

OXFORD STUDIES IN THE HISTORY OF ARCHAEOLOGY

FROM GENESIS TO PREHISTORY

The Archaeological Three Age System
and its Contested Reception in Denmark,
Britain, and Ireland



PETER ROWLEY-CONWY

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Preface

This book has come about as the result of two different things. One is an accident of birth and upbringing, the fact that I was brought up bilingual in English and Danish. This placed the Scandinavian archaeological literature within my reach, including the early publications of the Three Age System, and I started reading these simply for fun. As a student in Cambridge in the 1970s I had attended lectures by that great raconteur of archaeological history, Glyn Daniel, and subsequently explored his writings and those of others on the early history of archaeology. Comparing these with what I was encountering as I read the original material, I came to realize that the history of archaeology presented in English language textbooks is at the very least incomplete; often downright wrong. Quite a lot of work has been done over the years by Scandinavians interested in the history of their archaeology, and this has provided considerable insights to those who can read Danish or Swedish. Not much of this has been published in English, however, and a great deal remains to be done in general. The Scandinavian chapters of this book are therefore an attempt to redress the balance, and present a more comprehensive picture of the crucial period in Scandinavian archaeology.

The other major impetus was the realization that we know even less about the history of British and Irish archaeology, at least so far as the adoptions of the Three Age System is concerned. Histories of archaeology have, understandably and correctly, tended to concentrate on the great moves forward—the discovery of human antiquity, or the decipherment of long-forgotten scripts excavated from ancient city ruins. But how the Bronze Age, for example, became an accepted unit of time has received much less attention. It sometimes seems to be thought that, after its initial promulgation in Copenhagen and Lund, there is hardly any story worth telling. The Scottish pioneer Daniel Wilson is justly portrayed as a major figure, but few others are ever discussed. This was thrown into sharper focus by the research project in the Department of Archaeology in Durham University,

run and carried out by Dr Pam Graves and Dr Anne O'Connor, on William Greenwell. This led to my exploring him and many of the other figures of nineteenth-century English, Scottish, and Irish archaeology. This led once again to the realization that there was a huge untold story out there. The later chapters of this book are therefore an attempt to tell this story, and to see why things worked out so very differently in London, Edinburgh, and Dublin.

The raw data for this book are purely and simply the books and papers that people *wrote* at the time. All this literature has come from a variety of sources, and I have pleasure in acknowledging the various libraries. First must come Durham University Library, which houses a substantial selection of the early materials; a lot of items it does not contain were obtained for me by Judith Walton of the inter-library loans department. I also thank the libraries of Queen's University, Belfast (Special Collections); the British Library; Cambridge University Library; Glasgow University Library; Leeds University Library (the Brotherton Library); Newcastle University Library (the Robinson Library); and the National Library of Scotland. But a great deal of the material consulted had to be bought, which involved a lot of time searching antiquarian lists on the web. The major search engines I have used are <<http://www.antikvariat.net/>>, <<http://www.eurobuch.com/>>, and <<http://www.bookfinder.com/>>. A lot of time has also been spent in conventional bookshops; I am particularly grateful to John Turton and Ben Bainbridge of Turton's Bookshop, Willington, Co. Durham; and in Copenhagen to Christian Westergaard of Lynge & Søn and Anders Stensager of Kaabers. JSTOR is an invaluable web source for many of the early journals, as is the Thomson Gale database ECCO (Eighteenth Century Collections Online) for many eighteenth-century sources; and the Society of Antiquaries of Scotland, as ever the most progressive in Britain, has placed the entire contents of *Archaeologica Scotica* and *Proceedings of the Society of Antiquaries of Scotland* on the web. By these various means it is possible now to obtain in half a dozen years a library that formerly could not have been built up in a lifetime. But inevitably, it is not possible to locate every item one would like. In this book, if a cited work appears in the list of References it has been examined by the author; if not, the secondary source is quoted. When repaginated reprints are quoted, this is indicated in the text, e.g. (Rask 1818

[1993: ii–iii]). When facsimile reproductions have been consulted this is indicated in the references.

In Durham, numerous colleagues have put up with having their ears bent by exciting discoveries about nineteenth-century archaeology, and despite this have been happy to offer assistance, advice, and encouragement. Pam Graves and Anne O'Connor have already been mentioned; in addition I thank in particular Chris Brooks, Chris Caple, Margarita Diaz-Andreu, Derek Kennet, Dave Webster, Mark White, and the entire Department of Archaeology. Jeff Veitch produced superb scans from often very poor nineteenth-century original illustrations. Outside Durham, numerous people have helped greatly, including Stephen Briggs, James Graham-Campbell, Jytte Høstmark of the Kongelige nordiske Oldskriftselskab, Leif Fredensborg Nielsen (a walking dictionary just an email away), Poul Otto Nielsen, Pádraig Ó Macháin, Ian McBride, Tim Murray, and Kristian L. R. Pedersen. David Boyes read the manuscript and suggested many improvements. Two reviewers made suggestions which have substantially improved this book. Oxford University Press's *Ask Oxford* were able to tell me what Sir William Wilde's pampootees were. The drawings of skulls in figure 3.3 are reproduced from the article by Eschricht in *Dansk Folkeblad*, 3, 15 September 1837, by kind permission of the British Library (AC.9056 vol. 3 part 28–29; facing pp. 111 and 115). The letter from Greenwell to Lubbock quoted in chapter 7 (Add MS 49641/84–7) is quoted by kind permission of the British Library. The letter from Greenwell to Evans quoted in chapter 7 (JE/ B/ 1/7) is quoted by kind permission of the Ashmolean Museum, University of Oxford. Both of these were drawn to my attention by Dr Anne O'Connor. I thank the Arts and Humanities Research Board for an award under the Research Leave Scheme (APN 15880, AN 1759); and research leave given by the Department of Archaeology also played a significant part.

And finally I owe a huge debt of gratitude to my wife Debbie, who put up with me through the writing of this book and proofread much of it, and to my daughters Gabrielle and Eleanor, and to Eddie. They have all had a far greater exposure to nineteenth-century archaeology than anyone should be asked to tolerate.

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Durham 2006

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List of Abbreviations

Antiquaries	Society of Antiquaries (of London), founded 1717
<i>Arch. J.</i>	<i>Archaeological Journal</i> , published by the Archaeological Institute of Great Britain and Ireland from 1845
Association	British Archaeological Association, founded 1843
BAAS	British Association for the Advancement of Science, founded 1831
Commission	Kongelige Commission for Oldsagers Opbevaring (The Royal Commission for the Preservation of Antiquities), established 1807
Institute	Archaeological Institute of Great Britain and Ireland, founded 1845; became the Royal Archaeological Institute of Great Britain and Ireland in 1866
<i>JBAA</i>	<i>Journal of the British Archaeological Association</i> , published from 1846
<i>JRSAI</i>	<i>Journal of the Royal Society of Antiquaries of Ireland</i>
KNOS	Kongelige Nordiske Oldskriftselskab (Royal Society of Northern Antiquaries), founded 1825
<i>Mémoires</i>	<i>Mémoires de la Société Royale des Antiquaires du Nord</i> , published from 1837 by the KNOS in French, English, or German for its foreign members
<i>PRIA</i>	<i>Proceedings of the Royal Irish Academy</i> , published from 1836
<i>PSAL</i>	<i>Proceedings of the Society of Antiquaries of London</i> , published from 1843
<i>PSAS</i>	<i>Proceedings of the Society of Antiquaries of Scotland</i> , published from 1854
RIA	Royal Irish Academy, founded 1785
RSAI	Royal Society of Antiquaries of Ireland, founded 1890; grew from the Kilkenny Archaeological Society, founded 1849
SAS	Society of Antiquaries of Scotland, founded 1780
<i>TRIA</i>	<i>Transactions of the Royal Irish Academy</i> , published from 1787

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Chronologies in Conflict

This book is about a radically new scientific concept, how it was developed and promulgated, and finally came to be generally accepted. The concept in question is the archaeological Three Age System, the fundamental division of the prehistoric past into successive Ages of Stone, Bronze, and Iron. This is the basic chronology that now underpins the archaeology of most of the Old World. To be sure, we may question (for example) whether the transition from the Neolithic to the Early Bronze Age really marks as great a social and cultural change as that from Middle to Late Bronze Age; or we may debate whether the Mesolithic should really be so named, or should be referred to as the Epi-Palaeolithic. But the fact that we can even argue in such terms demonstrates the all-pervasive strength of the fundamental Stone–Bronze–Iron classification. Terms like ‘Mesolithic’ or ‘Late Bronze Age’ may create their own problems, and the precise definitions of such periods and the nature of the transitions between them are often keenly contested; but the debates they engender operate within the parameters of the Three Age System as a whole, and thus act to reinforce it. No-one, after all, doubts that the Stone Age preceded the Bronze Age.

But it was not always so. There is an archaeology even of the Three Age System itself. It was conceived in Denmark and southern Sweden; it was initially published there in the mid-1830s, and was fully accepted and operating in those countries in under a decade. Its acceptance in southern Scandinavia was remarkably rapid, and no serious assault was made there upon its fundamentals. The same cannot be said for its reception in the British Isles, however. Its acceptance and uptake here was variable and patchy, and some

leading British and Irish scholars shunned it for forty years. This is something which is almost always overlooked in histories of archaeology, which instead place emphasis on the people who *adopted* the Three Age System. This is entirely understandable, but it has led to the people who *rejected* the Three Age System being almost entirely written out of the history of the archaeology of the British Isles. Most contemporary British archaeologists will most probably be surprised to learn that Thomas Wright, one of the leading archaeologists of his time, could have expressed opinions like these into the 1870s:

[*Recent archaeologists are guilty of*] generalizing too hastily, and they thus form systems specious and attractive in appearance, but without foundation in truth. Such I am convinced is the system of archæological periods which has been adopted by the antiquaries of the north, and which a vain attempt has been made to introduce into this country. There is something we may perhaps say poetical, certainly imaginative, in talking of an age of stone, or an age of bronze, or an age of iron, but such divisions have no meaning in history, which cannot be treated as a physical science, and its objects arranged in genera and species. (Wright 1875: vi–vii)

This book is about the variable and patchy uptake of the Three Age System. It will address a number of questions. Why, for example, was the Three Age System such a rapid success in Scandinavia? In view of that success, why in contrast was its adoption so slow and so bitterly contested in England? What led to the Scottish archaeological establishment being the first in Britain to embrace it so enthusiastically? Why were the Irish, after an early encounter with it, the last to take it up? And how and why, finally, did it belatedly win through in England? Analogous questions could no doubt be posed in other regions as well. The countries discussed in this book are chosen partly because their literatures are within reach of the author's linguistic abilities, but over and above this they form a group that it is logical to consider together. Denmark was where the Three Age System originated. The archaeological traditions of England, Scotland, and Ireland were to a large extent separate one from another; but, more importantly, they were not much influenced by developments in continental Europe and can be considered separately. The exception was, as we shall see, the long-standing archaeological connection that existed between Scotland and Denmark. Events in the British Isles thus largely followed their own individual historical

trajectories; developments in other parts of Europe and in North America must be left to others to elucidate.

TOWARDS AN ARCHAEOLOGY OF THE THREE AGE SYSTEM

It is hugely difficult for contemporary archaeologists to conceive of the distant past outside the structure of the Three Age System. We labour under two handicaps of which we must try to divest ourselves. The first is our ingrained knowledge that the Three Age System is objectively *correct*. It is impossible for us to 'un-think' our unthinking understanding of it, but we must try at least to become aware that we operate entirely within that understanding, and realize that there was a time when the Three Age System was new and contentious; when it was just one of a number of competing ways of discussing the early human past. What Glyn Daniel (1964) has evocatively referred to as the 'idea of prehistory', the notion of a deep human past beyond all reach of history, is something we take for granted. From this perspective, we naturally assume that early nineteenth-century archaeologists also conceived of such a past, but simply failed to deal with it; and in their failure to structure it in such a way that it could be approached and studied systematically, they allowed it to remain a deep void. But in fact they conceived of no such void. As we shall see, archaeologists generally considered the pre-Roman past to be (a) short, and (b) dealt with by at least two other disciplines in addition to their own. The idea of prehistory was, quite simply, not required. This was what lay at the root of much of the opposition to the Three Age System: an absence of void, not a refusal to deal with the void.

Our second handicap is our knowledge of the subsequent history of archaeology, involving as it did the triumph of the Three Age System over those other ways of ordering the past. With hindsight, we unthinkingly accept that its triumph was *inevitable*. It did not, however, seem so to many of those involved at the time. Only when we understand that it was in the mid nineteenth century neither self-evidently correct, nor inevitably triumphant, can we

come to a somewhat clearer understanding of why particular individuals thought and wrote as they did. Such an understanding also helps us to avoid one other pitfall: the assumption that, once the Three Age System is alluded to or partially used by a particular individual, the archaeological community of which he was a part was thereafter 'on board'. As we shall see, in some places its adoption remained an area of contention for decades after its first appearance in the literature; some communities and indeed individuals, after an early hint of interest, actually hardened their attitudes *against* the stone-bronze-iron sequence as time went on.

Approaches that attempt to divest themselves of such handicaps have been quite widely and successfully employed in the study of other disciplines, but have yet to be applied so widely to archaeology. It is still far too easy for us to write off people who did not accept the Three Age System as blinkered diehards. In archaeology we can actually do this very simply, because we have a name for such people: we refer to them as 'antiquarians'. The connotations of this word are pretty clear, the term usually denoting an aged, dusty male, trammelled and hidebound by outmoded ways of thought, and unable to perceive what we know to be the most obvious truths. In contrast, those who adopted the Three Age System we term 'archaeologists', which term thus comes to denote (for once) a more innovative and go-ahead thinker, younger and perhaps less dusty, someone who could think in new ways, beyond the confines of tradition; in modern parlance, a breaker and changer of paradigms. By referring to ourselves as 'archaeologists' as well, we both bask in the glory of their innovative genius, and also confer upon them the accolade of being the pioneers on our subsequent road to disciplinary perfection.

If we seek to understand how the Three Age System was adopted, this unthinking caricature must be dispensed with, along with the two handicaps described above that gave rise to it. The men who have been termed 'antiquaries' were not uniformly blinkered and inept, any more than those whom we term 'archaeologists' were uniformly innovative and able. The same range of intellectual capacities and prowess was probably to be found in both camps. The chronologies employed at the time by the opponents of the Three Age System were just as viable, and just as potentially flawed, as the stone-bronze-iron sequence itself. These people thus had entirely good reasons for

thinking as they did, and it is these reasons we need to understand if we are to comprehend the course of events. For that reason, this book will dispense with the terms ‘antiquarian’ and ‘antiquary’ from now on, and refer to them all as ‘archaeologists’.¹

THREE CHRONOLOGIES, THREE CAPITALS

The Three Age System did not simply lay claim to a slice of time that no other discipline was dealing with. It emerged into an intellectual world in which there were other ways of considering the ancient human past; and furthermore geology and palaeontology were examining much more remote epochs. By the 1840s, geological time scales had become very long. The realization by Nicolaus Steno (1638–1686) and Georg Wilhelm Leibniz (1646–1716) that fossils were the remains of organic creatures led to the understanding that the earth had a history of change (Cohen 1996), and by the later eighteenth century most geologists accepted that this history comprised an immense (though indeterminate) length of time that was not compatible with the shorter biblical chronologies (Rudwick 2005: 115–31). The chronology of the ancient *human* past had however not lengthened, and was not to do so until the second half of the nineteenth century (Grayson 1983; Van Riper 1993). Until 1860 archaeologists, like historians and philologists, had no need for a timescale longer than that based on the Bible, and most remained chronologically conservative.

In the 1840s the Three Age System was just one of three main ways of approaching the ancient human past. The other two ways both had prior claims to speak for that past. The more venerable was what I will term ‘ancient history’, while the younger was the newer

¹ This rule will be followed except in quotation. This also goes for translations from Danish and Swedish, in which the terms for ‘antiquarian’ and ‘archaeologist’ are *antiquar* and *arkæolog* respectively (spellings vary between the languages and the centuries); *antiquar* will be rendered as ‘antiquarian’ and *arkæolog* as ‘archaeologist’. In addition there are the less common terms *oldforsker*, *oldgransker*, and *fornforsker*, which mean literally ‘researcher into ancient times’ and are here rendered as ‘archaeologist’.

discipline of ethnology. These two were by no means exclusive, and each made extensive use of philology as a means of reconstructing human history and migrations; as we shall see in subsequent chapters, however, they provided quite different types of reconstruction of the past, and these had a major influence on the adoption of the Three Age System in the British Isles. They will therefore be discussed as separate disciplines in the following. The conflict between these three ways of approaching the human past—ancient history, ethnology, and the Three Age System—forms the main theme of this book.

Ancient history comprised two main strands: history and chronology based on documentary evidence; and linguistic history based on philology. Chronologies calculated from the Bible had long been a part of European scholarship, but were transformed by the French scholar Joseph Scaliger (1540–1609). Grafton (1991) argues that Scaliger's importance was that he combined biblical evidence with that from other ancient sources to provide a more robust chronological scheme. Using the Septuagint (the Greek translation of the Old Testament), Byzantine scholars had calculated that the biblical Creation had occurred in c.5509 BC, and in his *Opus Novum de Emendatione Temporum* ('A New Work on the Correction of Chronology') of 1583 Scaliger adopted this. He also calculated that Alexander the Great's victory over the Persians at Gaugamela occurred in 331 BC, a date which is still a chronological cornerstone. He also demonstrated that the Babylonian king Nabonassar was not the same person as the biblical Salmanassar (as among others Copernicus had believed); this altered biblical chronology because Ptolemy dated his astronomical observations from the accession of Nabonassar on 26 February 747 BC (Grafton 1991: 129–31). Scaliger's intention was to show 'that the Bible was neither complete nor self-contained as a history of man. The chronologer could not date the events it mentioned—far less work out the histories of the non-Jewish nations it described—without constantly referring to nonbiblical sources' (Grafton 1991: 133).

The culmination of this sort of work was reached in 1650, when Archbishop Ussher of Armagh published his *Annales Veteris Testamenti* ('Annals of the Old Testament'). This book contained the famous calculation, based on the Hebrew version of the Bible, that identified 4004 BC as the date of the Creation (Barr 1985). Ussher also

had to use sources other than the Bible to construct his chronology, because there is no event in the New Testament that is given a precise date relative to one in the Old Testament. Old Testament history was thus a kind of 'floating chronology' that could not be fixed against later history by biblical means. Like Scaliger, Ussher therefore used Greek and Roman historical sources to connect the two and anchor the Old Testament chronologically (Barr 1985: 579–80).

Historians thus regarded other chronologies as providing necessary material to use alongside the biblical information. The chronology put forward by Ussher was the backbone onto which other histories were hung; to quote two examples, the Scot David Steuart and the Dane Peter Frederik Suhm both adopted Ussher's Creation date of 4004 BC and many other biblical dates: they agreed for instance that Noah's Flood occurred in 2348 BC, and that Moses was born in 1571 BC (Steuart 1814: 102; Suhm 1792: 1–2). They differed a little when other chronologies were tacked on—thus Steuart (1814: 19) placed the fall of Troy in 1209 BC, Suhm (1792: 3) in 1185 BC—but the clear implication was that other literary sources could be used to generate ancient history which was at least reasonably reliable. Steuart (1814: 39) placed the accession of Fergus I, the first king of Scotland, at 330 BC. As we shall see in chapter 2, Suhm (1770: 130–2, 318) calculated two alternative dates for the first occupation of Scandinavia, 1656 BC and 1397 BC, believing the latter to be the more accurate, and he dated the descent of the Danish royal house from the arrival in Scandinavia of Odin, the third historical personage of that name, in 70 BC (Suhm 1802: table II). In Ireland, as we shall see in chapter 6, Eugene O'Curry dated the victory of the Tuatha Dé Danaan over the Firbolg at the first Battle of Moytura to 1890 BC, and the arrival of the Milesians to 1694 BC (O'Curry 1873a: II, 3; 1873b: 250).

That events so deep in the past could be dated so precisely by historical means seems outlandish to the modern reader, but it was probably this very precision that made these schemes appear robust. In seeking to use other sources to provide precise dates, historians were merely trying to live up to the methodological model provided by Scaliger and Ussher. It would be easy to refer to such chronologies as 'legendary history' or 'saga history' and thus consign them to the same historical dustbin as 'antiquarian', but this would be to prejudge

the issue; for that reason 'ancient history', a term eminently respectable to modern sensibilities, is preferred.

The other strand of ancient history was linguistic history based on comparative philology. The study of comparative linguistics went back to the Renaissance, in the form of the search for the *Ursprache* or 'original language'. This derived from the biblically based understanding that originally there was only one language, so all contemporary languages must be descended from that original one. The geographical distributions of, and the degrees of interrelatedness between, contemporary languages provided evidence of their past divergences and history. By the eighteenth century, in the hands of major exponents like Johann Gottfried Herder (1744–1803), philology had therefore become a 'method of writing national cultural history' (Olender 1992: 5). One revolutionary development was the detection of parallels between the language of the ancient Sanskrit documents of India, and Greek, Latin, and other European languages, the foundation of the modern concept of the Indo-European language group (Trautmann 1997). Sir William Jones announced his discovery to the Asiatick Society in Calcutta on 2 February 1786, as follows:

The *Sanscrit* language, whatever be its antiquity, is of a wonderful structure; more perfect than the *Greek*, more copious than the *Latin*, and more exquisitely refined than either; yet bearing to both of them a stronger affinity, both in the roots of verbs, and in the forms of grammar, than could possibly have been produced by accident; so strong, indeed, that no philosopher could examine all three without believing them to have sprung from some common source, which, perhaps, no longer exists. There is a similar reason, though not quite so forcible, for supposing that both the *Gothick* and the *Celtick*, though blended with a very different idiom, has the same origin with the *Sanscrit*; and the old *Persian* might be added to the same family, if this were the place for discussing any question concerning the antiquities of *Persia*. (Jones 1786 [1806]: 422–3, original emphases)

Jones accepted the biblical narrative, believing that all humans descended from the original pair (Jones 1792 [1807]a: 480). The single family to survive the Noachian flood lived in northern Iran. Noah's three sons migrated in different directions, their descendants thus occupying different regions of the world: those of Shem, southern Asia; those of Ham, Europe, Phoenicia, Egypt, Iran, and India;

and those of Yáfet (or Japhet), northern Eurasia (Jones 1792 [1807]a: 490). Since in this scheme all languages had to be related, the discovery of the Indo-European links was a reaffirmation of the correctness of the biblical narrative; Jones indeed suggested even wider links, arguing that Chinese and the pre-Hispanic languages of Mexico and Peru would turn out to be also of Hamitic descent (1792 [1807]a: 491). Trautmann (1997: 42) concludes that 'the entire project is one of forming a rational defence of the Bible out of the materials collected by Orientalist scholarship, more specifically a defence of the Mosaic account of human history in its earliest times'.

Jones was unusual in regarding Ham as the ancestor of Europeans; most of his contemporaries regarded Europeans as of Japhetic descent. Two major historian-philologists whom we shall encounter in subsequent pages certainly did so. In Denmark, Peter Frederik Suhm provided a comprehensive picture of the descent of the European nations from Japhet, although he never adopted the implications of Jones's Indo-European connections and continued to regard the Indians as descendants of Ham until his death in 1798. In Ireland, Charles Vallancey also regarded the European nations as descended from Japhet, but in contrast to Suhm became an enthusiastic supporter of the Indo-European concept.

Ethnology was a younger discipline than ancient history. Stocking (1987: 48) characterizes ethnology as the fusion of two main pre-existing lines of study: the comparative philology also employed by ancient historians (see above); and comparative anatomy, principally craniology. Like ancient history, ethnology did not challenge the biblical narrative, but accepted and supplemented it, because both philology and craniology 'had begun, as it were, at the Tower of Babel' (Stocking 1987: 57); it has aptly been described as 'Mosaic ethnology' (Trautmann 1997: 9). Philology and craniology examined respectively the languages and the physical attributes of the subsequently diverging races. When the British Association for the Advancement of Science (BAAS) was founded in 1831 the discipline barely existed, and consequently was not included when the Association was formed into sections in 1832. The principal founder of ethnology was James Cowles Prichard, and he and a group of like-minded individuals founded the Ethnological Society in 1843. Individual papers on ethnological subjects had previously been presented

to various of the BAAS's sections, but in 1846 the ethnologists began to lobby for a section of their own. In this they were partially successful, being granted an ethnological sub-section within section D (zoology and botany) (Sillitoe 2005). In 1847 they held a major session, the publications from which marked an important milestone. In 1850 it was decided that ethnology and geography should in future jointly constitute section E (Cull 1854).

Prichard did not especially like ethnology being placed with the natural sciences, because he considered the discipline more closely allied to history:

ethnology is the history of human races, or of the various tribes of men who constitute the population of the world. It comprehends all that can be learned as to their origin and relations to each other. It is distinct from natural history, inasmuch as the object of its investigation is not *what is*, but *what has been*. (Prichard 1848a: 302, original emphases)

Ethnology examined the histories of human racial groups 'from the most remote times' (Prichard 1848a: 302), and sought to establish the interrelationships (or lack of them) between the various groups. 'All this rather belongs to archaeology than to natural history' (ibid.). Ethnology provided a racial sequence of occupations of Europe, based primarily on languages. The central theme was that of early Celtic occupants coming from the East, and being gradually pushed westwards by Germanic immigrants following on their heels. Who exactly peoples like the Basques or Finns were, and what their historical role and position were, remained a subject for discussion. The significance of ethnology, specifically craniology, as the conduit through which the Three Age System entered Britain has recently been stressed (Morse 1999, 2005). As we shall see in subsequent chapters, its significance was however both greater and more complex than has been realized; in England it may in fact have done as much to inhibit as to encourage the Three Age System's adoption.

Ethnology was seeing a boom in major publications in the crucial period of the 1840s and 1850s, a good deal of it from the pen of Prichard himself. He originally published *Researches into the Physical History of Man* in 1813 (Prichard 1813), in which physical aspects dominated his discussion, with ethnographical aspects such as mythology and language playing a lesser role. In the second edition of

1826 these latter aspects were much increased, and they were still further amplified in the 5-volume third edition, which appeared over the 11-year period 1836–47 (Stocking 1973). Prichard also wrote *The Eastern Origin of the Celtic Nations Proved by a Comparison of their Dialects with the Sanskrit, Greek, Latin, and Teutonic Languages* (Prichard 1831), in which he demonstrated that the Celtic languages were indeed part of Jones's Indo-European language grouping; he died in 1849 but a posthumous edition of this book appeared in 1857, edited by R. G. Latham (Prichard 1857). Finally, and in this context most importantly, Prichard produced *The Natural History of Man; comprising Inquiries into the Modifying Influence of Physical and Moral Agencies in the Different Tribes of the Human Family*. The first edition of this appeared in 1843 (Prichard 1843), followed by the second in 1845 and the third in 1848 (Prichard 1848b); once again there was a posthumous edition, the fourth, edited by Edwin Norris, which appeared in 1855. Several other authors were producing major works in these same years. Historians of archaeology have, it will be argued below, tended to overlook the importance of this spate of publications with regard to the adoption of the Three Age System in Britain.

Ancient history and ethnology were thus the two alternative providers of chronologies in the early and mid nineteenth century. The history of the differential adoption of the Three Age System is to a considerable extent the history of how communities of archaeologists in various parts of Britain adopted one or another of the three chronologies on offer, which they preferred, and how far they sought to reconcile the differences between them. Those who argued against the Three Age System were not simply ignoring chronological questions—they had more chronological schemes on offer than they could satisfactorily deal with—but were usually choosing to adopt one of the alternatives. By and large, the archaeological communities of London, Edinburgh, and Dublin each made a different choice, and each went in its own direction. In London, English archaeologists as the descendants of immigrant Anglo-Saxons had no particularly ancient history of their own, and adopted a short pre-Roman chronology based on ethnology. Dublin, like Copenhagen, had an ancient historical structure stretching far further back into the past. Scottish archaeologists had neither very much ancient history, nor much

interest in ethnology. Edinburgh had, however, produced a series of political economists in the later eighteenth century, and their general espousal of the concept of 'progress' was probably an important factor in the adoption of the Three Age System; the notion of progress had begun to enter mainstream history writing at about the same time (Momigliano 1977: 57), and was reinforced in the early nineteenth century when technological and economic progress was visibly occurring in contemporary society (Brooks 1998).

The archaeological communities of London, Edinburgh, and Dublin maintained their separateness throughout the nineteenth century, despite the continual improvement in communications. The burgeoning railway network meant that the different parts of the British Isles were becoming more closely linked than ever before. The Danish archaeologist J. J. A. Worsaae, whose travels around Britain in 1846 we shall be following, was (he wrote to his mother) impressed that he could travel from London to York in just nine hours (Worsaae 1934: 288). He travelled on to Newcastle upon Tyne, the final stretch of railway into Newcastle having been opened just two years before his visit. The section from Newcastle to Berwick-upon-Tweed on the Scottish border was the last to be completed, the Tyne and Tweed bridges being completed in 1849 and 1850 respectively (Middlebrook 1950: 190), after which Edinburgh and London were directly linked by rail; Worsaae had to do this stretch by coach. One major effect of the developing railway network was that the rival archaeological organizations based in London were increasingly able to hold their annual meetings in different towns and cities round the country, and in 1856 the Archaeological Institute even met in Edinburgh. However, not many people working on the remote past played active parts in the archaeological circles of more than one of the capitals. The Anglo-Scot Henry Rhind published papers in both the *Proceedings of the Society of Antiquaries of Scotland* (PSAS) and in the Archaeological Institute's *Archaeological Journal* (*Arch. J.*); William Lukis and George du Noyer published in London, in the *Journal of the British Archaeological Association* (JBAA) and *Arch. J.* respectively, and both also published in the journals ancestral to the *Journal of the Royal Society of Antiquaries of Ireland* (JRSAI), but these men were very much the exceptions. By far the most influential person to publish in two capitals was William Greenwell; from his intermediately located

base in Durham he was able to involve himself in the archaeological communities of both Edinburgh and London, and published influential papers in both *PSAS* and *Arch. J.*

TIME, CHANCE, AND WORSAAE

The reasons for the differences between the three capitals were complex. In the following analyses, factors such as nationalism and the historical context of research will emerge as issues of major importance. In some recent studies of disciplinary development, such aspects of 'social context' have been made paramount, and have become more or less the sole object of investigation. That will not, however, be the case here. While these aspects are undeniably important, to concentrate on them to the exclusion of all else would be to overlook two equally important things. One is the catalysing effect of particular important individuals. Particularly in numerically small communities such as those making up the archaeological cores in Copenhagen, London, Edinburgh, and Dublin, such stochastic factors could have effects out of all proportion to their scale. The other is sheer chance, and the ways it can affect both the actions of individuals and our subsequent perception of them. In the chapters that follow, I will use the activities of the Danish archaeologist J. J. A. Worsaae (fig. 1.1) as the thread that links the three capital cities together, considering both the early years of his career in Denmark, and his extended trip through England, Scotland, and Ireland in the years 1846–7. His activities exemplify two ways in which chance can play a major role. First, current perceptions of his importance are based on misunderstandings largely shaped by chance. Second, his visit to Britain and Ireland was itself the outcome of a series of chance events.

The importance of Worsaae as an individual has been much stressed. Rather in the way that Thomas Henry Huxley is portrayed as 'Darwin's bulldog', so Worsaae is presented as the man who fought the Three Age System through to general acceptance in a way that its originator, C. J. Thomsen, was too diffident to do. There is considerable truth in this, but the means by which Worsaae did it are often



Fig. 1.1. The opportunist: J. J. A. Worsaae. (From Müller 1886: frontispiece).

misunderstood. Histories of archaeology describe Thomsen as largely confined to his museum, while Worsaae was a field worker. This is certainly true, insofar as Worsaae did indeed excavate a considerable number of sites. He has however acquired the reputation of having proved the Three Age System correct by observing the superimposition of stratigraphic layers, in contrast to Thomsen who had only surmised it from the repeated associations of artefact types that he observed in the museum. Here is an example:

Time and time again, working in stone chambers deep in Danish mounds, excavating in fields and bogs and by the seaside, Worsaae's results were the same. Stratum upon stratum, lying one upon the other in the earth, burials that Thomsen's Three Age System identified as Bronze Age were found

above those of his Stone Age and underneath those of his Iron Age. So an idea born in a museum, a simple system of classification had been proved by excavation in the field. Reality had corresponded with theory, and the modern science of archaeology had begun. (Romer 2001: 25)

Similar sentiments are common to various histories of archaeology (e.g. Darvill 2002: 475; Fagan 1996: 761; Schnapp 1996: 301)—though others lay less stress on it (e.g. Murray 2001; Trigger 1989: 80–2).

The notion that Worsaae excavated sites with more than one stratigraphic layer, and used such sites to prove the correctness of the Three Age System, has become something of a ‘factoid’, a claim repeated so frequently that its veracity is largely unquestioned. It is uncertain where this tradition started; certainly it was clearly laid out by Glyn Daniel over half a century ago:

Worsaae himself demonstrated the validity of the three-age system by stratigraphical researches in the Danish bogs. He was also able to demonstrate a succession of vegetation types in the Danish bogs and peat-mosses, beginning with a layer where thinning aspen forest gave way to Scotch fir, a tree no longer growing in Denmark, and succeeded by a layer with oak, alder and birch, and then thirdly succeeded by a layer in which beech, the commonest tree in modern Denmark, appears. Only stone implements were found in the lowest or fir level, they persist into the oak–alder–birch level where bronze implements were found; iron implements were for the most part found only in the beech level. (Daniel 1950: 78)

Where Daniel obtained this misleading information from is not clear, but his first mention of Worsaae as a stratigraphist of peat bogs occurs even earlier (Daniel 1943: 9). The work described is apparently a garbled version of that of the natural historian Japetus Steenstrup, who as we shall see in chapter 3 first demonstrated a four-fold stratigraphic sequence of vegetation preserved in bogs; this started with aspen, which was followed by pine (not fir), then oak (without birch), and finally alder. Beech is hardly ever found in bogs, but since it forms the dominant forest tree of contemporary Denmark Steenstrup made it the marker for his fifth phase (Steenstrup 1842).

Worsaae’s excavations were in fact almost all of single-period sites, and his role in pioneering stratigraphic excavation is inconsiderable. In 1831 Thomsen had written and distributed round Denmark a pamphlet explaining how antiquities should be dealt with when

found, and how the excavation of burial mounds should proceed, stating that ‘one must take the greatest care to observe the relative positions of the deposited objects, since this is often more important than the objects themselves’ (Thomsen 1831: 2*).² This was to bear fruit just a few years later. At the end of 1837 a Mr Hage sent in some finds of amber, stone, bone, and bronze from a burial mound at Stege on the island of Møn, and because he had followed Thomsen’s instructions carefully, he provided Thomsen with the first recognized stratigraphic proof of the Three Age System:

At the BASE of this mound was a funerary chamber made of large boulders, in which the amber objects lay with several *uncremated corpses*, together with objects of *flint* . . . , pots and funerary vessels of clay, which did not contain burnt bone . . . but *absolutely nothing of metal*. Proof of great antiquity is also provided by the tools of bone found together with the stone objects . . . At the TOP of the same mound, completely separate from the funerary chamber, was placed a small stone cist, much too small for a body to have been placed in it. Inside was a clay urn . . . which contained *cremated human bones*, and on top of these were placed a pair of *tweezers* and two *knives of bronze* . . . The upper place of concealment thus belongs to the Bronze Age, while the lower large stone funerary chamber, in which the amber objects were found, in contrast belonged to the oldest period or Stone Age. (Thomsen 1839: 165–6*, original emphases and capitalization)

Thomsen was thus already using stratigraphic observations to allocate not just artefacts, but also grave types and burial rituals, to different periods. When Worsaae published a similar stratified burial mound a couple of years later he noted that such stratigraphy had ‘already been observed quite often’ (Worsaae 1841: 145*).

