivialisation and its effects.

So what is happening in these studies? The sociologists have found something significant in the lives of the workers they have observed and interviewed. But what exactly is this if it isn't emotional labour? The studies identify workers coping with their work environments, articulating their professional skills and interacting with a range of clients in different settings. And, perhaps most significantly, they have identified workers actually enjoying what they are doing. Perhaps what we are seeing is simply 'traction': people becoming caught up in their work and finding a mechanism, even a satisfaction to oppose the tedium they are experiencing.

Baldamus identifies a number of different sorts of traction (Baldamus 1961a: 57–65), all of which are directly related to the completion of tasks, usually physical tasks. I propose that in addition to object, batch, process, line and machine traction we add another form: interactional traction, where the worker is drawn into and feels pulled along by the personal interaction they must inevitably partake in as part of the labour process. This, rather than emotional labour, is what these studies have identified. Workers have tasks to complete which involve a number of different elements, one of which is the adjustment of the emotional state of the client. This is achieved through conversational interaction and the worker is drawn into this and receives a relative satisfaction. This is comparable to process traction: 'it should not be thought that the movements of the operator are simply forced on him. There is usually a distinctly pleasant sensation in being guided or pulled along by the process in completing a given work cycle.' (Ibid: 63) Baldamus goes on to note that traction tends to become a normal expectation; this aspect of the normalness of emotional interaction between clients and workers is also visible in the emotional labour studies examined above.

Finally, we must bear in mind that traction is the relative satisfaction that compensates for tedium. Although tedium was not identified in the emotional labour studies discussed above, we can predict that workers would feel tedium or boredom if they were not engaging in their conversational interactions, i.e. if they did not experience interactional traction.

By identifying a more precise, and situated, concept that explains the experiences of image consultants and beauty therapists it could be claimed that we are attempting to move towards sociological relevance and away from trivialisation. Perhaps, although it must be noted that what is offered above is an analysis based only on the data that was presented in these journal articles, not on a full reading of all the data collected. We should be wary about making definitive statements here concerning emotional labour or, indeed, traction. Instead, what we

should consider is how it is that sociology of work has become so taken with the 'new' phenomenon of emotional labour.

In addition, the question of why this misidentification of emotional labour has taken place remains. We might want to consider two possible explanations. The first revolves around the problem of empirical studies losing focus, or finding it difficult to identify a tight focus. Here researchers may find a retreat to pre-existing theory and categories (such as emotional labour) to be a useful tool in regaining a focus, albeit one that is incorrect. The second explanation starts from the opposite end of the research process: a commitment to a particular theory can lead to the 'discovery' of the key concepts and elements of that theory regardless of the data.

Conclusions

Baldamus was always a critical voice in sociology and it is likely that he would be critical of contemporary sociology of work. Although in this chapter we have only examined emotional labour similar processes of cumulative attenuation and the substitution of objective categories by subjective ones are visible in many other contemporary studies, even ones that have achieved a 'classic' status such as the Social Change and Economic Life Initiative (SCELI) (Penn, Rose and Rubery 1994).

But would we want to use Baldamus's sociology of work instead? Baldamus's approach is quite different from that of current sociology of work: the construction of 'objective' categories that provide ideal types that the world can be compared to, and the subsequent cross-classification of various objective categories so that causal relationships can be identified and analysis of why things happen can take place. Such a method produces a particular sort of sociology, and sociology of work, one which may be robust in terms of findings, but operates in a profoundly different way from contemporary analyses of work. It is technicist and restricted to just an examination of workplaces. Indeed, the most frustrating thing in reading *Efficiency and Effort* is that Baldamus talks of many interesting things, yet fails to examine them in detail. So, for example, the role of socialisation in the workplace is largely unexplored, the connections between attempts to achieve parity in workplaces and wider societal issues of social fairness and justice are also left undiscussed and, most strangely, the revelation that UK industry systematically under-rewards workers for effort is displayed only as a line on a diagram that is not explained until an unpublished paper of 1984.

Put simply, Baldamus's approach lacks political engagement and social analysis. His sociology of work skirts round significant issues of context, most notably the operation of capitalism itself, in favour of a quite narrow and technical focus on aspects of employment. His analysis is, from this perspective, much more about employment (i.e. how work is organised) than it is about work and how that is experienced. Baldamus, for example, would simply be unable to connect his analysis of employment with wider issues of discrimination and division,

such as gender. Although he does identify the power imbalance at the heart of the employment contract, he doesn't do much with this insight. But we must remember that this critique is coming from over 40 years in the future, and benefits from the development of an entire sub-discipline of sociology of work.

Yet there is another point that must be raised here: Baldamus's own critique of how sociological concepts and categories are constructed. For all that he is exceptionally critical of those who have focused on efficiency in their studies of industrial sociology without providing adequate (or any) definition of what this is, we can identify a similar problem with Baldamus's construction of the concept of effort. Baldamus did acknowledge the existence of a problem here.

> Thus the larger and more intangible question of overall efficiency can be narrowed down to the more specific problem of defining and analysing the meaning of effort. For 'effort', again, is merely a common-sense term that is scientifically useless. In pursuing this question, we shall, in fact, find that it is surprisingly difficult to determine the substance of effort. Though the particular defintion we shall suggest must, to some extent, remain tentative, the problem can no longer be avoided. Unless *some* concept of effort is analytically established, the student of industrial organization will have to admit that the essence of his subject-matter is unknown. (Baldamus 1961a: 5)

This is dangerous ground: Baldamus has dismissed the prevailing concept in sociology of work (efficiency) and has promoted another (effort) that can, apparently, explain the features of efficiency. Effort, the thing over which management and workers contest and co-operate in the effort bargain, is that which will promote or hinder efficiency and will also be the thing that is pre-eminently in the minds of those involved in the bargaining process. But as Baldamus works his way through different possible definitions of effort and finds problem after problem attaching to them he is gradually drawn towards the conclusion that the components of effort remain psychologically exceedingly complex (Ibid: 124) and the only objective aspect of effort that we can identify are the deprivational elements of tedium, impairment and weariness. This is pretty much the 'merely' common sense starting point that Baldamus spends much of the book trying to escape from. It is hard not to come to the conclusion that the vitally important concept of the effort bargain has been attenuated considerably through the course of *Efficiency and Effort* as Baldamus is reduced to identifying his three relative deprivations and noting that there are, of course, a large number of satisfactions but these are, largely, beyond the scope of his study or are 'substitute goals or rationalizations which are a function of deprivation, not an independent variable in the motivation to work' (Ibid: 124). Baldamus's assertion here is just that, and he offers nothing by way of evidence or explanation. And of the three relative deprivations we might want to ask just how exhaustive, or even valid, these categories are; as noted above, we can see these concepts themselves as being attenuated reflections of important and relevant actualities in the world of work.

Are these not also 'invented definitions'? Baldamus assures us that we will have experienced these ourselves (and perhaps most of us will have) but his dismissal of satisfactions at work as 'substitute goals' is strange: are we all *only* motivated by maximising our takings from the effort bargain? If we are not, then surely what Baldamus himself has done is produced a trivialised version of the world of work. Again, this is not to dismiss the value of such effort; rather it is to highlight that even the most self-aware investigator of the world of work may face such problems: 'Trivialisation is thus to some extent the unintended product of disciplined sociological enquiry.' (Baldamus 1971a: 33)

Sociology of work changed quite dramatically in the 1970s with the adoption of two critical frames of reference: labour process theory and feminism. In their wake they brought power, oppression and resistance to the heart of sociological analysis of work. They also pushed sociologists to look at contexts wider than just the individual workplace, and to focus on the experiences of individuals particularly with respect to the emerging concept of identity in the late 1980s. Baldamus's work, with its focus on the effort bargain and orientations to work in the workplace would struggle to address these issues, although it clearly responded to, and to some degree set, the agenda of its day. However, the question remains as to whether contemporary sociology of work's focus on wider contexts and personal identity is what we should be doing. Are we really explaining anything in this way? Are we really getting to the heart of *why* things are happening? Or are we, perhaps, replacing analysis with description, and the realities of work with invented definitions? Work has changed and sociology of work has also changed – but perhaps neither of these things have changed for the better.

Chapter 4
‘Gi’ Baldamus, Karl Mannheim, and the Idea of a Social Science

Peter Lassman

Gi Baldamus was a member of Karl Mannheim’s pre-war Frankfurt seminar in the sociology of knowledge. As the product of a former member of that seminar Baldamus’s work can be understood as a continuation of many of the themes and problems that were of concern to this ‘other Frankfurt’. Our understanding of the history of the social sciences in the twentieth century has suffered from the fact that the ‘excessive attention which has been heaped upon the so-called *Frankfurter Schule* in recent decades has unfortunately tended to eclipse this ‘other Frankfurt’ (Pels 1993: 46). Baldamus himself refers to the ‘silent, yet deeply rooted *ideological* hostility between Horkheimer and Adorno on the one hand, and the Mannheim circle-which included Norbert Elias, Adolph Löwe, and Hans Gerth – on the other’ that persisted during the highpoint of the old ‘Critical Theory’ during the crucial period of 1930–1933 (Baldamus 1992: 102). A mark of the continuation of this silent hostility was evident to Baldamus 50 years later in the fact that, despite its encyclopaedic scope, neither Mannheim nor Elias are mentioned in Jürgen Habermas’s magnum opus *The Theory of Communicative Action* (Habermas 1984, 1987).

In a central section of *Ideology and Utopia* Mannheim argued that ‘it was not often noticed that a *Weltanschauung* is not of necessity a source of error, but often gives access to spheres of knowledge otherwise closed’ (Mannheim 1960: 150). This insight provides a clue to the problem that fascinated Baldamus throughout his career. In trying to make sense of the modern development of the social sciences he never lost sight of the controversial implications of Karl Mannheim’s sociology of knowledge. What happens when we apply the methods of the sociology of knowledge to sociology itself?

It was not without reason that the first page of Baldamus’s *The Structure of Sociological Inference* is devoted to a quotation from Hans Christian Anderson’s ‘The Emperor’s New Clothes’: ‘No one would have admitted that he could see nothing, for that would have shown that he was not fit for office, or that he was very stupid’. The question that worried Baldamus was that of the status and achievements, or lack of them, of sociology as an autonomous discipline. Has the development of sociology created anything more than a series of obscure language games? In particular, the complete absence of genuine discoveries seemed to be an obvious but generally ignored fact. Furthermore, if there were any social science discoveries they would be very hard to recognise as they would almost

immediately be accepted as common sense knowledge. The sociologist would be rebuked for telling us what we already know or think we know. As far as Baldamus was concerned the fundamental problem was to see if anything of value could be retrieved from the inherent vagueness, lack of rigour, and ideological grounding of all sociological inquiry both theoretical and empirical.

It has long been recognised that sociology, if conceived as the science of something we vaguely call 'the social' or of an object called 'society', seems to be caught in an inescapable conceptual dilemma. In its claims to be accepted as an autonomous discipline sociology oscillates between two extremes. At one extreme it can be seen to be claiming to explain all features of all kinds of human action; at the other extreme, it is only claiming to explain some features of some kinds of action. Clearly, the first is unsustainable while the second is not very interesting (Descombes 1989). But perhaps, despite these enormous weaknesses, it is possible that the sociological *Weltanschauung* had, after all, in its long-term development, made some unexpected and unnoticed discoveries. Baldamus, instead of quoting Hans Christian Anderson, could equally have quoted Karl Marx as a way of indicating the general direction of his thinking. Baldamus gave several clues that he was inspired by the example of Marx, who in his critique of Hegel claimed that he was revealing 'the rational kernel within the mystical shell' (Marx 1961: 20). This, of course, is to assume that a 'rational kernel' can be found in the 'sociological paradigm' and that, once found, it is worth keeping.

Baldamus's discussion of the fate of sociology was marked by a deep anxiety. This was that, on close inspection, the study of sociology amounted to nothing more than learning 'a more or less obscure language'. Are students of modern sociology being taught how to write 'a special kind of literary essay, thereby to acquire the skilful, imaginary and clever use of a specialised vocabulary, a sociological vocabulary'? Students become good at connecting unfamiliar words such as 'social order' with analogues and images such as 'structure' and 'system' but do not acquire any kind of genuine knowledge or understanding from it (Baldamus 1976a: 13). When teaching sociology, especially to non-specialists, it is very hard, given the apparent absence of genuine discoveries, to convince them that it is not simply redefining common terms and telling them what they already know or think they know. For Baldamus the implicit assumption that lies behind this complaint is the thought that the writing of literary essays is somehow an inferior activity when compared with a genuine science or any other kind of intellectual activity that has a well-defined method. These worries are founded on the recognition that we do not have anything like a body of uncontroversial scientific knowledge to teach and discuss. If, then, sociology cannot be defined in terms of its substance, perhaps the answer is to define it in terms of its method.

There is an idea implicit in Baldamus's scepticism about the state of sociology, in part inherited from Mannheim and his circle, that sociology somehow ought to have much more to offer by way of a genuine understanding of the state of social reality than is available elsewhere. Sociology in the Mannheimian sense answers these anxieties by proposing that it be defined in terms of its synthesising

mission. This idea of a mission is far more demanding than the Weberian idea of an intellectual 'vocation', and the accompanying high expectations are bound to be met with a general scepticism and will inevitably lead to disappointment. In addition, there is the difficulty that sociological ideas cannot escape their being rooted in our common sense perceptions and ideological commitments.

It was a central claim of Mannheim's that influenced all members of the 'other Frankfurt' that the peculiar position of social scientists is that they are both observers and participants in the continuing struggle between competing ideologies. Unfortunately, despite the plethora of programmatic statements there is very little if anything in sociology that lives up to the expectation that it is capable of delivering anything that can transcend these difficulties. These observations imply two questions. The first is simply: if this is right then how do we explain it? The second is: despite these sceptical observations, can we completely disregard the institutionalisation of sociology and the social sciences?

Baldamus's suggestion is that we ask the Kantian question: How is sociology possible? Sociology exists as an institutionalised reality, a fact that is not easily dismissed. There is an implicit element of Mannheimian historicism here: sociology exists as a set of practices that is itself open to sociological investigation. In addition, this must be done while avoiding the trivialisation and 'the erosion of social reality' that has characterised much of the mainstream of sociological inquiry. Baldamus's argument is that if we look hard enough at the inner workings of 'the sociological paradigm' that has gradually and in a totally unintended way established itself across all the competing schools of theoretical and empirical sociology we might find among them the elusive creation of the makings of genuine social knowledge founded upon a distinct, if implicit, method.

The opening sentences of the central chapter in Mannheim's *Ideology and Utopia* – on the prospects for a science of politics – state that the 'emergence and disappearance of problems on our intellectual horizon are governed by a principle of which we are not yet fully aware. Even the rise and disappearance of whole systems of knowledge may ultimately be reduced to certain factors and thus become explicable' (Mannheim 1960: 97). The paradigm case for this kind of investigation, for Mannheim, was presented by the history of art. In the same way as historians of art have sought to comprehend how different styles of art become dominant in particular periods, sociologists of knowledge ought to investigate how particular problems and disciplines come into being and then fade from view. The sociologist:

> in the long run must be able to do better than to attribute the emergence and solution of problems to the mere existence of certain talented individuals. The existence of and the complex interrelationship between the problems of a given time and place must be viewed and understood against the background of the structure of the society in which they occur, although this may not always give us an understanding of every detail. (Mannheim 1960: 97)

In making sense of such facts the sociologist of knowledge advances over the earlier works of the historians of art by always referring to the idea of 'the ever-present but constantly changing configurations of experience' in which human conduct acquires meaning and significance.

The problem of the relationship between social theory and political practice is the focal point for Mannheim's proposal for the sociology of knowledge. It is a strange but often overlooked fact that the central discussion in *Ideology and Utopia* is concerned with the prospects for a 'science of politics'. It ought not be too surprising that the core of Mannheim's sociology of knowledge was itself fashioned in the struggle between competing political ideologies in the Weimar Republic. Mannheim generalised from this experience to his interpretation of all intellectual constructs. The idea of the sociology of knowledge was, and, of course, still is highly controversial. Mannheim was correct to see that if sociology claimed to be capable of being 'the master science' of the study of man as a social and political being then it could not avoid challenging the knowledge claims of other disciplines and ideologies. Furthermore, as Mannheim realised, the ultimate paradox was that the method of the sociology of knowledge must apply to itself. It is, therefore, not surprising that Mannheim's argument for the sociology of knowledge played a central role in the 'culture wars' in Weimar Germany. Clearly, his understanding of the nature and scope of politics was, among other things, meant to counter both the ideas of the 'Conservative Revolution' and those of the Marxist left represented most forcefully by Georg Lukács. For example, although Mannheim set out to counter the ideas of Carl Schmitt on the shortcomings of parliamentary democracy and his concept of 'the political' he was strongly influenced by his attempt to think through the consequences of living in a disenchanted world (Gangl 2008; Lepenies 1998; Hoeges 1994). However, it was the philosophical presuppositions and implications of the sociology of knowledge that attracted most controversy (Arendt 1994). Here Mannheim, to a large degree in an implicit debate with Max Weber, was developing a standpoint that was quite distinct. The divergence between Mannheim and Weber is indicative of a much deeper disagreement about the nature and limits of sociology. Mannheim's idea of sociology as the 'basic social science' which has 'as its *raison d'être* the construction of a consistent theory of society' is at odds with Weber's deep scepticism about the possibility or even the need for the synthesis of conflicting standpoints (Mannheim 1953: 203). Baldamus, despite his strong sense of the incommensurability of diverse theoretical languages and its lack of scientific success in conventional terms was also, at the same time, still drawn to the Mannheimian vision of the uniqueness and promise of the sociological imagination.

The deep conceptual uncertainty at the core of the idea of a social science was Baldamus's central problem. Despite all the time and energy expended on so-called 'research' we still have 'no adequate understanding of what is really specific about sociology' (Baldamus 1976a: 19). Is social science, and social theory in particular, 'nothing more than a discontinuous sequence of ad hoc inventions thrown up by the intuitive inspirations of prominent scholars' (Baldamus 1976a: 55)? Apart

from any anxieties about the relevance of causal or interpretive methods there is a more fundamental ambiguity about the meaning of the concept of 'the social'. Max Weber had seen this very clearly. In a rarely discussed passage he makes it clear that the word 'social' has no precise meaning unless it is provided with an additional qualifier. According to Weber the idea of 'the social' has 'a quite general meaning' and 'turns out to have, as soon as one carefully examines its application, a particular specifically coloured though often indefinite meaning. Its 'generality' rests on nothing but its ambiguity. It provides, when taken in its 'general' meaning, no specific *point of view*, from which the *significance* of given elements of culture can be analysed' (Weber 1949: 68). But the problem is that in doing so, with the use of such terms as the 'socio-economic' or the 'socio-political', it runs the risk of 'theoretical trivialisation' which amounts to the 'cumulative attenuation' of theoretical focus (Baldamus 1976a: 33).

Baldamus was struck by the inherent vagueness of sociological vocabulary. He made it quite clear that although 'the word 'society' is nowadays constantly used by laymen and social scientists in all sorts of political, economic, and ethical debates' it is 'easy to see that it has no recognizable substance. Nobody knows what it means' (Baldamus, 1976a: 5). Sociologists cope with the predicament of having to deal with empty but nonetheless indispensable concepts by looking at the method through which they have been arrived at. Of course, following on from these considerations the meaning of the word 'method' in social science is not clear either.

In an unpublished paper, 'Mannheim, Putnam, Habermas', Baldamus expressed his debt to Mannheim. He asked here what the term 'sociology of knowledge' could possibly mean in the contemporary world. 'What would be its subject matter, its methods, its audience? Who would want to know, today, that the way in which knowledge is acquired, accumulated, used or exploited is potentially conditioned by non-cognitive interests?' Baldamus remarks that he often urged students and colleagues to 'return to Mannheim'. However, this was unsuccessful. This led him to raise the question of what the 'non-cognitive interests' were behind the reluctance to revive the sociology of knowledge.

The situation, he argued, was different for Mannheim.

> Academic sociology though less fragmented was conceived as a vocation. This made it possible to discover new sociological approaches which could throw light on non-cognitive social determinants of knowledge, as notably political, economic, and commercial collective interests. But Mannheim realised very clearly that to question *all* types of knowledge would have ended up in a futile *philosophical* undertaking. Therefore his sociology of knowledge was grounded in a careful and penetrating attack on the dominant (Kantian) philosophical defenders of claims for interest-free, a priori types of knowledge. (Baldamus n.d.)