Here then is an example of how chance may influence our view of the catalyzing effect of individuals: if their works are misreported in the literature, problems may be unleashed. Aspects of the work of both Steenstrup and Thomsen have been ascribed to Worsaae in the Anglophone literature. This is not to deny Worsaae’s catalysing effect in Scandinavia, but in these early years this was, as we shall see in chapter 3, mainly directed towards the destruction of the ancient

² All quotations where an asterisk follows the citation of the original page numbers have been translated from Danish or Swedish by the present author.

historians' use of archaeological materials in their chronology; in this area his importance is undoubted, but little understood in the Anglophone literature.

Chance of a very different kind also played a major part in Worsaae's important trip to Britain and Ireland in 1846–7. It had not been his intention to undertake any such trip, and the circumstances under which it came about reveal how a quick-thinking opportunist can exploit the patronage of an absolute monarch. Some of Worsaae's previous research had involved travelling in southern Sweden in 1842 and 1844, and to publish the resulting book he had been awarded a royal bursary. Worsaae's book on the antiquities of the Swedish province of Blekinge duly appeared at the beginning of 1846 (Worsaae 1846a), and he arranged an audience with King Christian VIII to present him with a copy by way of thanks for his support. What happened at that meeting was of the greatest importance. One crucial chance factor was that Thomsen had gone down with a severe case of typhus in the autumn of 1845, and had left for Italy to recuperate in January 1846, accompanied by his museum assistant C. F. Herbst and his private secretary Conrad Engelhardt, and did not return until the end of September that year (Haugsted 1988; Worsaae 1934: 134). There were thus effectively no archaeologists remaining in Denmark except Worsaae; had this not been the case things might have turned out very differently. As it was, Worsaae was unimpeded; what happened during his meeting with the king is described in an enthusiastic letter Worsaae wrote to Thomsen in Italy on 19 March 1846. This has apparently never been translated into English, so the relevant passage is worth quoting in full:

[*The King*] received me most graciously. He spoke to me at length about various things and finally asked me if I wanted to travel this summer. I said that I planned to go up to Norway so I would have seen the whole of Scandinavia, before I undertook my long journey to Russia. Yes, he said, that was all very well, but did I not wish to visit *Scotland*? I naturally answered that it would interest me a great deal, because there were so many antiquities and because the Scandinavians in ancient times had ravaged it everywhere so much, and still inhabited the northern parts and the islands round about.—So, did I not have any connections in Scotland? In that instant it occurred to me to say that the Duke of Sutherland had the

previous year offered all his assistance to the KNOS, if a Scandinavian antiquarian would visit his estates in the Scottish highlands. But this is splendid—exclaimed the King, this is an opportunity that really should not be missed, why have you not mentioned it before? Because, Your Majesty, it is very expensive to travel in Scotland, and it would not do to visit an English Duke who has between 3 and 4 million a year with only a few pounds in one's pocket, even if one is invited. Yes, chuckled the King, that is true enough, but Rafn³ has not declined the invitation; what does he say? He says the same as I do, that it would be excellent if it could happen, except that it would cost money. Yes, said the King, but there might be a solution; it is a matter that interests me, just send in an application.—A couple of nights later the King met Rafn at Moltke's;⁴ he went up to Rafn, asked him about the Duke of Sutherland, and said that he wanted me to go; as far as the money was concerned, he would take care of it. So I made my application, which I sent him with the observation that I was asking for no less than 1000 rixdollars, because it was expensive travelling in that country; and I would be travelling through Norway, which was also expensive; 1000 rixdollars was in any case not much more than £100.⁵ The King thought this was rather a lot, but he admitted that this was due to the great expense of travelling in those countries. But he thought that I should go over to Scotland straight away, to get about there as much as possible; it could be that I might also get across to Ireland. He finished by saying that I should write saying I was coming. The King sent my application without delay to the Board of Finances with his mandate, with an addendum that the sum should not be reduced. The finance committee was not able to make the trip any cheaper, but the decision, which I received just 8 days after I had submitted the application, was that I should receive an extraordinary 800 rixdollars from the exchequer, so that I can apply later for the travel money proper. In the meantime they told me that I would easily get a supplement once I was on my way and sent some reports to the King. I must get to Ireland; I will therefore leave as early as May. The Society has already sent a letter about this, signed by the Crown Prince, to the Duke; it has been sent to Count Reventlow⁶ in London so that he can deliver it personally. In addition Rafn has written to the Duke's

³ Carl Christian Rafn (1795–1864), secretary of the Kongelige Nordiske Oldskriftselskab (KNOS).

⁴ Probably Count Adam Wilhelm Moltke (1785–1864), from 1845 the President of the Board of Revenue.

⁵ Actually £112 10s at 1845 rates.

⁶ Count Frederik Reventlow (1791–1851) was the Danish ambassador in London from 1841–51.

brother⁷ to arrange assistance for excavations etc. I am naturally very much looking forward to the trip, which, I hope, will be of great benefit to me, not to mention that I will master the language. I must now get intensive lessons in it. It is particularly pleasing that I am so unexpectedly to travel. (Worsaae 1934: 276–7*)

Thus did chance operate; less than three months later Worsaae found himself in London on a royal bursary.

The parameters for our investigation are thus defined. The raw data are straightforward: they comprise the publications that were produced in southern Scandinavia during the generation and rapid consolidation of the Three Age System, and in England, Scotland, and Ireland during the much more protracted period of its acceptance in the British Isles. Hardly any of the central figures wrote memoirs or autobiographies. Worsaae had begun writing his autobiography when he died in 1885; this was published half a century later, under the editorship of Victor Hermansen (Worsaae 1934). As luck would have it, at his death Worsaae had reached the end of 1846, as he was about to leave Scotland for Ireland. We thus have his descriptions of his time in England and Scotland, but not of his visit to Ireland. Some of his letters from Ireland (as well as from England and Scotland) do appear in this volume, however, and others were published by Clément (1930). His letters from England and Scotland are a valuable adjunct because, as we shall see, his later autobiographical comments do not tally with what he wrote in letters at the time. The methodology is simply to *read* the books and papers involved, and to attempt to see how developments unfolded. In Denmark and southern Sweden the rapid emergence of a robust and integrated system based on the fusion of the work of several individuals, followed by the undoing of the ancient historical chronology by Worsaae, was crucial. In London, however, things were very different. We shall see that despite his later claims, Worsaae's visit to London was not especially transforming of archaeology there. The London establishment already knew of the Three Age System via a different route, namely ethnology, but in transmission it had become blunted by involvement with the ethnological chronology to such an

⁷ Lord Francis Egerton (1800–1857), later the Earl of Ellesmere, who produced the English version of Thomsen's book (Ellesmere 1848).

extent that it was perceived as an unnecessary irrelevance. In Edinburgh, Worsaae's visit was transforming, but would probably have been much less so but for the emergence of a man who did not even meet Worsaae, namely Daniel Wilson. In Dublin, an ancient historical chronology was growing. After an early initial airing, the Irish archaeologists failed to integrate the Three Age System with this, and therefore shunned it for several decades. Finally, back in London, an unexpected assault on the received chronology was generated by the discovery and acceptance of deep human antiquity. This perforce brought the period *between* deep antiquity and the Roman Empire into sharp focus in a way it had never previously been. The battle over the subdivision of this period represents the final act in the acceptance of the Three Age System. In each capital we shall encounter some individuals whose roles in the history of the Three Age System are well known. But we shall encounter others who are less well known, or who are better known in other contexts; and in some cases we shall meet people whose names are by and large completely unknown to the history of archaeology. All these individuals have their relevance; the task this book sets itself is to reveal in each case what that was.

The Construction of Prehistory: Copenhagen to 1836

Copenhagen lies on the eastern shore of Zealand, Denmark's most easterly island. Scania, the southernmost province of Sweden, lies opposite; the city of Lund is just a few kilometres inland. They are separated only by the Sound, a body of water narrower than the English Channel, which narrows further to just 5 kilometres at a point some 40 kilometres north of Copenhagen. Lund and Copenhagen both have old universities, and an archaeologist travelling from one to the other can now make the journey via the new bridge over the Sound in less than an hour. In the early nineteenth century it took a little longer, but even in those days academic exchange was not difficult. For example, on 21 June 1830 the Swedish archaeologist Bror Emil Hildebrand embarked at 2 p.m. across the narrowest part of the Sound, and after spending that night in a hotel on the Danish side, reached Copenhagen on the afternoon of 22 June. Returning home on 17 August, he took a ferry direct from Copenhagen which departed at 8 a.m., but due to contrary winds he did not reach the Swedish side till that night (Hildebrand and Hermansen 1935). By 1842, steamships had speeded this up; the Danish historian Christian Molbech, visiting Lund, noted in his diary that he could be home in Copenhagen in just four hours (Molbech 1844a). Not surprisingly, the academic community of Lund was therefore much more closely linked to Copenhagen than it was to the Swedish capital, Stockholm, which is getting on for 600 kilometres from Lund as the crow flies. Molbech left Lund early on 9 June 1842 and travelled overland to Ystad, from where he took a steamship to Stockholm. This journey took him four days, and he doubted that even the introduction of

steamships would bring Copenhagen and Stockholm into close connection (Molbech 1844a: 274). (What Hildebrand learned during his visit, and how Molbech had contributed to prehistory, we shall see below).

The Three Age System emerged from the Copenhagen–Lund academic axis in the early nineteenth century. This chapter will examine the initial developments, which took place mainly in Copenhagen and culminated in Thomsen’s publication of the artefactual scheme in 1836. However, in contrast to the way these events are often presented in histories of archaeology, the Three Age System did not emerge into a vacuum, and was not initially regarded as reaching further back into the past than ancient history. In order to understand this, we must therefore examine the ancient historical chronology. The first part of the chapter will therefore present the ancient historical chronology that had already been in place for half a century, largely the work of Peter Frederik Suhm. The Three Age System initially grafted itself onto this chronology; it only became independent of it during the 1830s, the decade which saw the first florescence of the ‘idea of prehistory’—as well as the first use and establishment of the word ‘prehistory’ itself.

PETER FREDERIK SUHM’S ANCIENT HISTORICAL CHRONOLOGY

The chronological context into which the Three Age System emerged was provided by ancient historical studies. The most important figure involved in this was Peter Frederik Suhm (1728–1798) (fig. 2.1). The most significant historian in later eighteenth century Scandinavia, he returned to Denmark in 1765 after spending fifteen years living in Norway, and it was after this that his prolific writings started to appear (Jørgensen 1931: 216–20). Suhm’s ancient historical scheme is a classic eighteenth-century combination of documentary history and comparative philology of the kind outlined in chapter 1; an understanding of his achievements is essential if the Three Age System is to be placed in context.



Fig. 2.1. The historian as hero: Peter Frederik Suhm (1728–1798). A lithography by Bærentzen published in 1867, based on a painting by Juel.

Suhm exploited the various early Scandinavian writings in the confidence that they contained a core of historical fact—well-hidden though this might be by the poetic licence, later additions and so on that characterized these works. He was an encyclopaedic reader and collector of information; in his first major book he listed no fewer than 256 written sources he had consulted (Suhm 1769); his next, published the following year, listed an additional 289 (Suhm 1770); his third, from the year after, a further 118 (Suhm 1771). These three books were the first in a series of ten he was to produce by 1774. In 1782 he embarked on his most ambitious project, a multi-volume

history of Denmark, of which he had completed several volumes by the time of his death.

Suhm's first major work was on the origin and dispersal of peoples. His point of departure was a series of 36 rules or principles upon which his history was constructed. Rule 1 was that the Bible was the most fundamentally correct source; rule 2, that the Noachian flood was not localized but covered the entire world; and so on, gradually extending away from the security of Holy Writ. Rule 9, for example, stated that after Babel there were already enough people in the world to ensure that colonization proceeded in all directions; rule 10 that this colonization was mostly by land though some was by sea (Suhm 1769: 8–32). The dispersal from Babel of Ham, Shem, and Japhet (the three sons of Noah) and the division of their descendants into various peoples was Suhm's fundamental concern. He examined this from two chronological perspectives: *relatively*, by considering how closely peoples were interrelated; and *absolutely*, by historical dating.

Suhm's relative chronology was based on the degree to which peoples were related, which could be measured by how alike their languages were. Suhm's rule 35 was that:

Languages are one of the means which serve to reveal peoples' relatedness and origins, and thus also the routes they followed on their first migrations. When complete similarity of language can be demonstrated between two or more peoples, there arises a probability . . . that they are related to each other; but as the similarity becomes less and less, so the probability becomes weaker. (Suhm 1769: 70*)

Loan words presented a problem and should not be used. (He gave as an example the Danish word 'viin', meaning 'wine', a recently imported commodity, which clearly had no great antiquity in the Danish language.) More fundamental words such as those for parts of the body, names of people or places etc, were preferable (Suhm 1769: 81–2). He argued that there were twelve main language stocks descending from the tongues of the grandsons of Noah; fig. 2.2 presents a summary based on his tables (Suhm 1769: tables IVa & b, V, and VI). His knowledge was limited by the work and publications available at the time, so the better-known west Eurasian regions dominate the chart. The 'Indo-European' grouping had yet to be identified (see chapter 1), so he classed Indian languages as Hamitic,

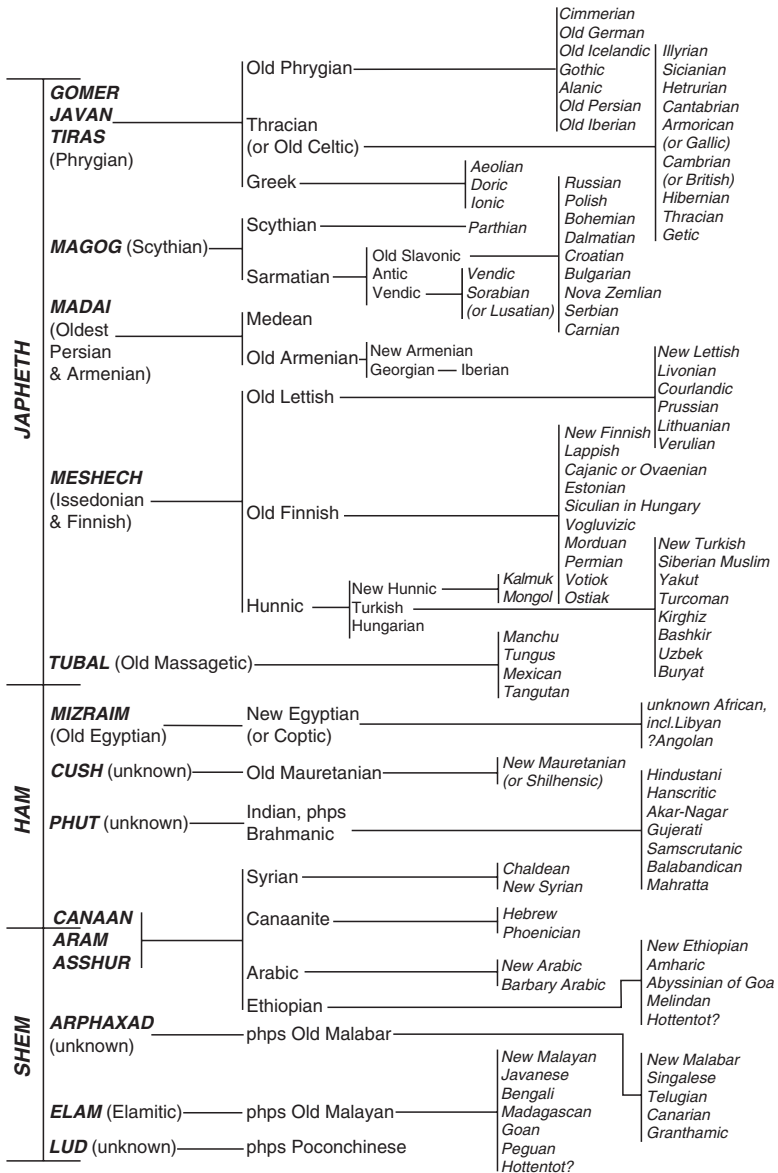


Fig. 2.2. Suhm's view of the history of languages based on descent into twelve main stocks from the grandsons of Noah. (Simplified from Suhm 1769: tables IVa & b, V, and VI).

while New World languages are absent except for 'Mexican', an offshoot from Tubal. The structure is, however, clearly dendritic, and was avowedly one of descent. Names provided a useful thread—from Gomer descended the ancient Cimmerians of eastern Europe, who moved westwards becoming the Cimbrians mentioned by Roman writers, and from whom is derived the modern name of Himmerland, a region in Jutland.

Suhm's absolute chronology was calculated from the Bible and extrapolated from there in a most ingenious manner. He identified the location of Mount Ararat, where the Ark came to rest, as the northern end of a mountain chain in Kurdistan, while Babel was identified with Babylon, which he knew to lie near Hilla in modern Iraq (Suhm 1769: 12–18). He accepted the date for the Flood of AM 1656 (AM is *Anno Mundi*, 'year of the world', or years after the Creation in 4004 BC), or 2348 BC, but calculated his own date for the confusion of languages and the dispersal from Babel of AM 1757 (2247 BC) from references in Genesis (1769: 28–9). This interval of 101 years was shorter than most previous authorities had thought, but was of vital importance since this was the time it took for people to traverse the distance from Ararat to Babel. What distance was this? Babel lay southwest of Ararat, but Genesis states that the people arrived at Babel from the East, not the Northeast; the solution had to be that the people initially followed the Ararat mountain range due South, only turning West when they reached its southern end (Suhm 1769: 15). On this basis Suhm calculated that the people covered 4 degrees of latitude in a southerly direction, followed by 2 degrees of longitude towards the West, or 6 degrees in all. And since one degree was 15 German miles, the total distance covered in 101 years was 90 German miles (Suhm 1769: 51–2).

This veritable tour de force provided a rate at which ancient pastoralists migrated, and from this could be calculated the dates of occupation of different places. In his next book, he used this principle to examine specifically the first colonization of Scandinavia (Suhm 1770). One potential objection might involve an insufficiency of colonists, but Suhm had an answer:

People in the expansion I have established [*that from Ararat to Babel*] were only expanding in one direction; but from Babel in contrast this was towards

all four corners of the world, so in the same time, 101 years, they would only be able to expand one quarter as far, or about 23 miles. But to this I answer that people were, in the beginning after the Flood, too few to expand too far, but 101 years later they already comprised some 1000 families, so they would have been able to occupy more space. (Suhm 1770: 125–6*)

This calculation was the basis for his rule 9 (see above). He also sought to check his calculated rate by calibrating it against known events. He stated (quoting Ussher) that Sicyon in Greece was founded 158 years after Babel, which at first sight implied too fast a movement—but the colonization of Greece was speeded up by being partially by sea. From Babel to the Mediterranean is 9 degrees, which people could have covered in something over one hundred and thirty years at the established rate of colonization. Some twenty years after this seemed a reasonable time for people to start voyaging by sea; particularly given the great age reached by many of the people in those days, which meant that some of them would have heard about ship-building directly from Noah and his sons—who after all had built the Ark. Hence Suhm's rule 10 (see above).

Scandinavia would however have been occupied first of all by land, which allowed Suhm to calculate dates with precision:

According to the aforementioned rule [rule 10], our ancestors could have reached the source of the Dvina about 390 years after Babel [1856 BC]; then they divided into two groups; one went west into Germany, to the border of which . . . it reached around 590 years after Babel [1656 BC]: the other went north around the Gulf of Finland to about the spot where Åbo¹ now lies, and arrived there at about the same time . . . After they had crossed over to the country we now call Sweden, they needed a period of about 75 years to reach the southernmost tip of Scania, which they consequently reached 660 years after Babel [1586 BC]. (Suhm 1770: 130–1*)

The need to follow river routes and avoid impenetrable bogs and forests might have slowed them up, however; so an alternative date for reaching the southern tip of Scania was 1276 BC. This reconnects chronology with linguistics, for it reflects Suhm's understanding that the German and the Scandinavian languages were fairly closely related:

¹ The modern city of Turku in Finland.

I have established with near-certainty that the Cimbrians and Teutons were German peoples, not truly Scandinavian . . . [but] it is a proven fact that the Germans and the Scandinavians were brothers, and in the most ancient times comprised one single people, before they separated, which probably occurred on the Baltic Sea where the Dvina flows into it. (Suhm 1770: 315*)

The movements to Scandinavia from the Black Sea region meant that a string of peoples presently or formerly living along the line of movement were related to Scandinavians (Suhm 1770: 85–112).

Within Scandinavia, Suhm sought to strip away from the early writings the flights of poetic fancy and the later additions to reveal the kernel of historical fact, often using the conclusions of other historians who had done the same before him. Mythological or supernatural beings were often based on historical personages, but an added complexity was that if several real people had the same name, the traditional stories would be likely to confuse and conflate them. Such was the case with Odin. Traditionally regarded as one divinity, Suhm disentangled three historical individuals from the various sources, placing each in a different period of early history (Suhm 1771). All three had originated at Tanais on the Black Sea, where they had lived with their people, the Asas, and whence they had migrated northwards (Suhm 1771: 80–7). When Odin I existed was largely unknown. Odin II had fled north as the result of the Persian emperor Darius' campaigns against the Scythians in about 500 BC; it was his people who, having defeated the previous inhabitants known as Joths or Jotuns, diversified in Scandinavia to become the Swedes, Danes, and Norwegians (Suhm 1771: 82–3). Finally, Odin III arrived in about 70 BC, a refugee from Roman expansion (Suhm 1771: 90–2); this Odin took over the whole of southern Scandinavia, and the subsequent royal lines descended from him.

Figure 2.3 shows Suhm's chronology in tabular form, taken from a book he wrote for students that was republished after his death (Suhm 1802). He defined the period before 70 BC as his 'dark' age, stating that 'most of what we know about it rests on *more or less historically* reasonable presumptions' (Suhm 1802: 1*, original emphasis). The arrival of the third Odin in 70 BC initiated the 'fabulous' age, for which much more information could be extracted from the sources. The 'historical' era began after AD 900, with the death of King Gorm and the conversion of the Danes to Christianity.

Suhm's chronology was thus both well thought through, and chronologically deep. He was in effect building on the conclusions of earlier scholars such as the Welshman Edward Lhwyd (whose work he cited), who in the early years of the eighteenth century had begun arguing that some peoples in Europe could demonstrate a greater antiquity in their contemporary locations than others; in Lhwyd's case this concerned the people he termed the 'Celts' of the Atlantic seaboard (James 1999; Morse 2005). The main problem all ancient historians faced was how to get people from Babel into Europe in a coherent and comprehensible manner, and Suhm's lengthy chapter on the descendants of Japhet (Suhm 1769: 139–326) was a systematic attempt to do this. He set the agenda for various aspects of later work. One noteworthy point is that he was unable to deal very satisfactorily with the Finns. Although of Japhetic descent, their language was very different and they were thus not close relatives of the Danes, Swedes, and Norwegians (fig. 2.2). Suhm argued that they had lived in present-day Poland 'in the very earliest times' (1770: 315*), though he did not make clear precisely when this was, or how they had got there; they had never lived in southern Scandinavia because there were no Finnish words in the other Scandinavian languages. This Finnish problem would repeatedly reappear both in Scandinavian and English publications. Also to reappear was the separation of the Germans and the Scandinavians into two migratory streams when the original people reached the Baltic; this was to rear its head fifty years after Suhm's death, and was to exercise Worsaae, when the descendants of the two streams went to war with each other in 1848.

The years following Suhm's death saw something of a recession in the writing of history in Denmark (Jørgensen 1943; J. C. H. R. Steenstrup 1889). Suhm's broad historical outline was, however, followed by Lauritz Vedel Simonsen, who, although he did not mention many historical dates, derived the Scandinavians from the line of Japhet (1813: Pt I, § 2: 18 ff.) and brought Odin, a refugee from Roman expansion, into Scandinavia in the first century bc (1813: 154 ff.). A few years later the Swedish historian Magnus Bruzelius placed Odin's arrival at around 100 bc (Bruzelius 1830: 15), and his countryman Erik Gustav Geijer argued for the fundamental

unknown	The first Odin lives at Tanais among the Asas and Alans, and introduces sun worship. All people in Scandinavia who are not Finns are called Joths.			
500	The middle Odin flees from Asgard at Tanais to northern Europe, and leaves Goths south of the Baltic in Prussia and Livonia, and also leads Goths with him into Sweden, where the Jothic name disappears.			
400	From these Goths in Sweden descend the Swedes, Norwegians and Danes.			
c. 150	Germanic peoples, Cimbrians from Jutland and Teutons from Holstein break out...			
100	...and are defeated in Italy by Marius. Goths from Zealand and the other islands then cross to northern Jutland and occupy it; and Angles, a Suevian people from Brunswick and Hanover, occupy southern Jutland.			
70	Odin, worshipped as a god and living in Uppsala, divides Scandinavia among his friends and sons			
	rulers in Zealand at Leire over the Goths	rulers in Scania over the true Danes	rulers in Trondheim or the true Norway	notable events
40	Skjold, Odin's son	Heimdal, Odin's son	Seming, Odin's son	Goths under Skjold move from Sweden to Zealand and Fyn, then inhabited by Joths, and establish a kingdom there. Lombards leave northernmost part of Jutland
BC	Fridleif I, Skjold's son	 O O O O 	Godhialt	
AD 1st cent	Frode I, Fredegode, Fridleif's son		Sverhialt	Goths leave Swedish Vester – and Øster-Gothland, and unite with the Goths of Prussia and Livonia
	Fridleif II, Frode I's son		Hodbrod	
	Frode II, Fridleif II's grandson		Himinleig	Rig expels the Heruls, who occupied Halland, and is the first in Scandinavia to call himself king
2nd cent	Vermund, Frode II's son	Rig, Heimdal's descendant	Vedrhall	
	Oluf, Vermund's son	Danp, Rig's son	Havar	

3rd cent	Dan Mykillatti, king in Scania, married to Oluf's daughter	Dan Mykillatti, m.Oluf's daughter & king in Zealand	Godgiest	Dan Mykillatti, king of the Scanian Danes inherits the Gothic kingdom in Zealand and the Jothic one in Jutland, calls the whole kingdom Denmark after the Danish people
			Hiemgiest	
			Gudlaug	
4th cent	Frode III, Dan's son	Frode III, Dan's son	Gylving	Hugleif, king of the Angles of Schleswig, unites with the Saxons, a Germanic people in Holstein, and with the Jutes, and his descendants rule all three peoples
	Fridleif III, Frode's grandson	Fridleif III, Frode's grandson	Mendill	
5th cent	Frode IV, Fridleif's son	Frode IV, Fridleif's son	Brand	Saxons, Angles and Jutes begin to go to Britain, AD 449
	Ingild, Frode's son	Halfdan, Frode IV's son	Godgiest & Brynjolf	
	Frode V, Ingild's son			
	Helge, Halfdan's son	Helge, Halfdan's son	Bard	Helge conquers Jutland and Angeln
6th cent			Hergild	
	Frode VI, Ingild's grandson	Frode VI, Ingild's grandson	Havar	
	Hrolf Krake, Helge's son	Hrolf Krake, Helge's son	Harald Thrygill	Hrolf Krake completes the conquest of Jutland and Angeln, which after his death are ruled by many small kings all of Danish ancestry. He was the first Danish king to make conquests in England
	Frode VII, Ingild's grandson	Baldar, Ingild's great-grandson	Thronð	
7th cent	Halfdan II, Frode VII's son	Harald I, Baldar's son		
	Roric Slyngebaug, Halfdan's son	Halfdan III, Harald's son	Harald	
	Ivar Vidfadme, Halfdan III's son	Ivar Vidfadme, Halfdan III's son		Ivar Vidfadme conquers all Denmark and Sweden
8th cent	Harald Hildetan, Hrærec Slyngebaug's son	Harald Hildetan, Hrærec Slyngebaug's son	Herlung	
	Sigurd Ring, Ivar Vidfadme's daughter's grandson	Sigurd Ring, Ivar Vidfadme's daughter's grandson	Herlung	Sigurd Ring, king of Sweden, conquers Denmark
	Regnar Lodbrog, Sigurd Ring's son,d.794	Regnar Lodbrog, Sigurd Ring's son,d.794	Griotgard, father of Hacon Jarl	
	Sigurd Snogoie, Regnar's son	Sigurd Snogoie, Regnar's son		

Fig. 2.3. Table of historical events and people in Suhm's dark period, from the earliest times to 70 BC, and his fabulous period, 70 BC–AD 800. (Combined and translated from Suhm 1802, tables I and II).

correctness of the scheme (Geijer 1832: 31–6). This was the chronological context into which the Three Age System emerged, and as we shall see in the next section, C. J. Thomsen was also initially to follow it.

THE DEVELOPMENT OF ARCHAEOLOGY, 1806–1816

Copenhagen University's librarian was a man who enjoyed making extended walking tours round various parts of Denmark. On these tours he took frequent note of antiquities, and by 1805 Rasmus Nyerup was a worried man. Agriculture and road-building were destroying ancient monuments at an unparalleled rate, and the numerous antiquities that were turned up were mostly accorded little attention and were usually dispersed and lost. Nyerup wrote a book in which he envisaged a programme of protection for field monuments, and the establishment of a National Museum to receive finds (Nyerup 1806).

The Bishop of Zealand was another man who worried about such things. Frederik Münter in the same year wrote a smaller book lamenting the destruction of the monuments at Leire, regarded as the capital established by one of Odin's sons (see fig. 2.3):

Eighteen centuries have run their course since Leire was mentioned in the history of Denmark; and we have reasonable grounds for the presumption that the place was already renowned in Scandinavia when Odin's son Skjold made it his residence... [*Archaeologists*] since the days of Worm² have visited and described the remains at Lejre. But the way these appeared in the seventeenth, and even in the mid-eighteenth, century, they appear no longer. Some even of what I saw in my young days twenty-five years ago, has in the intervening period been destroyed; and in the course of time, everything that can be destroyed, will disappear. (Münter 1806: 1, 3*)

These publications by Nyerup and Münter marked the beginning of the concern with antiquities that was to lead directly to the Three Age System a generation later. The government's Ministry for Home

² Ole Worm (1588–1654), prominent scientist and medical man, and the leading figure in Danish archaeology before the nineteenth century.

Affairs asked Münter for his recommendations on 12 March 1807, and on 31 March he responded with a remarkable document that laid the foundation for everything that followed. It recommended that reports about surviving antiquities be required from all parts of the Danish realm; that a commission be established to receive these reports and decide which monuments should be preserved, and to receive antiquities; and that a national museum be established (Hermansen 1931, 1949). This document has never been fully translated into English, so a full translation is presented in Appendix 1.

Münter's recommendations were rapidly accepted, which led to the immediate establishment of the Royal Commission for the Preservation of Antiquities on 22 May 1807. This had a membership of six, of whom Münter was one, with Nyerup as secretary (Hermansen 1931). It began work straight away; in the absence of premises of its own, and because all the members of the Commission except Nyerup were Freemasons, its first meeting on 4 June was in rooms attached to the Masonic Lodge in Copenhagen (Hermansen 1931: 310). It began receiving artefacts in the same year, which were initially housed by Nyerup in the University Library. The year 1807 is therefore regarded as the symbolic date of the foundation of the National Museum of Denmark; Sophus Müller, the pre-eminent Danish archaeologist of the late nineteenth and early twentieth century, celebrated its centenary in 1907 (Müller 1907). The Commission instituted a periodical, *Antikvariske Annaler*, which appeared intermittently starting in 1812; the first volume printed the Commission's reports from 1809 and 1811, listing monuments that had been protected by law, and some of the artefacts received. The first ones illustrated were a collection of bronzes from the island of Samsø (fig. 2.4).

A translated phrase from Rasmus Nyerup's book is often quoted in Anglophone histories of archaeology, to the effect that he saw everything from the heathen period as wrapped in 'mist' or 'fog' (e.g. Daniel 1964: 29; Daniel 1967: 80; Klindt-Jensen 1975: 6; Schnapp 1996: 284; Trigger 1989: 71). It is easy to read these quotes as if Nyerup was lamenting his inability to deal with the past from before the period of written records, and conclude that he had an

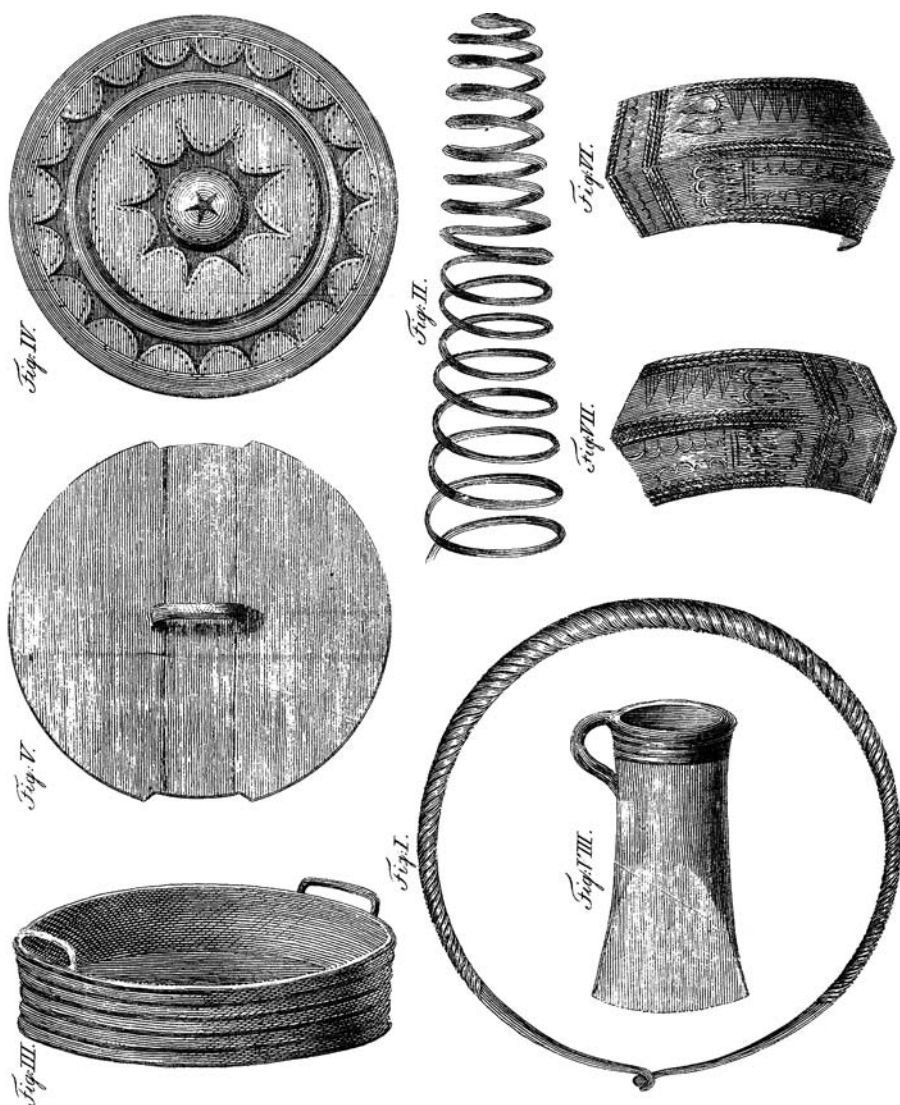


Fig. 2.4. Unintegrated antiquities: Nyerup's objects from Samsø, the first archaeological artefacts illustrated as a result of the work of the Antiquities Commission. (From Nyerup 1812: fig. 2).

inadequate 'idea of prehistory'. This was not, however, what Nyerup was saying. We must examine the passage in full; he was describing a room in his vision of a future museum:

This room contains monuments and remains that go so far back that they cannot be accurately dated. Everything that derives from an ancient period that precedes the introduction of the Christian faith in these countries can correctly be said to be infinitely old. With Christianity we get better writing media than stone or wood, faster writers than the runemasters of yore, time resolves itself into years and centuries, against which events, actions and monuments can be reckoned and divided. Everything preceding, everything from the earliest heathen period hangs before us as if in a thick fog, in an unmeasurable period of time. We know it is older than Christendom, but if by a few years or a few hundred years—even maybe over a thousand years—older, is sheer guesswork and at best only likely hypotheses. (Nyerup 1806: 1*)

Two points which emerge from this must be stressed. First, Nyerup was clearly discussing just objects and monuments, not all history. Second, as we have seen, Suhm's long ancient historical chronology was in place. Nyerup was an admirer of Suhm, and in these years was himself working up Suhm's notes for the posthumous publication of the unfinished volumes of the latter's *Historie af Danmark*—the final volume of which appeared in 1828, just one year before Nyerup's own death (Jørgensen 1943: 11). Nyerup was not in fact envisaging a very long chronology; since Denmark became Christian around AD 900, 'even one thousand years' before this was barely a century before Christ—well within the scope of Suhm's ancient historical chronology. Nyerup was thus lamenting not the absence of any chronology, but rather *the absence of a means of dating antiquities against the existing chronology*. He did not need an 'idea of prehistory' because he already had a sufficiently good 'idea of history'.

This is an important point which has led some writers astray; for example, in his English rendering of Münter's five recommendations (see Appendix 1), Klindt-Jensen's translator starts the first with the words 'Prehistoric monuments' (Klindt-Jensen 1975: 48). The original Danish wording is *monumenter fra oldtiden* (quoted in Hermansen 1931: 301), more correctly translated as 'monuments from

ancient times' or 'ancient monuments'. Münter and Nyerup did not consider them as falling before all recorded history; the Scandinavian word for 'prehistory' did not see the light of day for another thirty years (see below).

Nyerup's approach to chronology emerges clearly from a long two-part article he wrote on the history and antiquities of the island of Samsø (Nyerup 1812), from which the illustration in fig. 2.4 is taken. Following Suhm's outline, he divided the island's past into the dark, the fabulous, and the historical periods (Nyerup 1812: 21). Most of his discussion of the island's dark period involved a consideration of the etymology of its name, and of the names of places on it. In the fabulous period, the main event on Samsø was a famous battle, recorded in the sagas, in which Hialmar and Orvarodd fought against Angantyr and his eleven brothers. Nyerup was initially cautious about this: 'if, following Suhm, this is placed in the fifth century, it becomes, like everything in this period, somewhat apocryphal, but—it does not however in itself contain anything incredible or unlikely, it is described in ancient writings, it is reinforced by the monuments, supported by oral tradition' (1812: 30*). He explained his reference to monuments a few pages later, stating without any apparent reservations that the most important monuments on Samsø from the fabulous period were the burial mounds of Angantyr and his brothers, which had survived into recent times (Nyerup 1812: 34, 204–5). He then discussed individual antiquities, of which those in fig. 2.4 are the examples he illustrated, at considerable length (1812: 207–22) but without ascribing any to a particular period. Thus Suhm's chronology, albeit imprecise, provided Nyerup's framework; monuments could occasionally be hung onto it if any history was attached to them, but the artefacts were largely undatable and unusable except for anecdotal purposes.

Such was the situation when Nyerup retired as secretary of the Commission in 1816. His replacement was Christian Jürgensen Thomsen (fig. 2.5), who had a long-standing interest in numismatics and other antiquities, and was of sufficient independent means to accept the unremunerated office of secretary. In the next two decades he was to devise the fundamentals of the Three Age System.



Fig. 2.5. Christian Jürgensen Thomsen. Mid-nineteenth-century lithography by Bærentzen, copied from a photograph.

THE ORIGINS OF THE THREE AGE SYSTEM

Thomsen instituted a new method of working with the archaeological record, a method which has been examined by a number of recent researchers (especially Gräslund 1974, 1987; Jensen 1988, 1992; Street-Jensen 1988). His chronological scheme was not simply a means of arranging artefacts in museum cases, a means which subsequently turned out to have chronological foundation: it was created by him on the basis of repeated observations of material. The suggestion that stone, bronze, and iron formed a technological

sequence goes back at least to a mention by Lucretius in the first century BC, and various authors including a number of Scandinavians had mentioned it from the sixteenth century onwards (B. Hildebrand 1937/8: I, 119–29), most recently in very brief references by Suhm (1802: 11) and Vedel Simonsen (1813: pt I §2, 76 n. 1). Thomsen himself referred to it as an ‘old idea’ in 1825 in a letter to the German archaeologist J. G. G. Büsching (in Hermansen 1934: 101*; translated and presented in Appendix 2). What Thomsen did was to take this old idea and apply it in practice.

Thomsen’s principal publication of the scheme was in the second chapter of a book entitled *Ledetraad til Nordisk Oldkyndighed*, or ‘Guide to Scandinavian Archaeology’ (Thomsen 1836a). The first chapter in the book was a consideration of the extent and importance of the ancient Scandinavian literature, by the historian Niels Mathias Petersen (N. M. Petersen 1836).³ Gräslund (1974, 1987) points out that Thomsen’s chapter incorporates many more materials than just stone, bronze or iron. Thomsen included in his dating scheme artefacts made of a variety of other materials, such as amber (Stone Age only); gold (Bronze and Iron Ages); silver and glass (Iron Age only); and pottery (all periods); as well as grave type and method of burial (Gräslund 1987: table 1). To Gräslund’s list can be added decorative motifs, which differed between the periods (Thomsen 1836a: 62–3).⁴

³ Since it contained two chapters by different authors, the book was not ‘by’ Thomsen as is sometimes stated. The title page carries no editor’s name, but it was commissioned and edited by C. C. Rafn, secretary of the KNOS (Briggs forthcoming, n. 86); I therefore follow Briggs in crediting Rafn as editor in the references.

⁴ As Briggs (forthcoming, n. 86) emphasizes, despite the statements of many recent authors Thomsen’s chapter was *not* a museum guide, but a general consideration of archaeological monuments and artefacts. Magnus Petersen, employed in the museum as a draughtsman by Thomsen, stated in his memoirs that Thomsen was in fact opposed to the production of a museum guidebook and tried to dissuade one of his young assistants, Julius Sorterup, from writing one (J. M. Petersen 1909: 5). Sorterup, however, persisted, and it appeared in 1846 (Sorterup 1846). Petersen one day watched Thomsen approach three soldiers who had pooled their money to buy a copy, and their enthusiastic endorsement of the guidebook convinced Thomsen of its usefulness (J. M. Petersen 1909: 5–6). This anecdote has a curious afterlife: in 1859 the Museum lent Petersen’s services to the English traveller Horace Marryat, then staying in Copenhagen (J. M. Petersen 1909: 53–5). Petersen evidently told him this story, for Marryat reproduced it in the book he wrote about his travels, but transposing it to the Ethnographic Museum and making it appear that Thomsen had pointed out the soldiers with the guidebook to Marryat himself (Marryat 1860: I, 232).

The old laws of *danefæ* (a term best translated as ‘national treasure trove’) dated back to 1683, and claimed all finds of precious metals for the Crown. From 1752 the finders were paid the full cash value of their finds (Galster 1946). These laws were re-activated and extended, and Thomsen instituted a system of payment even for items not made of precious metals (Jensen 1992: 50). As a result an increasing number of artefacts began to flow into the museum, and a means of dealing with these was essential. One of Thomsen’s early innovations was to institute a standardized record for each group of finds received, something that had not previously been the case (1992: 52). It has been possible on the basis of these records to reconstruct a considerable number of the actual finds that Thomsen received from 1817 onwards (Street-Jensen 1988). Thomsen’s chronological scheme grew from his recording of what we would nowadays term *assemblages of finds*, and his observation that the same kinds of items occurred associated with each other not just repeatedly, but in the same contexts. This conception is what differentiates his work from that of all previous archaeologists (Gräslund 1974: 101–12, 1987: 20–9; Jensen 1988: 15, 1992: 51–5; Street-Jensen 1988).

Thomsen’s scheme was in fact well worked out long before its 1836 publication in the *Ledetraad* volume. It was implicit in an 1832 article he wrote on stone tools (Thomsen 1832). When Charlotte Williams Wynn, a lady from North Wales, visited his museum in 1827 he used the display to explicate the scheme for her (Rowley-Conwy 1984), and he had probably begun the reorganization of the display to reflect his conclusions as early as 1818 (Hermansen 1934), the year before the museum opened to the public (fig. 2.6). One of the earliest documentary records of his scheme was in a letter he wrote to the German archaeologist J. G. G. Büsching on 19 February 1825 (Hermansen 1934: 101–5). This revealing letter has never hitherto been published in English, so it is presented in full in Appendix 2. In it Thomsen makes it clear that he was making full use of the outline Three Age System by 1825, and furthermore that he was already considering grave types as varying between the periods. But it shows something else very significant, namely that in 1825 Thomsen still accepted the broad outlines of Suhm’s chronology, and was trying to fit the early part of the Three Age System into it. His earliest chronologically fixed point was the arrival of Odin ‘around the time



Fig. 2.6. Thomsen using the museum to explain the Three Age System to visitors. This drawing is by Magnus Petersen, who was attached to the museum as a draughtsman from the 1840s. (From J. M. Petersen 1909: 3). The younger man in the cap bears some resemblance to Thomsen's assistant C. F. Herbst (cf. J. M. Petersen 1909: 15) and may be intended to represent him.

of the birth of Christ', and he regarded Odin as the bringer of bronze to Scandinavia. Since the change from stone to bronze would take some time, the stone-bronze transition occurred at about AD 200. The transition to iron occurred around the fifth or sixth century AD in Germany, and a century or two later in Scandinavia. The notes made by Bror Emil Hildebrand during his visit in 1830, mentioned at the start of this chapter, reveal that Thomsen still considered these dates to be correct in that year (H. Hildebrand 1880: 146–7), but as we shall see, his views had changed substantially by 1836.

Thomsen's scheme was thus not chronologically independent to begin with, but was initially grafted onto the ancient historical chronology, which in the 1820s was still gaining support from a variety of intellectual directions. Ancient history remained dominant. An important development was the foundation in 1825 of (to give it its official English title) the 'Royal Society of Northern Antiquaries'. This is actually a less than accurate translation of its Danish name, *Det Kongelige nordiske Oldskriftselskab* (KNOS), which would be better rendered as 'The Royal Scandinavian Society for Ancient Texts'. The publication and study of ancient texts was its prime purpose (Steen Jensen 1975; Thrane 2001; Wiell 2001), and one of its founders, the energetic C. C. Rafn, personally published no fewer than twelve volumes of translated sagas between 1826 and 1837. The KNOS did, however, publish archaeological material. Although the Commission remained an independent organization until 1849, the last issue of its *Antikvariske Annaler* appeared in 1827, and from 1832 the KNOS's new periodical *Nordisk Tidskrift for Oldkyndighed* ('Scandinavian Journal of Archaeology') published the Commission's reports. Its first number also contained Thomsen's major article on stone tools mentioned above (Thomsen 1832).

Various lines of scholarship in the 1820s were broadly compatible with Suhm's outline. This was so both for racial history and for comparative philology, which would soon combine to form the science of ethnology. With regard to race, the German naturalist J. F. Blumenbach had in 1795 divided humans into five main races, coining the term 'Caucasian' for Europeans because of their supposed origins in the mountain range of that name (Gould 2003: 356–66). Blumenbach's theory would in due course raise the major question of whether humans had a single origin (monogeny) as

Suhm had supposed from the biblical account, or might have more than one (polygeny). This debate did not, however, affect Scandinavian history greatly, because whichever was the case, Scandinavians still originated from somewhere near the Black Sea; this therefore did not conflict with the derivation of an historical Odin from that region. With regard to philology, Rasmus Rask published a major work on the origin of the Scandinavian languages in 1818. He expressed some caution about the new idea that Persian and Indian languages were related to European ones (Rask 1818 [1993: ii–iii]), and dismissed the biblical account of human origins (1818 [1993: 2–5]). However, he continued to derive Odin from a southerly region near the Black Sea (1818 [1993: 108–9, 283]), and argued for a chain of languages related to Scandinavian, extending from the Baltic back to the Black Sea and marking the route of Odin’s migration. He dealt with the Finnish problem by arguing that they were the original inhabitants of Scandinavia, who were pushed northwards by the arrival of Odin’s Goths; they were referred to in the sagas under names such as trolls, dwarfs, and jotuns (Rask 1818 [1993: 109]). During the 1820s leading historians in Copenhagen, such as the Icelander Finn Magnúsen, also continued to base chronological arguments on the ancient writings. All in all it is therefore not surprising that Thomsen in this decade sought to fit his scheme into this overall outline.

In the 1830s the ancient historical structure began to be called into question. The historian Christian Molbech was the first important figure to do this (Borup 1954: 349–51; Jørgensen 1943: 71–2). He gave a lengthy series of lectures in 1833–4, of which only two sections were ever published (Molbech 1834, 1836). In the first of these he was very critical of the earlier sources. In the second he reviewed Scandinavian and Frankish sources for the eighth and ninth centuries AD, and concluded that only from about the year AD 872 was the history of Denmark based upon any reliable foundation (Molbech 1836: 421–2). In the unpublished notes of his other lectures he attacked the very existence of Odin: ‘We must establish that no person called Odin ever existed. It is a mixture of fragments of Scandinavian sagas that preserved the memory of the immigration into Scandinavia of Gothic tribes and their leaders, with myths of Odin as the chief of the gods’ (quoted in Jensen 1988: 14*).

The lectures contributed to a book Molbech published in 1837, in which he challenged Odin in print:

The fact that some of the old saga books of Norway and Iceland contain stories of the Asas, of Odin, of his family and descendants, and of the royal lines descended from these, of a kind suggesting that they were reliable historical sources, should not mislead us. Odin is not, and can never be, a *historical person*. Everything told about him, not as a god but as a person, was first written down *twelve or thirteen hundred* years after the time in which people presume he existed; and this with no authority except the weak support of epic tales and adventures collected and written down in a much later period. The principal source for everything which has led to Odin and his family being considered as historical personages in our recent histories is a narrative put together from legend and song which Snorri Sturleson added (about AD 1230) to his saga about the oldest royal line of the Sveas in Uppland. (Molbech 1837: 99–100*, original emphases)

Molbech's critical view of Odin was reflected in Thomsen's writings, which in the 1830s paid less attention to ancient history than previously. In *Ledetraad* Thomsen made only one oblique mention of Odin: when describing certain stone monuments he stated that 'some have regarded them as deriving from a religion that predated the Odinian' (Thomsen 1836a: 36*). He made one other reference to an immigration at the right period, but without naming Odin—while revealing that his absolute chronology had moved considerably: 'if one concludes that a people from southern lands immigrated into Scandinavia about the time of Julius Caesar, it is reasonable that the immigrants, who knew iron, which was in common use in the south, brought it with them here to Scandinavia' (1836a: 60*). If iron, rather than bronze (as he had written to Büsching in 1825 and Hildebrand in 1830), appeared in the first century BC, this inevitably pushed the Bronze and Stone Ages further back into the deeper past. Thomsen by now appreciated that his antiquities were producing a different kind of information from the historical texts, and reached back further. In his opening paragraph he wrote:

Since these [*antiquities*] can never provide us with new [*historical*] facts, they can neither confirm ancient royal successions nor fix points in time; but they can, collectively and comparatively, give us a clearer idea of our ancestors' religion, culture, way of life and so on, than the written sources; the latter can never be ascribed so great an antiquity, the ancient stories are mixed

with more recent additions, and because they were first written down at a later date must often be suspected of having been considerably distorted. Archaeological remains, which cannot be described as true written sources, thus supplement them in broadening the limits of our knowledge of a time for which the texts are just beginning to earn our trust, and to suggest or disprove ideas about the movements of peoples or connections concerning which written history is completely silent. (1836a: 27–8*)

Thomsen was in this way extending his own chronology just at the time when the ancient historical chronology was coming under attack. The 1830s thus saw the origin of the ‘idea of prehistory’ in Thomsen’s mind, although in *Ledetraad* he nowhere used the Danish word *forhistorisk*, of which ‘prehistoric’ is a direct translation; he everywhere used ‘oldtid’, the more generalized term meaning ‘ancient times’ we have already discussed above.

The origin of the term ‘prehistoric’ has been the subject of considerable discussion. Its first use in English was not by an Englishman at all, but by a Scot, Daniel Wilson, in his major book *The Archaeology and Prehistoric Annals of Scotland* which appeared in 1851; we shall be seeing much more of both the man and his book in chapter 5. The French word *préhistorique* appeared a few years earlier, in 1845 (Clermont and Smith 1990). Chippindale (1988: 310) alludes to a usage of *forhistorisk* by Molbech as early as 1837. In fact Molbech used *forhistorisk* routinely in his lectures of 1833. It first appeared in print no fewer than eight times in the sections of these lectures that were published as articles (Molbech 1834: 421, 427, 432, 437, 452, 453, 460; 1836: 443). Molbech used it a further three times in his 1837 book (1837: 41, 80, 109). It is however vital that we consider this word in its context, which was Suhm’s terminology for three successive periods (see above): the ‘dark’ period before Odin’s arrival in 70 BC, the ‘fabulous’ period from Odin to King Gorm at c. AD 900, and the ‘historical’ period from Gorm onwards. Molbech was down-playing the reliability of the sources from Suhm’s ‘fabulous’ period—he was not denying that there were any sources at all. Numerous bardic songs and legends had survived from before the historical period; thus ‘we find the bards, and their practice of staying at or travelling about and singing at the estates of the rich or the powerful, present in Scandinavia as early as the *prehistoric* period’ (Molbech 1834: 8*, added emphasis). Even the sagas of the earliest part of the

historical era were unreliable, because 'they still have too much of the poetic nature of the *prehistoric* age' (Molbech 1834: 20*, added emphasis). Molbech's employment of the word did thus not conform to modern definition: he used it to refer not to the period before all written history, but to a time for which there was some historical documentation, albeit unreliable.

This conclusion is reinforced by a consideration of the way Molbech used *forhistorisk* in his 1837 book. This innovative work opened with a chapter on antiquities, in which he mentioned the Egyptian pyramids and Greek cyclopean walls; Scandinavian monuments belonged to 'this same *prehistoric* period' (Molbech 1837: 41*, added emphasis). He did not apparently accept that there was a time beyond the reach of *any* documents, unreliable though they might be: 'we would know only a little, and even less of it reliably, about the earliest inhabitants of Denmark and Scandinavia and their circumstances, if we were to restrict ourselves to the accounts of foreigners and the legends of the natives' (Molbech 1837: 25*). In criticizing the historical existence of Odin, he wrote that:

Among the ancient legends of Scandinavia, in its oldest poetry and writings, and in its religion, we encounter a famous name known for millennia, which has also acquired great historical regard and value, even though it lies beyond definite knowledge, or far back in the *prehistoric* period. This name is that of Odin . . . ; and this being has been transferred to the real world, into the arena of historical accounts and narratives, as an ancestor for the oldest Scandinavian dynasties in all three countries. Thus we read in one song of Odin living among the Asas . . . ; in another, of how Odin and his Asas come from the Black Sea or Southeast Europe up into Scandinavia in the century before the birth of Christ. (Molbech 1837: 80–1*, added emphasis)

Later, discussing the time of the Odinian invasion, he considered the first-century BC date broadly acceptable, although precision was not possible; in this respect, 'it is the same as with any event that took place in the *prehistoric* period, or time of legends' (Molbech 1837: 109*, added emphasis).

Molbech made routine use of *forhistorisk* in publications in the next years, but always in the manner just described. In a lengthy review article of the major books by Nilsson (1838–43), which as we shall see also used *förhistorisk*, and by Worsaae (1843a), which

did not, Molbech used the word four times (Molbech 1843: 607, 618[twice], 663). He criticized Nilsson's statement that ancient history had been dismissed by current historians, using *forhistorisk* explicitly to refer to *written* material: 'nor does the current approach regard all prehistoric material as "myth, allegory or poetry" [*as Nilsson had stated*]; since the author here forgets traditional history, which may be the *truth written as poetry*, or a poetic growth germinating from a historical root, without having to be entirely or solely poetry' (Molbech 1843: 618*, original emphasis). He argued with regard to the Stone Age that it was unscientific that 'a class of actual monuments should be ascribed to a people existing only in the imagination' (Molbech 1843: 662*), indicating his unwillingness to consider the existence of unnamed people beyond the reach of historical sources. The next year he used the term twice in another article (Molbech 1844b: 230[twice]). In a review of *Blekingske Mindesmærker* (Worsaae 1846a) Molbech again referred explicitly to 'prehistoric traditions' (1847: 686*).

Whether Molbech therefore had an 'idea of prehistory' like Thomsen's is open to serious doubt. His chronology was entirely conventional; despite his rejection of Odin, the Danes arrived around the time of the birth of Christ (Molbech 1837: 6), and displaced the Finns and Lapps into the north (1837: 15). But since funerary chambers containing stone tools were found only in southern Scandinavia, not in the areas currently occupied by the Finns and Lapps, they must derive from yet another people, neither Danes nor Finns/Lapps; they were probably immigrant Celts, since similar graves were known in areas of Europe to the South and West (Molbech 1837: 16–18). The ancient monuments did not go back beyond the supposed era of Odin, since they dated back only 'a couple of thousand years' (Molbech 1837: 25*).

Molbech was thus not using the word 'prehistoric' in the way we now understand it; but as we shall see in chapter 3, it was used in its modern sense by Daniel Eschricht later in the same year, and in 1838 by the Swede Sven Nilsson. It was thus in routine use in the Copenhagen–Lund academic community by 1837–8, and its use became widespread in the next few years. This is the earliest date the term was generally used anywhere, a fact which has escaped modern commentators (Chippindale 1988; Clermont and Smith 1990; Daniel 1964).

But for Thomsen if not for Molbech, the Three Age System existed independently of Suhm's ancient historical chronology by the 1830s. Physical manifestations such as grave types and funerary method, and artefacts of other materials, were difficult to link to Suhm's history (as Nyerup had already found), but were clearly easier to link by assemblage association to Thomsen's scheme. Thomsen's Three Age System was thus potentially predatory and expansionist, capable of arrogating to itself further swathes of the physical record as these became better understood.

The next development was that within a few years, three other chronologies had appeared independently in Copenhagen and Lund, and had been grafted onto Thomsen's by their creators. This marked the point at which the Three Age System went over onto the attack.

The Three Age System as Predator: Copenhagen and Lund 1836–1850

We saw in the last chapter how Thomsen's Three Age System was establishing itself as the ancient historical chronology started to fail in the 1830s. The years immediately following its publication in 1836 saw two major developments that Thomsen could never have foreseen.

The first development was that three entirely separate chronologies came to maturity, and were grafted by their makers onto Thomsen's stone–bronze–iron sequence. These chronologies were Sven Nilsson's economic scheme of hunter-gatherers preceding farmers; Japetus Steenstrup's environmental scheme of successive forest types; and the craniological scheme of racial replacement devised by Daniel Eschricht and Anders Retzius, and championed by Sven Nilsson. None could easily be linked to the ancient historical chronology; but since all three were based on material remains rather than literary sources, they were easier to link with Thomsen's artefactual scheme, so they naturally gravitated towards it. Only Steenstrup's environmental scheme provided any hint of absolute chronology—and the hint it gave was so revolutionary that Steenstrup initially lacked the confidence to make much of it. But as it became more secure, it gradually became evident that the human time depth revealed by the broadened Three Age System dwarfed the conception of ancient history. The first part of this chapter examines how these chronologies developed and then attached themselves to Thomsen's.

The second development was that, having attracted to itself these other chronologies, the Three Age System (in the hands of J. J. A. Worsaae) went over to the attack against ancient history.

The second part of this chapter examines how Worsaae used archaeological excavation and data to wrest large parts of the material record from the ancient historians, by demonstrating that their use of it had been substantially inept. As a direct result, much of the ancient historical account lost its historical force and reverted to the status of literature and legend, leaving archaeology as the dominant voice speaking for the ancient past. In the later 1840s nationalist agendas were sharpening in various parts of Europe, and Worsaae used the archaeological voice to refute an aggressive historical claim by a German whose name is well-known in the Anglophone world—none other than Jacob Grimm, one of the brothers responsible for the fairy tales that are still so associated with their name.

THE ECONOMIC CHRONOLOGY: SVEN NILSSON

The economic chronology was put forward by Sven Nilsson (1787–1883), a senior academic figure in the University of Lund, who specialized in zoology. Word of Thomsen's ideas had certainly spread to Lund before *Ledetraad* appeared: after returning to Lund from his 1830 visit to Copenhagen, Bror Emil Hildebrand wrote to Thomsen in October of that year that he was about to start reorganizing the museum in Lund along Three Age System lines (B. Hildebrand 1937: II, 573). Nilsson cannot have been unaware of this—he had an interest in artefacts, and had begun a collection of his own in the 1820s (B. Hildebrand 1937: 704). His first publication of part of his economic scheme came in 1835, the year *before* Thomsen published in *Ledetraad*. This was in an essay on the history of hunting and fishing, which formed part of the introduction to the second edition of his major work on the birds of Sweden (Nilsson 1835). At the time of writing it, he was not aware of Thomsen's 1832 paper on stone tools, but he came across it in time to add some footnotes while his essay was in proof. His major publication came a few years later (Nilsson 1838–43), appearing first as a series of separate fascicles before being published in complete and expanded final form—though each chapter remained separately paginated (Rowley-Conwy 2004).

Nilsson was interested in the *function* of the stone tools. As an active naturalist and hunter, his primary conclusion was that the stone tools represented hunting and fishing equipment, and were thus the remains of people who had lived by hunting and fishing. He made extensive use of ethnographic descriptions of the technology of contemporary hunter-gatherers to back this up, and also travelled widely to visit ethnographic collections. His book is well and profusely illustrated; fig. 3.1 shows a few of the artefacts he depicted. His discussion of these started with no. 160, a find from Scania; this might, stated Nilsson, have been mounted as the head of an arrow, a throwing-spear, or a thrusting-spear, and would have made an effective hunting and (if necessary) fighting weapon. Find no. 157 could, however, *not* have served the same purpose, because it was curved; it must thus have been paired with another similar one to make a fish leister, and Nilsson depicted its mirror image in outline next to it to show this. Contemporary hunter-gatherers used identical items for this purpose: no. 155 was a complete example from the Northwest Coast of North America which he had drawn during a visit to the museum in Bristol in 1836; no. 156 was another North American example he had been given by Gideon Mantell, better known for his pioneering studies of dinosaurs (Nilsson 1838–43: 67–9). Much of Nilsson's ethnographic reading was concerned with the Eskimo, particularly those from Greenland, which was a Danish colony, so much of the ethnography was therefore in a language easily accessible to him. Nilsson refers throughout his text especially to three, namely Hans Egede (1741), David Cranz (1769), and Wilhelm Graah (1832). The first two in particular are well illustrated, and Nilsson drew many descriptions of hunting and fishing gear from them.

Nilsson's chronological contribution was to place his stone-using hunter-gatherers at the head of a developmental sequence which led on to pastoralism, thence to agriculture, and finally to contemporary commerce:

Every people has gone or will go through four stages before reaching its highest social development: they may be *savages*, or *nomads*, or *agriculturalists*, or people with writing and minted coinage, with labour divided among the members of society.

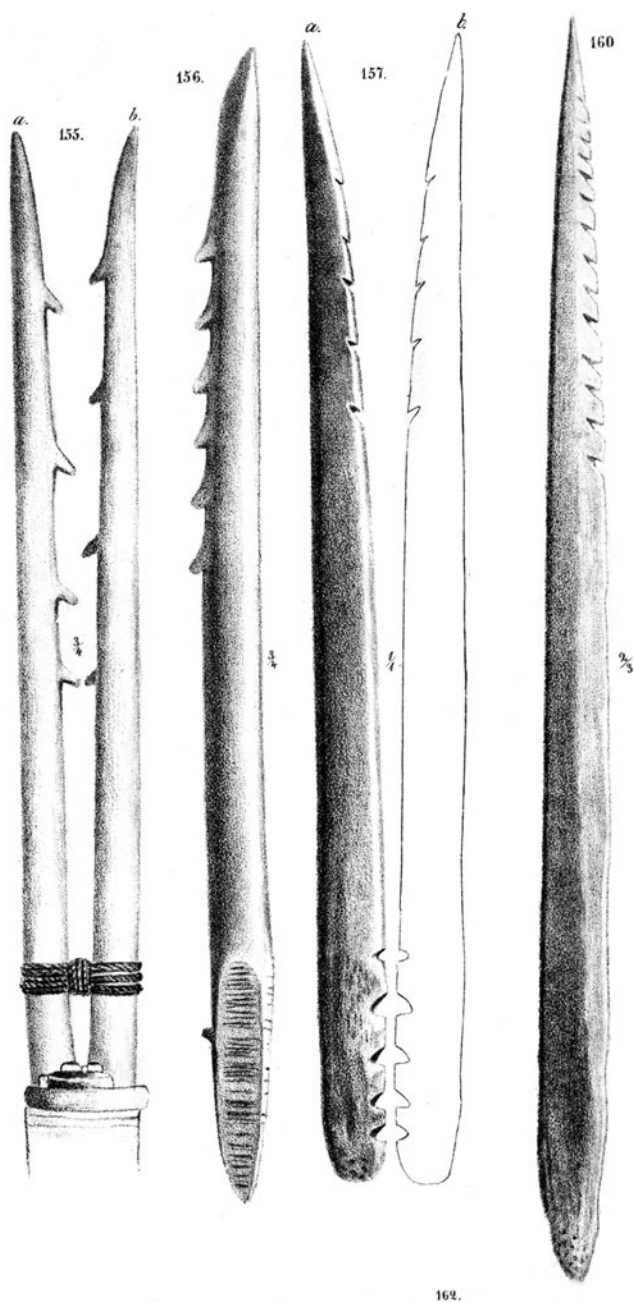


Fig. 3.1. Nilsson's drawings of archaeological and ethnographic bone points, showing the similarity between them. (From Nilsson 1838–43: fig. XIII).

1) *The savage* knows little other than his material needs, and he desires only to satisfy them on the instant. To satisfy his daily hunger: to protect his body so far as necessary from heat or cold: to find shelter for the night: to satisfy his need to procreate and instinctively protect his offspring—herein lie all his cares and all his pleasures. He thinks and acts only for *today*, not for *tomorrow*. In this situation the human is unconditionally a hunter and fisher... The savage everywhere finds the materials for hunting and fishing implements, and when he must he learns to prepare and use them... Experience slowly stimulates his thoughts; hunger is an unpleasant guest that arrives inevitably when one day he fails to catch his quarry. He therefore conceives the clever idea of saving the day's surplus for future needs; he conceives the even cleverer one of feeding up the young ox or reindeer calf, whose mother he has killed in the hunt: he collects more and bit by bit acquires a herd: he becomes

2) *The herder* (nomad), and lives mostly on the products of his flock: meat of domestic animals to eat, milk to drink, skins for clothing. Hunting and fishing, formerly his chief occupations, now become subsidiary... There are no ownership boundaries; property rights are limited to tents and flocks... Finally he tires of his nomadic way of life... he burns a patch of woodland and sows seedcorn in the ash. His first *field* is a swidden and his first *plough* a hoe. In this way the nomad slowly becomes

3) *The agriculturalist*, and as such develops a more settled social type. The mobile tent gives way to more regular permanent dwellings; the cultivated plot gives a better yield the more it is cultivated... The owner cultivates and cares for his territory; he has put his energy into it: it is *his*, he will and must keep it for himself and his descendents... A *boundary line* is drawn between properties: ownership becomes more pronounced, and also includes land... [Finally] the agriculturalist enters

4) the fourth stage of development, with a yet more structured society, in which labour is divided between the members of the society. (Nilsson 1838–43: v–vii*, original emphases)

Nilsson's book was about the savages of the Stone Age; he was not concerned with the later metallic eras in his 1838–43 book, but soon accepted that domestic animals and plants appeared in the Bronze Age—a period he did not however deal with till many years later (Nilsson 1862–4).

Nilsson was a scrupulous citer of his sources, and it is remarkable that in neither of the two publications where he listed the

stages of economic development (1835: xxiii–xxiv; 1838–43 as quoted above) did he state where this four-stage theory came from. It appears that he regarded it as too self-evident to require substantiation, and it was indeed a widely accepted theory. The so-called Four Stage Theory was a development of the intellectual movement that has become known as the Scottish Enlightenment. The basic elements of the theory were first published by Sir John Dalrymple and Lord Kames, in books of 1757 and 1758 respectively, although it is most probable that Adam Smith was including a version of it in his lectures several years before this (Meek 1976: 99); but Smith did not publish his own version until *The Wealth of Nations* appeared in 1776. Dalrymple's statement of the theory is as follows:

The first state of society is that of hunters and fishers; among such a people the idea of property will be confined to a few, and but a very few moveables; and subjects which are immovable, will be esteemed to be common. In accounts given of many American tribes we read, that one or two of the tribe will wander five or six hundred miles from his usual place of abode, plucking the fruit, destroying the game, and catching the fish throughout the fields and rivers adjoining to all the tribes which he passes, without any idea of such a property in them, as makes him guilty of infringing the rights of others.

The next state of society begins, when the inconveniences and dangers of such a life, lead men to the discovery of pasturage. During this period, as soon as a flock have brouzed [*sic*] upon one spot of the ground, their proprietors will remove them to another; and the place they have quitted will fall to the next who pleases to take possession of it: for this reason such shepherds will have no notion of property in immoveables, nor of right of possession longer than the act of possession lasts...