One potentially explosive implication of Baldamus's development of the Mannheimian sociology of knowledge for modern times is that, in order to

demonstrate its significance, it could not avoid being applied to sociology itself. Although this could be an exciting and revealing activity it has all the ingredients of a logical nightmare. The findings of the sociology of knowledge presumably applied to itself. If the sociology of knowledge was to make genuine knowledge claims there could be nothing to prevent those claims from being investigated from the standpoint of its own method. If the task of the sociology of knowledge is to uncover the non-cognitive interests that condition all claims to knowledge then it itself cannot be immune from such investigations. Baldamus referred to this difficulty as the 'sociological circle'. The most obvious way in which social and political theorists attempted to evade the paradoxical implications of the 'sociological circle' was to import hidden restrictions into their claims to knowledge. Thus, Mannheim attempted to avoid the inclusion of natural science, mathematics, and sociology itself within the scope of his theory. Clearly, the focus of Mannheim's sociology of knowledge was political ideology. However, the disturbing implication is that the insights of the sociology of knowledge might reveal that 'the Emperor had no clothes'.

In combining intellectual honesty and the ability to make explicit the anxieties of many of the more introspective social scientists, it is to Baldamus's credit that he indicated some of the undeniable but usually ignored barriers that stand in the way of the construction of a genuinely scientific study of 'society'. For example, it is clear to anyone apart from the most dogmatic or blinkered practitioner that the social sciences in general and sociology in particular seem to have failed to produce even one genuine discovery or anything that remotely resembles scientific knowledge (Baldamus 1976a: 23–8).

If the sociology of knowledge is to apply to sociology itself then, Baldamus argued, it is most important to 'keep the circle safely out of sight'. Of course, there is an irony here. Once this problem has been recognised we cannot honestly pretend that it does not exist. But, Baldamus seems to be saying, we must act as if we can. Controversially, Baldamus constantly contrasts 'vague epistemological' debates with 'thoroughly sociological' accounts. In so doing, he is suggesting that it is possible to avoid the well-known philosophical difficulties involved in sociological explanations. There is a pattern here that Baldamus shares to some degree with Norbert Elias. This is not too surprising as the sociological criticism of other disciplines, especially philosophy, has a long history in German thought (Goudsblom 1995; Meja et al. 1990). The problem is, of course, that the sociological criticism of philosophy could not escape from itself relying upon a series of philosophical concepts and presuppositions. For Mannheim, in keeping with this German tradition of seeking to find sociological or political answers to philosophical questions the transcendence of the philosophical dialectic could be put safely into the hands of the new status grouping of modern intellectuals.

Baldamus's unearthing of Ludwig Fleck's long forgotten work on the discovery of a scientific fact provided one crucial part of the jigsaw puzzle (Fleck 1979). Inspired less by Kuhn's well-known theory of paradigms and more by Fleck's account of 'collective styles of thought' (upon which Kuhn had leaned heavily)

Baldamus argued that 'stripped of unnecessary controversies' the idea of a 'ruling paradigm' could serve to make some sense of the development of social science. The idea that the social sciences could be characterised in terms of the possession of paradigms in the strict Kuhnian sense remains doubtful. However, it did make some sense to say that from around the 1930s to the 1950s a common framework of ideas and assumptions did hold sway. However, the most significant aspect of Fleck's monograph in Baldamus's view was his idea of 'collective errors'. Despite his statements about avoiding unnecessary philosophical detours Baldamus argued that Kuhn had adopted 'Fleck's apparent anti-scientific nominalism' (Baldamus 1976a: viii). This accounted for the problem that Kuhn and his followers had in explaining scientific progress. On the other hand, Fleck had provided Baldamus with a vocabulary that spoke of 'paradigmatic deceptions, false presuppositions, and irreproducible methods by overstating the single element of 'error' (or 'falsehood')' (Ibid: viii). It appeared that Fleck's 'forceful overstatement' of the principle of collective errors had, in effect, undermined his commitment to the radical nominalism of his own theory of paradigms. As far as Baldamus was concerned this meant that 'the realities of cumulative scientific knowledge are bound to reassert themselves' (Ibid: viii). In fact it seemed that Fleck realised this in his later research. The main point of this for Baldamus was that, perhaps, and despite all doubts and criticisms, if sociology was to be something other than a series of vaguely connected language games then there must be some evidence somewhere of unintended cumulative scientific or epistemic progress. In plain words, do we know more now about 'society' than we did in the past?

Baldamus's discovery of the curious and unrecognised place of Ludwig Fleck's contribution to the sociology and history of science provided him with the conceptual resources to argue that, perhaps, the emperor was not completely naked. Of course, there is a big conceptual leap taking place here. Whatever the value or truth in Fleck's work the simple fact that cannot be denied is that he was a natural scientist, a biologist and medical researcher who was analysing one particular discovery in the natural world. The question is whether it makes sense to transfer this insight to an analysis of the nature of sociological knowledge. Baldamus clearly thought that it did, and that in so doing he could avoid all the controversial philosophical questions that have plagued the modern social sciences since their early beginnings in the eighteenth century.

One way of clarifying the existence of the structure of theoretical methods in sociology would be, following Fleck, to seek to avoid the direct search for objective knowledge and concentrate, instead, on the existence of observable sequences of errors. Despite all talk of avoiding controversial philosophical theories Baldamus was appealing in an informal way to the neo-realism of philosophers such as Gustav Bergmann. More specifically, and in terms of the sociology of knowledge, Baldamus found support in the work of Norbert Elias, his fellow ex-member of the Mannheim circle. Elias was known for his deliberately provocative criticisms of most of the prevailing schools of thought in modern social theory and philosophy. In particular, he questioned what he saw as its bias against the analysis of long-term

structural change and its uncritical acceptance of the use of well-worn dichotomies such as 'objective' and 'subjective', or 'structure' and 'action'. However, and more importantly, Elias proposed an analysis of long-term trends not only in the growth of knowledge but also in our knowledge of how to gain knowledge. That is to say, the sociology of knowledge was necessarily at the core of Elias's developmental and 'figurational' sociology (Elias 1978).

As far as Baldamus was concerned these suggestions of Fleck and Elias provided support for his claim that it ought to be possible to detect a 'body of institutionalized sociological theory' with 'a long history'. Here, 'the first clue for dissolving the paradox [of its methods] is the plausible assumption that the invisible methods, like all other knowledge, must have evolved through a prolonged process of development' (Baldamus 1976a: 49). Exploration of the concept of 'method' was Baldamus's first move in demonstrating that perhaps sociology possessed enough similarities with the progress of natural science to make a comparison between them plausible. At first glance, the concept of 'method' in sociology appears to be vague and lacking in clarity, to say the least. However, Baldamus's innovation was to apply the concept of method to theorising as well as, as most would expect, to empirical inquiry. Just what would a method of theorising look like?

Baldamus's central claim is that there is, in reality, only one method. This is exemplified by Max Weber's concept of ideal types. The sceptic is bound to question this. This rather innocent sounding appeal to Weber's ideal types is highly controversial and reveals something about the implicit theory that informs Baldamus's argument. Although Baldamus was more than familiar with Weber's work he states, surprisingly, in a rather offhand way that a 'superficial acquaintance with Weber's method', which can be obtained from the secondary literature, is sufficient to show that it is a method in the accepted sense. This is to say that it is 'a procedure *consciously* carried out, on the basis of explicitly formulated *rules'* (Baldamus 1976a: 9). The main secondary literature on the ideal type that Baldamus refers to is Talcott Parsons's commentary in *The Structure of Social Action* (Parsons 1937), itself heavily dependent upon the work of Alexander von Schelting (von Schelting, 1934). Parsons followed von Schelting in being highly critical of what he saw as the shortcomings of the concept of ideal types. He saw that if Weber's argument was to be followed to its logical conclusion then it would rule out the construction of the kind of social science that he had in mind. Weber's ideal type method, informed as it was by his historicism and sceptical nominalism, would produce, at most, a fragmented array of images of social reality rather than the systematic theory that both Parsons and Mannheim desired although in very different forms and for very different reasons. The kind of sociology that Mannheim and, by implication, Baldamus thought possible and desirable would be undermined.

This focus upon Weber's ideal type method rests upon the assumption that it is desirable to have a single method of theorising. Mannheim had argued this case in support of the autonomy and value of the mission of sociology. Baldamus argued that it is generally true to say that it is in the nature of science that scientists

hope or assume that there is only one correct way of proceeding in their inquiries. Given what we know about the nature of scientific inquiry this seems to be a rather questionable idea. Nevertheless, he points to some other generally unsuccessful attempts to establish a single method as supporting evidence for his thesis that there must be some central method and that throughout the history of sociology there has been a search for such a method. Most well known among these is Durkheim's *The Rules of Sociological Method* (Durkheim 1964); less well known is Znaniecki's *The Method of Sociology* (Znaniecki 1934). Of course, neither was really about method. They were more like exercises in the philosophy of social science than works proposing a usable methodology.

There are several things that need saying at this point. First, there is the proposal that social science is, in some sense, comparable to natural science. Even if we accept this highly controversial notion the question remains of whether it is true or desirable that it is an essential mark of a science that it must have only one method. Many historians and philosophers of science are more inclined to speak of the disunity rather than of the unity of science that was in vogue when both Mannheim and Baldamus formed their ideas of science. In order to understand Baldamus's search for unity it has to be remembered that it was formulated in the context of the constant assertion that sociology was experiencing some kind of crisis. Many different symptoms and causes of this so-called 'crisis' were mentioned in the relevant debates, especially during the 1960's and 1970's. As far as Baldamus was concerned the key feature of this perceived crisis was the proliferation of 'approaches', 'perspectives', 'strategies', and 'methodologies'. This seemed to imply that somehow sociology as a discipline had lost its sense of direction and had become the battleground for various competing political ideologies and philosophical theories. This was, however, as far as Baldamus was concerned, a rather superficial understanding of the true situation.

The casual appeal to Weber's ideal types served Baldamus's purpose by allowing him to argue that what made sociology distinctive was that it possessed a unique method of theorising even if it remained unrecognised and implicit. However, this rather misleading use of Weber's ideal type serves to hide a deep division in theoretical attitudes.

In Baldamus's terminology the Weberian ideal type represents an influential version of what he saw as the dominant nominalistic tendency in modern social theory. This is understandable. Weber's account of the ideal type stresses its pragmatic and hypothetical character. It must also be remembered that Weber did not think that he was saying anything new, merely making explicit and clarifying the way in which historians did, in fact, form and use concepts. The ideal type serves, Weber tells us, 'to develop our skill in imputation in *research*: it is no 'hypothesis' but it offers guidance to the construction of hypotheses'. Further an ideal type is a 'conceptual construct which is neither historical reality nor even the 'true' reality' (Weber 1949: 93). The point of the ideal type is lost if we fail to recognise that it is simply a tool to be used for practical rather than truly theoretical purposes in the classical sense. The aim is not to achieve some form of general scientific

knowledge of 'society' but, rather, to understand situations and developments that are historically unique. The aim is to help us to make clear practical judgements. We must be on guard against 'naturalistic prejudices' that confuse history and theory. This 'confusion expresses itself firstly in the belief that the "true" content and the essence of historical reality is portrayed in such theoretical constructs or secondly, in the use of these constructs as a procrustean bed into which history is to be forced or thirdly, in the hypostatization of such "ideas" as real "forces" and as a "true" reality which operates behind the passage of events and which works itself out in history' (Weber, 1949: 94). As far as Weber was concerned any attempt to deny this and to claim to have some kind of privileged insight into the true direction and meaning of history or of the nature of 'society' was nothing more than a 'romantic swindle'.

It is clear that much of Mannheim's thought can be seen as a debate with Weber. However, it is also clear that in this debate there are often significant but overlooked differences between them in their respective understandings of the nature and task of sociology. It is the influence of this specifically Mannheimian way of thinking that had a deep influence upon many of the theorists of the 'other Frankfurt' although most of them did change their ideas in significant ways when they were forced to emigrate after the establishment of the Nazi dictatorship in Germany. Weber would surely, despite his interest in many of the same topics and problems as Mannheim, regard the underpinnings of the sociology of knowledge with some degree of scepticism. Did it not share the characteristics of the 'romantic' or 'metaphysical' swindle involved in those theories that he was so keen to expose? Mannheim's sociology of knowledge involved an attempt to re-Hegelianise Marx comparable to that of Georg Lukács and the critical theorists of The Frankfurt School. Mannheim's sociology of knowledge draws very heavily upon a strong dose of Marxist historicism. The hold that these ideas continued to exert upon Mannheim's ex-colleagues is evident. At one point for example, Baldamus makes the extraordinary statement that as a consequence of their separation from classical economics most sociologists failed to appreciate the theoretical advances of Malthus and Ricardo that culminated in 'the discovery of aggregate surplus value by Marx and Engels' (Baldamus, 1976a: 26).

Perhaps it is meant as a paraphrase of Weber's view of concept formation when Baldamus wonders whether the idea of the existence of theoretical methods in sociology might not be a fiction. Could it be that 'the development of theory is nothing more than a discontinuous sequence of ad hoc inventions thrown up by the intuitive inspirations of prominent scholars' (Baldamus 1976a: 33)? The obvious question to ask is: what is wrong with this? Is it such a bad thing if what we call sociology is itself nothing but a name for various, often barely related, kinds of inquiry that are always themselves reflections in some fairly straightforward sense of the 'practical cultural problems' of the age in which they are formulated? Clearly, Baldamus felt deeply troubled by this suggestion. If this was an accurate description of the true state of affairs then he feared that sociology would lose

any kind of claim to disciplinary identity that it might have ever entertained. The emperor would really be naked even though we do not like to admit it. Could it be just one vast 'collective error'?

Baldamus's answer to this predicament is as brilliant as it unexpected. His basic claim is that beneath the swamp of 'the allegedly intuitive thought processes' that produce conceptual vagueness and philosophical fuzziness there is 'an element of hardness' to be found. This previously unnoticed 'hard element' consists of 'recurring and reproducible components that may suitably be called 'structural' elements in the praxis of theorizing'. Looking at the major works of sociological theory it is possible, Baldamus claims, to extract a common pattern of concept formation marked by 'the formation of classificatory schemata' (Baldamus 1976a: 33). If this is true, is it enough to clothe the emperor?

The answer that Baldamus provides seems to be that while the Emperor might not be completely naked there is not much left to cover him. His clothing, such as it is, is decidedly skimpy. The fundamental assumption that Baldamus clings to is that with the decomposition of sociology as a unified discipline there still might be a 'non-verbal' residue. What this might mean is that sociological inquiry, despite all of its epistemic uncertainties, is unavoidably a part of what it studies. As such it cannot but find itself expressing, in very different and undisclosed ways, the development of societal complexity. The hope is that 'by long-term trial-and-error methods intended to articulate social reality ... reflect as it were developments in the asymmetric structure of *society*, 'structure' understood in a non-nominalistic sense' (Baldamus 1976a: 211).

Of course, it can be argued that Baldamus's own presuppositions, largely influenced by the Mannheimian idea of a social science, were too demanding. The idea of sociology as a kind of synthesising 'super science' while possibly attractive in the abstract was always bound to be disappointing in terms of what it could, in reality, deliver. In addition, the political implications of such a 'super science', if it were to exist, would be the object of endless disagreement. In addition, Baldamus's account contains an implicit bias against the idea that the social sciences, and sociology in particular, ought to be thought of as humanistic disciplines that owe more to the study of history and philosophy than they do to natural science.

This is curious because when we look at the core of the method that Baldamus detects in sociology it is essentially the pragmatic experimentation with what are in essence literary forms. The pleonasms, paradoxes, and metaphors that he points to have played an undeniable role in the history of sociology. The problem, however, is to demonstrate that they are distinctively sociological. For example, Herbert Marcuse's concept of 'repressive tolerance', to which Baldamus frequently refers, appears in an essay that has no sociological content in Baldamus's strict sense of the term. There are hints, in the concluding sections of *The Structure of Sociological Inference*, that methods of cross-classification are not really specifically sociological at all, but rather a general form of reasoning that some sociologists happen to have used at a particular time. Be that as it may, Baldamus

has challenged everyone working in the social sciences to think hard about what they are doing, what they are teaching, and with what authority they make their pronouncements. Perhaps the last laugh is on them. Maybe the Emperor really is naked. And if he is, so are we.

Chapter 5
The Structure Of Sociological Inference: A Forgotten Classic?

Charles Turner

Introduction

A classic work should found something: a school, a field of inquiry, a method, a tradition. The classics of literature tend to be founders of discursivity: they are never mute, one goes back to them constantly, perhaps for something we call truth, perhaps for a source of meaning or inspiration or world-orientation. We are never merely reading a literary classic but always rereading it, because it has never finished saying what it wants to say; what it discloses may vary, too, with the needs of the time in which it is reread (Calvino 1999). The classics of the natural sciences may be called founders of trans-discursivity, that is, they are works that are largely beyond discussion, that nobody needs to read because they are definitive conclusions or solutions to problems that establish a body of knowledge and provide a framework for the pursuit of knowledge in the future; if their basic assumptions are later called into question – or if there is a scientific revolution – this will happen on the basis of evidence or theoretical innovation and not because someone rereads Newton or Copernicus. The classics of the social sciences and philosophy exist somewhere between literature and science, discursivity and trans-discursivity; they all exhibit a striving for definitive knowledge but have been kept alive beyond their own present not only by the continuing use of an established method but also by debates about what they mean. Marxism and psychoanalysis are the most obvious intellectual movements to have profited from this dual status, relying on the idea of a rigorous, scientific method – dialectical materialism, the science of the psyche – while endlessly rereading and arguing about the masters' writings (Baehr 2002; Lepenies 1986).

I am not sure that *The Structure of Sociological Inference* (hereafter *SSI*) is a classic in any of these senses; indeed, more than 30 years after its publication, Google Scholar – how Baldamus would have loved it! – lists only 30 or so citations; to call it such one would have to extend Calvino's idea and say that there are classics that need several decades even to start saying what they want to say. Delayed recognition may also be a feature of prophetic works, though one hesitates to call it that either; it is hardly an attempt to restore sociology to the law of its founders, although it does contain what now look like warnings of what was to come; there is an anticipatory critique of interdisciplinarity, and when one reads

his analysis of functionalism one does not have to do much interpretative work to conclude that it is still being practised by far more people than would admit it.

It does lay down a method of inquiry, but more for the history of sociology rather than for sociological inquiry itself. And this method is, to say the least, idiosyncratic, entailing a willingness to find the key to an author's thought in randomly selected passages of text, and the reconstruction of difficult or incomprehensible theories by means of diagrams which are then deconstructed (Baldamus 1976a: 171, 181, 184). When one reviewer predicted that 'his syntheses and insights should have long-term positive consequences for sociology', he could not have been more wrong (Schumsky 1977: 722). Barry Hindess's dismissive judgment of 'a distinctly odd book' (Hindess 1977: 513) seems to have been shared by many: fans of *SSI*, even in sociological theory, are few and have been less fulsome in their praise of it than have industrial sociologists of Baldamus's earlier *Efficiency and Effort* (1961a).

In retrospect it is not hard to see why: had its insights and warnings been taken seriously by sociologists, many of them would have long since questioned the point not only of their research but of their professional existence,[1] while courses in sociological theory would be less likely to include references to Giddens or Bauman or Beck. As it is, one of the most brilliant and challenging accounts of what sociology is about has gathered dust in many a university library, as lesser names are repeatedly checked out and sometimes even read.

1 I was reminded of the warnings recently when my own university, Warwick, inaugurated a 'writing prize', to be awarded to the author of the best book, irrespective of genre, published in a given year on a topic of current interest. In 2009 it was 'complexity'. Now 'complexity' has also attracted the attention of prominent sociologists: 'Racing across the world are complex mobile connections that are more or less intense, more or less social, more or less 'networked' and more or less occurring 'at a distance'. There is a complex world, unpredictable yet irreversible, fearful and violent, disorderly but not simply anarchic. Small events in such systems are not forgotten but can reappear at different and highly unexpected points in time and space. I suggest that the way to think these notions through is via the concept of global complexity' (Urry 2003: x). Published in 2003, Global Complexity was ineligible for the Warwick prize but has already earned more citations than SSI. Although we can never know exactly what Baldamus would have made of this, we can, I think, guess … more or less: 'To the trained sociologist the mere suggestion that the complexity of society might be as real as the structure of atoms is unthinkable. Yet the idea of complexity is today among the most emphatically emphasised characteristics of the sociological discourse' (Baldamus 1976a: 55). As we will see, the idea of complexity was not the only one on which Baldamus had something prescient to say more than 30 years ago.

Theorists at Work

At the heart of *SSI* lies a deceptively simple question: is there something we can call a sociological method, a common mode of reasoning that is employed by both theorists and empirical researchers throughout the discipline? Is there, in other words, a sociological paradigm? So used have we become to the idea that sociology is either a broad church or a tangle of different and conflicting traditions that the question sounds absurd. But in the early seventies it was in the air, prompted by the reception of Kuhn, and also by interest in Lakatos, Feyerabend, Popper and Michael Polanyi (Martins 1972). Typically, Baldamus took less from these familiar names at well-endowed institutions than he did from the neglected work of Ludwik Fleck, a Polish medical scientist from Lwów, whose *Genesis and Development of a Scientific Fact* had appeared in 1935, seven years before he found himself a prisoner at Auschwitz. For Fleck, progress in science occurs not by means of consensus periodically shattered by scientific revolution, but by trial and error, wrong turnings and blind alleys (Fleck 1979; Cohen and Schnelle 1986). If that was true of the natural sciences, how much more so might it be true of the social sciences?

Published in 1976 when he was already 68, *SSI* represents the culmination of Baldamus's thoughts about this subject over the previous decade or so.[2] During that time he had read intensively in the philosophy of the social sciences, but he also brings the industrial sociologist's sensibility to bear; his question was not so much about the ontological or epistemological principles of sociology or its normative core, as about the working methods, both formal and informal, through which sociology is actually carried out.[3] Although an interest in informal methods and tacit knowledge is a prominent feature of the philosophy of science, Baldamus had fastened on it long before *SSI*; in an article from 1957 about production and personnel managers' introduction of piece-rates in Birmingham factories, he remarked on 'the contradiction between the claims which the practitioners make of scientific accuracy and their general admission of the extent to which their practice requires intuitive judgments' (Baldamus 1957: 193).

In his first papers devoted to the practice of sociology he puts the matter in similar terms:

> … it is obvious, at least superficially, that scientific work has something in common with administrative organisation … empirical research-activity and theory construction alike may be seen to be a specific form of discontinuous trial-and-error processes similar to the kind of decision-making activities with

2 The title is an obvious allusion to Mary Hesse, *The Structure of Scientific Inference* (Hesse 1975).

3 Baldamus did not share the Marxist view of theorising as 'intellectual labour'. He had long since rejected the idea of abstract labour even when analysing different types of work in the same factory (Baldamus 1951a).

> which we are familiar in the realm of administration in business, government and so on. (Baldamus 1984b: 279–80)

At the same time he developed an image of sociological theorising for which he became more well-known, again taken from the world of work, that of double-fitting: the theorist is not so much an administrator engaging in trial-and-error as a carpenter who, in trying to shape the door to fit the frame, unavoidably finds himself or herself shaping the frame as well (Baldamus 1972a).

It should be added that some of the flavour of *SSI* was already there in his earliest German papers from the 1930s, published oddly enough in *Ständisches Leben*, a journal edited by the Austrian conservative Othmar Spann. In his first, 'The Self-Destruction of Liberal Political Economy' he writes that 'the driving forces of the development of a science are not rooted solely in the realm of the intellect', an observation that foreshadows his later interest in informal methods of reasoning in all fields. Liberal political economy is subject to a 'deadly paralysis of cognitive creativity' that arises from its own subjectivism, perspectivism and ideas about value freedom.

> The individualist view of life comes to grief when it splits up into a chaos of numerous autarkic meanings for one and the same reality, and on an increasingly widespread anxiety concerning the load-bearing capacity of its foundations, an anxiety the resistance to which leads of necessity to helplessness and to a deeper entanglement in the relativity problematic. If thinking this question through to its self-destructive conclusion is bound to be ponderous and slow, this is explained psychologically by the fact that specialised disciplinary knowledge, which has accumulated over a century and a half and which is constantly developing, stands before us as something apparently self-evident and unchallengeable … (Baldamus 1933: 56)

In a letter from December 1986 he recalls this first paper, observing – with characteristic pedantry – that he had reread it 'in February 1982':

> … to my surprise, there was not a single word in it which I would have wanted to change. So here was a piece of enduring knowledge. And yet it is totally obsolete. With historical hindsight my (rather arrogant) prediction that the ruling paradigm of liberal (neo-classical) economics was in a state of self-destruction was plainly mistaken.[4]

This is typical Baldamus. Sometimes in the course of a single paragraph, doubts about being able to establish any sort of knowledge will be followed by an audacious knowledge claim. In the essay on the category of pragmatic knowledge he writes that extending the sociology of science to social science is 'a formidable

4 Letter to Irving Velody, private correspondence.

task of quite unexpected proportions, a task which is certainly unmanageable at present' (Baldamus 1984b: 279); discussing theoretical methods in *SSI* he writes that 'next to nothing is known of such methods' (Baldamus 1976a: 49), and that the development of theory may well be 'nothing more than a discontinuous sequence of ad hoc inventions thrown up by the intuitive inspirations of prominent scholars'. Yet he then allows himself the opposite conclusion:

> … as it happens, despite the large number and the diversity of topics that are covered by courses entitled 'Sociological Theories and Methods', there is only one single method. I know this sounds incredible. … This is Weber's celebrated method of constructing ideal types. … The professional theorist will readily agree. (Baldamus 1976a: 9)

The reason that it sounds incredible is that it is surely wrong; it may be true that all theorists resort to typologies, but not that they all use Weber's ideal type method; there is a significant difference between the ways in which typologies are deployed, corresponding to differences in the intellectual sensibilities, styles of thinking. At least three of these – classificatory, dialectical and ideal typical – may be identified. In fact, Baldamus explicitly recognises the distinction between the first two, that is to say between Parsonian/Kantian (classificatory) and Marxian/Hegelian (dialectical) theorising, but does not even discuss the third (ideal typical), as though it were simply the name of a master concept that embraced the other two.

Systematics of Theorising versus History of Ideas

That he should have chosen to regard this difference as unimportant is surprising in view of the fact that, at the start of the book, he suggests a very good reason why it might arise:

> Although the word 'society' is nowadays constantly used by laymen in all sorts of political, economic and ethical debates, it is easy to see that it has no recognizable substance. Nobody knows what it means. (Baldamus 1976a: 5)

As soon as one begins to theorise about society, for instance by saying Gemeinschaft and Gesellschaft, or agency and structure, or church and sect, one confronts the problem of 'dividing unknown entities'. Society is not like an apple that you can cut in half and study; that is not what is happening when a theorist operates with a conceptual dichotomy.[5] Today's reader should bear in mind that, although Baldamus did not intend this to be a purely epistemological point, it was made at a

5 The fact that Baldamus's reflections on the role of dichotomies in sociology are among the most important we have does not appear to have impressed the 24 contributors

time when sociologists were still interested in the philosophy of science and social science. When he says that 'society' has no substance and that nobody knows what it means, he is making a statement that is as much about the systematics of theorising as the history of ideas. To be sure, he wondered whether this distinction, introduced by Robert Merton, 'makes any sense at all' (Baldamus 1976a: 87). But he does discuss it, and does so because he still wants to ask whether sociology might be considered an explanatory science with a distinct set of conceptual tools, and if not, whether there is at least something resembling a sociological paradigm.

Today, it is not easy to appreciate the register in which he says that nobody knows what 'society' means; in the last two decades, all sorts of doubts have been expressed about the status of 'society' and 'the social', but in an entirely different, naïve realist idiom, one that has arisen as sociological theory has been supplanted in the minds of many by a more nebulous 'social theory', and interest in the philosophy of the social sciences – as well as in industrial sociology! – has waned. For this social theory, the distinction between systematics of theorising and the history of ideas is never even recognised as a possibility; instead, one simply says that 'society' is an entity which once did have some real solidity (say in America and Europe in the 1950s, or, for followers of longer-term trends, during European feudalism) but is now breaking up or dissolving or liquefying; or that globalisation has made the study of societies as bounded entities obsolete. In either case – society superseded by the post-social, or society superseded by globality – this is taken to mean that, although we may well need new tools, finding them is a matter of having a feeling for the times in which we live rather than mastering a particular disciplinary technique or skill. This new sensibility is expressed well by one of its representatives:

> If there are no longer any powerful societies then I try to establish what new rules of sociological method and theory are appropriate. In particular I elaborate some of the material transformations that are remaking the 'social', especially those diverse mobilities that, through multiple senses, imaginative travel, movements of images and information, virtuality and physical movement, are materially reconstructing the 'social as society' into 'the social as mobility'. (Urry 2000: 2)

While there are some who will want to say that this is not really theorising at all, I suspect that, had he been here to read it, Baldamus, tongue firmly in cheek, would have called it a major turning point in the history of sociology. But how can we know whether such turning points occur, how can we know whether sociology has produced lasting achievements?

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to the 433-page collection published in 1998 as Core Sociological Dichotomies, none of whom mentions him (Jenks 1998).

Theorising and Empirical Research: Double-headed Functionalism

Imre Lakatos's most notable contribution to the philosophy of science was the distinction between progressive and degenerative research programmes; his most notable contribution to the philosophy of the social sciences was to say that all that they could offer was degeneracy. In 1970 he writes that 'the methodology of research programmes might help us in devising laws for stemming this intellectual pollution which may destroy our cultural environment even earlier than industrial and traffic pollution destroys our physical environment' (Lakatos 1978: 89) Two years later Stanislav Andreski, operating from within sociology, wrote that the social sciences were the toughest disciplines, fit only for the most intellectually talented, but had degenerated through an expansion that left their doors open to the talentless (Andreski 1972).

Baldamus writes in the shadow of such observations. He quotes Lakatos's claim that social theory is 'a patched up arbitrary series of disconnected theories', and suggests that it be taken seriously. But he does so in order to show 'why for the most part it is wrong' (Baldamus 1976a: 57). Admittedly he does give Lakatos more than a run for his money. Lakatos asks: 'what *novel* fact has Marxism predicted since, say, 1917?' (Lakatos 1978: 88); Baldamus writes: '*there are no known sociological discoveries* … one only has to glance at the alphabetical author index of an introductory textbook, make a list of the most frequently quoted sociologists and then ask oneself: what did they ever discover?' (Baldamus 1976a: 25) Instead of discoveries comparable to those that are indubitably made in the natural sciences, it appears that 'the development of sociology is riddled with verbal innovations of a very fast rate of obsolescence.' (Baldamus 1976a: 37) Today, thirty years on, a glance at Google citation counts would seem to confirm the truth of both of these remarks.

Despite this, Baldamus claims that *The Structure of Sociological Inference* is not a dismissive book; that sociology may be capable of cumulative knowledge and of making discoveries, albeit that this will happen unintentionally, via a process of wrong turnings and mistakes. However, the central point is this: if we want to know whether there are any lasting achievements in sociology – and 'we must always remember that there may not be any at all' (Baldamus, 1976a: 119) – then we need to look in two, and only two places: its most robust theoretical statements and its most impressive empirical research over a long period. What we cannot do is investigate work that lies somewhere in between, because that is the region of those short-lived verbal innovations; these might inform us about the ideological biases of the age in which they were produced, but they will tell us nothing about long-term trends and hence nothing about possible achievements. Unfortunately, this region contains most of sociology.

This rejection of middle-range work explains why the material in *SSI* is so sparse for a study that makes such large claims. Much of it is devoted to grand theoretical statements like Parsons's *The Social System* (Parsons 1951) and Marx's *Capital* (Marx 1954) and empirical works like Goldthorpe et al's *The Affluent*

Worker (1968) and Ferdinand Tönnies' 1927 study of suicide in Schleswig-Holstein (Tönnies 1927). In the case of theory, he is interested in the technical aspects of Marx's and Parsons's use of concepts, in particular, whether the use of conceptual dyads and dichotomies can help the theorist arrive at something like discoveries. In the case of empirical research, he is interested in how it was that Tönnies, for instance, began his studies of suicide and of criminality with 44 explanatory independent variables and managed to whittle the number down to half a dozen, concluding that age, sex, class, urban and rural residence, education and religion are the most important factors in socialisation. The remarkable answer is that this selection of independent variables owed nothing at all to the elaborate theoretical apparatus that Tönnies himself had set down in his *Gemeinschaft und Gesellschaft*, itself a classic example of a delayed classic (Tönnies, [1898] 1955); instead, it owed everything to the practical and immediate problem he was addressing as an empirical researcher, in other words to his choice of dependent variables, such as rates of suicide in urban and rural settings and in men and women. By the same token, when theorists try to say something concrete about the social world, they make no more use of the most sophisticated empirical research than the empirical researchers make of the most sophisticated theories.

In fact, high-level theorising and low-level fact-finding are virtually autonomous domains of activity. The split between them has often been seen as a problem, with all manner of proposals being put forward to combine them. These proposals continue to this day but mostly in ignorance or neglect of Baldamus's point that this is pointless without addressing the real problem, which is not the split itself but the fact that both, in their own way, are prone to *error*. Combining error-ridden theory and error-ridden empirical research can only lead to an accumulation of errors, not their abolition. Two wrongs not only don't make a right, they take us further away from it, and faster.

Now error is not the worst problem that sociology could have, for 'error, in contrast to ideological biases, can be corrected' (Baldamus 1976a: 104), though how exactly is not made clear. It is the ideological biases that drive the work that takes place in the region between high theorising and empirical research, where sociology takes the form of advocacy supported by verbal innovations that pass for theorising and by bits and pieces of empirical research that are inserted where they can be made to fit. Here, what a research community considers important is driven by these uncorrectable ideological biases, so that 'the manner in which a problem is selected and inflated into a major issue of great significance is so unobtrusive and concealed that even the specialists in the same field are fooled by its apparent plausibility' (Baldamus 1976a: 59).[6] In an earlier working paper he had put the point more forcefully, in a formulation that ought to be required reading for those embarking on a career in sociology today:

6 Here readers can insert their own examples. One candidate might be 'mobilities' research, a field of inquiry boldly taking sociology beyond societies and into a world of global complexity. For a tourist gaze through the rear-view mirror see Urry 1998.

> Once the paradigmatic aspects of theoretical and empirical sociology have been identified, it is not difficult to account for the large residual of sociological output in the middle range of abstraction. Here we find writers who utilise and amalgamate in all sorts of ways some of the products of the two paradigms. Their methods and techniques are less rigorous, less specific and less standardized and their involvement in public debate on issues of major social and political importance is more openly disclosed. This is an area where the traditional preoccupation of the sociology of knowledge with systematic bias, ideological distortion and perspectivistic thought styles is readily applicable. (Baldamus, 1971a: 10)

If this looks like a case of Mannheimian bad news, the good news is not much better, for it turns out that the chief error to which both serious theorising and rigorous empirical research are prone is 'the erosion of social reality'.

Baldamus does not use the phrase 'degenerative research programme', but in addition to 'the erosion of social reality' he does speak of 'attenuation' and processes of trivialisation; all are unwanted methodological effects that can arise in the course of conducting an inquiry, be it theoretical or empirical. The theorist, for instance, will begin with a vague and general category and seek to make it more concrete by means of various theoretical devices, but the greater precision achieved as a result may be a precision of definition only, taking the theorist no closer to social reality than he was at the start; the empirical researcher may begin with something rather concrete and objective, if ill-defined, but in the course of the research process end up with a body of data that makes the causal explanation of social processes less possible than it was at the start. A notable example of this latter was the attempt before World War I by the *Verein für Sozialpolitik* to establish the psycho-physical causes of work behaviour by means of a massive accumulation of statistics, 'probably the most disastrous collective error in the history of social science'. Studies like *The Affluent Worker,* by contrast, succumb to error by reducing hard and 'non-mental' economic realities to 'perceived perceptions' of those realities by actors.

So are there in fact any sociological discoveries? There are of a sort, because by the end of the sixties something like a paradigm had emerged in sociology – functionalism. This is well known, but nobody before or since gave such an ingenious or apparently unbiased account of the trend towards functionalism: 'our principal criterion for the evaluation of trends is not the validity of empirical concepts or observations, but merely the changing frequency distribution of institutionalised (sociological) "activities"' (Baldamus 1976a: 158).

The important point here is that functionalism had what he calls a 'substantive core', a theory of primary socialisation, which emphasised the role played by factors such as age, sex, family and education in the formation of the person. The independent variables that Tönnies appealed to in order to make sense of suicide in Schleswig-Holstein – sex, class, age and so on – are routinely referred to in survey research to this day, with some new ones – such as ethnicity or sexual orientation

– added with changing times and a changing sense of what common knowledge requires us to be interested in. This theory of socialisation, which seems so banal to us today, has to count as a kind of discovery because it is almost entirely absent from sociological classics such as Marx and Weber, who theorise the social world as though it is populated from the start by fully formed adults. If, as they say, man is a societal being by definition, then 'the socialization process is superfluous' (Baldamus 1976a: 160). But the remarkable thing for Baldamus was that it seemed that this theory of socialisation had been arrived at by both grand theory and survey research over the course of several decades via entirely independent routes. This means that 'the coexistence of two versions of one and the same theory with identical value premises must rank among the most extraordinary events in the history of the social sciences' (Baldamus 1976a: 125).

That is one of those brilliant and baffling claims that, it must be said, Baldamus was rather too fond of. And in any case, he documents the implicit theory of socialisation at the heart of functionalism far more comprehensively for empirical research than for sociological theory. There is, for instance, only a passing mention of what Parsons took from Freud. When he writes about theorising he is far more concerned with the logic or illogic of theoretical categories. Here, despite his suspicion that the systematics of theorising and the history of ideas may not be separable, and for all the brevity of his account, it would to be hard to name anyone who has taken the systematics of Parsonian theorising as seriously.

Categories and Hierarchies in Theory

The core of functionalist theorising consists of two apparently simple devices, but ones that do an enormous amount of work, enabling the theorist to overcome some of the inevitable vagueness with which all inquiry into that unknown object, society, must begin. Firstly, dualistic constructions in the form of equivalent dichotomies are deployed in 'complex schemes of cross-classification'; secondly, apparently equivalent dichotomies may be arranged hierarchically. Dynamism may be introduced through a third move, giving priority to one side of a single dichotomy and using this as a kind of master dichotomy. That does not sound like much, and had Baldamus been more interested in the cultural anthropology of classification he might have been better equipped to make it sound like more.[7]

7 He does refer to this material, in his discussion of trichotomies. Parsons's distinction between cognitive, evaluative and cathectic faculties, for instance, is said to date back at least two millennia. Baldamus might, though, have added a reference to the tripartite distinction between ideology, politics and economics that became a staple of sociology and that can be traced back to indo-European mythology. As it happens Baldamus thought that triadic structures are 'too stable, too restrictive, to be combined with complex ranking procedures. This manifests itself also in the triads-within-triads system of Hegel's Logic, a system that owes its dynamic flexibility entirely to linguistic devices' (Baldamus 1976a:

As it is, in order to give it significance he relies on some intuitive, non-theoretical resources of his own.

Cross-classification is useful because a single concept is inevitably vague – think of globalisation, or capitalism, or patriarchy, or modernity; a conceptual dichotomy is less vague – think of reason/madness, agency/structure, church/sect, capital/labour, male/female; but a combination of two dichotomies is less vague still, giving you four boxes, the content of each of which should be twice as concrete as the content of two. Baldamus thought that pretty much anyone who had had anything distinctive to say in sociology had employed such devices in one way or another, whether or not they were grand theorists and whether or not they themselves acknowledged it. There is more truth to this claim than there is to the claim that everyone makes use of Weberian ideal types, but also more truth to either than to the speculations at the end of the book about a genetic propensity towards dichotomous thought.

One of his favourite examples was Marcuse's concept of 'repressive tolerance'. Marcuse was not a particularly sophisticated sociological theorist but Baldamus sought to show that 'repressive tolerance' was sophisticated enough to be reconstructable; it 'resulted' from the cross-classification of two asymmetric dichotomies, repressive-permissive and tolerance-intolerance. Initially this gives four boxes containing two pleonasms – repressive intolerance and permissive tolerance – and two paradoxes – permissive intolerance and repressive tolerance. The second paradox was fastened on by Marcuse and became one of those verbal innovations that is fashionable for a time. Although he does not offer any other examples of paradoxical formulations that have caught the imagination of sociologists for a limited period, they are not difficult to find: MacIntyre's 'bureaucratic individualism', Riesman's 'lonely crowd', Sennett's 'destructive Gemeinschaft', Rieff's 'negative community', Foucault's 'pastoral power', Bourdieu's 'symbolic violence'. There are of course thinkers of some renown who seem to prefer pleonasms, like 'liquid modernity'; and there are even prominent philosophers who satisfy themselves with one dichotomy alone and seek profundity in a simple contradiction, like 'inclusive exclusion'.