A third state of society is produced, when men become so numerous, that the flesh and milk of their cattle is insufficient for their subsistence, and when their more extended intercourse with each other, has made them strike out new arts of life, and particularly the art of agriculture. This art leading men to bestow thought and labour upon land, increases their connection with a single portion of it; this connection long continued, produces an affection; and this affection long continued, together with the other, produces the notion of property in land. (Dalrymple 1757: 86–8)

Others wrote in similar vein. Adam Ferguson reiterated that peoples who subsist by hunting and fishing 'have little attention to property', while others:

having possessed themselves of herds, and depending for their provision on pasture, know what it is to be poor and rich. They know the relations of patron and client, of servant and master, and suffer themselves to be classed according to their measures of wealth. This distinction must create a material difference of character, and may furnish two separate heads, under which to consider the history of mankind in their rudest state; that of the savage, who is not yet acquainted with property; and that of the barbarian, to whom it is, although not ascertained by laws, a principal object of care and desire. (Ferguson 1767: 123–4)

The wording of Dalrymple's statement is quite similar to Nilsson's, involving the same series of economic stages, and the same sequential development of property rights. There can be no doubt that Nilsson was so thoroughly imbued with the outlines of the Four Stage Theory of economic development that, once he made the connection between stone tools and hunting and gathering, it was inevitable that he should link this scheme to Thomsen's. The Four Stage Theory could never be linked to historical eras so long as these were based on the chronology of ancient history, but once the equation had been made between stone tools and hunter-gatherers, the artefactual and economic schemes fused naturally.

Perhaps because he was a zoologist rather than a historian, Nilsson had no qualms about the idea of a past beyond the reach of written records, and it is in this connection that he makes use of the word 'prehistoric', in Swedish *förhistorisk*. In the foreword that accompanied the 1838 publication of his first chapter, he defines his terms thus: 'by the *original inhabitants* of Scandinavia I mean not just the first people who immigrated or were originally present in the country, but all those who lived here throughout the time that went before history; I thus mean the *prehistoric people of Scandinavia*, of one or several tribes' (Nilsson 1838–43: i–ii*, original emphasis). *Förhistorisk* is used six times in Nilsson's book. Four of these are in the foreword to the section that appeared in 1838 (Nilsson 1838–43: ii [twice], iii, iv), the other two in the extension of this that first appeared when all the sections were brought together in 1843

(1838–43: v, vi). The way Nilsson used the word is clearly in its modern sense—in contrast to Molbech’s use (see chapter 2). But it is unlikely that he invented the term for himself. It is certainly possible that he did, but he may also have come across it in Molbech’s book published in 1837. Nowhere did he cite Molbech by name, however, but he did mention (and dismiss) the suggestion that the megalithic graves were constructed by Celtic peoples, noting that the suggestion had arisen because such graves were also found in France and Britain, regions previously occupied by Celts (Nilsson 1838–43: 2, 7–8). This was Molbech’s argument (see above), so it is most probable that Nilsson had read Molbech’s 1837 book. But it is perhaps more likely that he came across the term in the paper by Daniel Eschricht, which was also published in 1837 and which Nilsson had certainly read (see below).

Like Thomsen, Nilsson realized that he was elucidating very different things about the past from the ancient historians. He went one step further. He noted that historians were either, like Magnusen, believers in Suhm’s chronology, or, like Molbech, sceptics. His sort of archaeology might resolve this issue:

The method I have chosen—the comparative-natural historical—does not result in the acceptance of either of these views. This sort of research does not invalidate the evidence of the Eddas or the sagas, but seeks to remove them from their poetic context and display the naked truth that is contained in their prose; it does not reject colonizations at various times by different immigrant tribes into Scandinavia; but it does not accept them until traces of their remains are found in Scandinavia soil. (Nilsson 1838–43: vii*)

In other words, archaeology might in due course act as a *test* of the ancient historical chronology. The potential was thus that ancient history might now be hung onto the later part of the archaeological chronology, a revolutionary concept and an utter reversal of conventional views. Nilsson’s contribution to the Scandinavian ‘idea of prehistory’ was massive. But as we shall see in chapter 4, Nilsson’s work was not translated into English until much later, and was thus not able to have a similar impact in England, Scotland, or Ireland.

THE ENVIRONMENTAL CHRONOLOGY: JAPETUS STEENSTRUP

Environmental change provided the second chronological scheme to be linked to the Three Age System, and here there was nothing inevitable or natural about the linkage. It occurred as the result of unexpected observations by Japetus Steenstrup in a different academic sphere, namely peat bog stratigraphy. The significance of Steenstrup's work was twofold: first, it provided a link (initially tentative) between the earliest human occupation of Denmark and the most recent geological past; and second, by providing estimated dates for the geological sequence, it provided a timescale for human occupation far greater than that envisaged even by Thomsen and Nilsson.

In 1836 the Royal Danish Academy of Sciences and Letters offered a prize for the best essay written to answer the question of why the remains of pine trees were commonly found in Danish peatbogs. Pines are not native to Denmark, so their presence demanded explanation. The winner of the prize was Japetus Steenstrup, whose essay was summarized in the Academy's journal the next year (J. J. S. Steenstrup 1837), and then published in an extended form in 1842 (J. J. S. Steenstrup 1842). Peatbogs throughout Denmark were being dug out for their peat, and Steenstrup examined quite a few. He concentrated his efforts on two, in the area north of Copenhagen: Vidnesdam and Lillemose. He documented not just the presence of pine trees in these bogs, but realized that the pines occurred in a distinct layer towards the base of the peat (his section through Lillemose is shown in fig. 3.2). There were other forest layers too: below the pine layer was a layer containing aspen. Above the pine came a layer of oak trees, and above them a layer of alders. To this four-fold sequence Steenstrup added the beech as a fifth stage, since it had been the dominant tree in Denmark throughout the historical period; but it was hardly ever found in bogs.

What Steenstrup had observed was the outline of what we now term the postglacial forest succession. At that time, however, the geological understanding of glaciers did not go beyond the realization that Alpine valley glaciers had at some time in the past extended further than they now did. The existence of huge continental icesheets had

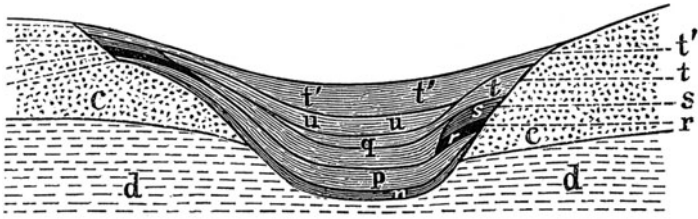


Fig. 3.2. Steenstrup's section through Lillemosse bog, showing the forest layers. c: gravel with boulders; d: gravel layer, the base of the bog; r, s, t: edge deposits formed by trees falling into the bog; r: layer of pine fragments; s: layer of oak fragments; t: layer of alder fragments; n: the aspens layer; p: the pine layer; q: the oak layer; u: layer of *Hypnum proliferum*; t': the alder layer. (From Steenstrup 1842: fig. V).

not yet been suggested (Bolles 1999, Frängsmyr 1976). Steenstrup did not therefore understand that the deposits underlying his peatbogs (c and d in fig. 9) were glacial moraine; he used the descriptive term *rullestensler*, later translated into English as 'boulder clay'. The peatbogs clearly post-dated these deposits, and thus belonged to the current geological epoch.

Geological explanations in the 1830s and 1840s were in a process of change. The theories of earlier decades had largely followed those of the Frenchman Georges Cuvier, who envisaged periods of geological stasis punctuated by major catastrophic events (Rudwick 1997). In Britain, William Buckland, a follower of Cuvier, called the boulder clay 'diluvium', the result of 'the last great convulsion which has affected our planet... a recent and transient inundation' compatible with the biblical narrative (Buckland 1823: 2). A few years later he considered that the event must have been more violent than the biblical narrative implied, though it was still 'the last of the many geological revolutions that have been produced by violent irruptions of water' (Buckland 1836: 95 n.). This 'catastrophism' was giving way to new theories of 'uniformitarianism' which argued that all geological change was the result of present-day processes operating over huge spans of time, without the need for catastrophes. Charles Lyell's *Principles of Geology*, the first volume of which was published in 1830, was a major landmark in this regard (Lyell 1830). A few years later Lyell considered the boulder clay, for which he preferred the gentler term 'drift', believing it to be 'accumulated on ground permanently submerged, and not by one or many transient rushes of water over land previously emerged... [deriving

from] those parts of every sea, where drift ice, charged with mud, sand, and blocks, melts, and the earthy materials are allowed to fall tranquilly to the bottom' (Lyell 1840: 176).

An analogous explanatory change was taking place in Denmark with regard to the formation of the peatbogs of the most recent geological past. The dominant figure in Danish geology at this time was Georg Forchhammer, who did not accept the hypothesis that melting drift ice had deposited the boulder clay. Had this been the case, he argued, the heavy boulders should be at the base of the sequence because they would sink more rapidly, and the sand higher up; but such was not the case. Furthermore, since southern Sweden was also covered in boulder clay, this would have to be an area where the icebergs were melting rather than forming; how, therefore, could this account for the many rocks of southern Swedish origin that were found in the Danish boulder clay? (Forchhammer 1835: 100–2). Forchhammer concluded that the boulder clay resulted from a 'catastrophe that disturbed the Scandinavian mountains and brought fragments of them down into the surrounding seas,' followed by geological uplift of the layers (1835: 103*).

Catastrophes, argued Forchhammer, also hit Denmark after the initial occupation of the country by people. He maintained that two catastrophic flood waves had hit Denmark *after* the time of the boulder clay, within the current geological epoch. The first or 'Baltic' flood came from the northeast, the second or 'Cimbrian' from the southwest (Garboe 1961: 99–100). The Baltic flood had cut the sounds that now separate the Danish islands and gouged out the bays of eastern Jutland, after which it had flooded over the centre of the Jutland peninsula, depositing the superficial deposits that cover its western regions (Forchhammer 1835: 106). The Cimbrian flood was the result of the sea breaking through the English Channel, which occurred in about the fourth century BC, because it was referred to by some classical writers. Forchhammer adopted the new archaeological chronology to date it: the tidal wave had impacted on a number of barrows which contained cremations in funerary urns, so 'the barrow was constructed in the so-called bronze age' (Forchhammer 1844 [1869: 154]*).

But after his minute examinations of many bogs, Steenstrup could see no traces of such catastrophes. He recorded the orientation of every fallen tree in each bog, and demonstrated that they usually lay

with their crowns pointing towards the middle of the bog, the direction in which they would naturally lie if they had aged, died, and fallen one by one (J. J. S. Steenstrup 1842: 94–7). Had a massive tidal wave overwhelmed them, they would all have been swept down in one direction and lain parallel. As a result:

I must declare my utter conviction that there was no violent catastrophic destruction of any of these forests, and must ascribe their disappearance to the normal peaceful course of nature, in which one complex develops and then declines, as if it gradually exhausts the favourable conditions for its own growth, thereby creating new conditions, and so calls forth the appearance and development of a new complex and hastens its own demise. (J. J. S. Steenstrup 1842: 107*)

Steenstrup's uniformitarianism had a hugely important chronological outcome:

our current [*beech*] forests, even if we put the lowest possible limit on it, have existed for 2–3 millennia . . . , and if we furthermore bear in mind that the transition from one type to another cannot be abrupt, but that each appears and grows to predominance only gradually . . . , each forest type did not consist of just one generation but several successive ones, each requiring centuries to reach maturity . . . ; then we cannot propose less than 1 or 2 millennia for each forest type. (J. J. S. Steenstrup 1842: 113*)

This required at least five or six millennia, and perhaps twice that, for Steenstrup's entire forest succession to run its course. The emerging prehistoric chronologies of neither Thomsen nor Nilsson involved timescales of anything like this magnitude. This is most probably what led Steenstrup to retreat to the inevitable conclusion that 'we must place these early forest coverings so many millennia back in time that the human occupation of Denmark cannot possibly go so far back' (1842: 112–13*).

This was however contradicted by an unexpected observation Steenstrup himself made with regard to human presence deep in the bogs. In 1837 he had reported categorically that he had found various artefacts in the oak layer (J. J. S. Steenstrup 1837: 19). However, in his next publications (1839, 1842) he was markedly more cautious, probably because he had by then realized that the time depth this implied for human presence in Denmark went far earlier than even the dates suggested by Thomsen and Nilsson. This

may be why he became more cautious, although in 1842 he depicted one artefact from the oak layer (J. J. S. Steenstrup 1842: 45). Steenstrup was well aware that artefacts could occur in the 'wrong' layers of a bog, and he was scrupulous to reject finds he considered potentially dubious.

However, within a few years Steenstrup had convinced himself that people *had* indeed been present as far back as the pine period: on 17 November 1848 he read a paper to the Royal Danish Academy stating that some of the bog pines showed clear signs of having been felled and burnt by people (J. J. S. Steenstrup 1848 [1851: 25]). Some time after this he found artefacts that he believed were genuinely coeval with the pines. He did not apparently publish this observation, however, so we cannot discuss it further; it was reported in English by Morlot (1861: 309), who had visited Denmark and discussed the matter with Steenstrup. But in the very small academic world of Copenhagen and Lund it cannot have gone unknown.

Steenstrup thus stumbled into an 'idea of prehistory' which he at first accepted, until he grasped that it was far deeper even than that envisaged by Thomsen or Nilsson. He therefore retreated from it until forced into acceptance by his own observations and growing self-belief. He knitted together the recent geological past and the earlier human past, thus drawing human history ever deeper into the past. But since this was the main aspect of the overall chronology that was *not* exported into English at the time, British archaeologists had no geological reason to expand their frames of reference. This was, as we shall see, particularly important with regard to the London archaeologists.

THE CRANIOLOGICAL CHRONOLOGY: ESCHRICHT, RETZIUS, AND NILSSON

Craniology emerged in both Copenhagen and Lund in the later 1830s. Skulls were turning up quite frequently during excavations, but all the authors described below lamented the fact that they were rarely retained. When they were kept they could, however, often be referred to one of Thomsen's newly published periods by virtue

of the associated grave goods, and sometimes by grave form. Skulls were thus naturally much more likely to be integrated into the Three Age System than into any form of ancient history.

The first to publish was the Danish physiologist Daniel Eschricht (1837). His article is little known even in Scandinavia; in view of its seminal importance—and of the confusion it caused in England—a full translation of it is presented in Appendix 3. Eschricht started with three skulls from a megalithic grave on the island of Møn (excavated by the same Mr Hage who had provided Thomsen with the stratified find described in chapter 1). To these Eschricht added three more from Maglehøj in Jutland, and up to twenty from another site on Møn. He adopted Thomsen's artefactual sequence, and believed all his skulls to be of Stone Age date. His craniological conclusion was that the facial regions of the skulls were small, while the braincase was high, spacious, and rounded (fig. 3.3 top). They did not match the skulls of either Mongols or Ethiopians, the two main racial groups he accepted other than Europeans, and he ruled out the Ethiopians on geographical grounds as well. His conclusion was 'that *these heads belonged to individuals of a noble tribe of the Caucasian race*' (Eschricht 1837: 112*, original emphasis). From the newly defined Bronze Age he had only one skull (fig. 3.3 bottom). It was very different, being elongated and low, with a low brow and long occiput; future finds would have to determine whether this was typical of the Bronze Age population or not (op. cit.: 116).

Like Nilsson, Eschricht was not an archaeologist, and perhaps for the same reason was unabashed by the notion of a time beyond the reach of history. He stated of the burial mounds that 'they are older not only than the history of our country, but furthermore are older than all the legends and myths that have been preserved until our day. It is therefore evident that they are the remains of people who occupied Denmark before the Danes' (Eschricht 1837: 109*). It was in this context that he made use of the adjective 'forhistorisk', or 'prehistoric', arguing that a concerted effort should be made to elucidate this period: 'if everyone would do their bit, knowledge of the country's circumstances in that *prehistoric* time may yet reach a much higher level of certainty than would seem possible at first glance' (Eschricht 1837: 109–10*, added emphasis). Since Eschricht

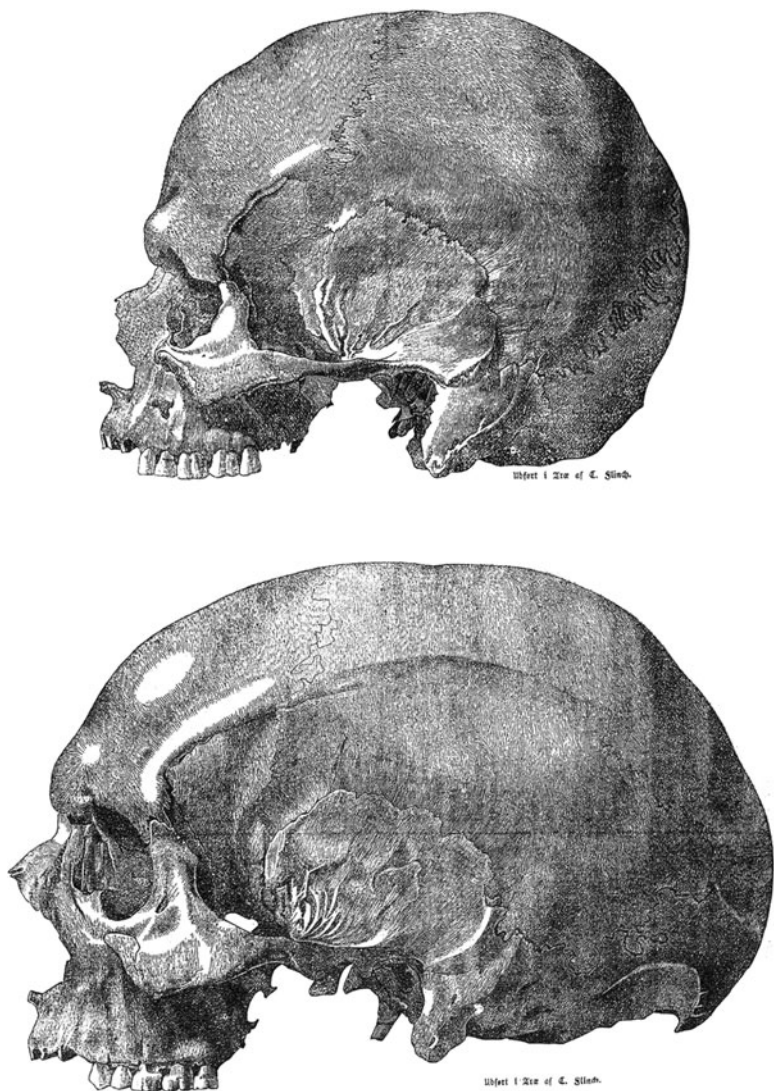


Fig. 3.3. Eschricht's drawings of the two successive skull shapes he identified. Top: spherical-headed Stone Age skull. (From Eschricht 1837: 111). Bottom: long-headed Bronze Age skull. (From Eschricht 1837: 115). © The British Library. All Rights reserved.

was undoubtedly envisaging a period beyond the reach of any written history whatsoever, to him must go the accolade of being the first person to use the word 'prehistoric' in its modern sense.

Sven Nilsson established a collaboration with the anatomist Anders Retzius, who worked almost entirely on modern skulls but whose methodology informed the work of Nilsson and many later craniometrists. Retzius divided skulls into dolichocephalic (or long), and brachycephalic (or round); jaws he classed as prognathous (projecting), and orthognathous (non-projecting). This produced a four-fold typology of peoples (Retzius 1843), as shown in fig. 3.4. His



Fig. 3.4. Retzius' view of skulls, jaws, and races. (Translated from Retzius 1843: 4).

sampling methodology appears rather piratical to modern sensibilities—from an available sample of between 200 and 300 modern Swedish crania he selected the *five* he considered most typical, and measured only them to provide his national benchmark (Retzius 1843: 5), thus entirely ignoring variability.

Nilsson built upon this in his own work. He had read Eschricht's paper and was rather patronizingly dismissive of it, stating that while he disagreed with its conclusions, it 'was a contribution in its day' (Nilsson 1838–43: vi*)—all of a year earlier! The sequence of cranial types that Nilsson put forward was, however, the same as Eschricht's; it was his interpretation that differed. Retzius' description of modern Swedish skulls enabled Nilsson to conclude that the rounded Stone Age skulls were *not* ancestral to the modern Scandinavians. He compared Eschricht's depiction of a round skull with one other Stone Age skull, which he had been given by Thomsen. This was one of a group of twenty from Møn (Nilsson 1838–43: 2, 4 n.), and was therefore one of the ones that Eschricht had also examined, though Nilsson was apparently unaware of this. He did not mention his total Stone Age sample size, beyond saying that he had examined 'a considerable number' (Nilsson 1838: 6*). On the basis of a comparison with the skulls of two Lapps—one which had belonged to a man called Johan Andersson Juff, the other to a woman from Lycksele—he concluded that his Stone Age skulls had belonged to Lapps (Nilsson 1838: 9–10). The Bronze Age was more difficult; he had ruled Celts out of his Stone Age discussion because they were a difficult group to define, and the term 'Celt' had apparently been applied to various peoples (Nilsson 1838: 6–8). But then he ruled them back in again during his Bronze Age discussion, defining them as being represented by contemporary Scottish Highlanders.

The case for the Bronze Age skulls being Celtic emerged from the similarities between just three skulls: a published drawing of a Highlander's skull; a skull in St Thomas's Hospital in London labelled 'Celtic', seen in the original by Retzius and as a cast by Nilsson; and the Bronze Age skull illustrated by Eschricht. On this basis Nilsson suggested that the Bronze Age inhabitants of Scandinavia were Celts (1838: 8).

The sample sizes considered above will strike modern readers as far too small to be the basis for any conclusions. However, the eagerness with which Eschricht and Nilsson sought to plug races

THOMSEN 1836	NILSSON 1838-43	STEENSTRUP 1842	CRANIOLOGY
Historical era	modern commerce	beech forest	elongated oval (modern Swedes)
Iron Age	farmers pastoralists	alder forest	
Bronze Age		oak forest	dolichocephalic (Celts)
Stone Age	hunter-gatherers	pine forest	brachycephalic (Lapps)
		aspen forest	

Fig. 3.5. Diagrammatic view of the integrated chronological structure of the Three Age System as it existed by the late 1840s. In the craniology column, both Nilsson and Eschricht agreed on the sequence of skull types, but the racial attributes are Nilsson's alone.

and nationalities into the Three Age System shows how firmly they accepted the veracity of Thomsen's scheme. With economy, ecology, and crania all at least tentatively grafted onto the artefactual scheme, the whole chronology was as depicted in fig. 3.5. This was a remarkable structure to have achieved and published in less than a decade, and the whole construction ranks as one of the key defining periods in the origins of the modern discipline of archaeology.

Once this structure was in place, the Three Age System was able to become an aggressive predator. In the person of J. J. A. Worsaae, it went over to the attack against the ancient historical chronology.

J. J. A. WORSAAE'S ASSAULT ON ANCIENT HISTORY

J. J. A. Worsaae (1821–1885) was just too young to be part of the events described in the previous sections. His first paper was published in 1839, when he was just 18 years old, describing his excavation of two megalithic graves. He was aware that they should belong to the oldest period, so he was troubled by finds of iron objects in both of them alongside the expected stone tools; in at least one case

he could not see how the iron tools could have been later intrusions. His recommendation was that future excavations of such tombs should look very carefully at the location of any iron objects (Worsaae 1839: 176). His first book (Worsaae 1843a) was, to be sure, the first in which the chapters were organized explicitly along Three Age System lines, but he was not the creator of any of the major chronological components that had come together by that year. A few years later he was to claim that in his 1843 book 'I sought to place the ancient monuments of Denmark into a scientific system for the first time' (Worsaae 1847a: 381*). However, as we have seen, Thomsen, Nilsson, Eschricht, and indeed Molbech were all aware of the datings of the major grave types some years earlier, so this claim appears presumptuous.

This is not to play down the importance of Worsaae or his book, however. His travels to Britain will be examined in subsequent chapters; his importance in Denmark in the 1840s was to attack the ancient historians' sometimes naive use of archaeological sites or finds in support of their historically based arguments (Ødegaard 1994; C. S. Petersen 1938). Five cases will be examined here. In the first three (the burial mounds at Leirskov Mark, the body claimed to be Queen Gunhild, and the Lejre dolmen) he detached archaeological remains from their previously ascribed places in the ancient historical scheme, and was able to demonstrate that they dated far further back than previously envisaged, and to eras far beyond the reach of any historical records. In so doing, he was a strenuous reinforcer of the 'idea of prehistory'—although nowhere in his 1843 book did he actually use the word 'forhistorisk'; his earliest use of the term may be as late as 1846, when, however, he used it nine times in two publications (Worsaae 1846a: 45, 54 [twice], 73, 74, 81; 1846b: 122, 140, 141). In the fourth instance, the Runamo affair, he launched an attack on a primary historical document, demonstrating that a major claimed 'runic inscription' was merely a series of natural fissures. Finally, his efforts to establish and protect a Danish national identity by archaeological means (analysed in detail by Briggs 2005) involved the presentation of archaeological arguments to counter ancient historical ones, as we shall see in the next section.

Worsaae's first assault on ancient history was published in 1841. His target was the leading historian N. M. Petersen, who as we saw in

chapter 2 had written the chapter on ancient Scandinavian literature which had preceded Thomsen's consideration of artefacts in the *Ledetraad* volume of 1836. Petersen had subsequently published a series of papers about the attacks on Denmark by the Vends (inhabitants of the southern Baltic shore) during the eleventh century AD. It was recorded that after invading Jutland, the Vendish army was catastrophically defeated by the Danes under King Magnus the Good at the Battle of Lyrskov Heath in 1043. Petersen identified Lyrskov Heath with modern Leirskov Mark, citing the many funerary mounds there as the burials of the slain Vends (N. M. Petersen 1839: 15). In 1841 Worsaae excavated eight of the mounds specifically to test this; all contained bronze objects and cremated bodies, and thus definitely dated from the Bronze Age. Worsaae believed that iron was introduced into Denmark much later than Thomsen had argued (see above), because very few iron objects had turned up in pagan burial mounds; iron therefore only arrived about the time Denmark became Christian (Worsaae 1841: 158–61), around AD 800. He was to persist in this belief until associations of imported Roman objects with native iron age products convinced him that Thomsen's chronology was correct (Worsaae 1849a). But at all events the Bronze Age dated well before the eleventh century, so Worsaae concluded that '*the mounds cannot possibly be regarded as resulting from the battle between Magnus the Good and the Vends or from the middle of the eleventh century*' (1849a: 161*, original emphasis). Earlier in the same paper he had reviewed other cases of burial mounds linked to claimed historical personages, concluding that in every case the mound was much older than the legend later associated with it (1849a: 149–52).

This was not calculated to endear Worsaae to the traditional historians, but there was a lot more to come. N. M. Petersen was also Worsaae's target over the Queen Gunhild affair. This began when a body, preserved by waterlogging, was found in a bog in Jutland in 1835. The forensic report established that it was that of a woman aged about 50 who had died violently, having been pegged down in the bog while still alive (Christens 1837). Archaeological examination revealed that she had been wearing a leather cape over woven clothing that microscopic examination identified as wool. The report of the examining committee was probably written by Thomsen

(C. S. Petersen 1938: 87–8), and cautiously concluded from the evidence of the clothing that the body probably dated to ‘the last period of heathendom, that is, to about the period of Queen Gunhild’ (Oldsag-Committeen 1837: 173*). The mention of Gunhild was because earlier in the same journal issue, N. M. Petersen (1837) had identified the body as that of the Norwegian Queen Gunhild, the widow of King Erik Bloodaxe. According to the historical sources, after a life of violence and betrayal impressive even by Viking standards, Gunhild was tempted to Denmark by an offer of marriage to King Harald Bluetooth; this was, however, a trap, and on her arrival she was seized and drowned in a bog. Petersen argued that the name of the bog in which the corpse was found, Gutslose, derived from Gunhilds Mose (‘Gunhild’s Bog’), while the name of the locality, Haraldskjær, conformed with the legend that Gunhild was killed near King Harald’s hall—and the ancient royal seat of Jelling was close by (N. M. Petersen 1837: 102–3). Most of Petersen’s paper dealt with the historical references, only the last two pages considering the body itself; the circumstances were in favour of its identification as Gunhild, and ‘it was wearing a valuable garment which does not belong to our era; . . . in short, all the circumstances show that it could be that of the Norwegian Queen’ (1837: 103*). The identification was rapidly accepted by most historians (Ødegaard 1994: 8; C. S. Petersen 1938: 88). The only dissenting voice was that of Molbech, who as we have seen above was sceptical about the historical value of the early sources. He questioned the reliability of the historical sources dealing with Gunhild—though only in a footnote (Molbech 1837: 289 n.). But Molbech had another asset at his disposal: he was editor of the journal *Historisk Tidsskrift*, and in its pages offered Worsaae the space for a major rebuttal.

Worsaae (1842: 252–3) was dismayed by the uncritical acceptance of Petersen’s arguments and attacked from two directions. First, he went through the ancient sources in detail to demonstrate how shaky they actually were. Gunhild would have been about 70 when she died, and Worsaae doubted that a woman of this age would be fooled by an offer of marriage (1842: 262). He also argued that the whole story of her death in Denmark was probably a late interpolation, because the two most reliable ancient sources stated that Gunhild had fled to the Orkney Islands and apparently died there of natural causes (Worsaae

1842: 269). Second, he examined the circumstances surrounding the body itself. The locations of King Harald's royal halls were impossible to establish, and there was no reason to suppose he had one either at Jelling or at Lejre (Worsaae 1842: 273–5); no historical connection had been drawn between Harald Bluetooth and Haraldskjær until the seventeenth century (1842: 280); the drowning of criminals in bogs was attested by Tacitus a millennium before Gunhild's time, and numerous similar corpses had been found (1842: 282–3); and the woollen garment that clothed the body was of low value, not the kind a queen would wear (1842: 287–8). His conclusion pulled no punches:

for the time being I believe I can reasonably state that the acceptance of the excavated body as Gunhild's is a delightful poetic hypothesis, which as such deserves recognition, but which, when the question is whether *it can be used to confirm or support an unreliable historical account*, is completely without value. (Worsaae 1842: 292*, original emphasis)

In the face of this systematic destruction of his case, Petersen was forced into a rather blustering retreat. Part of his reply (N. M. Petersen 1843) was measured, and he admitted specific points, for example that he had not personally examined the garment (1843: 274). But it was clear that he deeply resented being questioned by the younger man. In his discussion of whether there had been a royal hall at Lejre, Worsaae had stated in a couple of footnotes that Münter's (1806) book on Lejre was 'to a great extent uncritical' (Worsaae 1842: 276 n. 33*), and that he would in due course show that the antiquities at Lejre were many centuries older than the immigrant Gothic tribes (1842: 276, n. 34). This seems particularly to have annoyed Petersen, who responded in an outspoken and revealing passage:

[Worsaae] promises to show that the stone arrangements found there [Lejre] seem to be many centuries older, and do not even belong to the latest Gothic tribe to immigrate into this country. Of these proofs I can of course say nothing before I see them; his comments on this, even if they do not do what they claim, will I need hardly say be very welcome. But from what I have so far seen of similar investigations, I would like to state plainly that I do not place much reliance on them; and I absolutely do not understand where this criticism is leading, based as it is on destroying the results of the historical sources, in order to erect instead something that goes back before all history.

Our time has a great and overwhelming tendency to destroy what is already there, in order to replace it with its own postulations . . . If anything can be found out about the situation before the beginning of history, this must necessarily be brought into connection with history, and not used to oppose its results . . . Because what Münter and others said about Lejre was uncritical, as I willingly grant him, does it follow that the historical conclusions about Lejre's age as a royal seat are also unreliable? If an unprejudiced consideration concludes that these monuments must be regarded as older than the arrival of the Goths, does it follow that the place under the rule of the Goths would not continue to be one of the tribe's most important places? (N. M. Petersen 1843: 269–70*)

But overall he defended the identification of the body as Gunhild's, appealing in his closing paragraphs to the 'spirit of history' (1843: 325*). Worsaae's *coup de grâce* appeared later the same year, again courtesy of Molbech's periodical. After stating that it was inevitable that the earliest attempts at dealing with material remains from the ancient past would involve imagination, he sought to establish the *archaeological* context of the bog corpse. He did this by citing no fewer than ten further reports of such bodies from Denmark and elsewhere, some of which were pegged down in the bogs, and some of which were accompanied by similar clothing. The 'Gunhild' corpse was thus a typical example of a wider archaeological spectrum of finds, and likely (following Tacitus) to be criminals not royalty, and much older than the supposed date of Gunhild. He finished by once again questioning the veracity of the historical accounts (Worsaae 1843b); and this time Petersen had no answer.

Worsaae's third line of attack was his promised consideration of Lejre. As we saw in chapter 2, Frederik Münter had identified a large grave there as that of King Harald Hildetand, said to have been killed in the early eighth century AD by King Sigurd Ring at the Battle of Braavalle (see fig. 2.3). The grave had been partially destroyed by Münter's time (Münter 1806: 35 n.), but earlier descriptions enabled him to present a reconstruction of it (fig. 3.6 top). Worsaae however recognized it for what it was:

it will however be immediately apparent from this that the grave is a perfectly ordinary dolmen from the Stone Age, particularly since the earth dug out of the chamber contained wedges of flint, so it cannot possibly have

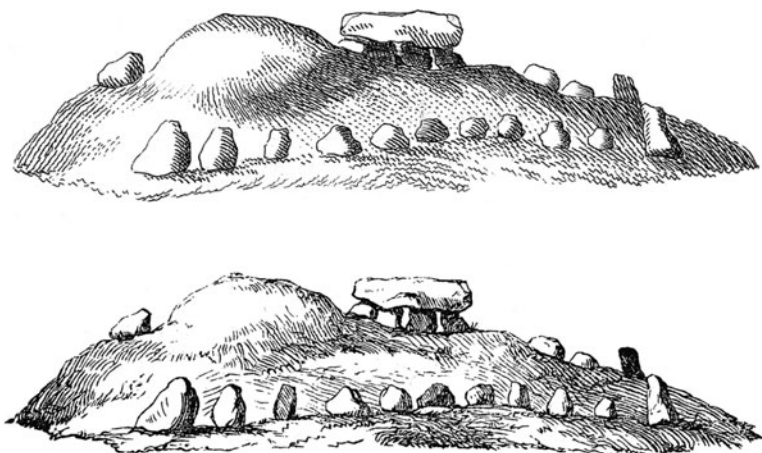


Fig. 3.6. Top: Münter's illustration of the grave at Lejre he believed to belong to King Harald Hildetand. (From Münter 1806: fig. 1) Bottom: Worsaae's drawing of the same grave, modified to make clearer its characteristics as a dolmen. (From Worsaae 1843a: 90).

been constructed for Harald Hildetand, who, according to the old stories, must have lived at a much later time. (Worsaae 1843a: 90*)

Worsaae also presented an illustration of it, at first site identical to Münter's, on which it must have been based. However, close scrutiny reveals that it was not printed from Münter's original block, but was redrawn (fig. 3.6 bottom). In Münter's drawing the capstone rested on a rather undifferentiated dark mass, but in Worsaae's this had been resolved into four clear supporting stones, thus enhancing the identification of the reconstructed grave as a typical dolmen—an interesting but understandable piece of artistic licence!