Parsons's theorising was and would have been more interesting to Baldamus than any of these; many of them may well have offered more insight into problems facing modern people, but none of them could match Parsons for the relentless way in which he employed such cross-classification devices; another element of fascination was 'the mysterious contrast between a highly formalized structure and the complete lack of any recognizable and reproducible method in its construction'. For instance, the pattern variable schema of *The Social System* consisted not of two but of four dichotomies each of which defined an action orientation choice:

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212). This is why, he says, in the most gnomic statement in the book, that 'triads have become obsolete' (Baldamus 1976a: 213). Their obsolescence did not prevent Baldamus from devoting two scandalously neglected papers to Habermas's use of triads.

universalism/particularism
achievement/ascription
specificity/diffuseness
affective neutrality/affectivity

Typically Baldamus says both that we have no idea where Parsons got them from, and that they are all a variation on Tönnies' *Gesellschaft/Gemeinschaft* distinction. That aside, key here is that Parsons tried various ways of representing the combination of these diagramatically as a series of nested boxes, a central feature of which was that he ended up displaying all sixteen of their possible permutations and combinations. Now these diagrams were seen by Baldamus not as a sign of cognitive weakness, nor even as a mere illustrative device, but as the key to the Parsonian theory; moreover, he seems to have thought that one could construct such diagrams for the work of any other theorist and that if you did so you would have a fair clue to the structure of sociological inference the theory contained.

At the same time, such diagrams are never innocent, and can conceal as much as they reveal, a point that Baldamus himself might have taken much further. For instance, the diagram on p.110 suggests that you can end up with 16 possible permutations and combinations of the four dichotomies no matter which two of the four you place on the outer rim of the diagram's frame. Because of this, what we might call the limited visibility of an implicit conceptual hierarchy, Baldamus rearranged the pattern variable choices as a tree diagram (Baldamus 1976a: 113). This manages to make a number of matters clear that are not so clear from Parsons's own mode of presentation, but it also introduces new and unnecessary mysteries. Firstly, a tree diagram consists of several levels at each of which the number of branches increases as another level of possibility is introduced; in this case the possibilities are doubled each time; secondly, a tree diagram proceeds down the page, which makes it easier to represent conceptual hierarchies and the idea of levels of abstraction; thirdly, however, at the bottom of the diagram are the sixteen possible combinations of choices all of which appear to have the same status, that is, to be equally possible.

Now Baldamus' tree diagram complicates matters in all sorts of ways, in particular by including two distinctions before the pattern variables even appear, with two on the third level (affectivity/affective neutrality, diffuseness/specificity) and two on the fourth (universalism/particularism and achievement/ascription). For us today the question of whether he was faithful to Parsons does not matter; far more important is that he thinks that by presenting Parsons in this way he can show that Parsons suppressed the idea of hierarchical levels of abstraction:

> … all the pattern variables are of equal rank in their relative degree of abstractness. Indeed it is precisely this implied notion of equivalent dichotomies that highlights the uniqueness of Parsons's methodological achievement. None before him had recognised the possibility of defining infinitely complex theoretical concepts in terms of dichotomies by using them interchangeably on a given level of abstraction. This achievement could be carried through only at the

> expense of forcefully suppressing the competing methods of hierarchical levels of abstraction. (Baldamus 1976a: 113)

This means that, although the tree diagram allows us to see the surprising fact that Parsons attached greater importance to the first two pattern variables – affectivity/affective neutrality, diffuseness/specificity – than to the last two – universalism/particularism and achievement/ascription – Parsons himself did not acknowledge this.

Here I think that Baldamus misleads the reader. Clearly he was trying to get to the core of *The Social System* via what he himself regarded as a legitimate shortcut that would save wading through the whole text, a method that he repeatedly recommended to any reader faced with hundreds of pages of difficult or incomprehensible prose, especially if it was German. But this leads him to neglect the existence of one chapter entitled 'Types of Social Structure', in which Parsons is quite openly hierarchical, completely abandoning the affectivity/affective neutrality and specificity/diffuseness pattern variables, and operating with just four types of social structure generated by the cross-classification of universalism/particularism and achievement/ascription. This enables him to assign actual societies – China, Russia, South American societies, the Western democracies – to the boxes, with any one society only being allowed to go into one box. In other words he creates a classic classificatory scheme. The typology that allowed Parsons to arrive at it is not itself a classification, rather the classification is generated by the cross-tabulation of pattern variable typologies. But note that it is a classificatory, and not an ideal typical analysis.

Baldamus's tree diagram could not show any more than Parsons could in his own diagrams which of the sixteen possibilities, each of which has equal status on the row at the bottom of the diagram, was socially or historically significant. They are simply logical possibilities derived by combination of the pattern variables, nothing more. Yet there was a concrete historical dimension to Parsons's thinking about modern Western societies; he says on numerous occasions that they are evolving, at one point even writing a book entitled *The Evolution of Societies* (Parsons 1977). Expressed in the language of pattern variables, he thought that the orientations of people towards one another were being governed more and more by the first item in each pair. Thus, with the rise of the professions and the spread of meritocratic institutions, modern industrial societies were acquiring a pattern of 'universalist, specific, affectively neutral, achievement-oriented roles' (Parsons 1949: 147). The dynamics of social change, of which Parsons, contrary to popular belief, seems to have been quite well aware, meant that not all of the sixteen possible combinations would be realised in any given society. Only some would be selected through pressures of social integration and social development.

But Baldamus's point is that Parsons could not represent this by means of his theoretical apparatus; which means that there is an alarming contrast between Parsons's striving for greater precision, by means of an apparently quite respectable device, and the result, which is that the only theoretical precision achieved is a

precision of definition of actors' orientations, with a closer approach to social reality being provided not by the theory at all but by the common knowledge (the rise of the professions and professionalism) that Parsons had as much access to as, but no more access to than, anyone else. The same is even more true of the later device of systems and subsystems, in which the possibility of subdividing each of four boxes into four smaller boxes can in principle continue, not to the bottom of a finite tree diagram, but forever, the apparent increase of content that occurs with each new subdivision being outweighed by the suspicion that nothing more concrete is ever being said. In the pattern variable scheme and in the system and subsystem scheme there is, then, only 'description, not explanation' (Baldamus 1976a: 99). No matter how elaborately you combine actors' orientations or subsystems, you will never get closer to 'the non-mental reality of social behaviour'. To do that you have to resort to the informal method of acquiring common knowledge and introducing it where it will fit. What had been true for the production and personnel managers in a Birmingham factory was just as true for Parsons in an office at Harvard University.

Nevertheless, Baldamus is also oddly generous to Parsons. He says, for instance, that Parsons's method of cross-classification is the adaptation of techniques of cross-tabulation developed in surveys and statistics, which at least places him within the ambit of modern science. Baldamus seems not to have wondered whether, in constructing a system in which all permutations and combinations of four initial variables could be generated repeatedly, Parsons might have been drawing on much older traditions. For instance, if you look at the work of the Catalan mystic and alchemist Ramon Lull, you will see that the diagrams he constructs of his *ars combinatoria* do not have to be manipulated a great deal in order for them to bear a striking resemblance to those of Parsons. Doubtless Parsons himself was unaware of this connection, but one does not have to be a Jungian to see that, if we are to take the diagram seriously, not only as a tool for reconstructing a theory but as tool of reasoning in its own right, then such historical connections are plausible. Curiously enough, Parsons's diagrams are not mentioned in a paper on this very theme that Baldamus published just two years later with Thomas Schnelle (Baldamus and Schnelle 1978).

Trivialisation Effects

Whatever the ultimate origins of Parsons's thinking, his virtue was to have highlighted, albeit unknowingly, that cross-classification of equivalent dichotomies is at the core of the implicit reasoning of both sophisticated theorists and the less sophisticated producers of short-lived verbal innovations. There is an identifiable theoretical method, from out of which, despite the danger of an erosion of social reality, genuine theoretical innovation might be possible.

Baldamus was less optimistic about the capacity for innovation in empirical research. Here, despite the availability of sophisticated statistical techniques, there

was a significant reliance on rather simple tables and common knowledge and none at all on sociological theory. This constant appeal to common knowledge meant in turn that, although one might be able to track the development of a long-term trend towards functionalism in both theory and empirical methods, at any given point in history methods were more likely to be an accurate mirror of changes in social reality than theory, being more compact and 'less fluctuating' (Baldamus 1976a: 151). By the same token, precisely because it was less of a mirror of social reality, and presumably owed a little less to common sense, theory was a truer guide to the state of the discipline.

This sense that empirical methods are more of a mirror on social reality than theory is makes his observations about trivialisation and attenuation effects in empirical research all the more damaging, not least because, as he says in his inaugural lecture:

> The true purpose of social research as I see it consists in making concealed institutional underpinnings explicit and thereby enabling the researcher to participate in the process of aggregate social change. This kind of personal involvement does not necessarily conflict with the canons of scientific detachment. (Baldamus 1971c: 15)

He was loyal to this principle, though he rarely made it explicit in published work, so much so that *SSI* might easily be mistaken for the work of a detached sceptic who, in his dotage, indulges in casual dismissals of entire realms of learning.

The fact that it is not is demonstrated by his discussion of trivialisation, or attenuation. 'A problem is deemed to be trivial', he says, 'when it requires no sustained theoretical treatment for its solution, when, for example, it can be solved by recourse to common sense' (Baldamus 1976a: 57). We have already seen the trivialisation effects attendant on Parsons's efforts to bring his theoretical vocabulary to bear on substantive problems. No matter how much additional refinement you introduce to the categories, no addition of content results; any necessary dynamism can only be introduced by means of common knowledge.

Baldamus's examples of trivialisation in empirical research are drawn from the industrial or economic sociology to which he had devoted most of his professional life, and had done so because the fate and situation of workers was something that his social democratic commitments drew him towards. The questions he asked – though this may also account for the astonishing dryness of some of his work – were always causal: what causes certain types of behaviour, such as absenteeism, or fatigue, or lack of commitment to work? The answers were always sought in what he would later call the non-mental reality of economic or industrial behaviour.

By the early seventies he found that two things were happening simultaneously: the functionalist paradigm was being challenged in theory by Marxism and phenomenology, and some empirical researchers were investigating the world of work in ways that produced serious errors. These were not a product of ideological bias, but collective errors whose result was trivialisation. Four types

of trivialisation effect could be identified. The first three form a pretty standard list of criticisms that believers in objectivist causal analysis of social processes have routinely levelled at their opponents for decades:

a. macro-concepts give way to micro- concepts;
b. objective categories give way to subjective categories;
c. dynamic analysis gives way to static analysis.

Few criticisms of *The Affluent Worker* can have been as merciless as Baldamus's; the different types of trivialisation reinforce each other so powerfully that the only possible conclusion is:

> that 'wages' and 'labour' are merely subjectively perceived epiphenomena. As this argument is linked with a general theory of 'conflict' and 'cooperation' between workers and employers, it follows by implication that profits and capital are also imaginary. (Baldamus 1976a: 65)

Beynon and Blackburn's *Perceptions of Work* (1972) is an even more spectacular case, because here the study does not even begin with objective categories that then degenerate; the categories by means of which the authors begin their account of different aspects of a job 'are *evaluative* appraisals of work factors from the viewpoint of respondents'. (Baldamus 1976a: 74) Workers are then interviewed about their perceptions of these work factors. But since the work factors they are being interviewed about are already perceptions rather than objective factors defined by the investigator, the result was a study devoted to *perceived perceptions*, the ultimate pleonasm.

At this point Baldamus makes what can only be described as a leap of the imagination, but one that is instructive for an understanding of his overall project. He says that work of this sort is part of a broader movement away from objectivist sociology of the sort that he was doing in the 1950s, one in which the use of statistical tables is a red herring, having nothing to do with 'established canons of probabilistic inference' (Baldamus 1976a: 77). He then claims that the roots of this emphasis on actors' own definitions of the situation would have to be traced back a long way and that doing so is almost impossibly complex, but also that much of it is due to the recent influence of phenomenological sociology. Today, students of the history of British sociology may be amused to learn that the errors made by Huw Beynon, Bob Blackburn, John Goldthorpe and David Lockwood are due to the influence of phenomenology on their work, but that is what Baldamus claims. Moreover, the errors made by these writers cannot be corrected from within the approach adopted: 'the inescapable circularity of an endless chain of perceived perceptions can only be noticed from *outside* the conceptual boundaries of that school' (Baldamus 1976a: 81). The remark is instructive because today it has become a commonplace to believe that accounts like Baldamus's, which emphasise the role of informal methods or common knowledge in the formation of theories,

are themselves inspired by phenomenology. Yet nothing could be further from the truth; phenomenology can indeed alert you to the presence of informal methods and to the continuities between formal theorising and 'everyday' categories, but the point of noticing these things is not to give yourself an excuse to celebrate them; on the contrary, when you do notice them you need to see them as errors. The lesson of his work is that in order to see the kinds of connections that he emphasises it is best not to be within the boundaries of any 'school'. Though he would hardly have thought of himself as a member of Mannheim's free-floating intelligentsia, there are traces of the spirit if not the letter of *Ideology and Utopia* all over *SSI*: the conditioning of science by non-scientific factors is openly acknowledged, but treated as a problem to which some sort of solution must be found if an explanatory sociology is to be possible.

Now one way in which the search for such a sociology will never get off the ground is if, in addition to the types of trivialisation effect that we have already mentioned, a fourth type is allowed to flourish, namely one that arises as a result of the importation or, in extreme cases, substitution of the categories from one discipline for those of another, what Baldamus calls 'extra-disciplinary transmutation' (Baldamus 1976a: 121). The problem that arises here can undermine the integrity of an individual piece of work, as he thinks happens with Parsons and Smelser's *Economy and Society* (1956), discussed in *SSI* but already dismantled in a review from 1959 (Baldamus 1959).

The more general point though is this: 'Anyone who has worked with an interdisciplinary team over a long period knows only too well that the most serious obstacles are rooted in conflicts over theory, not over methods of research. Such obstacles derive from the fact that the central theories of different disciplines are basically incommensurable' (Baldamus 1976a: 59). In the light of the fact that almost everyone in the social sciences today seems to want to extol the virtues of interdisciplinary work, Baldamus's remarks can only be read as prescient, so much so that one is prepared to forgive him the fact that he is unsure whether it is diverse theories or diverse methods that undermine it. His reference to 'the contrast between the administrators' simple-minded trust in interdisciplinary studies and the social scientists' experience of the incommensurability of diverse methods' (Baldamus 1976a: 118) should be pinned on the main noticeboard of every academic department.

Reading what he says about this one cannot help seeing it as a message in a bottle that might one day reach, but probably never will, at least one of those desert islands of triviality that many of today's sociologists seem to inhabit. On the other hand, it is also a piece of boundary maintenance that rather neglects the interdisciplinary origins of sociology itself, and the fact that sociologists as much as any other social scientists have routinely had resort to concepts from outside their own discipline. Many of Weber's main concepts owe much to his training as a lawyer; and before Parsons diluted his sociology by the use of economic categories, he had constructed the whole thing by means of the concept of system imported wholesale from cybernetics and information theory. 'The social system'

is, in other words, a metaphor. The use of metaphor may have provided examples of attenuation or trivialisation, but it has equally been at the heart of many of the more fruitful and illuminating accounts of the dilemmas facing modern men and women, as well as at the heart of those contributions to aggregate social change that Baldamus thought sociology should make possible.

Marxism and Functionalism

Baldamus does the same to Marx's *Capital* as he does to Parsons, concluding that Marx too adopted the method of dichotomisation and ranking (with the capital/labour distinction at the top and a series of further distinctions lower down). He also constructs a tree diagram that has the same structure as the one he constructs in order to represent Parsons's theorising (Baldamus 1976a: 169). Here again, on the third and fourth levels not one but two new dichotomies are introduced, each one of them being used in only one half of the tree diagram. It seems here that Baldamus, who coined the phrase double-fitting, is doing some double-fitting of his own, forcing Parsons and Marx together more tightly than they will go by first constructing a tree diagram, finding the categories that will fit into it, and then adjusting the diagram so that both Parsons and Marx can be slotted into it in the same way. Still, the broader point that Baldamus wants us to take from them is that there is dichotomisation and ranking in Marx as well as in Parsons.

The difference is that Parsons's dichotomies are symmetrical whereas Marx's are not. This means that labour/capital should read labour/*capital*. Baldamus seems to have thought that this, the use of asymmetric dichotomies, could be a genuine source of innovation within a basically restrictive functionalist method. It is restrictive because the use of pairs of opposites often presupposes that there is a unified object – say, society – that is there to be divided in the first place, when in fact our sense of the object under investigation may only arise as a result of this very division. So:

> … the only way of coping with this restriction lies in experimenting with non-equivalent, asymmetric pairs of concepts. For this permits the accentuation of one of two parts of a given pair of opposites so as to obliterate or suspend its pre-existing unity. So far that method has been used in sociology as an unobtrusive intuitive technique only. It is only in the day-to-day praxis of innovative theorizing, such as it is, that the benefits of accentuation, ranking or hierarchical stratification have been realized to some extent. (Baldamus 1976a: 199)

Examples of such are Gemeinschaft/*Gesellschaft*, mechanical solidarity/*organic solidarity*, wert/*zweckrational*, ideology/*utopia.* Baldamus, in a very obscure passage, says that the point is that these can work as a methodological substratum that controls the use of other concepts; 'whether or not these are dichotomous' (Baldamus 1976a: 208).

At the level of empirical research, too, Marxism seems to have assimilated much of the functionalist sensibility. As I will suggest below, this is one of the more genuinely prophetic features of the book, notwithstanding the bizarre manner in which he seeks to establish it, namely by references to a:

> remarkable Marxian document, written by East German social scientists, but published in West Germany. Its title is *Wörterbuch der marxistische-leninistische Soziologie*. There are 69 authors who between them cover no fewer than 283 entries. For the purpose of trend reconstruction, the Dictionary offers many advantages. It contains a wealth of concise information that would otherwise be very cumbersome to assemble. (Baldamus 1976a: 158)

Since these authors are exposing themselves to Western readers they must have been 'carefully selected in respect of technical competence and depth of knowledge'. Be that as it may, the point is that East German Marxism has assimilated the functionalist theory of primary socialisation, albeit according it a secondary role within the overall framework. Not the least of the advantages of this was that it enabled materialist Marxism to get a much firmer grip on the concept of 'societal consciousness' that had always troubled it, not least by shifting emphasis from the moral to the cognitive aspects of socialisation mechanisms. Yet Western functionalism and Marxism may be said to have supplemented rather than mirrored one another. In the West, the need for more precision in survey research led a conservatively biased approach to sociology to discover and constantly reinforce the idea of class structure as a major determinant of social behaviour, while in the communist countries where class was less problematic, 'the biological determinants of age and sex asserted themselves as indispensable' (Baldamus 1976a: 164) In both, however, the language of stratification (horizontal layers, as in geology) competed somewhat with that of structure (and the implied dynamics of structural change). Baldamus thought it 'surprising' that the same ambivalence over the meaning of class structure appeared in the writings of Eastern Marxist researchers, but admits that in order to get to the bottom of it he would have to know something about the informal methods employed by East German researchers, which is impossible because he has 'no empirical material from East Germany at my disposal to explore this matter'. So the rest is speculation, including the magnificent statement that 'it is also possible that the partial assimilation of functionalist concepts and methods in Marxist countries amounts to importing a Trojan Horse' (Baldamus 1976a: 162).

The details of Baldamus's reading of an East German dictionary of sociology are less important than the more general point that there are affinities between the dominant functionalist paradigm and those that would seek to compete with it. It is hard not to see this as a prophetic observation; since 1976, while fewer and fewer theorists or researchers make overt claims to be either functionalists or Marxists, in that large domain in between rigorous theory and rigorous methods, now even more populous than it was then, these affinities continue to flourish.

Nowhere is it more true than in the functionalist theory of socialisation, which was extensively adopted by Marxism and neo-Marxism and which continues to be a staple of much work that is carried out even in the name of an interdisciplinary sociology. The most influential version is the work of Pierre Bourdieu, which for all its apparent sophistication repeatedly reverts to the functionalist theory of primary socialisation: in all of Bourdieu's sociological inquiries the reproduction of society occurs precisely as a result of the ineffectiveness of culture or the education system, or any other feature of secondary or tertiary socialisation, in dislodging what has been established by class and upbringing. In the less sophisticated sorts of empirical research, especially in sociology doctorates, trivialisation effects are produced by the ritualised importation of extra-sociological theories – a process naively celebrated as interdisciplinarity – which are then compensated for by an equally ritualised appeal to a more stable method, centred on the standard repertoire of hard-core independent variables – class, gender, race – drawn straight from the functionalist roster.

Only One Method?