The Runamo affair was to dwarf all these controversies. The debate revolved around a horizontal exposure of trap (a variety of basalt) in the Swedish province of Blekinge, on which was a supposed runic inscription concerning the Battle of Braavalle. It had been a problem for centuries. Ole Worm, the renowned seventeenth-century Danish medical man, collector, and archaeologist, had been able to decipher virtually nothing of it. In the eighteenth century many dismissed it as a series of entirely natural cracks (Nyerup 1806: 90–5). When

Thomsen visited the site he was likewise unable to decipher anything (C. S. Petersen 1938: 91). In 1832 the Royal Danish Academy resolved to settle the issue once and for all, by appointing a committee to examine it. We have already met all its three members. Two were historians, the traditionalist Finn Magnusen and the more sceptical Christian Molbech; the third was the geologist Georg Forchhammer. They examined the site in July 1833; Forchhammer concluded that some of the lines had indeed been carved by human hand, so it had therefore to be a genuine inscription. The historians were not immediately able to decipher it, however; it was only when viewing the publication proofs of the drawings on 22 May 1834 that Finn Magnusen in a sudden flash of inspiration realized that it made sense if he read it *from right to left*—the opposite of the conventional direction. He announced his conclusion in the (delayed) 1833 issue of *Nordisk Tidsskrift for Oldkyndighed* (Magnusen 1833), but the full publication of the inscription complete with drawings did not appear until several years later.

After initial acceptance by the scholarly community, the first sceptical note came in 1836, when the Swedish chemist Baron Berzelius visited Copenhagen and was given a copy of the drawing. He then visited Runamo on his way back to Sweden, and in September 1836 announced to the Royal Academy in Stockholm that in his opinion the ‘inscription’ was in fact an entirely natural phenomenon. When his paper was published (Berzelius 1838), Sven Nilsson, who had previously accepted the claim, anxiously wrote to Forchhammer to ask for confirmation that the inscription was indeed real, and stated that he intended to visit the site himself. Forchhammer replied that the inscription was authentic; he described how he had determined which lines had been cut by human hand, and marked them in chalk for the draughtsman to draw, pointing out that as he himself could not read runes, he could in no way have been prejudiced about his determinations (letters published in Clément 1922: 16–20). Nilsson did not get to the site until 1840—but when he did he was immediately convinced that the ‘runes’ were natural cracks. He cut across some of them with a chisel, and found that below them all there were finer cracks extending deeper into the rock; the ‘runes’ were just the widening by erosion of these cracks near to the rock surface. He immediately wrote to Finn Magnusen

giving his opinion, but in his reply Magnusen reiterated his belief in the 'inscription' (C. S. Petersen 1938: 101–2). Nilsson's paper was published the next year (Nilsson 1841), the same year that Magnusen's final publication of the 'runes' appeared as a lengthy book (Magnusen 1841).

Despite the scepticism of the two Swedes, the reception of Magnusen's book in Denmark was generally positive (C. S. Petersen 1938: 103–4), although there are indications that the ever-cautious Thomsen, one of the few who had actually seen the 'inscription', was harbouring doubts as early as 1836 (Rowley-Conwy 2004). This remained the situation in 1842, when Worsaae made his first visit to Runamo. He recorded that he did not even take Magnusen's book with him, so confident was he that he would find a genuine inscription (Worsaae 1844: 21)—but, like Nilsson two years earlier, Worsaae immediately concluded that the 'inscription' was entirely composed of natural fissures. He returned in 1844 for a longer examination, bringing with him a draughtsman: 'I concluded at first glance what my later comparisons and investigations have fully confirmed, that *the representation made partly under Forchhammer's direction of the Runamo trap section are completely unreliable*' (Worsaae 1844: 21*, original emphasis). He was critical of Forchhammer's method of recording, because he found it quite difficult to see the cracks even after cleaning them up. 'And when I finally found them, how different they were from the representations!' (ibid.*). There were many intersecting lines of various depths: they all gradually tapered away at each end, none of them ending abruptly as the drawings made under Forchhammer's direction had shown them to do. Forchhammer's marking of his chosen lines with chalk had caused his draughtsman to depict the chalk lines, not the actual marks in the rock (1844: 21–2). And since the drawing of the 'inscription' was wrong: 'I could naturally neither harbour the slightest doubt that *Finn Magnusen's entire decipherment and interpretation of the inscription, which was based on the drawing, must also be completely fallacious*' (Worsaae 1844: 25*, original emphasis). He completed his demolition by pointing out that the first written mentions of the 'inscription' dated to some four hundred to five hundred years after the date of the supposed battle; the carving of an inscription on a horizontal rather than a vertical rock surface was highly unusual; and in any case

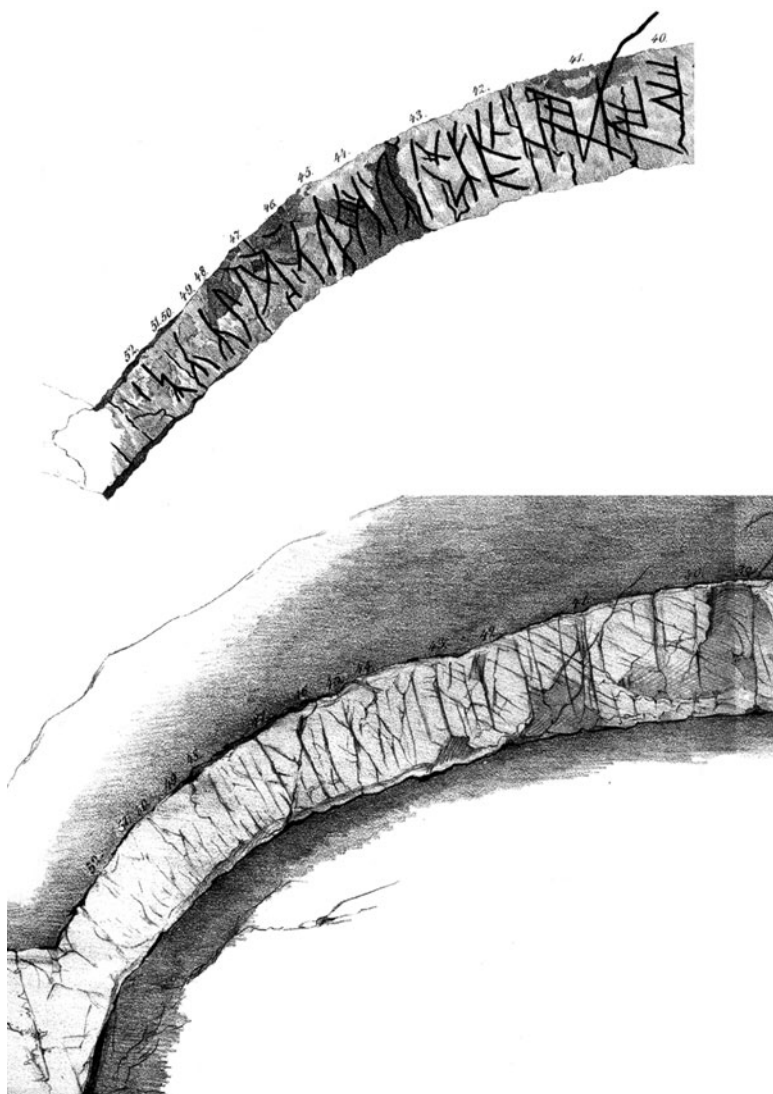


Fig. 3.7. Top: the drawing of the Runamo 'runic inscription' produced in 1833 under the direction of Georg Forchhammer, who marked the lines he believed were of human origin with chalk. (From Worsaae 1844: fig. I, where it is reproduced from Magnusen 1841: fig. II). Bottom: the drawing of the same section of trap produced by Worsaae's draughtsman. (From Worsaae 1844: fig. II).

the site was not on any sensible route that an army might take to or from the Braavalle battlefield.

A comparison between the drawing made under Forchhammer's direction, and that made by Worsaae's draughtsman, is given in fig. 3.7. The differences between them are evident. On his return to Copenhagen in August 1844 Worsaae took his drawings to King Christian VIII, who compared them closely with Forchhammer's version. Worsaae wrote later:

After a while the King said 'I both thank and congratulate you. From now on, nobody will be able to make me believe that there is even a single runic letter at Runamo. I would never previously have thought it credible that it was so clear a product of nature.' At this he began to laugh so strongly that in the end he clutched his stomach, repeating '*oh these academics*' and '*so much for the thick book about Runamo: this is definitely an unmatched story*'. (Worsaae 1934: 123–4*, original emphases)

Since the King was in such a good mood, the ever-opportunistic Worsaae took the chance to hit him for money to publish the results of his wider researches in Blekinge; and it was the publication of this book two years later (Worsaae 1846a) that led to the interview with the King described in chapter 1—from which arose Worsaae's trip to Britain and Ireland. Worsaae's shorter book specifically on Runamo appeared remarkably rapidly, before the end of 1844 (Worsaae 1844). It led to immediate retreat and recriminations among the previous proponents of the 'inscription'. In a meeting of the Royal Danish Academy on 29 November 1844, Forchhammer stated that all he had done was try to determine which marks were artificial; it was Magnussen who had decided they were runes. He accused Worsaae of unfairness, and suggested that Nilsson's chisel might have made things more confused; and he refused to accept any responsibility even though it was his own drawings that were responsible for the whole fiasco (Forchhammer 1845). Magnussen himself crumbled completely, saying that he had worked from the drawings and might have been too eager to accept the cracks as runes (Magnussen 1845). At the next meeting on 13 December Molbech distanced himself from the original conclusions, pointing out that nowhere in his contribution to the publication did he actually state in so many words that he believed in the runes or their decipherment (Molbech

BRAAVALLASLAGET.

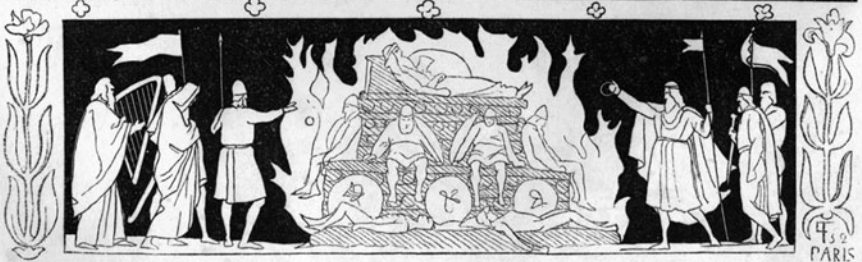


Fig. 3.8. The Battle of Braavalle reverts to legend. (From Fabricius 1854: I, 70).

1845). Worsaae, not yet a member of the Academy, was not present; his short final reply, published as a pamphlet, reiterated his conclusion that the marks were natural and that Forchhammer's drawing was incorrect (Worsaae 1845).

This series of blows doomed the claims of ancient history that it could study the earliest periods, and destroyed its chronology. The Battle of Braavalle reverted rapidly to legend. In his two-volume illustrated popular history of Denmark published in 1854, Fabricius devoted thirty pages to the Stone, Bronze, and Iron Ages, depicting numerous artefacts. He then devoted over one hundred pages to saga histories—but making no claims for their historical value, giving no dates, and not even placing them in any quasi-historical sequence; they appeared just as romantic legends, as his depiction of the Battle of Braavalle itself makes clear (fig. 3.8).

WORSAAE, THE 1848 WAR, AND GRIMM'S FAIRY TALE

Worsaae's demolition of ancient history had served to place archaeology in the front line, as the branch of learning that now spoke for the ancient past. Worsaae in fact soon had to defend its new-won territory in an unexpected context: the international crisis of the later 1840s, and the war with the Schleswig rebels and Prussia that broke out in 1848. Worsaae's nationalism has recently been examined by Briggs (2005); what is interesting in the current context is that the intellectual debate was one that pitted ancient history against archaeology.

In 1817 E. C. Kruse had published a paper arguing that the original language of the Jutland peninsula was not Danish, but German. In the absence at that time of nationalist feeling about such issues, the paper aroused no controversy; but when nationalism grew in the 1840s the issue was to return and occupy centre stage (Adriansen 1996: 122–3). In the later 1840s there were as many as three relevant nationalist agendas: the Norwegian, the Danish, and the German. Norway had been united with the kingdom of Denmark since the Middle Ages, but had been awarded to Sweden by the victorious Allies at the end of the Napoleonic Wars. Although Norway was not

to become an independent state until the beginning of the twentieth century, nationalist feelings were stirring. The Norwegian historian and philologist Peter Andreas Munch published a series of papers in 1846 and 1847, in which he argued that Norwegian was the original Scandinavian language. Following Suhm's outline, he accepted that the Germanic tribes had split when they arrived southeast of the Baltic; the Germans had moved to the West along the southern Baltic shore, while those who became the Scandinavians moved to the North and across the Baltic into Scandinavia—'all this can be regarded as for the most part definitely established' (Munch 1846a: 23*). What caused alarm in Denmark was that Munch renewed Kruse's claim that the German stream, which Munch termed the Gothic, *had occupied Denmark as well*, were its first inhabitants, and had emigrated only in the early centuries AD. Thus the modern Danes were a subsequent immigration from Norway (Munch 1846b, 1847).

The Danes were alarmed because the German-speaking element in Schleswig had been growing in numbers, and after several years of rising tension now threatened secession and closer alignment with the German states. The nationalist stakes were massively raised in 1848, because in April the Danish army crossed into the Duchy to suppress the pro-German rebels. Prussia supported the rebels for a time and invaded Jutland, before her withdrawal from the war allowed the Danes to win the decisive Battle of Isted on 25 July 1850. Both sides appealed to the distant past to support their positions. Worsaae published a pamphlet on the *Danevirke* (Worsaae 1848), a long Viking-age defensive line along the southern border of Schleswig, arguing that this marked the ancient southern border of what he termed *Danskheden*—'Danishness'. Not averse to using historical legends when they suited his nationalistic purpose, Worsaae recounted the old story of how the tenth-century Queen Thyra encouraged her son Harald Bluetooth to build the fortification to keep out an earlier German invasion, and how people from all over Denmark turned out to do the work.

From the German side in 1848 came a book entitled *Geschichte der deutschen Sprache*, 'History of the German Language', by Jacob Grimm. The author was one of the brothers who published the numerous fairy tales that are today so associated with their name.

It is less well appreciated that these fairy tales were part of a nationalist agenda aimed at reviving the ancient German past, both literary and historical. Jacob Grimm used his philological studies to make historical statements and claims, and during the war he devoted his efforts to the German cause. He followed Munch's argument that parts of southern Scandinavia had formerly been German speaking. His particular interest was Jutland: he argued that some aspects of the Jutland dialect of Danish reflected its supposed German-speaking past. He supported this by reference to Bede, the English eighth-century historian, who had stated that the Jutes (who occupied England along with the Angles and the Saxons) had come from Jutland and had occupied southeast England. Since the language spoken in southeast England today was derived from German, not Danish, it followed that the Jutes must have spoken German and not Danish. What made his book particularly confrontational was that he argued for a *Volksrecht* or 'national right' for a pan-German union to re-occupy and unite under itself *all the areas of Europe that had ever spoken German*; this included Alsace-Lorraine, Switzerland, Belgium, Holland—and of course Jutland; Denmark should actually be dismembered and cease to exist, the eastern islands properly being part of Sweden (Adriansen 1996; Ødegaard 1994; C. S. Petersen 1938).

One Danish historian has recently described Grimm's *Volksrecht* claims as 'hair-raising' (Ødegaard 1994: 18*), as well she might in view of the events of the century after 1848. Even without this, Worsaae found the whole idea outrageous, and he published a number of polemics in popular journals in 1848–50, which were also printed as separate pamphlets. He advanced various arguments to counter Grimm and Munch, of which the most important in this context were based on his use of archaeology to rebut their ancient historical claims. He pointed out that even if there were German speakers in Jutland in the early centuries AD (which he did not believe), this did not make Jutland in any sense 'originally' German. He used the longer timescale of archaeology to outflank Grimm:

Grimm indeed says with regard to Jutland that this area 'from the beginning and as far as history can reach has been occupied by purely Germanic tribes'. But that great academic, who must know the prehistoric period in Jutland far better than all the Scandinavian archaeologists put together, since he has

such definite information about the ancient German population of the peninsula, seems here to have had at the very least a lapse of memory. For it cannot be unknown to him that in Jutland, as in the other lands around the Baltic and North Seas, as well as in the countries on Europe's west coast, there are found numerous traces of a primitive original tribe, which did not know metals but used tools of stone and bone, and which consequently was at the very lowest level of culture. This tribe, which has not just left numerous tools scattered about the fields and in the lakes or bogs, but which also erected a great number of truly remarkable stone dolmens in which the corpses were buried in a unique and simple manner, cannot have belonged to the German tribe, which these days benefits so hugely from its Volksrecht at the expense of other peoples. (Worsaae 1850: 11–12*)

He pointed to the absurdity that would result from following Grimm's argument through to its logical conclusion, because people other than the Germans might also have a claim to Jutland:

the successors of the ancient Celts in Wales, Scotland, and Ireland have a still better claim on Jutland than the Germans, not to mention all the Finnish folk in Russia, who, many argue, occupied Scandinavia and thus the Jutland peninsula in the very earliest period, the so-called Stone Age. Grimm cannot use the argument that the extent to which the Finns and the Celts once occupied Jutland is very uncertain, because the Finnish and Celtic occupations are just as certain, or more correctly uncertain, as the German immigration. It would completely correspond to Grimm's claim of Jutland on behalf of the Germans, if the English on behalf of the Celts, or the Russians on behalf of the Finns, one fine day came along with an equally 'irrefutable' and even older 'Volksrecht' to the peninsula. (Worsaae 1850: 12–13*)

In a move calculated to enrage German nationalists, Worsaae even pointed out that such arguments might also work *against* Germany; Slavic peoples had occupied areas now so quintessentially German as Rügen, Pomerania, and Mecklenburg, right down to the twelfth and thirteenth centuries, though with tongue in cheek he doubted that Grimm would extend to the Slavs any kind of Volksrecht-type claim to these territories (Worsaae 1848: 11).

The extent to which Worsaae was using the longer time depth of archaeology to outflank the ancient historical chronologies of Munch and Grimm can be seen in the fact that he used the term *forhistorisk* or 'prehistoric' in excess of 40 times at various places in these pamphlets (1848: 10, 11; 1849b: 3 [twice], 4 [twice], 5, 6, 7, 19, 22,

26, 28, 29 [twice], 31 [four times] 32; 1849c: 3, 4, 5, 6 [twice], 7 [five times], 16, 17, 21, 22 [twice], 30; 1850: 11 [twice], 14 [twice], 15). One of these publications has two further claims to importance in this regard. Published in the 29 May–2 June 1849 issue of the periodical *Fædrelandet* ('The Fatherland') as well as in pamphlet form, it is entitled in translation 'On a prehistoric, so-called "German" population in Denmark. With reference to political movements of the present day' (Worsaae 1849b). This was apparently the first time that 'prehistoric' was used in the title to any work. Furthermore, while all previous usages had been adjectival, in two places in this paper Worsaae described archaeologists as *forhistorikerne*, 'the prehistorians' (1849: 7, 31). This was over twenty years before the noun *préhistorien* first appeared in French (Clermont and Smith 1990: 97).

The 1840s had thus seen the Three Age System go over onto the offensive, and oust ancient history as the discipline that dealt with the earliest times. This can be argued to mark the maturing of archaeology as an independent discipline, capable of debating with and defeating major opponents on the international stage. The coherence of Thomsen's scheme supported by the other chronologies it had attracted, supplemented by the aggressive way Worsaae had then used the construction, meant that in Scandinavia there was no going back.

The Disinterested Gentlemen: England to 1860

In 1852 Thomas Wright reviewed Europe's ancient past of Europe in his book *The Celt, the Roman, and the Saxon* (Wright 1852). Wright was an archaeologist who worked in a variety of fields. During his active life he did a great deal of work on medieval manuscripts, history, antiquities, folklore, arts, and sciences; he wrote full-length histories of Scotland, Ireland, and France; he excavated at the Roman town of Viriconium; and finally, he took an interest in the pre-Roman past.

Wright typified a generation of mid-nineteenth century archaeological scholars whose interest in pre-Roman matters amounted to no more than a minor sideline. There were arguably two main reasons why most of the London archaeologists paid little or no attention to the pre-Roman past. The first was that, as Englishmen themselves, they had no nationalist axe to grind by stressing the earliest archaeology of England. The ancient Celtic past had been firmly claimed by the Welsh, the Irish, and the Scots ever since the 'Celtic Revival' of the mid eighteenth century (Morse 2005: 41–7), while the English were post-Roman immigrants. The pre-Roman or Celtic past was therefore the past of other people—the ancestors of the Welsh or Irish, nationalities not generally held in high esteem by anyone but themselves. To emphasize the Celtic past was thus to exalt the inferior—and perhaps also, by emphasizing the relatively recent arrival of the English, to play into the hands of the nascent Celtic nationalisms. Such views were by no means articulated in the publications of Wright and his generation, and we can at this remove only guess how consciously motivating such concerns really were;

but it remains true that the pre-Roman past got little attention. In Wright's *The Celt, the Roman, and the Saxon*, the pre-Roman Celts were dealt with in just forty-four pages, or 9 per cent of the total book, the post-Roman Celts in a mere five pages, or 1 per cent.

The second reason for the Londoners' lack of concern with the pre-Roman past emerges from the very first sentences in Wright's book:

According to the system now generally adopted by ethnologists, Europe was peopled by several successive migrations . . . , all flowing from one point in the east. Of these two the principal were the Celts and the Germans, both branches of the same great race, which has been popularly termed the Japhetan, because, according to the scriptural account, the various peoples which belonged to it were all descended from Japhet. (Wright 1852: 1)

This chronological structure derived, as Wright indicated, not from archaeology but from ethnology. Wright and his colleagues were not concerned with the generation of chronologies, because as we shall see, ethnology provided a ready-made one. We saw in chapter 1 that the 1840s witnessed a boom in ethnological publication, as comparative philology and comparative anatomy united to form the science of ethnology. The sheer weight of this publishing endeavour meant that ethnology set the agenda, and predominated over the Three Age System, which was poorly disseminated abroad by the Scandinavians. The resultant reliance on ethnology meant the consigning of all things pre-Roman into one category: the 'Celtic', or, if one preferred, the 'British'. The time depth allocated to this was, as we shall see, pretty shallow. Thus with one brief pre-Roman phase in place, there was no scope for the further ramifications inherent in the Three Age System.

THE THREE AGE SYSTEM: LOST IN TRANSLATION

In examining the receptions of the Three Age System in Britain, our first task is to examine its accessibility to foreign readers. All the publications described in the last two chapters were, after all, in Danish or Swedish, languages which could be read by very few

Anglophone archaeologists. In fact remarkably little of the scheme made it into English in the early years; a considerable quantity of Scandinavian archaeological work *was* being translated into the major languages at this time, but this was in pursuit of a very different agenda.

The Royal Society of Northern Antiquaries (KNOS) was attracting a large and varied foreign membership under the direction of its energetic secretary C. C. Rafn. By the late 1840s the membership list was intercontinental and highly distinguished. Lists were published in the new journal *Antiquarisk Tidsskrift*, which appeared first in 1843–45. The 1849–51 list included no fewer than thirty members of royalty. These were not all just the heads of minor European principalities, but included the King of Prussia (who maintained his membership despite invading Denmark in 1849), the Czar of Russia, the Emperor of Brazil, and the Shah of Persia. The general membership extended over most of the world, and included the Presidents of Chile and Ecuador, the Bishops of Bogota, Guatemala, Kharkov, Tobolsk, and Calcutta, the chancellor of the Austro-Hungarian Empire, a Parsee man of letters from Bombay, the Governors of western Siberia, Newfoundland, and Bermuda, professors from Brazil and Argentina, the Hospodars of Moldavia and Wallachia, the former Prime Minister of Spain, the ambassadors of both Russia and the United States to Constantinople, and many Americans including former president Martin Van Buren. With a readership like this, publications in more widely accessible languages were essential.

The KNOS produced a variety of foreign-language publications from 1836—but they contained hardly anything of the Three Age System. Rafn had a different agenda altogether: the documenting of the early activities of Scandinavians on the world stage, which to a great extent meant the forays and explorations of the Vikings. A volume in English appeared in 1836, entitled *Report addressed by the Royal Society of Northern Antiquaries to its British and American Readers*. It contained nine papers, several of them by traditional ancient historians whom we have already encountered in chapter 3. Over half the volume was given over to a paper by Finn Magnussen on the Ruthwell Cross and Anglo-Saxon runic inscriptions. The same author contributed a summary of his claims concerning the Runamo ‘inscription’, and N. M. Petersen a paper on ancient Scandinavian accounts of Ireland. There were some short chapters on antiquities,

including a translation of Thomsen's (1832) article on stone tools; the opening sentence was translated as 'Of all our Old-Northern antiquities, the most ancient are, unquestionably, those of stone' (Thomsen 1836b: 18). This was the only hint in the entire book of any chronological dimension with regard to artefacts.

The following years saw the unfolding of Rafn's vision. In 1837 he published the magisterial volume *Antiqvitates Americanæ* (Rafn 1837a). This contained all the information then available about the Viking discovery of America. The bulk comprised the sagas of Erik the Red and Thorfin Karlsefni, each published *trilingually*: in the original Icelandic; in modern Danish; and in Latin for the international scholarly readership. Shorter sections described indubitable archaeological remains left by the Vikings in Greenland, as well as possible traces in Massachusetts. This last was based on Rafn's correspondence with Thomas Webb, secretary of the Rhode Island Historical Society, to whom Rafn had first written in 1829. Webb's reply, dated 22 September 1830, was published in full in *Antiqvitates Americanæ*, and gave details of the so-called Dighton Writing Rock, on which were inscriptions of unknown origin.

In the next years Rafn was to push the Viking discovery of America to its limits. The KNOS initiated an international journal, *Mémoires de la Société Royale des Antiquaires du Nord*, with selected publications from its Danish publications translated into French, English, or German. The first volume's first part, for 1836–7, contained a lengthy French summary of *Antiqvitates Americanæ* (Rafn 1837b). This was to appear in book form in the original French (published in Paris in 1838); and Spanish (Caracas in 1839, Madrid in 1840, Havana in 1845), Portuguese (Rio de Janeiro in 1840), Dutch (The Hague in 1838), English (New York in 1838), German (Stralsund in 1838), Italian (Pisa in 1839), Polish (Cracow in 1838), and Hungarian (Pest in 1842). Part two of the first volume of *Mémoires* contained an article by Thomas Webb, suggesting that a curious round stone tower in Newport, Rhode Island, might actually be of Viking origin (Webb 1839). This tower has subsequently been adopted by various 'fringe' or 'alternative' causes, which claimed that it was constructed by pre-Columbian groups as disparate as Knight Templar explorers (Knight and Lomas 1996: 289–90) or Chinese circumnavigators (Menzies 2002: 331–3). It is not generally realized that the first published

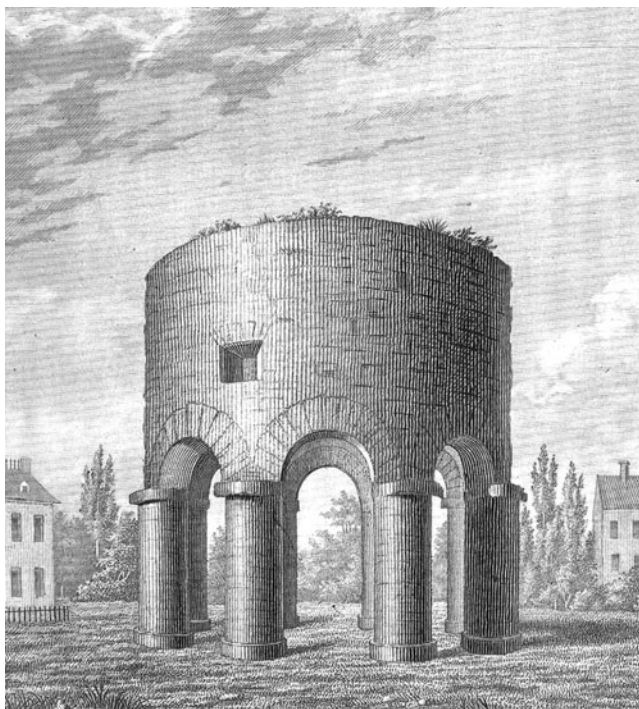


Fig. 4.1. The circular tower at Newport, Rhode Island, claimed as a possible Viking structure. Reproduced from its first published image in the Danish periodical *Mémoires de la Société Royale des Antiquaires du Nord*. (From Webb 1839: fig. III).

description and illustration of this famous (or, more accurately, infamous) structure was in this Danish periodical (fig. 4.1).

This first volume of *Mémoires* also contained various other papers translated into French and German, papers we have already encountered in their Danish incarnations. We saw in chapter 2 that Thomsen's primary publication of the Three Age System was the second chapter of the *Ledetraad* volume (Thomsen 1836a), the first chapter being by N. M. Petersen (1836) on the extent and importance of ancient Scandinavian literature. In *Mémoires*, Petersen's chapter was translated into French (N. M. Petersen 1839b)—but Thomsen's was not. Petersen's paper on the Vends' attacks on Denmark was also published, in German translation (N. M. Petersen 1839c); this was

where he identified burial mounds containing the warriors slain at the Battle of Lyrskov Heath in 1043. As we saw in chapter 3, Worsaae's excavations demonstrated that the mounds were Bronze Age; but Worsaae's paper was not in a KNOS publication and was never translated. *Mémoires* also contained an unattributed paper about the 'Queen Gunhild' bog corpse (Anon. 1839); the first part described the find and the artefacts, as reported by the Committee (Oldsag-Committeén 1837), but most of it contained Petersen's (1837) argument identifying the corpse as Gunhild. Once again, Worsaae's rebuttals remained untranslated. No mention of the Three Age System found its way into this first volume of *Mémoires*.

The second volume of *Mémoires* (1840–4) saw the first two mentions of the Three Age System. One occurred in a short paper on brooches, which must surely be by Thomsen although he was not credited by name. The beautifully illustrated brooches (fig. 4.2) were of bronze, but Thomsen stated that they were not Bronze Age. Bronze continued in use for decorative purposes after iron cutting weapons and tools appeared; Thomsen dated these brooches to the Iron Age because their pins were iron, and to the last part of the pagan period because similar decorative motifs occurred in the earliest Christian contexts (Thomsen 1844: 76). The second mention, also uncredited, comes from the description in French of the Society's annual meeting of 1840. A report was read from a clergyman called Masch, on his excavations near Grönau in the Duchy of Lauenborg. These provided stratigraphic evidence that the Bronze Age succeeded the Stone Age, similar to the instance reported from Møn in 1837 (Anon. 1844) (see chapter 1). Both these mentions were, however, decidedly low-key. Worsaae's 1843 book was published in German in 1844, and this was a more major statement; he was gratified in 1846 to find that British Museum staff knew the German version, but it was not to appear in English until 1849 (Worsaae 1849d). The *Ledetraad* volume, containing Thomsen's chapter, was translated (with modifications) under the auspices of the Earl of Ellesmere in 1848 (Ellesmere 1848), only a year before Worsaae's book. Thomsen spent some weeks in the British Museum in 1843. He felt that the display was jumbled and the staff had little knowledge or expertise (Jensen 1992); he does not seem to have proselytized the Three Age System (Briggs forthcoming).



Fig. 4.2. The first illustration of artefacts dated to a specific period of the Three Age System to appear in an article in a non-Scandinavian language. Thomsen dated these bronze brooches to the Iron Age because the pins on their rear sides were of iron, and to the end of the pagan period because of their decorative motifs. (From Thomsen 1844: fig. I).

Overall, the Three Age System must therefore have come across as a quite minor aspect of Danish archaeology. What was totally lacking in English were the two key things that had made the Three Age System so strong at home: the accretion to it of the other chronologies based on economy, environment, and craniology; and the subsequent assault on ancient history by Worsaae. Nilsson's economic scheme did not appear in English until 1868 (Nilsson 1868), when the intellectual circumstances were entirely different, and in a much altered form (Rowley-Conwy 2004). Steenstrup's environmental scheme has never been translated. The craniological scheme did trickle through, but (as we shall see in the next section) in such a way as to cause confusion rather than clarification. None of

Worsaae's assaults on ancient history was translated. They appeared either in independent books, or in Molbech's journal *Historisk Tidskrift*, so that even had Rafn been interested, they were not the KNOS's to translate. Having appeared at home with a bang, the Three Age System appeared abroad with little more than a whimper; and it would take more than that to have much impact on the ethnological chronology.

ETHNOLOGY: JAMES COWLES PRICHARD, THE CELTS, AND THE ALLOPHYLIANS

In 1840s England the Three Age System was up against an alternative chronology devised not by archaeologists, but by ethnologists. We saw in chapter 1 how the science of ethnology emerged from the uniting of comparative philology and comparative anatomy (Stocking 1987: 48). Its leading proponent in England was James Cowles Prichard, who saw it as a *historical* science (Prichard 1848a: 302); however, it mostly examined the past through the philology and anatomies of *contemporary* peoples. Where possible, it supplemented these with historical and epigraphic information, and in the Near East and Egypt was by this means able to achieve a considerable time depth. But in peripheral areas like Europe, the reach of history was much less. The picture created by ethnology tended therefore to be both rather amorphous and generalized (because based mainly on contemporary material), and chronologically rather shallow.

The London archaeologists, for whom the pre-Roman past was a relatively minor concern, were however content to shelter under the ethnological scheme, and allow ethnology to speak for this period of the past; hence the comments of Thomas Wright, quoted above, to the effect that successive westward migrations of Celts and Germans formed the main events of early European history. Paradoxically, it was, as Morse (1999, 2005) notes, through Prichard himself that the Three Age System first got into print in England; but the partial understanding of it that Prichard had, and the way he embedded this

within his previously held ideas, arguably as much hindered as hastened its adoption.

In order to understand how this happened, we must examine Prichard's views in some detail. As a committed monogenist, he championed the single origin of mankind, followed by division and dispersal in accordance with the scriptural account. Much of his work was devoted to the main consequent problem: how to account for the diversity of human physical types if all people were descended from one original couple. Prichard resolved this by proposing physical change through time—but in those pre-Darwinian times, only within the parameters of the individual species. He explicated this as Darwin was also later to do: through the changes in animals consequent upon their domestication. Animals could in fact change in either direction, either ennoblement towards the domestic form, or reversion towards the wild one. A good example was provided by the pig, which was heavily modified under domestication, but which rapidly reverted to the wild form when released. Prichard illustrated the contrast by depicting a wild boar and a domestic Hampshire sow (fig. 4.3); the bone structure of the head showed great variety, and provided 'some of the leading characters which distinguish particular races' (Prichard 1848b: 63). Drawings of the skulls of wild boar and domestic pigs (fig. 4.4) demonstrated just how massive and thoroughgoing such changes could be:

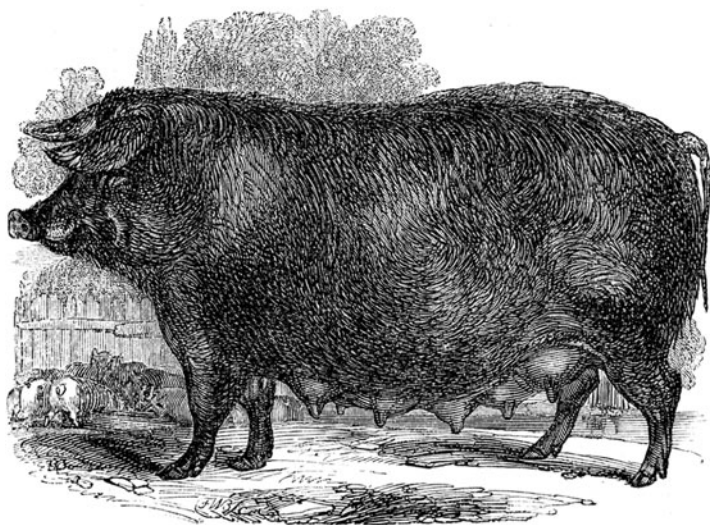
The difference in the shape of the head between the wild and domestic hog of America is very remarkable. Blumenbach long ago pointed out the great difference between the cranium of our swine and that of the primitive wild boar. He remarked that this difference is quite equal to that which has been observed between the skull of the Negro and the European. (Prichard 1848b: 31)

This was to serve as Prichard's model for human variability: skull shape reflected not just ancestry, but also way of life. Since changes induced by way of life were transmitted to the next generation, his view of physical change was closer to Lamarck's theory of the 'inheritance of acquired characteristics' than to the evolutionary theory later propounded by Darwin.

Prichard's fundamental division of humans was three-fold, and his nomenclature assigned each to a son of Noah: the Semitic or

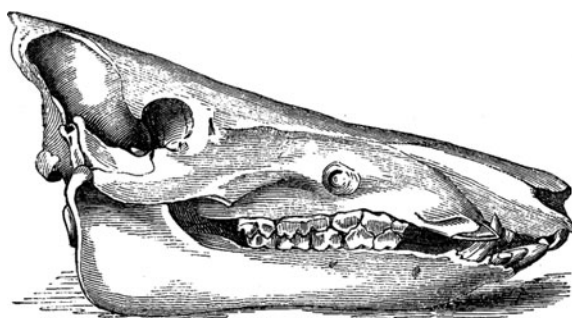


Wild Boar.

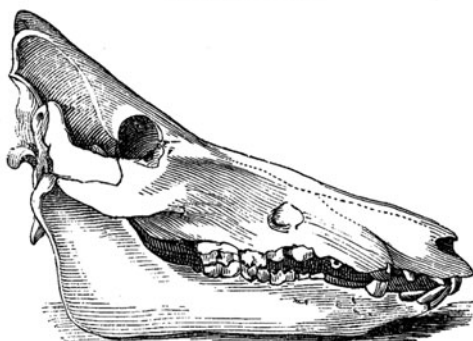


Hampshire Sow.

Fig. 4.3. Prichard's illustrations of wild boar and domestic pigs, showing how animals could change physically as a result of domestication. (From Prichard 1848b: figs. 3 and 4).



Skull of a Wild Boar.



Skull of a Domestic Hog.

Fig. 4.4. Prichard's illustrations of the skulls of wild boar and domestic pigs, showing how physical changes associated with domestication could affect the skeletal structure, especially the skull. (From Prichard 1848b: figs. 5 and 6).

Syro-Arabian; the Japetic or Indo-European; and the Hamitic, or Egyptian and African. Skull morphology did not however group neatly into three corresponding with these, because in the Near Eastern heartland they all lived in a state of civilization—and *the alternative ways of life: civilization, nomadism, or hunting, were major determinants of skull shape*, just as domestication was in animals:

[*the Near-Easterners*] were neither nomades [*sic*] nor savages, nor do they display in their crania either of the forms principally belonging to races in those different states of existence. They all had heads of an *oval or ellipto-spherical form, which we have observed to prevail chiefly among nations who*

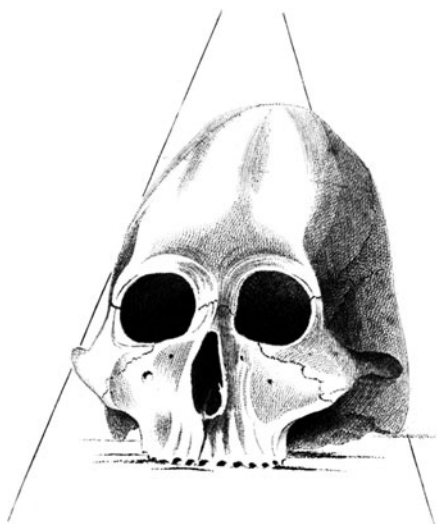
have their faculties developed by civilization. (Prichard 1848b: 138, added emphases).

Other forms of skull were associated with the other two ways of life, determined by which part of the face was most strongly developed: laterally projecting cheekbones created skulls of pyramidal form, which characterized nomads; while a forward projection of the jaw (prognathism) created a long skull, which characterized hunters. The projecting cheekbones of the pyramidal skull meant that lines drawn upwards from the outer points of the cheekbones and touching the temples, met above the head; and Prichard depicted Eskimo and American Indian skulls with the lines in place (fig. 4.5). In Europeans by contrast the two lines were parallel, because the European forehead was wider, and the cheekbones less projecting (Prichard 1841a: 281–2; 1848b: 119–21).

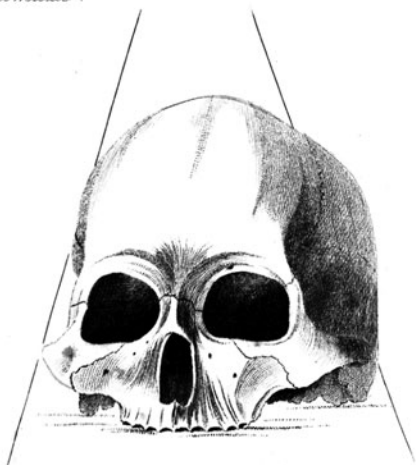
Just as animals could become domesticated or revert to the wild, so too could humans ascend or descend through these ways of life:

The greater relative developement [*sic*] of the jaws and zygomatic bones, and of the bones of the face altogether, in comparison with the size of the brain, indicates, in the pyramidal and prognathous skulls, a more ample extension of the organs subservient to sensation and the animal faculties. Such a configuration is adapted, by its results, to the condition of human tribes in the nomadic state, and in that of savage hunters... The physical characteristics of these last-mentioned races bear some analogy to those of the wild and uncultivated breeds of animals. But we have seen that the peculiarities of wild tribes are reproduced by a return to the wild and savage condition. (Prichard 1848b: 121–2)

The influence of way of life on skull form was often clearly visible. Originally central Asian nomads, the Turks would have had pyramidal skulls when they moved into the Near East and established their empire. What happened subsequently was that Turks in the eastern regions, who had continued as nomads, showed no change in skull form. The western Turks, however, had settled down and become civilized, and in consequence their skulls had become globular, like those of Europeans (Prichard 1848b: 210–13). A reviewer of Prichard's book added another example: the Hungarians. Language indicated that they were 'closely allied in blood to the stupid and



Cranium of an Esquimaux.



Cranium from an ancient tomb near Niagara.

Fig. 4.5. Prichard's illustrations of two skulls of pyramidal shape showing how lines drawn from the outermost point of the cheekbone and touching the temples meet above the top of the head. (From Prichard 1848b: facing p. 119).

feeble Ostiaks and the untameable Laplanders' (Carpenter 1848: 460), but when they came to Europe they

laid aside the rude and savage habits which they are recorded to have brought with them, and adopted a settled mode of life. In the course of a thousand years, their type of cranial conformation has been changed from the pyramidal to the elliptical, and they have become a handsome people, of fine stature and regular European features. (Carpenter 1848: 440).

Way of life thus accounted for the contemporary skull-shape of the northern Asian nomads; but in terms of ancestry they were more difficult to fit into the three main families of mankind—though in Prichard's monogenist perspective they had to fit somewhere. Prichard's solution involved radiations from the Near Eastern heartlands. The Hamites went to Egypt and degenerated into long-headed hunters as they moved onward into Africa. The northern Asians resulted from a spread and degeneration of the Japetic group. Differences in skull shape were due to their degeneration into nomads; the key evidence was the linguistic connection between the nomads and the main Japetic group, albeit quite remote (Prichard 1848b: 138–9). This linguistic connection was supported by another major philologist, the German diplomat Baron Bunsen, resident in Britain at the time. A monogenist like Prichard, he regarded the central Asian languages as very inferior to the Indo-European ones, but definitely related, comprising the other main branch of the Japetic group. At the BAAS in 1847 he proposed that the central Asian language branch be termed the 'Turanian', in contrast to the Indo-European one for which he proposed 'Iranian'—following a suggestion of Prichard's a few years earlier (Prichard 1841a: 257). Bunsen stressed their historical links: 'the Iranian and the Turanian, though always in opposition to each other, are to be considered as but diverging lines from one common centre' (Bunsen 1848: 296). He reiterated this in his major book of 1854, using the term 'Arian' instead of 'Iranian' (Bunsen 1854).

The two key aspects of Prichard's thinking that determined his treatment of pre-Roman Europe were thus: variation of human skull forms with way of life; and the Asian nomads being an inferior offshoot of the Japetic line.

Prichard (1831) had himself shown that the Celtic languages were Indo-European. But those of the Finns, Lapps, Hungarians, Ostyaks,

and Siberian Tschudes were not (1831: 17). This raised the question of whether the Celts, as the first Indo-Europeans in Europe, had found Europe empty, or whether the ancestors of these other peoples were there before them. Prichard concluded that Europe *did* have an earlier non-Indo-European population. For these he coined the term *Allophylian*, as a neutral label not implying any affiliation (1841b: 8–9)—the term is simply derived from the Greek for ‘other lineage’; the Danish philologist Rasmus Rask (whom we encountered in chapter 2) had earlier labelled them ‘Scythian’, but Prichard preferred ‘Allophylian’ because not all were necessarily Scythian (1843: 185 n.). Prichard’s 1841 discussion of the Allophylians comprised just two pages, and he made no more of them two years later (1843: 185–6; repeated verbatim in 1848b: 185–6). They must have preceded even the Celts, because had the Celts been the first to possess Spain, the Allophylian Basques would have been far too primitive to wrest any territory from them (Prichard 1843: 185). However, the earliest Indo-Europeans, although ‘everywhere superior in mental endowments’ to the Allophylians, had often themselves ‘retained *or acquired* many characteristics of barbarism and ferocity’ (Prichard 1843: 186, added emphasis). They did not know metals, because the words for ‘iron’ and ‘gold’ differed between the Indo-European languages of Europe, suggesting that these languages had already diverged before metals were discovered (*ibid.*).

This is the context into which the Three Age System made its entry into English. It first appeared, as Morse (1999) identifies, in volume III of the third edition of Prichard’s *Researches in the Physical History of Mankind* (Prichard 1841b: xvii–xxii). It is not in the volume’s main text, but is an extended plate caption in advance of the main text. Both plate and caption appear to have been added as an afterthought. In view of the historic importance of the text and the scarcity of the volume in which it is found, it is transcribed in Appendix 4. The plate shows the skull of ‘an ancient Cimbrian’, appearing, however, in a chapter on Italy (Prichard 1841b: facing p. 204), a topic to which it is not relevant. The skull was in fact from Denmark, being the first of the two illustrated by Eschricht (1837). Prichard saw a cast of the skull in the Royal College of Surgeons, and noted that he received a translation of *part* of Eschricht’s paper from Richard Owen, curator of the College’s Hunterian Museum; had he

had access to the whole of Eschricht's paper things might have turned out very differently for British archaeology. It is not clear who carried out the translation, but it was not a Dane because it contains an elementary error: near the end of the part translated, Mr Hage is mentioned digging two mounds 'near Byen', as if this was a place name. But 'Byen' simply means 'the town'; the correct translation is 'in the vicinity of the town'. The Royal College of Surgeons apparently received a cast of the skull, along with Eschricht's *Danish* text. It appears that only the section dealing with this skull was translated and made available to Prichard. He cannot have seen the original paper, because he stated that it contained a front view of the skull, which it did not. Prichard quoted Eschricht's identification of this skull as Caucasian; but he evidently *remained unaware that Eschricht had identified another physical type appearing in the Bronze Age*. In *The Natural History of Man* two years later, Prichard incorporated the substance of his 1841 plate caption in the main text (1843: 191–2; repeated verbatim in 1848b: 191–2), but did not correct this simplification of Eschricht's more complex scheme.

This was to have major consequences. Had Prichard had to fit *two* earlier human types into his scheme, one would have to be Allophylian; but with only one type, the Allophylian category might not be needed. So where did Eschricht's Stone Age skull belong? It had, said Prichard, some contrasting features. On the one hand, the cheekbones were quite prominent, so that the diagnostic lines did converge somewhat above the head (fig. 4.6), as in an Asian nomad (cf. fig. 4.5). On the other hand, the large cranium, high forehead, and prominent nasal bones suggested a more advanced race. Thus these skulls were 'probably the crania of *Celtic races; in Denmark, those of the Cimbrians*. The tombs containing ornaments of the precious metals are referred to a later age; but it is uncertain as yet whether they belonged to the same race as the former' (Prichard 1843: 192–3, added emphasis). The skull's somewhat convergent lines would be expected from an Indo-European group that had, as he argued, 'retained or acquired many characteristics of barbarism and ferocity' (Prichard 1843: 186)—they were not as convergent as in true pyramidal skulls. Prichard confirmed his identification in 1844, when he examined four modern Finn and Lapp skulls. He concluded that the Finns and the Lapps were essentially one population, and unlike the

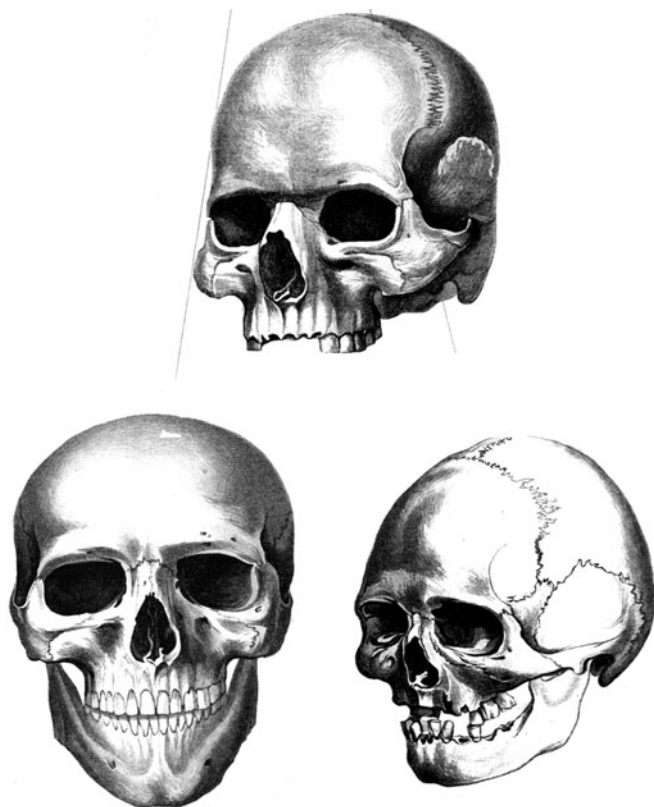


Fig. 4.6. Prichard's illustration of the cast of Eschricht's Stone Age skull in the Royal College of Surgeons, with the moderately converging lines indicating something of a tendency towards a pyramidal appearance (top), compared with the skull of a contemporary Estonian Finn (bottom left) and that of a Lapp (bottom right). After Prichard (1848b: 206; also 1843: 206).

Indo-Europeans: 'we are hence able to lay down, that there is no important difference between the skulls of the Finns and Lappes [*sic*], but that, on the contrary, there is a very great resemblance between them; that altogether they are more nearly allied to the Hyperborean¹ form than to the European' (Prichard 1844: 134–5). Thus when faced

¹ A term used by Prichard to refer to northern Asian or Allophylian peoples.

with genuine Allophylian skulls, he was clearly able to identify them as such, and distinguish them from the Celts.

Prichard's belief that human skull form was plastic therefore enabled him to interpret Eschricht's Stone Age skull as an Indo-European gone somewhat primitive. The Celts were the first Indo-Europeans to reach Europe, so the identification of the skull as a Celt was inevitable. And because the Allophylians and the Indo-Europeans were branches of the main Japhetic family, the range between them formed the only relevant axis of variation. To Prichard, Eschricht's Stone Age skull was, simply, more of a Celt than a Finn.

Prichard's diagnosis of this and by implication other very ancient skulls as Celtic allowed English archaeologists to continue to lump everything pre-Roman as 'Celtic' or 'British'. Any Allophylians in England had thus left no archaeological record, and need not be considered further—and as we shall see, they were almost completely ignored. The pre-Roman past therefore needed no chronological subdivision, and need not encompass a particularly long time period. Horizontal or tribal differences were thus more important than vertical chronological ones. This was the conclusion of ethnology, the new science that, more than archaeology, was dealing with Europe's most ancient past. This picture could hardly be more different to that which Worsaae brought with him in 1846; but he was to find that archaeology in London had other things on its mind.

THE ASSOCIATION AND THE INSTITUTE

The archaeological fraternity that Worsaae encountered in London in 1846 was in serious disarray. It had just gone through an acrimonious split and ended up as two hostile organizations, the British Archaeological Association and the Archaeological Institute of Great Britain and Ireland (abbreviated to the 'Association' and the 'Institute' respectively). From 1770 the Antiquaries had sporadically published the *Archaeologia*, its journal, but by the 1840s had long been moribund (Briggs forthcoming). In 1843 frustrated elements of

its membership founded the British Archaeological Association. The impetus came from three men: Thomas Wright, whom we met at the start of this chapter, Charles Roach Smith, and William Bromet. They met at Wright's house on 5 December 1843, and agreed to invite others to join them. These included Albert Way (the director of the Antiquaries in the period 1842–6), Thomas Pettigrew, and Thomas Crofton Croker (*JBAA* 1: i; E. R. Taylor 1932; Wetherall 1994). As Albert Way explained, the intention was not to supplant the Antiquaries but to supplement them, by concentrating on two things the Antiquaries were unable to do due to the nature of their constitution: protect monuments from the ever-increasing destruction to which they were subject; and involve people outside London. Thus the Association was 'wholly independent of [*the Antiquaries*], yet wholly subsidiary to its efforts, and in extension thereof' (Way 1845: 3). The self-selected Central Committee comprised twenty men: the Presidents of the Association's four sub-sections (primeval, medieval, architectural, and historical), and sixteen others including Wright and Croker. Pettigrew was treasurer, Roach Smith and Way the two secretaries. Lord Albert Conyngham, an active and wealthy archaeologist and president of the Numismatic Society, was invited to become President.

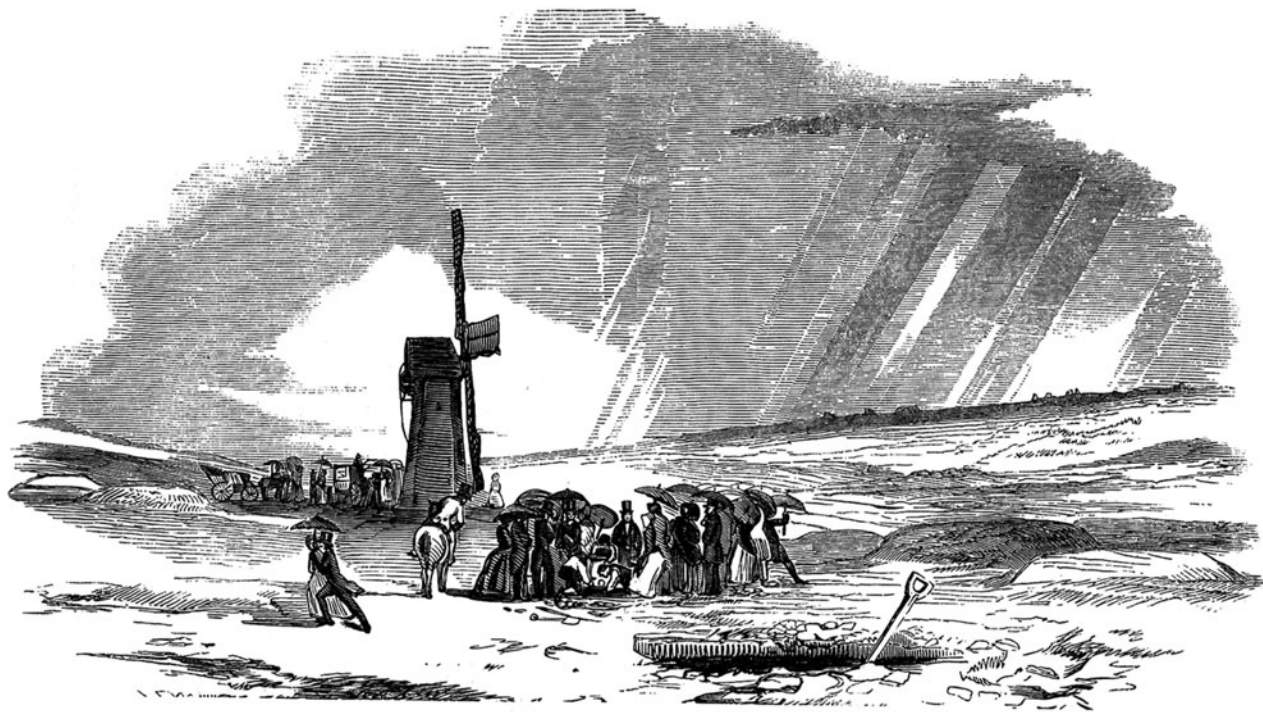
Despite (or perhaps because of) this rapid and idealistic start, the Association had no constitution or regulations, no membership dues, and indeed no membership list at all apart from the names of individuals who had written letters of support; the intention was that it should be funded by the sale of its journal, the *Archaeological Journal* (*Arch. J.*). 'Consequently the members had no rights at all; they never met, and matters were entirely in the hands of the self-appointed Central Committee, who met regularly twice a month' (E. R. Taylor 1932: 186). The Central Committee appointed a Printing Committee of six, including Way, Wright, and Roach Smith, without making any one of them editor-in-chief (Wetherall 1994: 10). In this ramshackle organization lay the seeds of disaster, and the Association was soon to polarize into two factions, one around Albert Way, the other around Thomas Wright. The two factions had different ideas about the running of the Association's two major ventures: the journal, and the Canterbury congress.

Way initially assumed responsibility for the journal, and edited the first of the four parts of volume 1, for 1845. His health then deteriorated, the first part went to press late, and Wright had to take over. The situation was exacerbated by the differing financial situations of the two men: Way had extensive independent means, but Wright did not; the Wright faction later made much of the fact that Wright himself spent much time editing the journal without ever asking for recompense (*JBAA* 1: ii). The journal's organization and finances were curious. Way made a verbal agreement with the Oxford publisher J. H. Parker that Parker would publish *Arch. J.* at his own expense; if it made a loss Parker would sustain it, while if it made a profit this would be divided equally between Parker and the Association. The cost of woodcuts and other illustrations was to be limited to £25 per number, or £100 per annual volume, with the proviso that an individual number might exceptionally go to £30. The Wright faction claimed that the cost of illustrations for the first volume actually amounted to more than twice what was agreed, totalling £217 0s 6d. They accused Parker of commissioning expensive woodcuts at the Association's expense, woodcuts that he would subsequently be able to use elsewhere; Parker had even inserted on his own initiative an entire article with many expensive illustrations into the fourth number of volume 1, an article that neither the Central Committee nor the Printing Committee had even seen prior to its publication. Way and Parker arranged for the printing of a membership list; Parker added advertisements for his own publications and inserted them into various periodicals—and then billed the Association for £207 6s 3d for the paper, printing, and publication, though this was later reduced by £103 13s (*JBAA* 1: xii–xiii). If this account by the opposing faction is accurate, Way was evidently both unilateral and naive in his dealings with the wily Parker, and this annoyed the more business-minded Wright. At all events, at the Association's second congress in Winchester in 1845, Thomas Pettigrew the treasurer was again to highlight the financial aspects of the journal as the main cause of the split (Pettigrew 1846: 9).

The other cause of conflict was the first congress, held at Canterbury on 9–14 September 1844. Not all members of the Association were convinced that such a venture would be successful, or whether

it was even the right sort of event to hold at all. Chief among the doubters was Albert Way, and over half the Central Committee (largely the emerging Way faction) stayed away from Canterbury. One hundred and forty-one members did, however, attend, the majority being accompanied by the ladies of their families (Dunkin 1845: 18–21), so the total attending approached three hundred. The meeting was a success due to the hard work of the local committee and the other Central Committee members, principally Thomas Wright (Wetherall 1994: 12). At 3.30 p.m. on 9 September Lord Conyngham opened the meeting with a short welcoming address. He was followed by Roach Smith, who regretted the absence of Albert Way on health grounds, and proceeded to give a summary of the Association's aims. When Roach Smith finished there was still some time available, so some Central Committee members proposed that a paper could be read immediately (Dunkin 1845: 35)—and up stood Sir William Betham, a visitor from Ireland, who gave a paper stating that on linguistic grounds the Irish were not related to the Welsh, but were the descendants of Phoenician colonists (Betham 1845). It is not clear what his English audience made of this, but as we shall see in chapter 6 Betham's views were widely accepted in Ireland. The rest of the meeting proceeded more according to plan; on the same evening the primeval section heard two lectures on barrows, the first by John Bathurst Deane (1845), the second by a man of whom we shall hear more, the prolific barrow digger Thomas Bateman (Bateman 1845). The next day Lord Conyngham courteously invited all the ladies and gentlemen present to excavate eight barrows on his nearby property (fig. 4.7).

The Wright and the Way factions split apart early in 1845 over the publication of the Canterbury meeting. Most of the Central Committee felt that the proceedings should be described in *Arch. J.*, and the best papers forwarded to the Antiquaries for publication in *Archaeologia* (Wetherall 1994: 13). A 16-page summary of the meetings and excursions duly appeared in the journal (*Arch. J.* 1: 267–83). Matters took an unexpected turn when an enthusiastic rank and file member, Alfred Dunkin, produced his own book-length account of the meeting containing many of the papers presented (Dunkin 1845). Events only became seriously unpleasant, however, when Thomas Wright published his own account, forming the first 42 pages of a



Opening of the Barrows on Breach Downs.

Fig. 4.7. Members of the British Archaeological Association excavating eight barrows on Lord Albert Conyngham's property in inclement weather; Canterbury, 10 September 1844. (From Dunkin 1845: facing p. 92).

book entitled *The Archaeological Album* (Wright 1845). J. H. Parker, the devious Oxford publisher of *Arch. J.*, may have seen Wright as a potential threat to his lucrative relationship with Way; at all events, he took exception to the *Archaeological Album* on the grounds that it would prejudice the account in the journal, and asked that the next issue of *Arch. J.* contain a declaration on the title page to the effect that the journal was the only official publication of the Association. The Wright faction later pointed out that Dunkin's much larger publication had aroused no such adverse comment, and argued that since Wright's *Archaeological Album* was intended as a summary of the year's archaeological developments in Britain, it could hardly ignore the Canterbury Congress; nor could it be confused with *Arch. J.*, 'from which it differs in size, colour, shape, appearance, &c.' (*JBAA* 1, iii). Wright may also have considered any profits to be a reasonable recompense for all his unpaid work.

On 18 December 1844 the Central Committee declined to issue the declaration Parker wanted (*JBAA* 1, iii). This might have been the end of the matter; but Albert Way decided to pursue it at the meeting of the Printing Committee on 28 December 1844, which passed a motion censuring Wright by three votes to two (one of its six members was absent). Wright and Roach Smith consequently resigned from this committee. Parker immediately published a circular condemning Wright; this purported to come from the Central Committee, 'to the great astonishment of the members of that Board, who knew of no such intention on the part of the governing body to which they belonged' (*JBAA* 1: v). Lord Conyngham presided over the next meeting of the Central Committee in person to calm the situation, and persuaded Wright and Roach Smith to rejoin the Printing Committee. But at the Central Committee's next meeting on 22 January 1845, Edward Hawkins, a Keeper of Antiquities at the British Museum, raised the matter again. The Way faction had packed this meeting with members of the Central Committee who otherwise rarely attended, and consequently had the majority required to pass Hawkins's motion requiring Wright again to resign from the Printing Committee. Lord Conyngham, who was not present, afterwards wrote to Hawkins asking him to withdraw the motion, but Hawkins refused. Following another row at the Central Committee meeting on 12 February

1845, Lord Conyngham resigned the presidency, feeling that Wright had suffered an injustice; Wright, Roach Smith, and Pettigrew also determined to resign (*JBAA* 1: v–vi; E. R. Taylor 1932: 221; Wetherall 1994: 14).

But rather than resign, the Wright faction immediately counter-attacked. As treasurer, Thomas Pettigrew was (in the absence of a president) the senior office holder. What happened next was later presented by the Wright faction as an unexpected reaction by the grass-roots membership: ‘the treasurer was entreated not to give in his resignation, and was made acquainted with the intention of many to address to him a requisition to call together the members at large’ (*JBAA* 1: vii); in ten days, Pettigrew received 162 such requisitions from all over England. The scale and speed of this response however suggests planning rather than spontaneity; the Wright faction was evidently just as willing to politick as was its opponents. Pettigrew duly called a Special General Meeting for 5 March 1845. Way and twelve members of the Central Committee met on 28 February and denounced Pettigrew’s meeting in the press, stating that only the Central Committee had the power to call such a meeting. At the 5 March meeting Pettigrew countered that in the absence of any regulations to the contrary he had a duty to the membership, and had no choice but to call the meeting. The Wright faction then selected a new Central Committee which did not include Albert Way, and invited Lord Conyngham to reassume the presidency. The Way faction continued to maintain that these proceedings had no validity and that they represented the Central Committee (E. R. Taylor 1932: 221–3; Wetherall 1994: 15–16).

There were now two bodies claiming to be the Central Committee of the British Archaeological Association, and so it remained for the next few months. Pettigrew retained control of the funds, and some members of the Way faction who had paid their 1845 subscriptions early in the year asked for them to be returned; but Pettigrew refused. The Way faction, dominant on the Printing Committee and allied with Parker the publisher, retained control of *Arch. J.* but needed a new president; they trumped the Wright faction by appointing an even more eminent peer, the Marquis of Northampton, president of the Royal Society. After the success of the Canterbury meeting, plans had been set in motion for one in Winchester in

1845, but it was now inconceivable that this could go ahead as planned. Winchester thus became the scene of *two* archaeological meetings in 1845, the Wright faction's on 4–9 August, the Way faction following on 9–15 September. On 15 September, the last day of the latter meeting, Lord Northampton proposed after consultation with Albert Way that this faction adopt a new name, suggesting 'The Archaeological Institute of Great Britain', but 'at the suggestion of Mr. BABINGTON the words "and Ireland" were added after "Great Britain"' (*Arch. J.* 2: 316–17; Institute 1846: xxxi). This was significant: the Institute's 1846 membership lists included 29 Irishmen including two Kilkenny men whom we shall meet again in chapter 6, James Graves and John Prim. Presumably mindful of the chaos earlier in the year, twenty-one regulations, evidently drafted in advance, were then promptly read out. The next year Wright's residual Association decided to launch its own journal, the *Journal of the British Archaeological Association*, but resolved to keep tight control of the publishing to avoid the sort of problem that had arisen with Parker (*JBAA* 1: x, xi).

Both the Institute and the Association (as we may now refer to them) published full accounts of their separate Winchester meetings despite the Way faction's earlier misgivings about such things (Association 1846: Institute 1846;). Each also retrospectively claimed the 1844 Canterbury meeting as its own. The Association had two published accounts of it, in *Arch. J.* and in Wright's *Archaeological Album*. Although Dunkin stayed with the Association, the Institute seems to have favoured his account, reporting the publication of his book (*Arch. J.* 2: 115); Wright's *Archaeological Album* was never mentioned in *Arch. J.*, although naturally it was fulsomely reviewed for the Association by Roach Smith (*JBAA* 1: 269–71). It is symptomatic of their mutual animosity that the favoured versions of the two organizations could not even agree what the interior of Canterbury's town hall looked like (cf. figs. 4.8 and 4.9). Each side expressed hopes of reunification, but each thought the opening move should come from the other (*Arch. J.* 2: 315; *JBAA* 1: xiii) so no move was ever made. In 1846 they held their annual meetings in different towns, the Institute in York and the Association in Gloucester; and separate they have remained ever since.

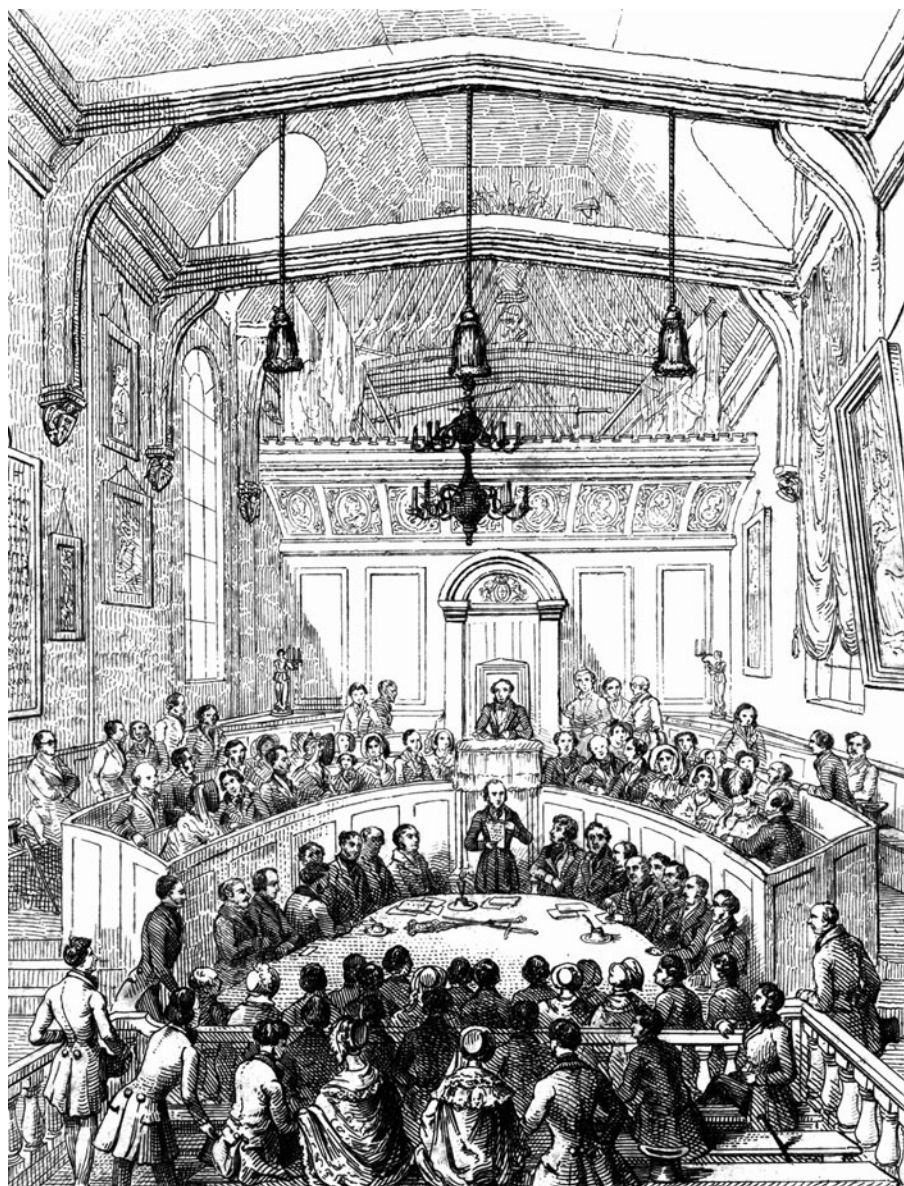
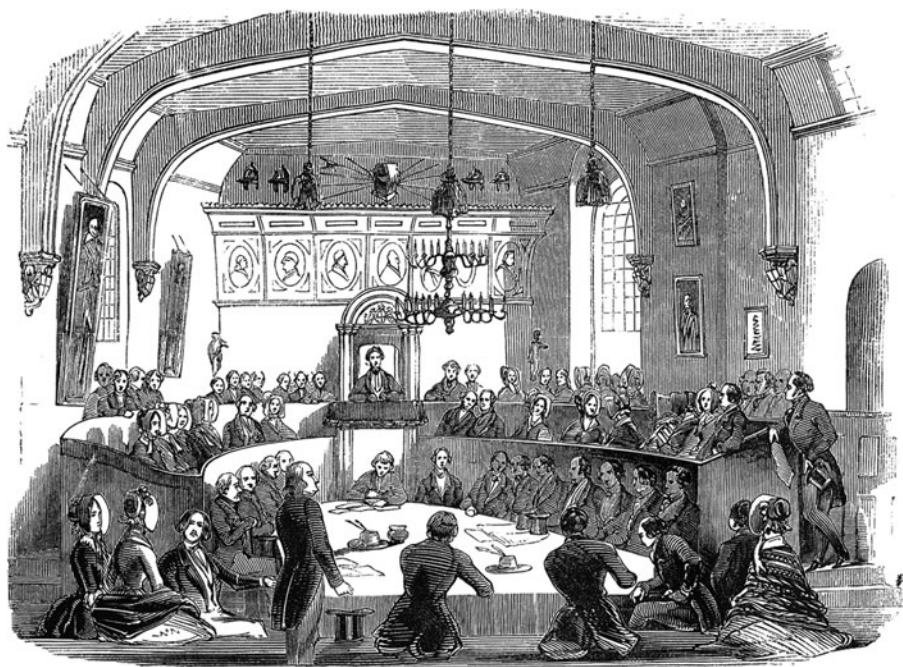


Fig. 4.8. How the Association was to remember the opening meeting in Canterbury town hall. (From Wright 1845: facing p. 1).