Before closing we should turn back to Weber and the grandiose claim that there is only one method, Weber's ideal types. The first point that Baldamus neglected was that for Weber ideal types are not classificatory concepts at all, but the one-sided exaggeration of social reality; they are deployed quite often in dyads, but often in triads, and never as part of a cross-classificatory, definitional project (Parsons) or a cross-classificatory attempt to generate paradoxical verbal innovations (Marcuse, Bourdieu, MacIntyre, Foucault). Nor are Weber's dyadic categories available for the sort of negative dialectical analysis of categorical mutations to be found in Adorno.

On the one hand, the transcendence of merely classificatory concepts is 'the coming of age of social science'. In this respect Weber's *Economy and Society* is far in advance of Parsons, whose most concrete statements about social reality are the sections on 'types of social structure' in *The Social System*. There, two symmetrical dichotomies generate four types of social structure so completely that there is little more to say than what needs to be said to fit individual societies into one of four boxes. Compare that with Weber's sociology of religion: the dichotomies of church and sect, asceticism and mysticism, and otherworldly/innerworldly do get combined in various ways, but only as a means of orienting inquiry, which then takes off in the way we know.

Thus, on the other hand, although many of Weber's ideal type combinations are asymmetric dichotomies, in contrast to dialectics they are not experimental or designed to introduce dynamism into an otherwise stable image of society consisting of equivalent pairs, or designed to show the mutation of one concept into its opposite, as Adorno tries to show in his analysis of culture. Rather, the point of them is to orient comparative historical inquiry so that sociology can

answer questions of cultural significance: in church/*sect*, mysticism/*asceticism*, or otherworldly/*innerworldly*, the greater weight was given to the second because that was considered significant for the emergence of the modern world. One can say that Weber was introducing a piece of common knowledge here, but he is hardly doing so surreptitiously. As he says quite openly, 'the light of the great cultural problems moves on'.

Reflection on what Weber was doing seems to me to point up the chief weakness in Baldamus's work, one that is a mirror image of one contained in a famous reflection on the status of social scientific knowledge, Winch's *The Idea of a Social Science* (1958). Winch, like Baldamus, made a sharp distinction between the conceptual and the empirical, but did so in order to show that the problems of sociology were really conceptual problems and therefore a matter for philosophy; social science should be about meaning and not about causality. For Baldamus sociology could only be a science if it could resist a flight into philosophical speculation and explain 'the non-mental reality of social behaviour'; he had no time for hermeneutics. But with Weber, in contrast to both, you get cause and meaning, conceptual flexibility and empirical inquiry. That is what the first sentence of *Economy and Society* is all about. Despite the brilliance of Baldamus's reflection on sociological knowledge, and despite the interest in long term trends in the history of sociology, he never practised sociology itself as an historical discipline. He says that Elias's sociology of knowledge is the method being used in *SSI* (Baldamus 1976a: 49–50) but he could never have claimed that his own empirical work bore the slightest resemblance to Elias' *The Civilising Process*, even though this book appears in the bibliography, in the original German edition of 1939.

Conclusion

In this chapter I have approached *SSI* not only as one who looks back and asks how a well-known work has stood the test of time, whether its claims still bear scrutiny; I have not only looked upon it as Baldamus himself might have done, identifying what is lasting and what is obsolete. While trying to think about its relevance today I have found myself repeatedly drawn back into the text itself, as though it were newly published; in order to fathom what it means and what it wants to say; I have found myself closing the book to reflect on it and having to open it again immediately in order to remind myself of its wording. And in many places I have been reduced to the sort of summaries one might find in an extended book review, as well as to a measure of repetition. Then again, there is much that is worth repeating. Such as? Such as the idea that theoretical concepts may be put to work or left idling; such as the idea that functionalism is recalcitrant enough to be still with us and that the ritual references to race, gender and class sustain as much as challenge it; such as the idea that diagrammatic representations of theories may hold the key to their structure; that interdisciplinarity may be a

source of trivialisation; or that, if you combine two asymmetric dichotomies the resulting four boxes will contain two pleonasms and two paradoxes, and that the use of the paradoxical formulations is an important way in which innovation can be generated solely by means of a theoretical method; that Marcuse's 'repressive tolerance' was the product of such a procedure, and that it is not difficult to find others; that 'the lonely crowd' is one, as are 'pastoral power', 'destructive Gemeinschaft', 'bureaucratic individualism', and 'symbolic violence'; and that so, for that matter, is 'forgotten classic'.

Chapter 6
The Exoteric Paradox: A Contribution to Ludwik Fleck's Theory of Science[1]

Wilhelm Baldamus[2]

Editors' Introduction

The resemblance between Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) and Ludwik Fleck's *Genesis and Development of a Scientific Fact* (1935) is notorious, notwithstanding Kuhn's acknowledgement of the provenance of his ideas. In this chapter Baldamus, who was one of the first to notice the similarities, alludes initially to the possibility of plagiarism but then notes that the real issue that requires explanation is why it was that Fleck's work remained neglected until the 1970s while Kuhn's received widespread fame. He interprets the different reception that these two books received through the distinction made by Fleck himself between esoteric and exoteric knowledge. While esoteric knowledge is comprehensible only to specialists, exoteric knowledge is characterised by persistent attempts to define scientific or technical terminology in a way comprehensible to a broad public. The paradoxical result is frequently an increase rather than a decrease in ambiguity of meaning attached to key terms and expressions. Baldamus traces the development of Kuhn's work from the 1950s to the late 1960s and reveals an increasing use of exoteric, popular terminology. The most well known example of an exoteric, widely discussed and thoroughly ambiguous expression is Kuhn's 'paradigm'. In contrast, Fleck's work retained its esoteric form, largely due to the lack of an appropriate audience in the 1930s and Fleck's status as an outsider to the philosophy and sociology of science communities.

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In this chapter an attempt will be made to demonstrate the practical possibility of taking an 'external' view of the problems of a theory of science. In order to avoid the appearance of a naïve instrumentalism, two explanatory notes may be made at the outset:

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1 Editors' note: This is a slightly edited version of an article first published as Baldamus 1979a. It was translated for this volume by Charles Turner.

2 I owe enormous thanks to Thomas Schnelle for his indispensable help in the development and ordering of the ideas and material presented here; without it this publication would never have seen the light of day.

1. It is to be assumed that three distinct disciplines are making an effort to be received or at least understood beyond their own limits by a public of non-specialists. That is, each discipline is directed towards the generally educated reader without realising that both of the others have the *same* aim. This implies the possibility of investigating unexamined claims to general comprehensibility. Accordingly it can be assumed that the other side of this striving for general comprehensibility is an increasing vagueness of the object of scientific knowledge.
2. An external characteristic of our procedure is the frequent use of the word 'vocabulary', taken from everyday speech. This is simply a device designed to avoid a cumbersome linguistic analysis. The aim is merely to establish that the vocabularies being investigated display structural regularities that cannot be explained from within existing research into science. Similarly, the statistical mode of presentation is only a provisional means for making these observations more directly accessible, it is a scaffolding that, in Wittgenstein's (or Hegel's) sense, is no longer required when its work is done.

The extensive discussion in the last few years about the cognitive status of research into science has brought with it so much confusion that it is now hard to say what the arguments are about (Schnädelbach 1972; Shanin 1972; Krüger 1974; Schäfer 1974; Sandkühler 1975; Wisdom 1975; Harding 1976). The increasing lack of clarity is demonstrated by the fact that three disciplines have claimed this domain as their central object of inquiry: *philosophy* of science*, history* of science and *sociology* of science. Clearly there are basic questions here that cannot themselves be answered from within the conventional boundaries of the three competing disciplines. So one would require a procedure that is not tied to disciplinary boundaries – a procedure by means of which one could in a certain sense 'observe' a common object 'externally'.

The tendency towards general comprehensibility in this area is a relatively recent phenomenon. The decisive impetus was given by Thomas S. Kuhn in his 1962 study, *The Structure of Scientific Revolutions*, a work whose popular science stamp was made clear by its extraordinary sales figures (Kuhn, 1962). And with this the relaxation of disciplinary boundaries begins. For while Kuhn repeatedly and explicitly presented himself as an historian of science, the decisive controversies and arguments have been set in motion by philosophers and sociologists of science.

All this becomes clearer if we contrast Kuhn's aims and influence with those of Ludwik Fleck, whose *Genesis and Development of a Scientific Fact* appeared in 1935. To justify this some preliminary remarks are in order. Whether it is still necessary to recall the classic status of the discoverer of 'the scientific community' is difficult to say. At any rate, Fleck's demonstrable influence on Kuhn has been rediscovered at least four times (Baldamus 1972a; von der Ohe 1971; Merton 1977; Schafer 1977). Here Kuhn's recognition of Fleck in the foreword to his main work is regularly cited, as in the following remarks by Lothar Schäfer:

> Kuhn mentions Fleck in the foreword to his well-known *The Structure of Scientific Revolutions*, where he presents the 'Society of Fellows of Harvard University' as the breeding ground for his work. He writes: That is the sort of random exploration that the Society of Fellows permits and only through it could I have encountered Ludwik Fleck's almost unknown monograph, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Basel, 1935), an essay that anticipates many of my own ideas. ... Fleck's work made me realize that those ideas might require to be set in the sociology of the scientific community. Though readers will find few references to either these works or conversations below, I am indebted to them in more ways than I can now reconstruct or evaluate'. (Kuhn 1962: vi–vii) Yet because in the course of the book Kuhn makes not one reference to Fleck's work, this connection has never been investigated. Kuhn is widely cited as a revolutionary in the theory of science. Whoever takes account of Kuhn's dependence on Fleck can only conclude that the primary basis for revolutions in the theory of science lies in the ahistorical consciousness of a community of reception. (Schäfer, 1978: 29)

As we will see, the question of Kuhn's dependence on Fleck is complicated by the fact that he adopted only certain *fragments* of Fleck's study, namely those observations and concepts that would make his own position more convincing, and above all comprehensible to a wide audience. These fragments essentially make up his theory of 'normal science' and the concept of the 'scientific community' that is connected with it. In Fleck's work both of these elements are extraordinarily complex and, being weighed down with technical detail, difficult to understand. In particular Fleck's explanation of the connection between them is difficult to grasp without a constant reference back to his extensive natural scientific observational data. We cannot go further into this here, since from today's standpoint the connection between a pragmatic and a theoretical concept of science has to be seen as questionable. Be that as it may, the basic question of the possibility of generally comprehensible statements about specialised natural scientific knowledge, be they historical, philosophical or sociological, must in all cases be regarded as primary. For in view of the apparently interminable debates we must make the hypothetical assumption that an adequate translation of technically specialised statements into the language of the educated layman *is in no way possible*. Clearly such a possibility would depend on whether there was an appropriate procedure (or whether one was conceivable) with whose aid such a translation might be carried out.

A medical practitioner by education and profession, Ludwik Fleck specialised in bacteriology and serology. In the middle of the twenties, towards the end of his professional training, he began an intensive study of philosophy (Carnap, Mach, Schlick, Reichenbach) and sociology (Scheler, Simmel, Durkheim etc.). It was as a result of this wide-ranging set of interests that he was familiar with the problem we are discussing here. What mattered to him was not the attempt to solve it, for the necessary preparatory work had not been done, but rather a clear description of

the difficulties that would have to be overcome. A few pointers may be given that make clear what he proposed:

> Popular science is an especially complicated subject. Since the speculative theory of knowledge has never investigated actual knowledge but merely an imaginary picture of it, the epistemological investigation of popular science has yet to occur – at least according to my knowledge. Here is not the place to fill this gap. (Fleck 1979: 112, translation altered)[3] ...
>
> Independently of the substantive or formal organization of a stable thought collective (such as that of a church community or a trade union), there are common structural characteristics of all communities of thought as such. This general structure of the thought collective consists in the fact that around every object of thought, be it a dogma, a scientific idea, or an artistic thought, there forms a small esoteric and a larger exoteric circle made up of the thought collective's members. A thought collective consists of many such intersecting circles, while an individual belongs to several exoteric circles and to few, and possibly no, esoteric ones (Fleck, 1979: 105, translation altered) ...
>
> Take the case of a researcher who creatively approaches a problem and is a 'specialised expert' informed in the greatest depth – for example, a radium specialist in the science of radioactivity. He constitutes the center of the esoteric circle of this problem. The circle includes, as 'general experts', scientists working on related problems – all physicists, for instance. The exoteric circle comprises the more or less 'educated dilettantes'. For science, then, a contrast between *expert* and *popular* knowledge is the first effect of the general structure of the thought collective. However, the richness of this field means that even within the specialized esoteric circle the sphere of specialist experts may be distinguished from that of general ones: let us say that expert science consists of *journal* science and *vademecum* science. (Fleck 1979: 111–112, translation altered)

Later we will make use of these suggestions about the comparative content analysis of vocabularies, based on the distinction between esoteric and exoteric vocabularies. The simplification that this involves is, however, only for ease of presentation. It must always be borne in mind that in principle we are dealing with a difference of degree that, strictly speaking, would have to be presented as a scale or hierarchy, as has already been sketched by Fleck. Furthermore our analyses are concerned in the first instance only with 'vocabularies', that is with bundles or collections of isolated *words*, and not with (esoteric or exoteric) statements. This implies a conscious confrontation with the dominant language-analytic logic

3 Baldamus cites Fleck in the original German. The published English translation of Fleck's book has been altered where the editors thought it appropriate, as here.

and metaphysics, whose thoroughgoing influence on the current theory of science is generally overlooked. Clearly the attempt to treat collections of words as the object of investigation, rather than the logical or semantic connections between them, can only be defended by comparing its results with those hitherto obtained. In the limiting case one has to reckon with the possibility that these results can be neither explained nor rejected within the framework of traditional concepts. In that case one would face an irresolvable pragmatic paradox.

Now in order at least to anticipate some especially useful applications, we can cite Fleck at this point:

> Words as such constitute a special medium of intercollective communication. Since all words bear a more or less distinctive colouring conforming to a given thought style, a character which changes during their passage from one collective to the next, they always undergo a certain change in their meaning as they circulate intercollectively. One could compare the meaning of the words 'force', 'energy', or 'experiment' for a physicist, a philologist, or a sportsman; the word 'explain' for a philosopher and a chemist, 'ray' for an artist and a physicist, or 'law' for a jurist and a scientist. (Fleck 1979: 109)

While Fleck here treats 'words as such' only as a question of semantics, with the counterposition of esoteric and exoteric knowledge he attains a decidedly more comprehensive perspective, though he himself sees it only as conjecture:

> No matter how a given case may be described, the description is always a matter of simplification permeated with apodictic and graphic elements. *Every communication and indeed all nomenclature tends to make any item of knowledge more exoteric and popular*. Otherwise each word would require a footnote to assign limitations and provide explanations. Each word of the footnote would need in turn a second word pyramid. If continued, this would produce a structure that could be presented only in multidimensional space. Such exhaustive expert knowledge completely lacks clarity and is unsuitable in any practical case. It must be remembered that such a pyramidal structure does not yield more general and recurrent elements, which would basically simplify the construction if they could be described separately. (Fleck 1979: 114–5, emphasis in original)

For a fruitful development of Fleck's ideas we would have to remember that at the time when he was formulating them a consistent realisation of such thought experiments was still not possible. For Fleck the only purpose they could serve was to push the exaggerated claims of early logical empiricism towards fictive conclusions *ad absurdum*. Nobody at the time could have imagined that something as inconceivable as 'word pyramids' might one day be discovered in the empirical world. It is easy to see that each of the tables of vocabulary set down in sequence would tend towards the form of a pyramid (placed on its head). In so far as our first efforts have proceeded from conventional subject indices, in which words

are ordered alphabetically, that is, not according to any statistical procedure, the pyramidal shape can be treated as an accidental result. And the fact that we are dealing here with an observation that is as fundamental as it is puzzling, would be underlined rather than undermined by pointing out that it could not be otherwise. In order to see this one need only – in another thought experiment – attempt to write a book or an essay in which indispensable words appeared with the same, maximal, average or minimal frequency. The pyramidal structure of vocabularies is manifested more clearly in the fact that they assert themselves against every polar division according to analytic criteria: in our examples it is reproduced in the groups of both esoteric and exoteric words.

The remarkable obstinacy with which differently constituted word pyramids appear to reproduce themselves is important for the following reasons. At first it appeared that the choice of criteria for comparing the frequency distribution of observed word groupings was highly subjective. That already applies to any original subject index, for an author can always justify his own choice of keywords intuitively. (This also explains why, as we will see later, a given subject index can be reshaped by means of supplementary criteria). And so a vocabulary that has been constructed without any pre-given model, that is artificially, must appear even more subjective. In each case the statistical Universum is unknown. That makes it advisable to make explicit the *principle of relativity* that is at the heart of content analysis: there can only ever be differences of degree within an arbitrarily selected comparative pair, such as esoteric-exoteric. A given vocabulary can be polarised just as well by means of dichotomies like old v. new, abstract v. concrete, short v. long, clear v. unclear, endemic v. foreign, trivial v. important and so on. Even when several such dichotomies are combined with one another, all that is increased is the descriptive complexity of the vocabulary tables. Thus out of the universe of possible observations only those features are chosen that can be seen as more or less relevant according to given conventions. Now since our object of inquiry, the word pyramids sardonically postulated by Fleck, cannot be defined conventionally, their relevance for a theory of science appears all the more precarious.

However in certain situations there is a comparative procedure that may be superior to the usual comparison between opposing terms. This involves the introduction of a *temporal* dimension to the (simple or cumulative) comparison, as a result of which the relativity criterion is restricted. Should it become clear for instance that, for a given disciplinary specialisation at a given point in time, the relationship between an esoteric and an exoteric vocabulary is not determinable, this does not exclude the possibility that a temporal shift may be observed, such that the esoteric components have expanded at the expense of the exoteric, or vice-versa. Naturally these would only be empirical regularities through which the hitherto undefinable explanatory scope might be more concretely described. This apparently rather unlikely possibility can be seen more clearly from regular subject indices. Clearly the task of the subject index, an institution created originally by the natural sciences, is to enable the reader who already has some knowledge of the

area to judge the type or direction of the author's specialisation from the relative frequency with which certain 'keywords' appear. Particularly in a first publication the number of pages assigned to the most important keywords points roughly to where the centre of gravity of a new specialism lies. If one then takes into account the temporal succession of several texts within a given discipline or school, one may under certain circumstances be able to read off *certain developmental trends* from the changing frequency distribution of important keywords. To take a fictive example from the area that concerns us here: should it be possible to establish that, from a random sample of the most influential texts in research into science, the keyword 'theory of science' first appeared say at the end of the sixties and then ever more frequently, it might strengthen the impression of the existence of a problem that, though admittedly insoluble, was increasingly urgent.

So much for procedure. The most appropriate case to which it can be applied is Kuhn's *Structure* (1962), since it touches on the most important of the questions raised here. In particular it gradually makes us aware of the persistent difficulty of drawing a boundary between the three competing disciplines. Moreover, it represents the peculiar case of a well-known author feeling himself entitled to refuse out of hand to construct a subject index. In an autobiographical article from 1974 Kuhn put it like this:

> Reactions to it have been varied and occasionally strident, but the book continues to be widely read and much discussed … One aspect of the response does, however, from time to time dismay me. Monitoring conversations, particularly among the book's enthusiasts, I have sometimes found it hard to believe that all parties to the discussion had been engaged with the same volume. Part of the reason for its success is, I regretfully conclude, that it can be too nearly all things to all people. … For that excessive plasticity, no aspect of the book is so much responsible as its introduction of the term 'paradigm', a word which figures more often than any other, excepting the grammatical particles, in its pages. Challenged to explain the absence of an index, I regularly point out that its most frequently consulted entry would be 'paradigm, 1–172, *passim*'. Critics, whether sympathetic or not, have been unanimous in underscoring the large number of senses in which the term is used. (Kuhn 1974: 459)

Flicking through the book it might appear that 'scientific paradigm' appears on almost every one of its 173 pages. However, as will be seen, there is another word that occurs with (significantly) equal frequency, namely 'scientific theories'. *This error* of Kuhn's is no accident. Both expressions have one characteristic in common: they are both, despite their centrality, extremely ambiguous, albeit for different reasons. These reasons can, however, only be understood by means of a polar division of the reconstructed vocabulary into esoteric and exoteric words.

Up to this point we have accepted Fleck's impressionistic description of this dichotomy, recalling that Fleck's statements regarding the *exoteric* component are very vivid and relatively detailed, whereas the *esoteric* remains in the background.