Interior of the Town-Hall, Canterbury.

Fig. 4.9. How the Institute was to remember the opening meeting in Canterbury town hall. (From Dunkin 1845: facing p. 22).

WORSAAE'S 'VIKING RAID' ON LONDON

Worsaae's arrival in London early in June 1846 precipitated him into the midst of this acrimony. Towards the end of his life, Worsaae was to claim that his visit had a major impact on the archaeological scene. In his unfinished memoirs (written in the early 1880s but published only in 1934) he wrote of this trip that:

There was absolutely nothing in the way of a generally accepted archaeological system . . . [*most archaeologists were*] utter dilettantes, who had no concept of the chronological sequence of the monuments and antiquities . . . I think I can state without being immodest that my trip was a sort of archaeological Viking

raid, which served . . . to establish the foundations of the . . . Danish system's influence on the British Isles. (Worsaae 1934: 137*)

Because of my especial knowledge of prehistoric artefacts, to which little attention was paid in England and which did not even have a room of their own in the excellent Museum, my expositions were listened to with considerable attention both in the Museum and in the learned societies, yes even at a solemn meeting of the great 'Society of Antiquaries' I unexpectedly gave, at the request of the President, a long lecture in English, for which I received general acclaim. (Worsaae 1934: 139*)

But the Society of Antiquaries has no record of a lecture by Worsaae on this occasion; he may have confused his memories of his 1846–7 visit with those of 1852—when he did lecture to them (Briggs 2005: 10; forthcoming). Worsaae did not in fact spend much time in London. The dates can be calculated fairly precisely from his letters to his mother. He arrived in London from Hamburg on the steamship *John Bull*, sailing up the Thames on the evening of Friday 5 June. He left for Scotland on about 19 June, so this stay amounted only to a fortnight. On his way home in 1847, he arrived in London on the evening of Tuesday 23 February, and left on Saturday 3 April; during this stay he travelled out to visit Oxford, Stonehenge, and Salisbury (Worsaae 1934). There were after all limits to the impact an opinionated 25-year-old foreigner could have in mid-nineteenth-century London, even if his name was J. J. A. Worsaae, particularly in context of bitter internecine strife between the Association and the Institute.

The cautious Thomsen had managed to maintain good relations with both the Institute and the Association, his name (misspelt 'Thompson') appearing in the list of foreign members of both in 1846. He had evidently briefed Worsaae about the situation, because on 13 August Worsaae wrote from Kirkwall to Thomsen in Italy, describing how the rival organizations had each snapped him up: 'I followed your advice and kept out of the squabbles of the English antiquarians; I said plainly that I had not come to take sides, but to meet and establish contact with *all* antiquarians. The result of this was that both the warring societies made me an honorary member; you can imagine how I secretly laughed' (Worsaae 1934: 304*, original emphasis). The courting of Worsaae by both groups may indeed have helped him—he wrote later that 'I was as if by magic

placed as a scientific authority in the midst of England's leading literary circles' (Worsaae 1934: 140*), although this may be another example of later hyperbole. He was taken by the Danish ambassador, Count Reventlow, to meet Bunsen, the philologist whom we have already encountered as a supporter of Prichard's views. But Bunsen was also the Prussian ambassador in London; the tensions that were to lead to war between Denmark and Prussia in 1848 were already building up, and Worsaae described the meeting as decidedly frosty (1934: 144).

Worsaae was seriously unimpressed by the archaeologists he met in London; he described this in his 13 August letter to Thomsen, starting with the situation in the British Museum:

The British Museum is an utter shambles! Only the Egyptian and Roman antiquities are beginning to be improved by Dr Birch; but there is no prospect of this for their national antiquities. I remonstrated at length to Hawkins and a young man, Mr Newton, about the importance of paying them more regard than has previously been the case. They promised they would, but the situation is awkward... I saw several small collections and small, very small, antiquarians in London, of whom I do not expect much. (Worsaae 1934: 304*)

We can only guess who the 'small, very small, antiquarians' were, but they presumably included leading members of both the Institute and the Association. One was evidently Edward Hawkins, whose refusal to withdraw his motion at the Association the year before had been the immediate cause of the schism. Hawkins showed Worsaae the hoard from Cuerdale, which had been found in 1840. This major tenth-century find comprised over 30 kilos of silver fragments and nearly 7,000 coins. Hawkins had published the coins at length, noting in passing a few oriental ones (Hawkins 1843: 95). But as he wrote to Thomsen on 13 August, when Worsaae saw them he was galvanized: 'some of the silver objects from Cuerdale undeniably resemble most remarkably our silver objects, but there were also 29 Kufic coins!! You can imagine what a flea in his ear I gave Hawkins about this. I think he has recently given a lecture about this in York' (Worsaae 1934: 304*). Kufic coins with Arabic inscriptions were turning up in large numbers quite regularly in Denmark, and were an important source of information about the commercial reach and power of the Vikings.

They were discussed in *Ledetraad* (Thomsen 1836a: 82–3). One hundred and sixty had been found at Vaalse in southern Denmark in 1835, and were published with many illustrations (Lindberg 1842) (fig. 4.10). These publications were in Danish, but Hawkins could not plead the language barrier to account for his apparent ignorance: the KNOS's *Mémoires* (1840–4: 20) contained a report in French of Kufic coins found as far away as Iceland. The 1844 German translation of Worsaae's book also contained a discussion of them, and we know from the same letter to Thomsen that the British Museum staff knew this book (Worsaae 1934: 304). No wonder Worsaae felt that more attention should have been given to these coins. Hawkins's lecture in York to which Worsaae alluded was in fact delivered at the third annual meeting of the Institute, to which Hawkins had naturally gravitated as a member of the Way faction. Worsaae was not present to hear Hawkins's talk, since the meeting started on 17 July 1846, by which time Worsaae was in the Hebrides (see chapter 5). Its publication (Hawkins 1847), however, contained nothing about the Kufic coins, because Worsaae inserted a paper on them in the same number of *Arch. J.* (Worsaae 1847b). He stressed the frequency with which Kufic coins were found in Scandinavia, and their importance as indicators of medieval contact and trade. He finished with a wake-up call to British archaeologists:

I cannot conclude these few remarks without expressing the hope that British archaeologists will at a future time take great care to ascertain the localities where Cufic coins and silver ornaments have been found in England and Ireland. By such facts we should be enabled to give a still clearer and more detailed account of the remarkable trade between the east and the north of Europe which existed at so early a period, and of the influence which this connection with the Levant had upon the civilization of the north of Europe. (Worsaae 1847b: 203)

Worsaae's visit might have brought the Three Age System to the attention of English archaeologists who had not previously come across it, though there is no indication that it did so. The translations of both *Ledetraad* (Ellesmere 1848) and Worsaae's book (Worsaae 1849d) gave it further exposure. Worsaae's book was not reviewed in *JBAA*, but it was in *Arch. J.*, once in its German incarnation (Anon. 1846) and again when it appeared in English (Anon. 1850). The English version was reviewed in the *Gentleman's Magazine* for 1850



Fig. 4.10. Illustrations of some of the numerous Kufic coins with Arabic inscriptions from the find at Vaalse. (From Lindberg 1842: fig. 6).

(Briggs 2005). But despite Worsaae's later claim, his 'Viking raid' did not convert the English. Sheltering under the ethnological umbrella term 'Celtic' few saw much need to subdivide the pre-Roman past along technological grounds. When one provincial member of the Association stepped out of line in 1852, he was rapidly reined in.

ENGLISH RESISTANCE: BRONZES AND BARROWS 1846–1860

The reception of the Three Age System by English archaeologists will be assessed by a consideration of their publications. Most were interested in the pre-Roman past only as a minor sideline. They rarely wrote about it extensively, and most publications were excavation reports. As we shall see, we must avoid making the assumption that as soon as an individual used Three Age System terminology, he was thereafter committed to the scheme, or even necessarily understood it in the way its Scandinavian proposers did.

A decade after Worsaae's visit, Henry Rhind (1856) reviewed archaeological chronologies, attributing the current trend to Thomsen. He resorted to the classic double rebuttal: first, it was not new, because Lucretius had mentioned the stone, bronze, and iron sequence as long ago as the first century BC; second, he doubted whether it worked anyway. Rhind's paper reveals three important aspects of his thinking. First, he simply did not grasp what Thomsen had done. C. S. Petersen (1938: 8–9) distinguishes between *tredeelingstanken* ('tripartite thinking') and *treperiodesystemet* ('the three age system'). The former was the philosophical history of earlier authors, unrelated to any practical endeavour. The latter was the systematic hands-on research agenda developed by Thomsen, linked to the other lines of chronology and used to order museums, monuments, and monographs. Rhind never understood the difference. Second, in seeking English antecedents to tripartite thinking, Rhind (1856: 213) could name only two: Thomas Pennant's *Tour in Scotland* (Pennant 1774–6) and William Borlase's *Antiquities of Cornwall* (W. Borlase 1769: 289). Many others had however at least suggested that stone preceded metal. For example, Charles Lyttleton, the bishop of

Carlisle, considered stone axes older than metal ones (1773: 118); William Charles Little (1792) agreed, adding that since Caesar mentioned that the Britons used iron, the use of stone must date much earlier; William Cunnington (1802) stated that a barrow at Upton Lovell dated to before the use of metals; a point reiterated by Sir Richard Colt Hoare (1810: 76); and in a remarkable paper, John Hodgson argued from classical sources and artefacts that bronze preceded iron as the metal used for cutting weapons around the Mediterranean (J. Hodgson 1822). Similar ideas can be found in James Douglas's *Nenia Britannica* of 1793 (D. Murray 1904: 232), William Camden's *Britannia* of 1607, and William Dugdale's *Antiquities of Warwickshire* of 1656 (B. Hildebrand 1937, I: 119–20), and others could doubtless be added. Rhind's apparent unawareness of these shows how alien even tripartite thinking had become. He stressed in contrast that 'there were at the opening of the present century many dissentients, just as there are now on broader principles' (Rhind 1856: 213). Third, he did not accept that the Three Age System could be extended beyond artefacts to other aspects of the archaeological record. He was openly dismissive of the classification of burials into the different periods: 'it will, of course, be seen, that I do not advert to the subsequent graft upon this simple outline, the corresponding ascription of sepulchral usages, and still more comprehensive generalizations which, even if we admit their applicability to Scandinavia, in deference to native investigators, *are quite untenable with us*' (1856: 213, added emphasis).

A survey of the major journals and of the various books published in the 1850s reveals that Rhind's attitude was typical. Two trends emerge. The first is that, like Rhind, archaeologists were evidently happier to utilize the Three Age System when dealing with isolated artefacts, than when considering barrows. The second is that opinions *against* the system seem to have hardened during the 1850s. Both these trends will first be examined in the writings of John Akerman. In his 1847 *Archaeological Index* he mentioned that some argued that barrows should be divided into three periods, aceramic, stone, and metallic, but dismissed this because it took no account of the rank of the dead person (Akerman 1847: 11). His illustrations reflected this, disparate items appearing in the same figure with no age indication other than 'Celtic' (fig. 4.11). But in a subsequent

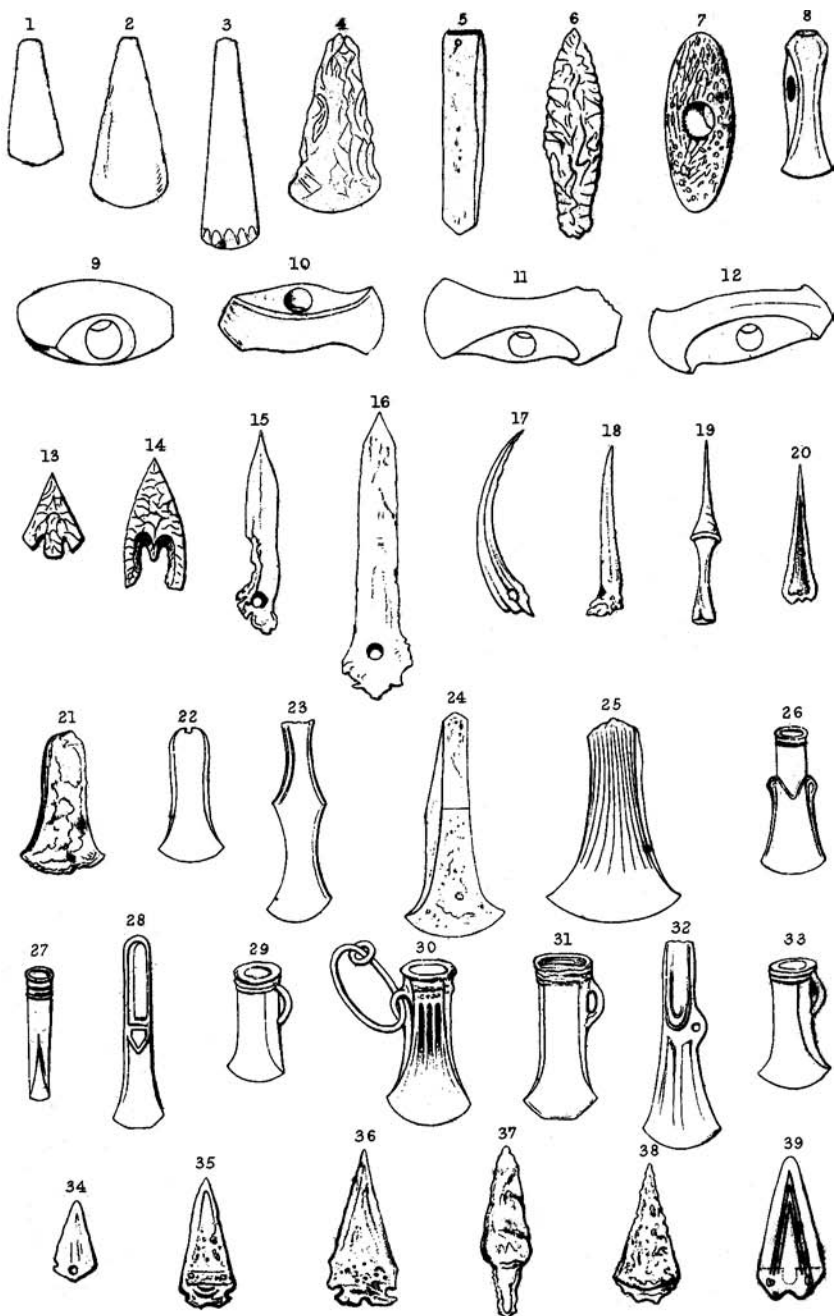


Fig. 4.11. One of Akerman's illustrations of objects belonging to the 'Celtic' period. Items 1-14 are of stone, 15-20 of bone, 21-39 of bronze. (From Akerman 1847: fig. IV).

article on artefacts he *did* classify them according to the Three Age System (Akerman 1852). It would be easy to conclude from this that Akerman had become a convert to the Three Age System; but that was not the case. In 1855 he published a barrow containing a bronze dagger, once again resorting to rank rather than chronology to explain its presence, because the dagger 'would seem to indicate that the individual... was a person of some rank..., since it is very evident, from the presence of flint implements in the mound, that the use of metal was not common among them' (Akerman 1855: 176). Describing other barrows two years later, he did not even consider chronology, using only the general catch-all 'ancient British' (Akerman 1857).

These two trends are also found in the writings of others. With regard first to artefacts, George du Noyer (1847) published a paper on axes in which he stated that stone ones preceded bronze ones. Morse (1999: 6) highlights this as an indication that 'it is only during Worsaae's visit that the [*three age*] system gained recognition', implying that this recognition was rather late in coming but had now finally arrived. The events of the next dozen years suggest however that this is something of an over-simplification. Augustus Wollaston Franks joined the British Museum in 1851 (Cook 1997), and in subsequent years published reports on the nascent collection of British antiquities. In his first he used the stone-bronze-iron classification (Franks 1852); but he actually dispensed with it the following year when describing new additions, using only the term 'Celtic' for pre-Roman items (Franks 1853). This practice he continued until late in the decade (Franks 1858). An anonymous reviewer of William Wilde's (1857) catalogue of the Royal Irish Academy's museum (of which we shall see more in chapter 6) singled out for praise the absence of any chronological theme to the display (Anon. 1857: 389). This tendency to ignore the Three Age System reached its climax in 1859, when J. O. Westwood published a description of his archaeological travels in Denmark. He was quite prepared to describe the way the *ethnographic* museum was laid out: '1. Nations not possessing or previous to possessing the use of metal. 2. Nations possessing the use of metal but destitute of literature. 3. Nations possessing the use of metal, and having a literature of some kind' (Westwood 1859: 139). But when he described the archaeological museum, he assiduously

avoided any allusion to its organizational principle, even though it was staring him in the face. He must also have known the museum's successive guidebooks, the first by J. B. Sorterup (1846), the other two by Worsaae (1854, 1859), the second appearing the year Westwood visited. These were all organized explicitly along Three Age System lines, and could have left Westwood in no doubt that this was the fundamental theme of the entire museum.

The few English papers on artefacts from this period that did use the Three Age System either discussed foreign material, or were inconsistent to the point of eccentricity. Two papers on lake dwellings were organized along Three Age System lines—but one dealt with the Irish crannog of Lagore (Talbot 1849), the other with Swiss material (Wylie 1860). In two back-to-back papers in *JBAA*, Henry Syer Cuming in the first studiously ignored the Three Age System, merely referring all metal objects to the 'Celtic' period (Cuming 1859a)—but in the second stated that stone and bronze objects 'for the sake of convenience and chronological order may be divided into two groups,—one appertaining to the ancient British savages, the other to an age of civilization; or, in other words, to what have been called the STONE and BRONZE PERIODS' (Cuming 1859b: 231, original capitalization). It is not clear to which of these he would have assigned the Celts of his first paper.

With regard to barrows, authors were generally even more reluctant to fit them into a specific Three Age System period. Arthur Trollope made a confused allusion, suggesting that his barrows might belong to the stone period, but some contained rare bronze arrowheads, which 'might have been obtained from some tribe or people in a more advanced state than themselves' (1851: 351)—leaving the precise nature of his periodical ideas in confusion. Most diggers of barrows simply ignored chronological classifications. This transcended the division into warring factions: in *Arch. J.* it was the case for Harry Longueville-Jones (1846), the anonymous 'I. W.' (1848), John Thurnam (1854), and Harry Scarth (1859); and in *JBAA* for two members of the Lukis dynasty of barrow diggers from the Channel Islands, Frederick Lukis (F. C. Lukis 1846a and b, 1848—although he alluded to chronology in a small way in F. C. Lukis 1849), and his son John Lukis (J. W. Lukis 1848), Lord Albert Conyngham (1849), John Tissiman (1851), and finally the charmingly and appropriately

named Benjamin Barrow, Esq. (Barrow 1855). It was also the general practice in the Antiquaries' *Archaeologia* (F. C. Lukis 1853; Thurnam 1860).

A rare example of an attempt to date a barrow by its content was provided by Thomas Wright's consideration of a burial in a log coffin from Gristhorpe in Yorkshire. Excavated in 1834, it contained a number of stone tools and a bronze dagger; the excavator regarded the latter as the result of Phoenician contact, and placed the date somewhere between 600 and 200 BC (Williamson 1836: 16). Wright would have none of this; twenty years later he went fully into the question of the date, perhaps because his article appeared in *The Gentleman's Magazine*, and he wished his non-specialist audience to avoid making mistakes like those of Williamson. He considered the objects:

None of these articles gives us any assistance in fixing the age of this curious interment, except the dagger, and that is not very certain. Chipped flints are found very frequently in Roman interments, both in this country and on the continent; and I have also found them in Saxon graves; but the dagger belongs to a type of which...we should be led to ascribe to a remote date, perhaps to the earlier period of the Roman occupation of the island. (Wright 1857 [1886: 82])

Wright's exceptionally short chronology, his ascription of worked flint to very late dates, and of bronzes to the Roman period, are points that re-emerged in the controversy of the 1860s. Gristhorpe had, however, come to the attention of C. J. Thomsen the year it was published; it was discussed in a paper which, although anonymous, must have been written by Thomsen himself (Thomsen 1836c). Thomsen compared Gristhorpe to a similar find from Bjolderup in Denmark which contained various bronze objects, and he argued that the remarkable similarity between them meant that they must be of the same date; Bjolderup belonged to 'the earlier periods, before iron came into use in Scandinavia' (Thomsen 1836c: 286*).²

² A combination of confusion of placenames and a misreading of Gothic type has caused one Danish site to appear as two different ones in the English literature. In southern Jutland, the confusingly named villages of Bjolderup, Bolderslev, and Vollerup lie within 5 kilometres of each other, in the vicinity of the larger towns of Aabenraa and Haderslev. Thomsen (1836c) used 'Bjolderup' in the title of his paper,

This effectively dated Gristhorpe to the Bronze Age. In terms of absolute chronology this was not necessarily much earlier than Wright's early Roman period, because Thomsen believed that iron reached Scandinavia in the late first millennium BC (see chapter 2). But conceptually he was way ahead of Wright, who never admitted even the existence of a Bronze Age, and who had no means of ascribing the find to an epoch not named after a people known to history.

Books written by people outside mainstream archaeology reflect these trends. William Saull (1845) was aware of the Four Stage Theory, arguing that the hunting and pastoralist stages preceded the Roman conquest; the Romans introduced iron, while the Phoenicians might have been responsible for bronze. In two books published in 1858, there was less consideration of chronology. William Barnes (1858) mentioned that stone axes might be older than bronze ones; he mentioned in passing that barrows containing only stone implements were likely older than ones containing metal (Barnes 1858: 28) but this in no way informed the rest of his book. Charles Boutell (1858) did not even go this far; following Wright's chronology, he regarded bronze as a Roman introduction, but referred to all barrows simply as the 'burial-places of the Celtic Britons' (Boutell 1858: 306).

There is little indication in all this that Worsaae's 'Viking raid' had much impact on English archaeology at the time. The practitioners were clearly aware of the Three Age System but appear to have felt no need to adopt it, and indeed shunned it particularly with regard to burials. The only exception was the Derbyshire squire Thomas

'Bollerslev' in his text. Worsaae (1843a: 75) used 'Bollerslev'. Both Danes stated that the site was near Haderslev. The confusion was created by Worsaae's translator, W. J. Thoms; the letters B and V are similar in Gothic type, and he apparently confused the two. In his editor's preface he quoted Thomsen's original paper, mentioning 'Bolderup' near Haderslev (in Worsaae 1849d: xvi). In the main text, however, he rendered the name as 'Vollerslev', stating that it was near Aabenraa (Worsaae 1849d: 96). Thoms added a footnote at this point stating that Lord Ellesmere mentioned another site, 'Biolderup'. The mention by Ellesmere (1848: 121) is to 'Biolderup' near Haderslev. 'Vollerslev' and 'Bolderup' thus became two different sites; Daniel Wilson mentioned both in different works (1851a: 461; 1878: II, 137). William Greenwell (1877: 377 n.) appears to be the only British archaeologist to have consulted the original publication, correctly referring to 'Bolderup, near Haderslev' and giving the correct reference.

Bateman, whose stimulus, as we shall see, was not Worsaae. But even his work was not going to lead to the adoption of the Three Age System.

ENGLISH RELUCTANCE: SKULLS AND RACES 1846–1860

We saw above how the partial translation of Eschricht's paper on Danish craniology influenced the views of James Cowles Prichard. The work of the other two Scandinavian pioneers made it into English just after Worsaae's visit, but only in small publications.

The first paper was by Anders Retzius (1847), the anatomist collaborating with Sven Nilsson (see chapter 3). This filled less than one page of text, and simply presented a list of peoples whose skulls were classified as brachycephalic or dolicocephalic, orthognathous or prognathous. It was organized geographically rather than craniologically, but the pattern was that shown in fig. 3.4. There was no consideration of history, or of possible change through time. If skull type was indeed immutable and determined by race, as this implied, then Retzius was a polygenist, i.e. one who believed in the separate origins of the different racial types. This was utterly contrary to the avowed monogenism of Prichard, who addressed the issue briefly in two of the last papers he was to write before his death at the end of 1848 (Prichard 1848a, 1848c). These papers overlapped a good deal, and in both Prichard pointed out that Retzius had made a point that was interesting in a way that he (Retzius) had probably not contemplated: there were differences between the skull forms of peoples known to be historically related (1848a: 307; 1848c: 233). Prichard did not enlarge on this, but it is likely that he was referring to his Japetic family; in Retzius' scheme these peoples might be dolicocephalic and orthognathous (Scandinavians, Celts); brachycephalic and orthognathous (Finns, Turks); brachycephalic and prognathous (Kalmuks, Tartars); and even dolicocephalic and prognathous (if American Indians were included). With this amount of variability within the Japetic family, skull form had clearly changed through time—which suited Prichard's monogenism.

The next year saw Sven Nilsson's English-language publication (Nilsson 1848). We saw in chapter 3 that Nilsson's first proposal (in Swedish) involved Stone Age Finns, then Bronze Age Celts, and finally Iron Age Goths (Nilsson 1838–43). His 1848 paper, however, listed *four* different races. The first was the brachycephalic group, related to Finns. The second was a new interpolation, comprising dolichocephalic early Goths; these people were the first agriculturalists, and they acquired bronze objects from more advanced neighbours. The third had intermediate skulls, which were 'longer than the first and broader than the second' (Nilsson 1848: 32); these were Celts, and were responsible for 'introducing bronze into the country' (ibid.). The fourth were Gothic, being the true Svea, ancestors of the modern Swedes, who immigrated in the sixth century AD and brought iron with them.

This article by Nilsson has led to considerable confusion, because it appeared to envisage two introductions of bronze into Scandinavia, and a double wave of Goths separated by a Celtic wave. But the paper was so truncated that it misrepresented the views Nilsson held in the late 1840s. To understand these we must consider first, Worsaae's criticism of Nilsson's 1838–43 scenario; and second, how Nilsson's views changed as a result. Worsaae (1843a: 105) demolished the identification of the Stone Age population as Finnish, by pointing out that Stone Age dolmens and passage graves were (a) found across large areas of western Europe, where Finns had never lived; but (b) not in northern Scandinavia or Finland, where they did live. The Stone Age population was too early even to be named in historical sources; and, as we saw in chapter 3, this was the argument Worsaae later employed against the 'Volksrecht' arguments of Jacob Grimm. Worsaae also questioned the link between the Celts and the Bronze Age. In the late 1840s Worsaae considered that the Bronze Age continued until the eighth or ninth century AD (see chapter 3), and it was historically well-attested that Goths, not Celts, lived in Denmark at this time (Worsaae 1843a: 110).

Nilsson absorbed these points and modified his views, and presented his new synthesis in a series of public lectures delivered in Stockholm in May 1847. In the introduction to a book on mammals published that year, he stated (Nilsson 1847: I n. 2) that he would be producing a book entitled *Sverige och dess Inbyggare före den*

Historiska Tiden ('Sweden and its Inhabitants before the Historical Period'), but no such book ever materialized. The 1847 lecture series had the same title; Nilsson's extensive notes were edited and published in 1923 (Nilsson 1847 [1923]). His Stone Age population was now not Finnish as such, but was part of the larger grouping of whom Finns, Basques, Iberians etc. were a part. The Stone Age occupants of southern Sweden could not have been Finns coming from the North, because Nilsson's geological studies showed that northern Sweden was under water at that time. Thus the Stone Age people came from the South; the modern Finns had come in from the northeast after the waters receded. This Stone Age group thus corresponded to Prichard's Allophylians, though Nilsson did not use that term. His crucial new departure came after this. In the later Stone Age, new immigrants arrived; these were the long-headed early Goths, who were cultivators and who drove out the round-headed hunters of the previous group. The Celts also arrived at about this time, and *lived alongside* the early Goths, and both their skull forms were found. The Celts were the bringers of bronze, maritime colonists who mainly settled in coastal regions, and it was from them that the long-headed Goths acquired bronze objects. The second and third groups in the 1848 translation were therefore actually largely concurrent, not consecutive. The picture of advanced mercantile colonizers encountering primitive indigenes might have gone down quite well with Victorian English archaeologists, but this was absent from the 1848 English summary. To the English, Nilsson was now presented as arguing for a *four*-fold sequence going Lapp-like–Gothic–Celtic–Svea. His identification of Celtic skulls was on firmer ground than before, because he had acquired a better specimen from an Irish researcher. As he declaimed in his lecture of 18 May 1847:

Here is the cast of the skull of an O'Connor, the last descendant of the Irish royal line; he is buried in Dublin. I received the cast via Prof. Retzius, who got it from a Dr Wilde in Ireland. *This* I regard as the type of true *Celt* in the way I use the word—a man of a tall blonde race who followed the Iberians in most of the countries of Europe. (Nilsson 1847 [1923]: 61*, original emphasis)

We shall encounter Dr Wilde again in chapter 6.

In the face of this apparently burgeoning complexity from Scandinavia, English ethnologists remained sceptical over the very existence of a pre-Celtic phase. In his last papers Prichard stated that the Scandinavians argued that their Stone Age skulls came from a pre-Celtic people, while the French argued that theirs were Celts; consequently, 'whether these oldest sepulchres were the tombs of a Celtic race, is a question not yet decided' (1848a: 312; 1848c: 237). In the same year Hodgkin stated that there was insufficient evidence to ascribe the early skulls to any particular race (1848: 37), while nevertheless implying that the earliest population was Celtic (1848: 40). Two years later R. G. Latham stated that the Celts were the oldest race in the areas they had occupied (1850: 528). He reiterated this in 1857 in an introduction to a posthumous edition of Prichard's 1831 book on Celtic languages. He mentioned the Scandinavian claims that the oldest skulls were 'Lap or Fin [*sic*] in form; and, as such, indicative of a population other and earlier than the present Indo-Europeans'—but disagreed sharply: 'the present writer commits himself to the doctrine that it is wrong' (Latham 1857: 52). This stemmed from his conviction that the Indo-Europeans originated not in Asia but in Europe, so the Celts had to be first; he was to persist in this until at least 1863, although by then admitting that his was a minority view (Latham 1863).

Under this ethnological guidance, there was no incentive for English archaeologists to worry about Allophylians. In Scotland, as we shall see, Daniel Wilson espoused the Allophylian concept enthusiastically, but the Anglo-Scot Henry Rhind was apparently one of only two people to mention the term in an English publication. He described the excavation of a broch (which he termed a 'Picts' house') at Kettleburn in Scotland. In explicit response to Wilson, he decried:

a tendency among archæologists to ascribe [*the Picts' houses*] to a more remote antiquity than existing data will warrant. Dr. Wilson, for instance, incorporates them in the first section of his recent excellent work, 'The Prehistoric Annals of Scotland', implying that they date from the earliest human antiquity... Now admitting the previous existence in Scotland of an allophylian race ignorant of the metallurgical arts, such as that indicated by the Professor, what evidence is there for assigning the structures in question to a people so low in the scale of civilization? (Rhind 1853: 221–2)

He was clearly following Prichard in doubting whether the Allophylians had left any archaeological record at all. And no English archaeologist went even this far. As we shall see, one English craniologist did mention the Allophylian hypothesis, but was similarly dismissive. English craniology was in its infancy in the 1850s, suffering from a severe shortage of skulls. However, a shortage of skulls had not inhibited the Scandinavian craniologists when they developed their schemes (as we saw in chapter 3). The difference was that they had an accepted *archaeological* structure into which to fit their skulls; and once the skulls' sequence could be established, racial discussion could follow. But in Britain the archaeological chronology was not, as we saw above, being resolved; so a skull sequence was for the time being unattainable.

The two leading English craniologists of the 1850s, John Thurnam and Joseph Barnard Davis, were both medical men. Thurnam's first venture into craniology was his description of a series of Anglo-Saxon skulls from tumuli at Lamel-Hill (Thurnam 1849). He concluded that they were, in Retzius' terminology, dolicocephalic and orthognathous, as expected of a Teutonic population. They differed from earlier skulls; the few he had seen conformed to Nilsson's (1848) definition of the Celtic skull as intermediate between the dolichocephalic and the brachycephalic. He did not consider any non-Celtic groups; the skull from Gristhorpe (the barrow with log coffin mentioned above and dated to the Roman period by Thomas Wright) was of this Celtic type, and the barrow was an 'undoubtedly British tumulus' (Thurnam 1849: 129). The next year Thurnam announced that he was collecting crania with a view to elucidating the skull forms (Thurnam 1850). He published his excavation of a chambered tomb at Uley in 1854, stating that the skulls were dolichocephalic; he mentioned various European finds and even cited Worsaae, but drew no chronological conclusions except to suggest that the Uley skulls might be contemporary with what were later termed hillforts (Thurnam 1854).