For him the really new problem was the exoteric, for which he made use of several synonymous expressions: 'popular scientific', 'generally comprehensible', 'intuitive', 'emotion-laden', 'simplification', 'evaluation', 'world view' and so on (see especially Fleck 1979: 121). For the practical purposes of a content analysis of vocabulary, this meant initially that it was relatively easy to identify typical exoteric keywords, but that esoteric expressions could be identified only negatively, as residuals. This made for an unreliable analysis. Only by means of a series of detailed comparisons *within* the category of 'intuitive', 'emotion-laden', 'simplifying' and so on, would it have been possible to arrive at a reliable *common* characteristic. This characteristic manifests itself in the invention of new *metaphors*. In hindsight, it is especially instructive that overtly popular texts about science are dependent to a high degree upon the most striking and indeed exaggerated analogies. But this also means that the reception of newly invented metaphors is subject to numerous misunderstandings. And this explains why they are, as a rule, introduced by means of detailed formal definitions. The quintessence of the exoteric, then, is that it consists of two complementary features: on the one hand the appearance of newly minted metaphors; on the other their being linked to explicit definitions. As a result, the residual category of the esoteric is characterised by conventional expressions which appear in *undefined* form, *surreptitiously* so to speak.

Here Kuhn's vocabulary from 1962 is construed in a deliberately simplifying way so as to make it easier to give a provisional illustration of our procedure. Hence we have restricted ourselves to just three exoteric keywords: 'scientific paradigms', 'scientific communities', 'scientific revolutions'. These are distinguished by the fact that they are over-defined and have an explicitly metaphorical character. Naturally the text contains further exoteric concepts, above all 'normal science', 'puzzle-solving', 'conversion', 'novelties', 'crisis', 'world views', and 'Gestalt switch'. It was, however, only in the course of their philosophical reception that these concepts would come to be defined, if, that is, they were not distorted to the point of unrecognisability.

The immediate purpose of these schematic and simplified tables is to bring an as-yet-untested procedure to the reader's attention with a minimum of methodological discussion. In so far as we proceed through example, extracting some of the better-known terms and comparing them quantitatively by means of a simple sub-division, the distinctiveness of our procedure should to an extent 'speak for itself'. This perhaps rather primitive, pragmatic-instrumental attitude is not only advisable in itself, in order to avoid making premature conclusions from a single case study. A more important reason lies in a logically insuperable dilemma: the foregoing remarks have already suggested that the essence of an exoteric concept manifests itself in the technically *unsolvable* problem of definition. The appearance of such keywords becomes immediately apparent through their being *over-defined*. To see this we need not concern ourselves with their so-called logical or semantic substance. We simply observe a specific *mode of action*, namely the repeated and repeatedly unsuccessful attempt to define one and the same object.

One can perhaps see this failure not only in Kuhn but in the reception of his work, that is, in the constant efforts of commentators to extract from Kuhn's conceptual universe some sort of unambiguous and communicable sense. This characteristic failure to define central exoteric concepts becomes clearer if one casts a glance at their esoteric counterparts, that group of non-defined, apparently conventional keywords. Even an incredibly difficult concept like 'scientific theories', which refers to the entire development of the natural sciences from Ptolemy to Einstein, is adopted by Kuhn with astonishing carelessness. The expression is first used on page 1. There he writes: 'the content of science is uniquely exemplified by the observations, laws, and theories described in these pages'. On the next page, similarly, he writes: 'The historian … must determine at what point in time each contemporary scientific fact, law and theory was discovered or invented'. And once again on the next page: 'Out-of-date theories are not in principle unscientific because they have been discarded'. So if it appears unnecessary to provide a definition of a concept as complex as 'scientific theories', and if there is no sense that one *would have to* define it, then it is not surprising that the relatively less complex expressions 'scientific textbooks', 'scientific traditions', and 'scientific education' are treated likewise as unproblematic, purely descriptive elements. In the case of 'scientific practices', the third most frequent, the case is somewhat different. Because it gives the impression of having been taken from everyday language we experience it as unproblematic and therefore not in need of definition. However, as we will see later, even this impression may be misleading. The two other expressions, 'esoteric vocabularies' and 'philosophy (of science)' are included in the table only so that they may be taken into account in the later comparisons of vocabularies over time.

In order to guarantee a minimum of comparability, changes in the type and frequency distribution of keywords over time would have to be registered, where possible, for a particular discipline. Hence in Kuhn's case his development as a historian of science will be emphasised. His first publications date back to the beginning of the fifties, so that we have available a long series of readily comparable material. Unfortunately there are only two authentic subject indices authorised by Kuhn himself, one for *The Copernican Revolution* of 1957 (Kuhn, 1957), one for the book which appeared in German in 1977 as *The Emergence of the New* (Kuhn 1977). In order to get a handle on the entire development, then, it was necessary to construe some of the keywords artificially. A further difficulty was that we had to refer to articles as well as books. Clearly articles are too short to be treated by means of the usual method of the subject index. In order to circumvent this problem, here we have used the actual frequency of the word rather than the number of pages on which it appears. This makes it possible to include even short articles. In order not to over-complicate the presentation of structural changes over time we have concentrated on four points: 1952, 1957, 1958–61 and 1977.

Table 6.1 Kuhn's 1962 vocabulary

	Pages	Esoteric	Exoteric
Scientific Theories	1 2 3 6 7 8 9 10 11 12 13 14 15 16 17 18 19 24 25 26 27 28 29 30 31 32 33 39 40 41 42 43 46 47 48 50 52 53 54 55 56 58 59 60 61 62 65 66 67 68 69 70 71 72 73 74 75 76 77 78 79 80 81 82 84 85 86 87 88 90 92 94 95 96 97 98 99 100 101 102 107 108 118 124 125 126 129 130 131 133 134 135 137 138 139 140 142 143 144 145 146 147 148 150 153 154 155 156 170 171	111	
Scientific Paradigms	10 11 13 18 23 28 29 32 33 35 42 44 46 48 19 50 51 52 53 56 57 58 59 60 61 62 64 65 66 67 68 69 72 74 75 76 77 78 79 80 82 83 84 85 86 87 88 89 90 91 92 94 95 96 99 101 102 103 104 105 106 107 108 109 110 112 114 117 119 121 122 123 128 129 130 131 132 133 134 135 136 137 139 140 141 143 144 145 146 147 148 149 150 151 152 153 154 155 156 157 158 159 161 162 164 165 166 168 172		109
Scientific Practices	4 5 6 7 10 11 12 13 18 20 22 23 24 30 31 32 33 34 36 37 41 42 46 47 49 51 58 60 70 72 73 74 75 76 77 80 81 83 84 85 90 93 94 98 99 100 102 103 104 110 111 112 122 124 125 126 127 128 131 133 135 136 140 141 143 149 150 151 153 154 156 157 158 160 161 162 164 165 166 167 168 171	82	
Scientific	4 5 6 7 8 10 11 14 19 20 21 22 23 37 43 45 46 47 49 61 65 91 93 100 102 106 107 110 121 127 135 139 144 149 151 152 155 157 158 160 161 162 163 164 165 166 167 168 170 171 172		52
Scientific Revolutions	3 6 7 8 12 34 39 48 49 50 69 90 91 92 93 97 100 102 106 110 111 114 128 132 134 135 136 137 138 139 140 142 143 147 155 156 162 165 169 171		42
Scientific Traditions	3 6 7 8 9 10 11 19 42 43 44 45 46 50 65 68 69 85 88 90 93 102 111 120 136 140 141 143 147 148 150 155 156 157	34	
Scientific Education	1 3 5 12 19 46 47 49 50 51 53 80 95 101 108 110 111 118 127 132 136 138 139 141 142 152 164 165 166 167	30	
Scientific Textbooks	1 8 10 11 12 20 43 46 47 49 50 80 135 136 137 139 140 141 142 143 155 158 164 165 166	25	
Philosophy (of Science)	78 87 88 90 95 97 100 120 125 126 136 144 146 148 159 161 164 170		18
Esoteric Research	11 15 18 20 23 24 35 42 64 163	10	

Table 6.2 Kuhn's 1952 vocabulary

	Pages	Esoteric	Exoteric
Scientific Theories	1 1 1 1 1 1 1 2 2 2 2 2 2 3 3 3 3 3 3 3 3 3 3 4 4 4 4 4 5 5 5 5 5 5 5 5 5 5 6 6 6 6 6 6 6 6 7 7 8 8 10 10 10 11 11 11 11 11 12 15 19 21 21 21 21 21 21 21 22 22 22 22 22 24 24 24 24 24 25 25 25 25	82	
Experiments in Chemistry	2 2 2 3 3 3 3 3 4 6 7 7 7 7 7 8 8 8 10 10 10 11 11 12 14 15 15 16 17 17 17 18 18 18 19 19 19 19 19 19 20 20 20 20 20 20 20 20 20 20 22 22 22 24 25 25 25 25 25	59	
Scientific Metaphysics	1 7 7 8 9 9 10 13 14 15 18 18 18 25		14
Traditions (scientific, philosophical)	4 4 6 6 6 10 11 11 11 13 23 23 25	13	
Novel conceptions, novelties	9 12 16 20 25	5	
Revolution (chemical)	3 3 4 21 25	5	
Conceptual schemes	16 16 25		3
Scientific Textbooks	3	1	

The table for 1952 concerns a specialist article with the title 'Robert Boyle and Structural Chemistry in the Seventeenth Century' (Kuhn, 1952). Its central question is whether and to what extent Boyle's research makes him a pioneer of modern experimental inorganic chemistry. In view of the specialist material one might assume that the vocabulary of this article is exclusively esoteric. Here Fleck's observation about an 'intersecting' esoteric-exoteric 'style of thought' is relevant. Although the article is certainly not aimed at a broad public interested in popular science, it is nevertheless exoteric to the extent that it appeals to the particular thought collective of *research chemists as a whole*. Here Kuhn sees the historian's task as one of translating the archaic and foreign mode of thought in which Boyle describes his hypotheses and experimental observations into that of modern chemical theory. This explains the fact that in the table the keyword 'theories' appears frequently and in many variations: 'corpuscular', 'particulate', peripatetic', 'iatro-chemical', 'mystical', mechanical' theories and so forth. From the standpoint of the layman with no training in chemistry the article is overwhelmingly esoteric; only two keywords, 'scientific metaphysics' and 'conceptual schemes', appearing relatively infrequently, could be described as universally understandable. And since moreover the discussion is pedantic and monotonous, this vocabulary stands in the starkest contrast to that of *Structure*. The

comparability is to be found only in the exoteric oxymoron 'scientific metaphysics', which to a certain extent is the later concept of paradigm *in statu nascendi*.

The next table deals with *The Copernican Revolution*, published in 1957. For our purposes it is instructive that the index to this work, consisting of around 280 keywords and compiled by an assistant, on closer inspection requires further elaboration. For a casual overview already makes it clear that the far more numerous and frequent keywords are of a purely specialist-esoteric nature, while only a few exoteric concepts are to be found and these with a lower frequency. The first group includes a large variety of concepts conveying objective information, such as 'astronomy', 'prehistoric', 'hellenic', 'mystic', 'Muslim', 'medieval', 'Renaissance', and so on, while the second includes only three: 'conceptual schemes', 'scientific revolutions', and 'world views, fabrics of thought', all of them with low frequency. This mismatch between these two categories was so striking that we thought it necessary completely to rework the subject index, especially when the original index turned out to contain a number of inaccuracies. Naturally, the reconstruction was aided by the benefit of hindsight, which allowed us to compare this book with the later and much more successful *The Structure of Scientific Revolutions*.

Bearing in mind our 'defined/undefined' distinction, there was a considerable number of keywords that were undefined but, according to their frequency, indispensable. These were headed by 'observations' (66), 'traditions' (48), and theories' (43). An initial difficulty was the fact that problematic concepts such as 'theories', 'systems', and 'models' are distinguished by their high frequency and yet at the same time are very similar to the clearly exoteric concept 'conceptual schemes'. Only if one recalls that the appearance of 'defined' exoteric concepts in scientific texts always indicates an unsuccessful struggle with ambiguity, does it become clear that a central position was accorded to 'conceptual schemes' by means of an autonomous, epistemologically unjustified and arbitrary decision, of which the author was fully aware. Thus in Kuhn's original index the keyword 'theory' is supplemented with the remark 'see conceptual schemes'. The beginnings of a definition of 'conceptual schemes' appear first on pages 25–6, and then only in the form of a very provisional, implicit contrast with the expression 'observations'. An explicit definition appears only on page 35, in the section called 'The Functions of a Conceptual Scheme': 'Unlike the observations [sic!] described in the early sections of this chapter, the (Copernican) two-sphere universe is a product of the human imagination. It is a conceptual scheme, *a theory* deriving from observations but simultaneously transcending them' (Kuhn 1957: 35, emphasis added). Here, then, 'theory' is used synonymously with 'conceptual scheme'. But 'theory' also appears from page 1 onwards, and already on page 4 the very imprecise but equally fundamental role of this concept is set out with exemplary clarity.

Table 6.3 Kuhn's 1957 vocabulary

	Pages	Esoteric	Exoteric
***Observations**	4–5 7–8 11–13 17 25 30–31 34 36 40 45 47 50–51 54 56 62 64–66 69 73–76 85 92 94 119 125 131 139 156–9 162–3 166 169–70 172 174 200–201 205–9 211 215 216 219–22 225 226 236 247 261	66	
***Traditions**	2 54 75 77 82 90 98–105 116–7 119 123 126–8 132 134 135 139–40 147 149 159 163 170 181–2 188 190–4 200 206 214 230–231 233 243 245 264	48	
***Theories (of scientists)**	1–4 12–13 26–7 35 37–8 40 47 50 55 69 72–73 85–86 89 90 93 100–101 105 113–15 117–22 125 153 201 208 211 217 246 252	43	
***Systems, models**	39 56 58–9 64 66 74 125 139–40 162 164 166 168–74 176–77 186–7 196 202–5 208–11 226 232 240 242 244 247–8 263	41	
Conceptual Schemes	25–26 35–41 54 64–66 73–76 82 89 96 104 112 117 168 208–9 212–213 226 261–2 264–5 273 278		35
***Successes, Achievements**	1 7 38 40–41 54–55 71 76 94 99 114 116 121–2 136 139 168 171 180 200 231 245 252 261	27	
***Innovations**	42 76 83 101 124 131 133–36 138 143 154 164 170 182 187–9 194–5 200 231 245 252 261	26	
***Incompatibilities, Incongruities, Irregularities**	2 41 65 67 71 75 77 82 86 103–4 106 114 118 133 136 142 153–4 168–69 182–83 210 228 235	26	
***Education**	43 53 77 79 89 92 94–95 99 101–2 105 108 126 132 135 144 168–9 182 188 190 196 227 260	25	
***Scientific Revolutions**	2–4 74–75 93 104 108 116 133–34 182–83 227–31 260–65		24
***World Views, Fabrics of thought**	6 76–77 83 93–95 98 108 192 231 236 260 264		14
***Discoveries**	4 14 45 127 197 199 211 220 256–58	11	

Note: The keywords marked with * indicate concepts introduced for the purposes of comparison but not found in the book's original index.

> This book deals primarily with astronomical concepts, but they are much like those employed in many other sciences, and by scrutinizing their development we learn something of scientific theories in general. For example: what is a scientific theory? On what would it be based to command our respect? What is its function, its use? What is its staying power? Historical analysis may bit answer questions like these, but it can illuminate them and give them meaning. (Kuhn 1957: 4)

This remark confirms our suspicion that Kuhn was well aware of the impossibility of defining 'scientific theory' adequately, and that his efforts to reduce its unfathomable and broad complexity to the concept of 'conceptual scheme' were a makeshift measure at best. Clearly this might have been achieved by a corresponding accentuation of other theory-like substitute concepts such as 'system' or 'model'. But for him what mattered was the fundamental and in our sense exoteric opposition between scientific 'observations' and scientific 'theories'. This is clear from the frequent occurrence of 'observations' and their 'theory'-like counterpart keywords. The latter include 'theories' (43), 'systems' models' (41), and 'conceptual schemes' (35). Naturally, this calculation is somewhat questionable; but it suffices for a comparison between the peculiar structure of the Copernicus book of 1957 and those of the 1952 article and the monograph of 1962. In each case an *unsuccessful* attempt is being made to overcome the impossibly multifarious character of the theoretical development of modern science by means of definitions that are universally comprehensible. From 1952 to 1962 a parallel process occurs in which Kuhn's *own* theory of science is gradually *expanded* beyond the sub-regions of the natural sciences that were the object of his observations. While the article on Boyle sought to generalise the theoretical basis of *chemical* experiments in the seventeenth century, the Copernicus book pursued a similar goal in the area of classical *astronomy*; *Structure* potentially went far beyond this to the *entire* development of the modern natural sciences. Hence the structural comparison of the three vocabularies demonstrates that Kuhn's esoteric programme was progressively overshadowed by being increasingly burdened with descriptive-exoteric material. This should in no way be understood as a criticism. We wish, rather, to point out a basic and insoluble dilemma faced by every history of science that seeks a universally comprehensible, i.e. exoteric theory of science that transcends that history. In that very sober, specialist, esoteric paper from 1952 on Boyle, to us an obscure thinker, one finds in embryonic form practically all of the exoteric problems that would come to occupy the philosophy of science in the early sixties.

There is another sense in which the Copernicus book is instructive for Kuhn's development. It has a foreword that, amongst all the autobiographical forewords, introductions and afterwords warrants special attention. On page ix two expressions that appear nowhere in the main text are mentioned entirely in passing: '*paradigms'* (in the sense of model examples) and '*scientific communities'*. This foreword is dated 1956 and therefore gives a more precise meaning to the somewhat mysterious hints about the origins of these two innovations contained in the much discussed foreword to *Structure* (Kuhn 1962: ix–x). There it is not stated at what *time* Kuhn took them over from Fleck and accorded them a central position in his own work. Later we

will discuss in more detail why this turning point in the development of his theory of science is important. For now it suffices to date it as November 1956 at the earliest, i.e. shortly before the final plans for *Structure*. The classical phase in the astonishingly rapid career of these concepts begins effectively the two articles written in 1957–58, 'The Essential Tension: Tradition and Innovation in Scientific Research', and 'The Function of Measurement in Modern Physical Science'. Both appeared in German translation in the collection entitled *Die Entstehung des Neuen* (Kuhn 1977);[4] this has a detailed subject index. In order to use it for comparison of vocabularies over time, we have divided the relevant keywords into two periods: *before* and *after* the publication of *Structure* in 1962. Thus the page numbers 125–168 and 239–326 cited in the subject index refer to 1958–61 and the rest to 1963–77. The result is this (since the corresponding page numbers can be looked up in the book itself, we will confine ourselves here to the frequency with which the keywords appear):

Table 6.4 Kuhn's 1958–1961 vocabulary

	Esoteric	Exoteric
Scientific Textbooks	18	
Scientific Traditions	18	
Normal Measurement		15
Paradigms (of textbooks)	8	
Normal Science	7	
Scientific Education	7	
Scientific Community	6	

Table 6.5 Kuhn's 1963–1967 vocabulary

	Esoteric	Exoteric
Paradigm		43
Philosophy, Philosophy of Science		26
Normal Science		15
Theory of Science		14
Scientific Community		12
Sociology		6
Scientific Education	6	
Scientific Tradition	5	
Scientific Textbooks	4	

4 Editors' note: Here Baldamus is referring German readers to the translation of Kuhn's *The Essential Tension*.

As one can see, the concepts 'paradigm' and 'scientific community' play a role in both periods, with the notable difference that they are less frequent before the publication of *Structure* than *after* it. Moreover it can be shown that in the early period they were introduced, along with other new terms such as 'scientific textbooks' and 'scientific education', as *undefined*, specialist-esoteric expressions. Each of them is explicated only through being placed in a context provided by specialist-technical illustrations from the history of the modern natural science. At this stage the only term that is defined with care is 'normal measurement'. Apart from that the concept 'paradigm' has a narrower meaning than it would have in the next phase: it refers only to knowledge derived from natural scientific textbooks, as opposed to the knowledge contained in journals, 'in which there appear no finished and recognised theories, merely those that are being developed' (Kuhn 1977: 255).

This is even more striking if we look at the vocabulary used between 1963 and 1977. Here there appear three (less frequent) terms that are just as undefined, unproblematic and self-evident as they were before: 'scientific education', 'tradition', and 'textbooks'. Among the remaining concepts, the exoteric, defined ones, the keyword 'paradigm' has finally made it to the top. If we look at the whole time span from 1952 to 1977, the gradual reduction in the number of specialist-esoteric and corresponding increase in the number of universally comprehensible, exoteric terms can be seen as a *continuous* development. We can give a step-wise demonstration of this trend in the structural transformation of the Kuhnian vocabulary by concentrating on five points:

Table 6.6 Kuhn's vocabulary over time (absolute frequency)

	1952	1957	1958–1961	1962	1963–1977
Exoteric Expressions	17	59	15	203	116
Esoteric Expressions	165	327	64	309	15

Table 6.7 Kuhn's vocabulary over time (%)

	1952	1957	1958–1961	1962	1963–1977
N	182	386	79	512	131
Exoteric Expressions	9	15	19	40	89
Esoteric Expressions	91	85	81	60	11

Clearly we cannot 'read off' from this table a connection with the current problem of the unclarified status of research into science. One must go further, in order to make more explicit the puzzling transformation in the relationship between esoteric and exoteric terms.

What is initially striking is the *continuity* of the relative increase in the number of exoteric elements. We are not dealing, then, with groups of words that suddenly appear but with long-term *series* of words in the manner of statistical time-series. Without wanting to anticipate an exhausting explanation, we can at once adopt a point of view that our material might easily cause us to neglect. It would be wrong to attribute this trend to the creative originality of the individual author, in our case Kuhn. Clearly the idiosyncratic features of a literary style play a role in all vocabularies. But against this it must be remembered that even Kuhn's most 'successful' paradoxical metaphors were borrowed from predecessors, especially Fleck. So we must assume that even here collective factors predominate. Although it would be laborious and time-consuming, studying the work of other historians of science – such as E.J. Dijkersthuis, H. Butterfield, J.B. Conant, A. Koyré, P.M. Rattansi or F.A. Yates – by the same method, comparing early and late vocabularies, would lead to similar results. It is always the case that an early work of an explicitly esoteric and specialist nature is superseded by subsequent explication by means of increasingly exoteric elements. And the deeper one goes into these writings, the stronger is the impression that this trend is *irreversible*. Even if a historian were to start on a new specialist topic, the exoteric expansion would be repeated in subsequent publications.

Now since today it is increasingly difficult to justify drawing a disciplinary boundary between the history of science and the philosophy and sociology of science, the tendency towards an increase in exoteric elements is by no means confined to strictly historical work. We have already seen that the most important of Kuhn's innovations, though originally grounded in history, found an increasing resonance in popular philosophy and sociology. With this, the fact that an exoteric basic vocabulary is ineliminable becomes an acute problem, one that none of the three disciplines is able to resolve. In order to tackle this problem in the most productive way one has to begin with the observation that those vocabularies that set the direction of highly specialised research in the modern natural sciences are marked by increasing precision, lack of ambiguity, and specificity. To the outsider, they appear to be increasingly 'esoteric'. One only need recall here the decisive role played by mathematics. Consequently, *within* the natural sciences *themselves* the creation of exoteric basic concepts has become increasingly precarious. On the other hand this means that the potential demand for 'universally comprehensible' and also comprehensive concepts becomes all the greater, and it appears to have opened up an historic opportunity for those disciplines that understand their task to be that of meeting this growing demand for exoteric interpretations.

Our analyses of vocabulary are an initial expression of doubt that such a demand can ever be satisfied. To be sure, from a quantitative, superficial point of view, one can observe that the number of exoteric novelties is constantly on the increase. However, if one examines their qualitative features, such as unsuccessful over-definition or the

predominance of paradoxical metaphors, it is hard to disagree that the prospects for the exoteric disciplines look bleak. Put in more general terms, the contradictory trend in the relationship between esoteric and exoteric vocabularies can be formulated like this: *the increasing precision of the esoteric vocabularies of the natural sciences is accompanied by a growing vagueness in the corresponding vocabularies of the exoteric disciplines*. Within the framework of the dominant philosophy of natural science, one might describe these counter-movements more sharply by saying that the more the natural sciences attain to empirical truth the more uncertain are their cognitive foundations. That is, we are dealing with a pragmatic paradox: despite the obvious plausibility of each of these trends, their factual coincidence is unacceptable.

At the present time there is no obvious way of resolving the exoteric paradox. If one wanted to start with the assumption that the alleged connection between esoteric and exoteric vocabularies was an illusion, because it was only observed in the frequency distributions of series of words within an unknown statistical universum, then the scope or selection of material would in any case be insufficient. Clearly, it cannot be denied that in order to explain why this procedure is incommensurable with the traditional paradigm of the philosophy of language, its realm of application would have to be considerably expanded.

The basic incompatibility of the two approaches can now be anticipated: whatever more comprehensive studies may bring to light, it will remain undeniable that the fateful increase in exoteric vocabularies cannot be reduced to a transformation of speech acts. For the remarkable fact about this trend is that it always occurs *behind the back* of the 'subject of language', against the intentions of those who are producing generally comprehensible innovations in the various disciplines of research into science (Baldamus 1976a: 186, 191). In any case it remains an open question in which way, if at all, an unambiguous and firm connection could come about between certain esoteric terminologies in the natural sciences and corresponding word series in the exoteric disciplines.

Certain points of departure for a future solution of this problem can be derived from the extreme difference between the reception of Fleck's work and that of Kuhn. *Genesis*, despite almost exclusively supportive reviews, was a complete failure. It was simply killed off by the dominant reception community of the 1930s, logical positivism.[5] When, finally, Kuhn discovered Fleck's importance, his reworking of Fleck's ideas led to an equally remarkable success. This peculiar turnaround is probably partly explained by the fact that between 1935 and 1962 the exoteric trend, particularly in the philosophy of science, became ever more marked, and critical interpretations of the natural sciences became increasingly influential. This allows for a further, supplementary hypothesis: one may assume that Fleck's critique of the presuppositions of logical positivist epistemology was so penetrating that it could not be understood, let alone rejected, using the tools available at the time. When Kuhn took from Fleck only those components

5 A particular influence here was Reichenbach 1938. I am grateful to Nico Stehr for pointing this out.

that could be made to fit with the increasing exoteric demands of the sixties, that critique was effectively blunted and thereby made into something that could finally be discussed, i.e. received. The fact that by means of this process of purification Fleck's discoveries in the philosophy of science, such as the interdependence of esoteric and exoteric circles, were brushed under the carpet, seems to be the price that the philosophy of science community must pay in order to confront the growing competition from sociology.

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Chapter 7
Networks

Wilhelm Baldamus

Editors' Introduction

Actor network theory, and other sociological approaches that describe social reality in terms of networks and other metaphors of connectivity, comes under Baldamus's scrutiny in this chapter. The fact that such approaches only appeared in the decades *after* the chapter was written is surprising, highlighting how Baldamus's thought was prescient in a number of areas. The chapter presents an extensive and in-depth discussion of the role of metaphors in the construction of sociological theory and presents an archaeology of 'net-like' metaphors, beginning with Georg Simmel and Pierre Duhem. Baldamus introduces a number of useful methods for constructing his critique of the concept of network, perhaps the most useful of which is his use of the social sciences citation index. Starting from this point Baldamus deconstructs a number of disparate sociological approaches that have deployed the concept of network. Throughout the chapter Baldamus points out that 'network' is left undefined, or defined synonymously, by those who deploy the concept.

* * *

The social sciences abound with metaphors. There is little doubt that the often remarkable longevity particularly of spatial metaphors is a contributing factor to the survival of obsolete theoretical conventions. The concept of 'networks' is of special interest here because it shows that even a metaphor which had hardly any explanatory power to start with can maintain its popularity for long periods for no tangible reason. It will also be seen from this example how an apparently unambiguous metaphor may circulate from one speciality to another without anyone noticing the delusions, pretences or sheer inanities which it entails. As there are several synonyms for the term net or network, like web, fabric, texture, mesh, tissue, etc., it would be difficult to track the beginning of regular uses of net-like metaphors. I shall therefore start with 'citation networks' in the sociology of science as the most recent instance and then look at earlier precedents in other specialisms.

The idea of analyzing citation practices for the purpose of sociological studies of the natural sciences popped up in the mid 1960s. At first the vocabulary fluctuated uncertainly between citation networks, clusters, patterns, contexts, data and behaviour. Within a decade the network image had proved itself as the most prevalent version. But there was as yet no reflection on the assumptions underlying its usage. It was at first simply a readily available tool for descriptive citation counting. Nor was it noted that

the rapid growth of the citation literature had led to all kinds of unconvincing theoretical speculations in a 'structuralist' vein. The tangle of widely conflicting extensions of the network metaphor was exposed by Susan E. Cozzens (Cozzens 1982) who voiced a strong criticism of the naïve preoccupation with citation measurements by stressing the undisclosed *theoretical* implications of the problem:

> Since the creation of the annual *Science Citation Index* (SCI) in 1961, citation analysis has been used as a tool to study numerous aspects of the social structure of science … Derek Price (1965) claimed that the 'fabric' of new scientific knowledge was being 'knit' through citations, and that social networks ('Invisible Colleges') accounted for 'networks' of papers created by citation 'ties'… A single well-known work on this topic used citations in five different ways, according to its subject index: as measures of diffusion, influence, quality, recognition and utilization (Cole and Cole 1973). But in fact we know very little about who cites whom in science, and why … (Cozzens 1982: 233)

While the invention of a standardised citation index in 1961 was a consciously planned act, the *practice* of more or less standardised reference habits, which is a silent convention, is of course much older. It emerged spasmodically and unsystematically in isolated fields of the natural sciences in the second half of the 19th century. The convention was (and still is) based on the ethical principle that private individual claims to new knowledge, discoveries for instance, must be offered as potential public knowledge to the professional community. At the same time it was assumed in this convention that knowledge grows cumulatively by accretion. Our next problem is this: what happens to these practices in a situation of proliferation, outwardly indicated by the dilemma that specialists in numerous fields are no longer able to keep abreast in their own specialisation, yet must continue to provide references of relevant previous researches?

It is only natural that sociologists of science are influenced in their own approach by the subject matter they are investigating. Scientists typically believe that the cognitive net-proceeds of research tend to be cumulative in the course of time. All references to past research are therefore recorded in a chronological sequence: Jones (1970), Smith (1971), Miller (1972) and so forth. If in certain scientific fields the rate of successive contributions is faster than in others this is an indication of progress. Obsolescence will reveal itself simply by the gradual disappearance of older references. The prevailing inductivist climate of opinion in the sociology of science is strengthened by the fact that sociologists who by temperament or education take a special interest in the natural sciences are prone to find this branch particularly attractive. The essentially physical properties of the network metaphor would seem to make it very compatible with science-based orientations. Consequently one would expect a totally different attitude among specialists in sociological *theory*. But this is not so. The following quotations, taken from mathematical sociology (a sub-branch of the central theoretical tradition) demonstrate that the capabilities of the network metaphor are nearly boundless.

In 1976 the *American Journal of Sociology* published a two-part paper of over one hundred pages by a team of mathematical sociologists at Harvard and Pennsylvania University, H.C. White, R.L. Breiger and S.A. Boorman, entitled 'Social Structure from Multiple Networks. I. Blockmodels of Roles and Positions. II. Role Structures'. The project was financed by the National Science Foundation. To minimise the mathematical technicalities of the text, I have selected statements which are self-explanatory for the purposes of illustrating the far-reaching adaptability of the imagery of 'nets', 'strands', 'lattices', 'interstices' and 'nodes' in theoretical contexts:

> Abstract: Networks of several distinct types of social tie are aggregated by a dual model that partitions a population while simultaneously identifying patterns of relations. Concepts and algorithms are demonstrated in five case studies involving up to 100 persons and up to eight types of tie, over as many as 15 time periods. In each case the model identifies a concrete social structure. Role and position concepts are then identified and interpreted in terms of these new models of concrete social structure. Part II, to be published in the May issue of this Journal (Boorman and White 1976), will show how the operational meaning of role structures in small populations can be generated from the sociometric blockmodels of Part I. (White et al. 1976: 730)

> … Network metaphors date back at least to Simmel (1950, 1955; first published in 1908) and the so-called formal school of German sociologists. Simmel emphasized the ubiquity of social networks based on 'the actual similarity of talents, inclinations, activities, and so on (Simmel 1955: 128) and which cross-cut the categorical attributes of persons. Von Wiese, strongly influenced by Simmel, stressed the multiplicity of types of social ties and the analytic desirability of reducing network structures. If the 'constantly flowing stream of inter-human activity' were halted in its course for one moment, von Wiese (1941, pp. 29–30) suggested, we would observe 'an apparently impenetrable network of lines between men. There is not only a line connecting A with B, and B with C, etc., but C is directly connected with A, and, moreover, A, B, and C are enclosed within a circle. Not only is there *one* line connecting A with B, and not only *one* circle in which they are both enclosed, but there are many connecting lines. … A static analysis of the sphere of the interhuman will … consist in the dismemberment and reconstruction of this system of relations. Outside this network, above and below it, there can be nothing that is social, unless we leave the plane of empirical observation'. (White et al. 1976: 730–1)

> During the past decade, the network metaphor has become increasingly popular with social scientists; it has even penetrated the conservative precincts of economics (Boorman 1975; Marschak and Radner 1972; Schelling 1971; see also Leijonhufvud 1968). Sociologists' and anthropologists' attempts to develop the metaphor into operational concepts have taken two directions. One has

> emphasized the paths or 'threads' in a single network: the manner in which long chains of contact wind their way through large social systems (Milgram 1967; Pool and Kochen 1958; Rapoport 1963; Coleman 1964; Hunter and Shotland 1974; White 1970a, 1970b; Lee 1969; Granovetter 1973, 1974). The second has emphasized the 'knittedness' of interconnections within a network and the overlaps between multiple (many-stranded) types of networks for a given population (typically small: see Theoretical Background section, below). Our operational concepts follow the second tradition but are consistent with the first. (White et al. 1976: 730–1)

The reference to the school of formal sociology, inaugurated by Simmel and von Wiese, and the claim that this study is consistent with the post-classical concept of social systems, gives the impression that the subject matter is strictly theoretical. A high level of abstraction is also suggested by the mathematical vocabulary of 'concepts and algorithms' in the Abstract and by several technical footnotes on the types of algebras and algorithms which proved most productive (e.g. note 14, p. 737, explains that both the MDSCAP and the INDSCAL algorithm were found to be indispensable in operationalising the blockmodel network device). Yet in a section entitled 'Theoretical Background' Maine, Tönnies, Durkheim, Weber and Linton are jointly accused of having presented 'categorical' descriptions of social structure 'without any solid theoretical grounding'. By contrast White et al. argue:

> In our view the major problem with postclassical social theory has been that its concepts remain wedded to categorical imagery. All sociologists' discourse rests on primitive terms – 'status', 'role', 'group', 'social control', 'Interaction', and 'society' do not begin to exhaust the list – which *require* an aggregation principle in that their referents are aggregates of persons, collectivities, interrelated 'positions', or 'generalized actors' … In contrast to the standard wisdom, there is a growing list of empirical findings regarding the effect (and frequency) of 'accidents' and 'luck' in the actual functioning of societies: the transmission of useful information among scientists (Menzel 1962), the attainment of general economic success (Jencks et al. 1972), and the location of desirable jobs (Granovetter 1974; see also Boorman 1975). These findings force us to ask whether the stuff of social action is, in fact, waiting to be discovered in the network of interstices that exist outside the normative constructs and the attribute breakdowns of our everyday categories. (White et al. 1976: 733)

Here we can see a peculiar epistemological difficulty which occurs on several occasions in both parts of the paper. Verbally this problem is that the neo-Kantian dichotomy of contrasting 'theoretical' concepts with 'empirical' experience is found to be unacceptable. Considering that this component of the Kantian legacy has been an unquestionable doctrine throughout the development of sociology as a separate discipline, the unmitigated refusal by White et al. to adhere to it is important. It means that the entire history of mainstream theory as well as survey

methods is obliterated in one stroke. My first reading of the paper, concentrating on the methodological statements and the concluding paragraphs, nearly prompted me to file it away under the label 'Sundries' (in the sense that whatever they are trying to do, it can't be sociology). But eventually I decided otherwise, for several reasons. The paper had been accepted by a major sociological journal; it was supported by a prestigious funding organisation; the bibliography of 119 titles left no doubt that this was certainly a growth area, especially as a surprisingly large number used an apparently well established vocabulary of network-like imagery such as 'clustering', 'webs', 'chains', 'patterns', 'bonds', 'ties', or 'blocks', in addition to interesting-sounding network specialities: contact networks, social networks, social cooperation networks, multiple networks, interpersonal attraction networks, network flows, acquaintance process networks, communication networks, role networks, team networks, job information networks and others. A few of the titles are, it would seem, deliberately pungent: 'The Strength of Weak Ties'; 'Some Muddles in the Models'; 'On Being Just Complicated Enough'; 'Crisis in a Cloister'. The diversity of topics is impressive, ranging from 'The Search for an Abortionist' or 'The Care and Exploitation of Non-Human Primate Infants by Conspecifics Other than the Mother' to 'Island Models for Takeover by a Social Trait Facing a Frequency-dependent Selection Barrier in a Mendelian Population'.

The great variety of topics accessible to a network analysis is in fact used by White et al. as an argument to expose the inadequacy of the traditional work of professional sociologists and anthropologists on role structure (pp. 733–37, 1384–88) or similar theoretical conceptions. Briefly, the argument is this. Numerous kinds and types of roles have been investigated in the past: Italian immigrants, sick people, trade unionists, diplomats, federal judges, soldiers, bureaucrats, scientists, industrial workers, or whatever. However insightful and meticulous, these empirical studies failed to generate a general theory of role structure. The conceptual vacuity of this approach is repeatedly emphasised, but on each occasion the alleged superiority of network studies is brought out only by a *descriptive* vocabulary in which synonymous words like 'concrete', 'observable populations', 'inductive', 'actually exist', 'phenomenological', 'particular', are reiterated without explicit definitions. The most frequently occurring term is simply 'person' (or 'concrete person' or 'individual').

Hence to understand the significance of the transition from conventional structural-functional theory to networks of persons (or populations) we have to consider the *contexts* in which this vocabulary is employed. The most conspicuous context is clearly indicated by the use of special mathematical models, as notably matrix algebra. The particular aspect of matrix algebra which is relevant for network analyses is that it appears in the shape of a rectangular array of symbols, letters or numbers each of which exhibits a particular *binary* property, i.e. it is *either* present *or* absent. The selected property may be, for instance, a 'contact', 'acquaintance', 'communication', 'awareness', etc., between persons which can be recorded by an observer. A typical case of this sort, called 'blockmodelling', is described as follows:

Consider ties of one type, from one person to another, arrayed as a square matrix, with a row *and the corresponding column* of the matrix assigned to each person in the population. Create a separate matrix for each type of tie. Figure 1 presents three kinds of tie among biomedical researchers specializing in the neural control of food and water intake (B.C. Griffith, V.L. Maier and A.J. Miller, *Describing Communication Networks through the use of Matrix-based Measures*. Mimeographed. Drexel University, Philadelphia, Graduate School of Library Science 1973). An entry of *X* means that a tie is present; a blank space that a tie is absent. The left-hand matrix represents all ties of mutual personal contact. The other two matrices represent unawareness of man or his work, distinguishing pairs of individuals who reciprocate unawareness (the right-hand matrix) from pairs in which only one individual indicated an unawareness tie (the middle matrix). Only an arbitrarily chosen subset of 28 members of the full sample (n=107) is included.' (White et al. 1976: 737–9)

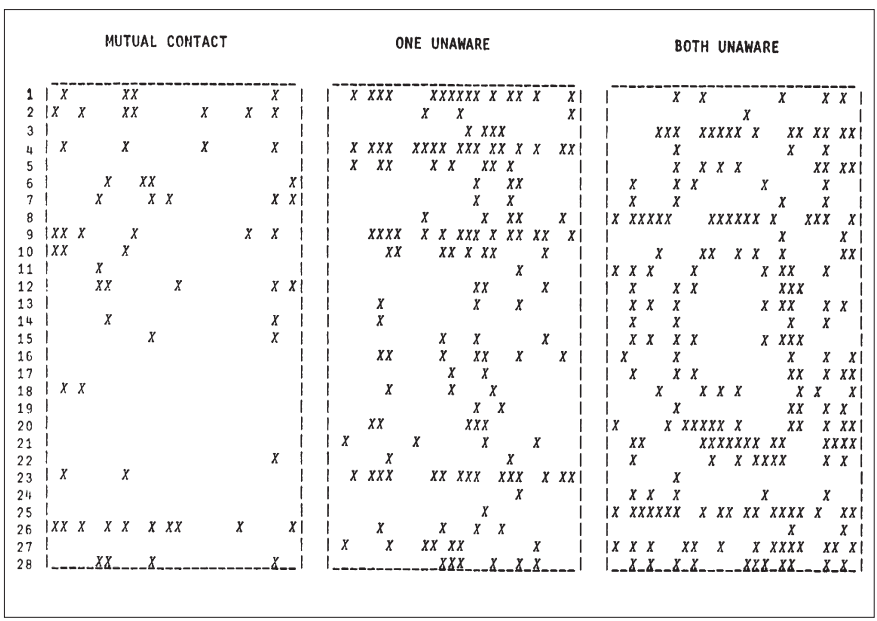

Figure 7.1 White et al.'s matrix diagram

For the benefit of nonmathematical sociologists a few instructions about binary matrix tables may be in order. Ordinarily a single 'block model' would be shown as a square, rather than a rectangle. Thus, the numbers 1 to 28 (representing the selected subset of the sample) are displayed not only as a vertical column in the

left-hand margin, but also as a *horizontal row* at the top of the diagram. A given block would then look like a square consisting of 28 x 28 sub-squares, and the purpose of the binary ('present/absent') principle can be seen more clearly.

Table 7.1 Example of a binary matrix table

present / absent	1 Bill	2 Tom	3 Bob	4 Sam
1 Bill				
2 Tom				
3 Bob				
4 Sam				

The convention of a square in which a binary division is shown by a diagonal stroke in the upper left-hand corner suggests a – surprising – resemblance with Habermas's (1981) sociological schemata, but this will be treated elsewhere.[1] I have supplemented the numbers with personal names; this has been done to separate the descriptive observations as clearly as possible from the mathematical methodology. The most striking feature of the whole paper is in fact its overcrowding with long descriptive passages where the network idea is proliferated into all kinds of more or less synonymous metaphors taken from everyday language.

No reasons are given for this procedure, and it is therefore very difficult to explain why the paper is so long. Part of the explanation might be inferred from the self-imposed impossible task of the authors to articulate essentially subjective experience in terms of physically 'observable' representations which then in turn can be manipulated by quantitative computations. These difficulties are frequently acknowledged, but only in an offhand manner. A typical example is the concept of role reciprocity:

> … the usual concept of role reciprocity remains heavily tied to subjective perceptions by the parties which are at once difficult to establish and often not relevant to the objective structure. … Is my liking for James a social fact or only a psychological quirk if any of the following hold true: (1) he does not

1 Editors' note: Baldamus pursued this theme in the two late articles he published in *History of the Human Sciences* (Baldamus 1988 and 1992).

> reciprocate my liking; (2) he does not reciprocate with any other type of tie; (3) he is not aware of my sentiment toward him; (4) he does not know my name; (5) he does not know who I am tied into; (6) he does not recognize me; (7) no one lese in the population is aware of my liking for Bill? Can such questions be answered by either observation of questionnaire? (Boorman and White 1976: 1391)

The emphatic disdain for psychological quirks and subjectivist premises runs through the entire paper as a contrast to the 'robustness' and 'density' of the aggregative ties and bonds of the blockmodel images (e.g. p. 1395, 1407, 1424, 1426). Another reason for the prolixity and length of this work is the vacuity of the binary 'present/absent' criterion. While in functionalist role theory the irrelevance of subjective-individual attributes was overcome by focussing on the *normative* end products of socialisation processes, the notion of merely existing or non-existing network contacts is so capacious that it eliminates not only the psychological but virtually *all* contents of social structures. The examples of case studies exploring mutual awareness/unawareness, likes/dislikes, enumerable/zero contacts, friends/enemies, etc., illustrate very clearly the enormous range of the descriptive applicability of binary aggregation techniques. But they also suggest that their explanatory value, relative to the amount of work and costs, is very small. Contrary to the ambitious claims in the introductory and theoretical sections of the paper, the sparsity of 'nontrivial applications' is frequently admitted in the mathematical contexts of the case studies, as also in the concluding paragraph:

> What makes a society human? Speaking as a sociologist, one is tempted to seek the answer in the existence of roles. The problem is not so simple: … it is clear that at least the higher primates have well-developed complexes of stable social relationships which seem to behave much like human roles … A somewhat more sophisticated hypothesis is that the characteristic features of human society lie in the peculiarly intricate complexes of interlocking roles which only men can sustain …
>
> We see at present no intelligent way to develop role interlock for open networks extending through large populations, even thought this topic is much closer to the heart of sociology than is small-group structure (Milgram 1967; White 1970; Granovetter 1976). From an analytic standpoint, the present machinery is suggestive of social castes and classes and their interrelations on a macroscopic level of large-scale social structure (Mayer 1960; Boyd 1969a). Various classical hypotheses lend themselves to possible blockmodel and algebraic reformulations: can the classical Aristotelian theory of revolution (as the result of separation between political and economic elites) be made operational through tracing the emergence of certain kinds of zeroblocks? The next analytic task is to provide ways to probe how role structures of the kind we have identified actually come into being, through the continuing accommodations and manipulations of all

> individuals acting simultaneously (Leach 1954). Some first steps have recently been taken in this direction. (Lorrain 1971; White 1973; Spence 1974; Boorman 1975a) (Boorman and White 1976: 1441–2)

The failure of the network metaphor to advance sociological theory by means of mathematical aggregation techniques is not really surprising. Even the most imaginative manipulation of its literal meaning cannot get rid of the strictly physical intuitive implications. We have seen the same limitation of this metaphor when it is used to provide a theoretical basis for scientific citation practices, though in that context the deceptions which it may cause are less serious. It should be noted that these two contexts – sociological theory and scientific citations – are completely incompatible. Outwardly this can be inferred from the fact that there are no cross-disciplinary references.

I will now present a *third* application of network imagery, its usage in philosophy of science to describe the connectedness of *theories* within particular fields of science at certain stages of their development. The oldest and most widely discussed has become known as the Duhem network model. It was invented by the French scientist and philosopher Pierre Duhem in 1906 in *La théorie physique, son object et sa structure* (Duhem 1954). Duhem made important contributions to specialised natural science fields (e.g. thermodynamics) since about 1880. But in this work his aim was to reach a wider public of educated laymen. As an elementary introduction to the history and the then current practices of physicists, the book was an unquestionable success. The second edition of 1908 was translated into German, Italian and English. The network model is presented in chapter X, paragraph 2: 'A Physicalist Experiment Can Never Refute an Isolated Hypothesis, but only a Whole Group of Theories'. When one reads this today, one is struck by the vagueness and ambiguity of the approach. There are no stipulative definitions and the basic idea emerges only from the synonymous use of various holistic images, such as 'group', 'network', 'organic structure' and 'framework', which are illustrated by simple episodes from the history of experimental physics.

It is difficult to account for the enduring popularity of the holistic tenets of Duhem's philosophy of science. Although nothing else from his work has been resuscitated, the network idea was repeatedly extended or revised and on each occasions widely debated. After three decades it had reached the status of an eponymous invention, 'the Duhem thesis'. Yet the ambiguities of the network vocabulary increased with each new interpretation. The most influential facelift operation was effected in 1951 by W.V. Quine (Quine, 1951), strangely enough through affiliating Duhem's simplistic holism with the sophisticated logic of sentential calculi and semantic networks. Additional critical revisions from different metaphysical standpoints followed suit but attracted less attention. Then, in 1960, when the debate on the Duhem-Quine thesis was still in full swing, a new controversy was stirred up by A. Grünbaum with a paper 'The Duhemian Argument' (Grünbaum, 1960). Grünbaum and his followers side-stepped the tangle of philosophical difficulties by shifting the focus back to Duhem's concern

with *physics*. Inevitably his eclectic and out-of-date illustrations were replaced by current mathematical space-time theories. The key term in this movement is 'the D-thesis'. In less than a decade the technical literature reverberates with several 'nontrivial' re-interpretations of the old 'trivial' D-thesis. A survey by R. M. Yoshida, published in 1975 in vol.42 (p. 29–45) of the same journal tries to reduce the total of as yet unrefuted Duhemian theses to *five*. However, a new edition of the German (1907) translation of Duhem's work, published by L. Schäffer in 1978, provides a bibliography of well over a hundred Duhem titles and indicates an exponential growth rate from the fifties onwards.

I became interested in the Duhem literature in 1979 when I was looking for historical material to clarify the problem of obsolescence and proliferation in the philosophy of science. At that time it seemed to me that the story of the network model could be used as a paradigmatic example to show that the demolition of a redundant invention does not come about by the realisation that a mistake had been made. What really happens is a process of makeshift *repairs* during which successive generations search for new applications. This results in an unintended and uncontrollable proliferation of the literature until a point is reached where the surviving tenets of the initial model are barely recognisable. In the present example this stage was clearly apparent in the aftermath of the Yoshida (1975) paper 'Five Duhemian Theses'. Though no one admitted that the holistic network image had been an impediment right from the start, any attempt at making yet a further repair or adjustment could not have been contemplated without a laborious clearing-up operation. Even for a sociological case-study on the proliferation syndrome the preliminary task of sorting out the extensive literature of the end-phase seemed hardly worth the effort. I spent three months on making an annotated bibliography and then drafted a short paper entitled 'Duhem's Error'. However, when I wrote up the conclusions, I was not satisfied. The drift of my argument was that there was as yet no agreement about the difference between the trivial and non-trivial versions of the network model. This was unconvincing because it left the possibility open that a reliable criterion to define 'triviality' as a necessary and sufficient condition of proliferation might still emerge in the future. Alternatively the community of the philosophy of science would eventually reach a consensus to abandon the model altogether.

In the end I settled for the second alternative. This meant I could distinguish four phases in the career of the philosophical network model. Initially it was introduced to suppress major epistemological difficulties which, if admitted, would have threatened the holistic opposition to the school of logical positivism. The second stage was a period of proliferation in which the metaphor acted as a catalyst generating new applications and further ramifications. But eventually the unsolved difficulties come to the surface and there is a long phase of all kinds of makeshift repairs which compete and overlap with each other. In the fourth phase a tacit consensus emerges to abandon the model and all its deceptive implications. It is replaced by descriptive technical terms.

The above is a summary of diary notes I made in January and February 1980. The main reason however why I had lost all interest in this matter was an external event which happened unexpectedly and which changed my whole outlook on the philosophy of science. On the 2 March I received N.I. Kondakow's *Woerterbuch der Logik*, published in Leipzig (East Germany) 1978 (third edition; the first edition was in Russian and published in Moscow in 1971). It was sent to me by Thomas Schnelle from Warsaw where he had seen it displayed in the window of an academic bookshop. I had been working for some time on mathematical and logical eponyms, collected from English language journals and textbooks, and Thomas had promised to look out for textbooks available in East Germany or any other communist country. He knew that I was hoping to show that even the most abstract and highly formalised forms of knowledge were not immune to ideological bias and that therefore a sociology of mathematics and logic would in principle be feasible. When I opened the book I knew within minutes that I had been on the right track. However, I also realised in the course of the following weeks that the intertwining of orthodox communist ideology and very technical mathematical logic was so extraordinary and improbable that western non-mathematical sociologists would simply not believe that the mathematical entries should be taken seriously. And there were many other problems: of the 168 items of the list of references in the appendix, over a third are in Russian; as pointed out in the Preface, the dictionary is addressed to readers of different levels of competence, from A-level students to various professional specialists; a quantitative content analysis would have been misleading because it could not possibly cope with the symbolic notations, diagrams and tables.

I got so involved in the preliminary work of selecting, translating and classifying the most interesting entries that I forgot all about 'Duhem's Error', and the final demise of the holistic Duhem-Grünberg-Quine thesis. It was only in April 1981 that I discovered I had been wrong: a resuscitation of the whole network imagery had occurred; not only that, the revived model extended far beyond the physical sciences: in the same collection, *Wissenssoziologie* (Stehr and Meja 1981) in which my paper on formal logic was printed I found two substantive contributions, both translated from English manuscripts, which acknowledge, in similar terms, this model as the most crucial inspiration to their work. Equally surprising was the immense *philosophical* scope of these two papers, apparently undaunted by the long and dismal path of the Duhem controversies which were confined to classical (i.e. non-mathematical) physics only.

The first paper is by David Bloor. It is called 'Klassification und Wissenssoziologie: Durkheim und Mauss neu betrachtet' (Bloor 2005: 67–92).[2]

2 Editors' note: In the original paper from 1982 Baldamus translates, 'roughly', passages from Bloor and Barnes that were originally published in German in 1981 in the collection edited by Stehr and Meja and entitled *Wissenssoziologie*. In 1984 the English version of *Wissenssoziologie* was published as *Society and Knowledge* (Stehr and Meja 1984). In 2005 a second edition appeared with some of the original contributions removed

The point of departure is a study by Durkheim and Mauss, *Primitive Classsification*, first published in 1903 and translated into English and introduced by Rodney Needham in 1963. In this work an attempt was made to *reduce* all classifications of physical entities to the *social* divisions in human societies: the classificatory system of *things* reproduces or mirrors the classification of *human beings*. One of the prominent examples of Durkheim and Mauss is the seven-part system of the cosmological mythology of the Zuni which was derived from the corresponding seven-part social organisation of their settlements. Bloor indicates that the history of this thesis is a story of numerous attacks from anthropologists, ethnographers and sociologists. He remarks that a review of these criticisms would be pointless, because it is now possible to re-formulate and extend the thesis so as to give a basic account not only of classification but also 'knowledge' in the widest sense – for, essentially, all knowledge (mythical, scientific, and philosophical) *is* classification. He supports this view with familiar, but newly interpreted, examples from the history of classical physics, chiefly Newton and Robert Boyle. Realising that any sociology of knowledge which aspires to account for *all* knowledge leads to a self-contradictory epistemological *regressus*, in the sense that if all knowledge is primarily a social product, then this law itself may be such a product, Bloor refines his thesis by the notion of 'coherent' knowledge. Thus his central argument is that any kind of 'coherent knowledge' requires necessarily a *universal* system of classification. He then goes on as follows:

> Fortunately such a model exists. It is called the network model and has been developed by the philosopher of science Mary Hesse following the work of Duhem and Quine. Originally offered as an analysis of scientific inference, it applies as much to myth or common sense as to science. (Bloor 2005: 68)

After discussing a number of illustrations of the Hesse model, Bloor arrives at the following conclusion:

> … predicates and classifications are organized into laws, so laws themselves exist within systems of a still higher order. They form networks. The simplest network comprises two or more laws referring to the same classification … This…system of knowledge … consists of a number of names or classes that can be taught by pointing out accepted instances. The names are joined by laws, which sometimes give cross-references to the same classification. These could

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and some new ones added, to reflect what the editors called 'developments in the sociology of knowledge'. Baldamus's was one of those that fell by the wayside, something that might well have amused him, particularly as those by Bloor and Barnes were retained. The quotations from Bloor and Barnes are from the published English versions of their essays as they appear in the 2005 edition of *Society and Knowledge*, now available online. They differ slightly from Baldamus's renderings of them in his original manuscript but not so as to make a difference to the argument.

> be said to form a network if one thinks of the cross-references as the knots of a net, and the laws as the strings that connect them. (Bloor 2005: 70)

Considering that classification systems are claimed to be 'universal', it is strange that nothing is said about the long and rich history of formal classification systems of *natural* species of plants and animals, beginning in the sixteenth century. How can such classifications be derived from *social* divisions? And how can we apply the Hesse model to the development of quantified atomic theory, set in motion (around 1803) by Dalton's discovery that atoms of different elements can be identified by precise *numerical* differences in their *weights*? I mention this example because, according to Bloor, the sole basis of classification is our capacity to perceive *qualitative* likeness (Aehnlichkeiten) and difference. In this view there is no way of grasping our capacity for counting. It is therefore hardly surprising that the entire realm of the logic of classes, sets or types, of number theory, topology, etc., is simply ignored. Consequently, the continuous impact of mathematical advances on modern physics and cosmology is also disregarded.

The other contribution to *Wissenssoziologie* which is similarly grounded in the Hesse network model is a paper by Barry Barnes, 'Über den konventionellen Charakter von Wissen und Erkenntnis' (Barnes 2005). Here again the aim is to provide a universal holistic model, applicable to all types of knowledge:

> There are of course many forms of knowledge, and it is difficult to propose arguments and offer illustrations which encompass all of them. … Fortunately, the severity of this problem has been much alleviated by the demonstration that no absolute distinction can be drawn between theoretical and observational knowledge. This finding, carefully considered and justified by Mary Hesse in her study *The Structure of Scientific Inference*, suggests a general analogy between a great range of concepts and forms of knowledge, including empirical common sense and esoteric scientific theory. It indicates that conclusions of general significance can be derived from the study of specific, everyday, readily intelligible concepts. (Barnes 2005: 152)

From these premises Barnes constructs a general model of the process by which human beings come to know and use concepts. This process is called 'learning'. Barnes's fundamental argument is that the learning process has two, and only two, elements which are in essence identical: learning how to use concepts and learning how to classify.

> Firstly, people learn as they move around in an indefinitely (sic) complex physical environment of which they are aware; learning takes place in the course of the reception of complex inputs of information from 'experience' or 'the world' … Second, learning always initially occurs within a social context; to learn to classify is to employ the classifications of some community or culture,

> and this involves interaction with competent members of the culture. (Barnes 2005: 152)

These two propositions are then explicated in detail by constructing a situation in which a novice is confronted with the task of learning the forms of classification of the natural environment as it is accepted by his culture at a given point of time. This rudimentary situation is described by showing how the incompetent learner L interacts with a competent member or teacher T within a particular environment. Barnes does not define his notion of 'competence' but the context makes it clear enough that this notion supplies the essential link between the learning process of everyday knowledge and the learning process in a scientific community. We are then shown by recourse to very simple examples that the total learning process has only two necessary constituents. (1) It has to produce a direct relation between a concept and an object of the environment; this is done by the act of 'pointing'. (2) The concepts have to be connected with each other; this is achieved by the act of 'generalization'. It would be very difficult, according to Barnes, to imagine any other strategies which might be available to T. He postulates therefore, for the purposes of the subsequent elaboration of the model, that no other strategies of teaching exist. Thus (he argues) L learns the entire information of his group on concepts by means of those two processes. 'And, since every T was once an L, it follows that all competent members of a community have built up their knowledge via such process. This suggests that the knowledge of any competent member can be modelled by the scheme in figure 1, which I call a 'Hesse net'' (Barnes 2005: 153–4).

The remainder of the paper follows the well-known tradition of a strictly conventionalist theory of knowledge. There are no criteria of truth, empirical adequacy or logical coherence. Knowledge consists of collectively accepted (or 'received') concepts and generalisations. The scheme of the Hesse net pictures the concepts as knots and the generalisations as the threads which connect the knots with each other. It is only fair, however, to mention that Barnes in a footnote points out that his use of the Hesse net is of a different kind than Hesse's 'network model of universals'. In contrast to Hesse's standpoint, his own use of the network is 'instrumentalist': the question whether or not the network is composed of true or false statements is not applicable (Barnes 2005: 172).

In passing, it is interesting to note that both Bloor and Barnes are using the net metaphor as a strictly two-dimensional image: the threads connecting the knots are spread out across a flat surface. As a result the net cannot capture the temporal dimension of the genesis of new concepts and new levels of abstraction. Thus, the problem of the growth and the vanishing of 'knowledge' remains inaccessible and undecidable in this approach. As regards the Barnes thesis, one might possibly argue that a temporal dimension could be incorporated by assuming that there exists an infinite succession of (competent) teachers and (incompetent) learners forever engaged in transmitting knowledge in terms of innumerable classifications.

Obviously, to clarify the origin of the Bloor-Barnes approach I had to take a look at the original Hesse model of universals. This was initially presented as an

extension of the Duhem-Quine thesis in a paper entitled 'Is there an Independent Observation Language?' in 1970 which has been reprinted as Chapter III in a collection of essays, *Revolutions and Reconstructions in the Philosophy of Science* (Hesse 1980). The paper is divided into six sections, the third of which is entitled 'The Network Model'. Here we are told:

> The foregoing account of theories, which has been presented as more adequate than the deductive two-language model, may be dubbed the *network model* of theories. It is an account that was first explicit in Duhem and more recently reinforced by Quine. Neither in Duhem nor in Quine, however, is it quite clear that the netlike interrelations between more directly observable predicates and their laws are, in principle, just as subject to modifications from the rest of the network as are those that are relatively theoretical. (Hesse 1980: 91)

Although the vocabulary and the context of this statement made it evident to me that Mary Hesse was addressing exclusively a philosophical audience, it certainly confirmed that it had been her version of the Duhemian tradition which Bloor and Barnes had reinterpreted to transplant the network metaphor into sociological contexts. But the really remarkable thing at that time was that here at last I had stumbled upon an eponymous innovation *in statu nascendi*. Whether or not Bloor and Barnes have succeeded in transforming Hesse's philosophical innovation into a sociological one remains to be seen; their *intention* was clearly to establish a (datable and relatively durable) *eponym*.

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