Davis displayed a somewhat ambivalent acceptance of the Three Age System—but did not, however, use it to place skulls in sequence, and specifically denied that any skulls might have Allophylian connections. In a paper read to the BAAS, he described a skull as of an 'ancient Brigantian (?) Briton...belonging to the "stone period"'

(Davis 1855: 127). He was thus assuming in advance not just the racial but even the tribal affinity of a Stone Age skull—and a Brigantian tribesman was undoubtedly British, Celtic, and Indo-European. Davis argued against the Scandinavian theory of racial sequence because the sample size was too small and the dating evidence too shaky. The Scandinavians had consequently overlooked variability in skull form:

A prolific source of error consists in overlooking the great diversities of form which present themselves regularly in every family of the European races, and assuming that we shall find the cranial character more stereotyped as we ascend to primitive times. This assumption has probably led men of great distinction, upon slender evidence for the difference of antiquity of certain skulls, to refer them to a succession of races. A pleasing speculation, but more satisfactory grounds seem to be required for its establishment. (Davis 1855: 127, original emphasis)

In fact every period displayed such a wide range of cranial variation that the Allophylian hypothesis could not be sustained. Each population would contain various types, such as:

the *abbreviated*, or strictly *brachy-cephalic*; the *elongated*, or *dolicho-cephalic*; the *elevated*, or, to continue the terms, the *acro-cephalic*; and the *expanded*, or *platy-cephalic*. Notwithstanding these aberrant forms, the whole series bears the impress of so many similar features, as to show that it constitutes one natural group. The dolicho-cephalic has been supposed to indicate an 'Allophylian' or 'pre-Celtic' race, but it may probably be regarded as more properly a family peculiarity in some cases, and accidental in others, in which it has been met with in the same Barrow, and in a position proving the interment to be equally ancient, with a calvarium of the normal form. (Davis 1855: 128, original emphases)

Davis had evidently not understood Nilsson very well, because if there were Allophylians in Europe they were of course represented by the brachycephalic, not the dolichocephalic, skulls. Perhaps he too had fallen victim to the over-complex English presentation of Nilsson's views. The root of his chronological hesitancy he revealed in two subsequent papers. Davis was a committed polygenist who stressed the differences between races, and disagreed with the Prichardian doctrine of progressive development induced by civilization (Davis 1857: 45). All races might very well have used stone tools to

start with; but because of their different intellectual endowments, some races would have continued to use them until a very late period, 'the civilizable races having abandoned them soon' (Davis 1856: 325). How good a chronological marker could stone tools therefore be? This meshed with archaeological conclusions: we saw above that Thomas Wright stated that stone tools were found even in Anglo-Saxon contexts. In a paper read in the USA, Davis revealed just how short a chronology he envisaged: all the skulls he considered he believed belonged to the tribes mentioned by the Roman author Ptolemy, even those from the chambered long barrows (Davis 1857: 41).

English craniology was thus just beginning to feel its way. In the absence of a secure chronology and sequence of skulls, it was impossible to propose any racial sequence along Scandinavian lines. And with influential people like Davis attacking even the principles upon which the Scandinavian craniological system was based, there was no more impetus to seek a craniological than an archaeological chronology.

ENGLISH RETRENCHMENT: BATEMAN, WRIGHT, AND WAY 1846–1860

Thomas Bateman was the most prolific barrow digger of the 1840s and 1850s, and he also made use of craniology. He provides an instructive insight into the English archaeology of the 1840s and 1850s. It has been stated that he pioneered the Three Age System in England (Marsden 1988: 21; Morse 1999: 6; 2005: 116). This is true so far as it goes, which is up to 1852; after that Bateman followed the trend identified above, and his use of the Three Age System actually *diminished* almost to vanishing point during the 1850s.

We have already seen that Bateman gave a paper on barrows on the first evening of the Association's first meeting at Canterbury on 9 September 1844 (Bateman 1845). This offered no hint of chronology. Bateman was not actually present, and his paper was read by Roach Smith, who ended by congratulating Bateman because the paper contained an 'immense mass of facts unalloyed with premature

or ill-digested theories', before adding that some of Bateman's 'Celtic' barrows were actually Romano-British or early Anglo-Saxon (1845: 80). After the split between the Association and the Institute, Bateman remained with Wright's Association, and at its next meeting in Winchester in 1845 gave two more papers in the same vein (Bateman 1846; Bateman and Isaacson 1846). In Bateman's first book there is a short mention of the Three Age System near the start, with the statement that it was 'the Phoenicians who first supplied them [*the British*] with brass weapons... This induced the Britons to discontinue the flint and stone weapons' (1848: 10–11); iron came later still. However, this in no way informs the rest of the book, which is simply a list of excavated barrows, without any chronological dimension. Roach Smith reviewed his book in *JBAA*, once again praising Bateman for simply presenting a huge array of facts, while avoiding the 'prolix theorizing to which antiquarians of past generations were too prone' (Roach Smith 1848: 350).

Bateman's faith in the Three Age System increased up to 1852. As we shall see in chapter 5, Daniel Wilson published his *Archaeology and the Prehistoric Annals of Scotland* in Edinburgh in 1851, a book explicitly organized along Three Age System lines. This book induced Bateman to make his clearest statement—which is indeed by far the clearest of any English writer in the 1850s. This occurred when the Association's annual meeting was in his native Derbyshire. As the expert on the local barrows, he gave a paper stating that 'one of the most satisfactorily ascertained facts seems to be the existence of at least two strongly marked varieties of the human family in these parts, long previous to the times in which the Celtic migrations to the west took place, which have hitherto been considered as the *ne plus ultra* of our inquiries' (Bateman 1852: 211). He believed that both differed from the later Celtic form. Regarding burials, 'we shall probably be correct in assigning to the most remote antiquity such barrows as are found to contain chambers and galleries formed of immense stones' (1852: 211). These contained long-skulled individuals. Smaller round barrows came next, with round-skulled people: 'it hence appears that we may safely place these barrows towards the close of the stone period, when the instruments of flint, stone, and bone, now arrived at great perfection, were about to be superseded by the bronze dagger and celt of the most archaic type' (Bateman

1852: 214). Subsequent users of bronze had similar skulls; after that the sequence was broken, when cremation became the norm. Finally there appeared tumuli containing iron objects, with skulls that ‘approximate much more nearly to the oval form of the heads of the modern race now inhabiting the same country’ (Bateman 1852: 218). Wilson’s influence is clear. Bateman named him only once (1852: 211); however, he used the terms ‘stone period’ and ‘bronze period’ on one occasion each (1852: 214 [quoted above] and 217), and also mentioned ‘boat-shaped’ skulls (1852: 212). As we shall see in chapter 5, this terminology is clearly Wilsonian, and reveals just how much Bateman had drawn from Wilson’s book; and in chapter 6 we will track Wilson’s use of ‘period’ rather than ‘age’ back to Worsaae’s second presentation to the Royal Irish Academy on 14 December 1846.

Thus the basic outline of the Three Age System was present in Bateman’s article—but it was not to last. The next year, the Association met in the neighbouring county of Nottinghamshire, and Bateman again presented an overview of the local barrows—but *with absolutely no mention of chronology* other than the term ‘Celtic’ (Bateman 1853). This was also true of his next (and final) article in *JBAA* (Bateman 1859). His two further books are also revealing. In his catalogue of his own collection of antiquities (Bateman 1855), he lumped stone and bronze artefacts together into the ‘Celtic Period,’ and discussed them before the ‘Roman and Romano-British Period’; the only chronological mention in the entire book was a note (in a reduced font size) about a bronze dagger found with a stone axe:

In these primitive weapons of thin bronze without sockets, usually found in connection with implements of stone, we undoubtedly see the first efforts of the ancient inhabitants of Britain in metallurgy at a time when bronze was so rare and precious as not to be commonly employed for the larger and heavier articles, such as axe heads, though occasionally we find the stone axe replaced by the earliest type of bronze celt. (Bateman 1855: 6)

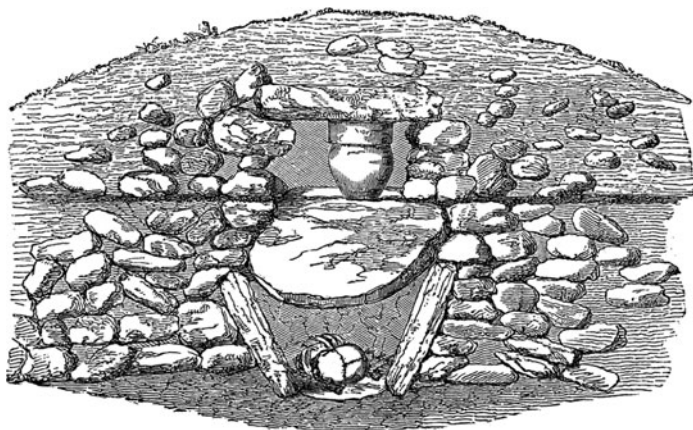
His collection was not dedicated to prehistory, but was a jumble of various materials of all periods. It included such exotica as olive leaves from the Mount of Olives, iron grape shot from the Waterloo battlefield, and the highwayman Dick Turpin’s jawbone. Bateman’s last book was published in the year of his death (Bateman 1861).

After eschewing theory (1861: vi) he did present a few chronological hints. In one case, an earthen barrow was constructed against the side of one containing a megalith; since the earthen barrow was therefore clearly younger, and contained 'a bronze dagger of the most archaic form' (1861: 57), the megalith must be even older—but he presented no finds from it and drew no conclusion. The nearest he came to a chronological conclusion appeared later:

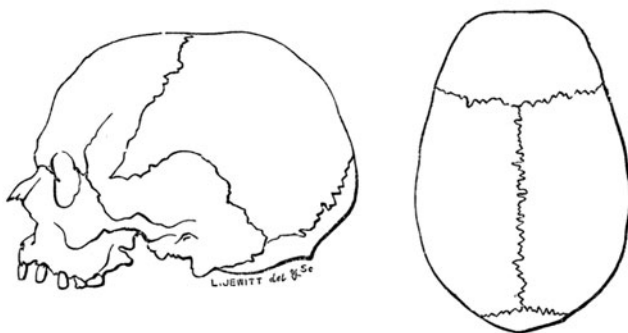
The few stone axes found during our researches have uniformly been associated with the brazen daggers, and were replaced by the plain axe-shaped celt at a slightly later period, but in no other instance have they accompanied an interment by cremation; indeed the instances in which the brass dagger has been found with burnt bones bear so small a proportion to those in which it accompanies the skeleton, that we may conclude there was a marked, though gradual change in the mode of burial introduced about the time when the knowledge of metallurgy was acquired. (Bateman 1861: 155)

Once again, however, the book is entirely a list of barrow excavations not organized along chronological lines. In no publication after 1852 did Bateman even mention a Three Age System period by name. His sole reference to Worsaae was to quote the latter's description of a helmet—not to champion his chronology (Bateman 1861: 32).

Despite his 1852 paper, Bateman appears curiously uninterested in the Three Age System. He may have been a more competent excavator than many in his day—he certainly had more experience than most—and he was aware that not all items in a barrow need be contemporary. He mentioned that during the backfilling of one barrow, his wife had lost a gold ring with a classical onyx cameo. This might, he surmised, cause a future excavator mistakenly to believe that the barrow was of Roman date—'many theories are based on foundations equally fallacious' (Bateman 1861: 79). He published a considerable number of plans and illustrations of his excavations, including one that showed clear evidence of stratigraphic superimposition (fig. 4.12) that a Thomsen or a Worsaae would immediately have recognized as a Bronze Age cremation overlying an earlier inhumation. But the cremation urn in the upper burial contained no bronze objects (although others he had examined had done so), and he drew no conclusions (Bateman 1861: 59–60).



Section of Barrow, Ballidon Moor.



Kumbe-Kephalic Skull, from Longlow.

Fig. 4.12. Top: Bateman's section of a barrow on Ballidon Moor, showing a clear instance of stratification from which he, however, drew no conclusions. (From Bateman 1861: 60). Bottom: Bateman's only illustration of a skull after 1852, one of those from Long Low. (From Bateman 1861: 146).

In craniology he seems similarly uncommitted. He had established a collaboration with Joseph Barnard Davis as early as 1849, when Davis recorded in his diary that he visited Bateman and realized that his private collection contained numerous potentially useful skulls (Stocking 1987: 66). But in his 1852 paper Bateman relied entirely on the cranial sequence from Wilson's book, while Davis (as we saw above) did not accept a sequence of any kind. Bateman did nothing

to fight his ideas through. Despite having a number of skulls from his excavations, he seems never to have measured them; certainly he published no table of measurements. After 1852 he only ever illustrated one skull (Bateman 1861: 146), and this was a thumbnail sketch rather than a technical drawing (fig. 4.12). In the accompanying text he again used Wilson's highbrow term for the boat-shaped skull, 'kumbe-kephalic', but his attribution was supported by no data and appears to have been based either on surmise or on what Davis told him. He did note that while Nilsson placed skulls of this type second in the Scandinavian skull sequence, 'our observations seem to indicate a period more strictly primeval' (Bateman 1861: 147). But this was all he said; there was no further argument of any kind.

What had happened to Bateman after 1852? It is difficult to avoid the impression that he was 'sat on' and advised not to continue publishing in this vein, although there is apparently no direct evidence that this was so. The leading figures in British archaeology certainly remained implacably hostile to the Three Age System through the 1850s. Albert Way, the leading figure in the Institute, followed both the trends identified above. Describing gold objects in 1849, he was prepared to hint that there might have been a time when metals were not in use (Way 1849a: 54). But he was not prepared to date barrows; in the same journal volume he wrote that 'there is, at present, no sufficient evidence on record to enable the archaeologist to assign vestiges of this class [*burials*] to distinct periods, or to classify these sepulchres in accordance with any peculiarity of their construction, or their contents' (Way 1849b: 227). The Institute held its 1856 meeting in Edinburgh, where Daniel Wilson's museum had already been arranged on Three Age System lines for several years (see chapter 5). By this time Way's attitude even towards dating objects had hardened against the Three Age System; when he published the catalogue of items exhibited at the meeting, he stated that 'it has not been thought advisable in the present imperfect state of archaeological classification, to attempt any minute distribution under Periods, which have not as yet been satisfactorily established' (Way 1859: x). Tellingly, his definition of 'early antiquities' was strictly based on negative criteria: they were, he stated, 'with the exception only of such as are of Roman character, all that appear

not properly to be classed with Mediæval objects' (Way 1859: x). His own interests emerged clearly from his preface: after this brief mention of these objects, he immediately launched into a lengthy discussion of portraits of Mary Queen of Scots (1859: xi–xxvii). This mirrored the interests of the Institute as a whole, as indicated by the objects the members contributed to the exhibition at the Edinburgh meeting. Only 51 pages of the 224 in the catalogue dealt with 'early antiquities'; neither the text nor the illustrations in this section contained any chronological element, juxtaposing items of various dates (fig. 4.13 presents an example). The remaining 173 pages, or 77 per cent, covered foreign, Roman, medieval, or Stuart items—including more on Mary Stuart's portraits.

Way's chronology, like Thomas Wright's, was very short. He presented a rare example of good dating evidence, based on an early Welsh legend (Way 1849b). He quoted a cremation burial found by the River Alaw in Anglesey, at a place called 'Ynys Bronwen' or 'Bronwen's Island'. This was, according to legend, where a princess called Bronwen had been buried. Crucially, legend made her the aunt of a person referred to by Roman authors, none other than Caratacus, the king who had resisted the Roman conquest of Wales for many years in the first century AD. The burial on the bank of the Alaw could only be Bronwen's; Way examined and drew the funerary urn it had contained. Because of the identification with Bronwen, he ascribed to it a date of c. AD 50 (fig. 4.14); and in the same article he noted another find of a similar cremation urn that contained a fragment of bronze (1849b: 239). In this way a cremation burial that would now be regarded as a classic Bronze Age find came to be placed in the Roman period—along with, by implication, many similar ones from round the country.

Thomas Bateman was a member of the Association, however, not of Albert Way's Institute. The leading figure in the Association, Thomas Wright, was much more outspoken even than Way; and if Bateman was indeed gagged, Wright must top the list of suspects. Wright's critique of the Three Age System (quoted near the start of chapter 1, above) appears in all editions of *The Celt, the Roman, and the Saxon*, from its first edition in 1852 to the posthumous sixth edition in 1902. In this book, he gave six reasons why the Three Age System did not work (Wright 1852: vii–viii):

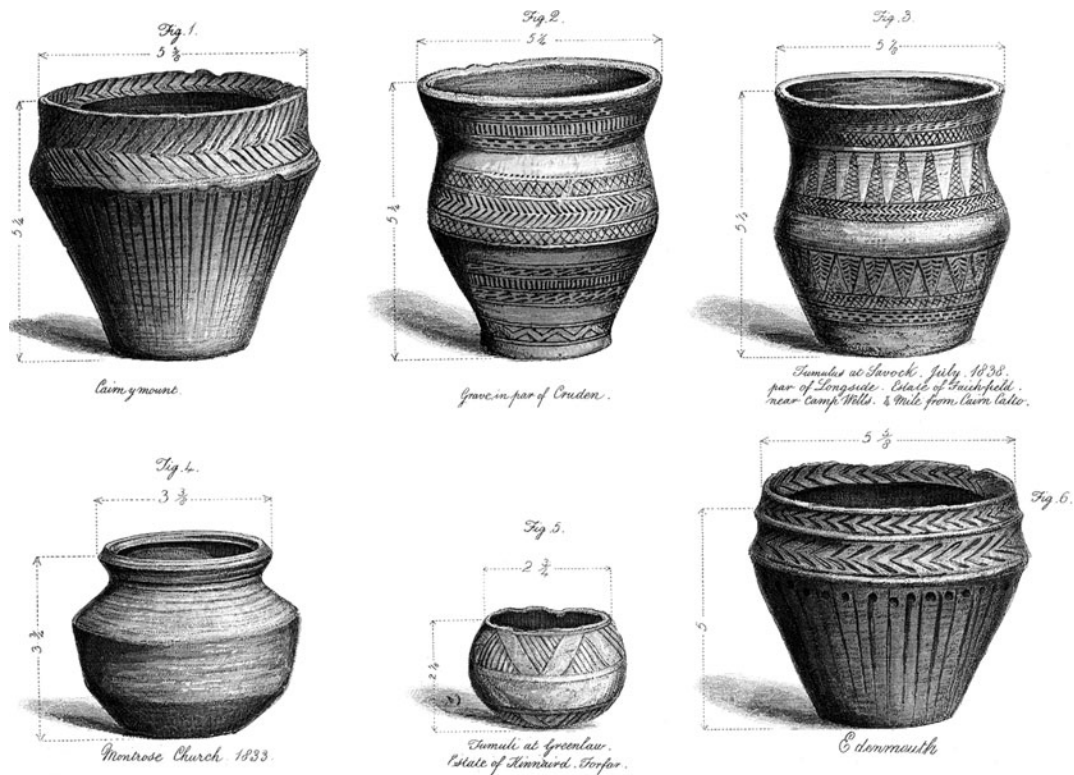
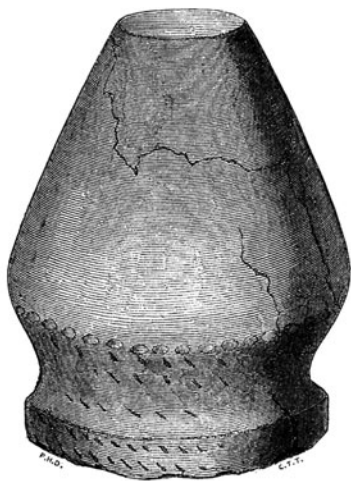


Fig. 4.13. Pots exhibited at the meeting in Edinburgh in 1856 of the Archaeological Institute, dated only to the 'earlier periods'. (From Way 1859: facing p. 11).



**Urn of Bronwen the Fair. Date, circa A.D. 50.
(One-sixth original size.)**

Fig. 4.14. Way's drawing of the funerary urn supposedly containing the ashes of Bronwen, identified in legend as the aunt of King Caratacus, the first-century king referred to by Roman authors. This was the basis for the date of *c.* AD 50. (From Way 1849b: 238).

1. Stone and metal tools were often found together, so there was no chronological separation.
2. Stone would be used where metal was locally unavailable, so the differences were spatial rather than chronological.
3. Bronze was preferred for funerary contexts because it was more precious, so the differences were contextual rather than chronological.
4. Bronze was simply hardened copper used in regions where iron was unavailable, so the differences were spatial rather than chronological.
5. Bronze is an alloy and more complex to produce, and so could not have been used in a period before elementary iron.
6. Iron corrodes away quicker than stone or bronze and could have been more widespread, so the differences were taphonomic rather than chronological.

In 1861 he added two more technological reasons (Wright 1861b: 13):

7. Many of the more sophisticated stone implements were manufactured using iron tools.
8. Many stone tools such as axes, barbed arrowheads, and fish hooks were copies of metal tools.

In support of proposition 1, the most empirical, he mentioned stone tools found in Roman and Anglo-Saxon contexts (Wright 1861b: 12, and see his discussion of the Gristhorpe find, above). He concluded by arguing that the Three Age System classified antiquities in a pseudo-geological character; but his ethnological perspective convinced him that the ethnic origin of their makers was the correct system to use:

The proper, and the only correct, arrangement of a museum of antiquities is, no doubt, the *ethnological* one . . . But people have been adopting a practice of placing flint implements with flint implements, bronze with bronze, and iron with iron, until, forgetting entirely the real elements which give them an individual meaning, they begin to look at them just as if they were so many fossils belonging to such and such geological strata, and thus form systems which are pretty and attractive to look at, but which, in truth, belong only to the imagination. (Wright 1861b: 11)

This was a sophisticated and effective series of arguments, and it is not surprising they were widely accepted. The Three Age System chronology only made sense if some technological overlap between its periods was nevertheless admitted. But for Wright, a stone axe found with a bronze dagger was simple proof that there were no stone or bronze periods, because he misrepresented the Three Age System as a rigid separation of stone from bronze, and bronze from iron.

Thomas Bateman did not attempt to make any headway against the archaeological arguments of Wright or the craniological ones of Davis, but accepted them passively, apart from the occasional hint noted above. Perhaps he felt in 1852 that his paper at the Association required a modicum of theory, despite the fact that Charles Roach Smith had praised him for its absence on two previous occasions. If so, his reliance on Wilson's theoretical structure seems naive, in view

of the opposition of Wright (the leading light in the Association) to the archaeological sequence, and of Davis (his craniological collaborator) to the racial sequence. But after 1852 he never fought his corner; he seems to have been quite happy just to excavate more, and more, and ever more, Derbyshire barrows. Bateman appears to have been, in the very worst sense of the term, an *amateur*. He was no threat to the professional establishment, and his work was never going to lead to the adoption of the Three Age System in England.

In the 1850s English archaeology was thus not creating a chronology onto which the crania could be attached. English craniology was likewise not creating a model of racial replacement which might provoke an archaeological counterpart. But a huge threat to Wright and Davis was, however, to arise almost immediately. It came from a direction neither of them could remotely have foreseen, one that was to drop the early history of humankind deep into the geological past and leave their short chronologies stranded. But before we examine this, we must see what the other major archaeological communities were making of the Three Age System at this time. We will follow Worsaae on his journey through Scotland and Ireland, before returning to England.

Scotland: The Creation of a Nation's Prehistory

Scotland was the part of Britain that adopted the Three Age System most rapidly and completely. This chapter will argue that there were probably two main reasons for this. The first was that there were some intellectual similarities between Scotland and Denmark, and there had for a long time been strong links between the archaeologists of Edinburgh and Copenhagen—far stronger than ever existed between London and Copenhagen. The second was that, like Denmark, Scotland was seeking an identity rooted in its past.

Scotland and Denmark share a number of characteristics. Both were (and are) small northern nations overshadowed by larger southern neighbours, and both were (and are) using their early history to protect their national identities. Although the Romans made inroads into Scotland, neither country had been incorporated into the Roman Empire, so their emergence into history was much more gradual. As scholars looked back through time, there was therefore a more gradual 'greying out' of historical knowledge, rather than an abrupt and brightly lit Roman threshold preceded by darkness. Both therefore had a greater willingness to use archaeological materials to shed light in the 'grey-out'. This is probably one reason for the many archaeological links that had been established between Denmark and Scotland long before Worsaae's visit. One important link between two key individuals worked in a rather different way, however: the friendship between the Norwegian Peter Andreas Munch and Daniel Wilson was partly based on their common mistrust of Copenhagen's mid-century archaeological hegemony. But there were

also differences between Scotland and Denmark, and these also had their effect on the course of events.

The seeking of an identity rooted in the past was in Scotland an endeavour that was potentially fraught with problems. The deposed Stuart monarchy had last invaded Britain only a century before, and many Scots, particularly from the Gaelic-speaking Celtic Highlands, had supported the claim of Charles Stuart, 'Bonnie Prince Charlie', in his bid to become King Charles III. His defeat at the Battle of Culloden did not immediately remove the threat of a renewed invasion, and Jacobite agents remained active in the Highlands for some time afterwards (Maclean 1982). This meant that the invocation of a 'Celtic' past was potentially problematic. Furthermore, many lowland Scots were monoglot English speakers who supported the union with England, and the maintenance of the rule of the Hanoverian dynasty from London. Many lowland Scots had in fact fought *against* Charles Stuart, and their descendants had no wish to champion an identity based on a Celtic past. Scotland in any case did not have anything like the quantity of early documents that would have allowed the construction of an ancient history of the kind Suhm had created in Denmark, or that the Irish would later undertake. Where, therefore, was such a nation to seek a past? Prehistory was not politically loaded, so its physical remains provided an appropriate arena.

THE SCOTTISH–DANISH ARCHAEOLOGICAL AXIS

There were substantial intellectual links between Scotland and Denmark in the later eighteenth and earlier nineteenth centuries. We saw in chapter 3 how the Scandinavians promulgating the Three Age System were ready to accept the grafting of the Four Stage Theory developed in mid- and later eighteenth-century Scotland, onto the emerging system of archaeological ages; this was particularly the contribution of Sven Nilsson (1838–43). In adopting the Three Age System in their turn, the Scots were performing the same grafting operation in reverse.

The Four Stage Theory is a chronological scenario that with modern eyes we would consider ripe to be projected into the prehistoric

period, and rapidly linked with that other branch of the study of early times, archaeology. It may seem strange that this development did not happen rather quickly, and that when it finally did three quarters of a century after its first promulgation, it was first to happen in Scandinavia rather than Scotland. The principal reason for this is that the Four Stage Theory was a means for examining not so much the past, but rather the present. The chief object of political economists and lawyers like Adam Smith, Sir John Dalrymple, Adam Ferguson, and their contemporaries was an understanding of their *own* society and economy, based as it was on property and commerce. One way to place their own society in perspective was to see it as the culmination of a series of developments *towards* the state in which they found themselves. 'It must appear very evident, that property is a matter of *progress*. It requires, among other particulars *which are the effects of time*, some method of defining progression', wrote Adam Ferguson (1767: 124, added emphases). Thus the first three stages of the Four Stage Theory involved not definition by a series of independent criteria, but rather thinking backwards from the present day. Moving back into earlier stages therefore involved the stripping away of more and more contemporary complexities. The commerce of the eighteenth-century economy was clearly the most complex aspect of the economy, and was thus the first to be removed. What remained was fixed property, i.e. agricultural fields; these were therefore what defined stage 3. The stripping away of these fixed assets left only mobile property, namely herds of domestic animals; the ownership of herds was therefore what defined stage 2. This is the reason why pastoralism, that aspect of the Four Stage Theory that appears to modern perspectives to be a particularly 'wrong' interpolation, became accepted as an entire stage in economic progress. Finally, when even such mobile property was stripped away, what was left by default was stage 1, comprising hunter-gatherers with little notion of property at all.

The origin of the eighteenth-century notion of property and commerce was thus the main concern of the Scottish Enlightenment authors—not the examination of past societies for their own sake. Their discussions of the preceding stages therefore remained very brief, such as that from Dalrymple quoted in chapter 3; the main thrust of their books was the functioning of their own society, not

research into previous ones. As far as study of the past was concerned, the Four Stage Theory was therefore entirely theoretical, and in no way constituted the setting of a research agenda. This was just as true for the eighteenth-century Edinburgh archaeologists, who in the limited academic circles of the day could not avoid coming across the Four Stage Theory, as it was for most others elsewhere in Europe. As we have seen, only when Danish archaeology independently produced its own series of stages under the auspices of Thomsen's Three Age System, was there anything tangible to which the Four Stage Theory could be linked. The crucial factor that led to Danish archaeology developing its own chronological stages was, as we saw in chapter 2, the law of *Danefæ*. This ensured that Thomsen was provided with a series of what are today called archaeological assemblages, which provided him with the repeated associations of find types that formed the basis for his understanding of the Three Age System. He was thus supplied with the kind of raw data which Scotland, in the absence of any corresponding law of *Scottefæ*, could never provide for her own archaeologists—Daniel Wilson was later to praise the Danish legal situation but deprecate the shortcomings in Scotland (D. Wilson 1851a: xix–xx). Consequently any archaeological chronology could not be generated internally; it had to be imported.

Scotland did duly import the Three Age System, being by some decades the first part of Britain to do so unequivocally and enthusiastically. Scotland's predisposition, thanks to the propagators of the Four Stage Theory, to think of the most ancient past in terms of a series of phases was undoubtedly a major contributory factor. Thus it was significant that J. J. A. Worsaae extended his 'Viking raid' to Scotland in 1846 (Morse 1999; Wilkins 1961); but this cannot be considered the only or even the major factor. Worsaae also visited London and Dublin; the only public lectures he is known to have given were delivered in Dublin, and put forward the Three Age System; yet both London and Dublin eschewed the Three Age System until much later. Worsaae's avowed main purpose in Scotland was in any case to find traces of the Vikings, not to promulgate the Three Age System, as Briggs (2005) most clearly documents. Why, then, were Scottish archaeologists such willing converts?

The Society of Antiquaries of Scotland (SAS) was founded in 1780, and within three years had its first Scandinavian member, Grímur

Thorkelin (Stevenson 1981). Thorkelin was an Icelandic who was living in Copenhagen, and he obtained royal funding to spend five years in England, Scotland, and Ireland in order to seek out early historical and literary sources relevant to Denmark. One of his most noteworthy pieces of work was the first publication in 1815 of the Anglo-Saxon *Beowulf* epic, the manuscript of which he located in the British Museum (Kiernan 1983). He seems to have found Scotland more to his taste than England. He became friendly with George Dempster of Dunnichen, who was provost of the burgh of St Andrews in 1787; Dempster may have been instrumental in arranging for Thorkelin to receive an honorary doctorate from St Andrews University in that year, making him the first Icelandic to receive a higher degree from a university outside Scandinavia since Sveinn Pétursson the Wise, bishop of Skálholt from 1465 to 1476 (Benedikc 1970).

The reason Thorkelin was so well received in Scotland was connected to that country's search for an historically based national identity. Such a search was not without its potential hazards—in the 1780s it was only a generation since Scotland had made her last military bid for independence—and in the Scottish historical context provided a particular problem: should Scottish historians look to a Celtic or to an Anglo-Saxon past? Neither was particularly satisfactory. To stress the Anglo-Saxon meant admitting that the language spoken by non-Gaelic Scots was nothing more than a dialect of English, but to many in Edinburgh the Celtic alternative was even worse: Gaeldom had connotations of primitiveness, and the Irish origin of the Scotti made them anathema to many Lowland Scots (Cowan 1972). In 1787 John Pinkerton published his *Dissertation on the Origin and Progress of the Scythians or Goths being an Introduction to the Ancient and Modern History of Europe*, in which he derided the Celts as the historical reason for Scotland's status as a cultural backwater. The Celts of his own day, he wrote, were:

not yet advanced even to the state of barbarism; and if any foreigner doubts this, he has only to step into the Celtic part of Wales, Ireland or Scotland, and look at them, for they are just as they were, incapable of industry or civilization even after half their blood is Gothic and remain as marked by the ancients, fond of lies, and enemies of truth . . . For the Celts were so inferior a people, being to the Scythians as a negro to the European,

that, as all history shows, to see them was to conquer them. (quoted by Cowan 1972: 113–14)

But Pinkerton had a way out of the problem: he argued that the shadowy Picts of early historical times *were in fact of Scandinavian origin*. This provided an avenue for Scots to explore that was neither Celtic nor Anglo-Saxon, and by the turn of the nineteenth century it was thought possible by some academics that the lowland Scots dialect might not derive from Anglo-Saxon at all. One man who thought along these lines was John Jamieson, who compiled his *Scots Dictionary* at Grímur Thorkelin's behest; by 1802 he was seriously considering whether the Scots dialect might be a branch of Scandinavian (Cowan 1972: 115). An argument between fictional antiquaries on whether the Pictish language was of Celtic or of Gothic origin appeared in Walter Scott's novel *The Antiquary*, published in 1815. The Scandinavian hypothesis continued to be taken seriously until much later; in Lord Ellesmere's introduction to his translation of *Ledetraad* he stated that both Pictish and the lowland Scots dialect were of Scandinavian origin (Ellesmere 1848: vi); and in a letter written to the Norwegian historian Peter Andreas Munch on 12 October 1852, Daniel Wilson mentioned that R. G. Latham had recently claimed that the Picts were of Scandinavian origin, though Wilson disagreed. But Wilson continued by saying that Latham was 'a sort of Will-o-the Whisp; one is never sure of him, or safe to follow him' (Indrebø et al. 1955: 92–3); and sure enough, by 1857 Latham had changed his mind (Latham 1857: 151–3).

Sir Walter Scott himself had a considerable interest in Scandinavian literature and antiquities, and in 1814 published an abstract translated from the *Eyrbyggja Saga*. The general level of interest in Scandinavian connections led to continued correspondence with Grímur Thorkelin after the latter returned to Copenhagen in 1791. Thorkelin's later career did not see the anticipated number of publications coming from his pen, and the bombardment of Copenhagen by the British fleet in 1807 caused the destruction of his library (Kornerup 1942). He began collecting anew, however, and one of his other activities was the obtaining and despatching to Scotland of lists of books requested by archaeologists there. Some of these volumes could be obtained in Edinburgh, but only at a much higher price:

Robert Jamieson, a friend of Walter Scott with an interest in Icelandic literature, wrote to Thorkelin that the Edinburgh booksellers were 'the veriest Jews in the world: perfect sharks!' (quoted in Cowan 1972: 120). As if to confirm this image, the Edinburgh bookseller William Laing visited Copenhagen in 1799 in order to buy duplicate volumes being sold off by the Royal Library, presumably for profitable resale in Scotland (1972: 120). In 1819 his son David, a leading historian and archaeologist, also visited Copenhagen. Thorkelin had married an expensive wife and suffered continual financial problems (Kornerup 1942), and David Laing bought the library of some 1,500 volumes which he had collected since 1807. This was not for Laing's personal use, however; he sold it on to the National Library of Scotland, where it still remains, forming the nucleus of the Scandinavian collection (Ash 1981; Cowan 1972).

There were various other connections across the North Sea. In 1787 the SAS unexpectedly took delivery of a large runestone from Lilla Ramsjö in Sweden, sent over by Alexander Seton (1768–1828), the 19-year-old son of a Scottish resident of Stockholm. The Swedish connection had arisen because Alexander's father (also called Alexander) was formally adopted in 1785 by his maternal uncle George Seton (1696–1786), who was a merchant in Stockholm. In that same year he (Alexander the father) was elevated to the Swedish nobility. It was intended that the young Alexander should go into the family business but he showed no aptitude for this, preferring to work as an archaeologist. As a young man he became interested in runes, and it was this interest that prompted him to send the Lilla Ramsjö runestone to Edinburgh (Selling 1945). However, this handsome gift caused dismay on its arrival in Edinburgh, because the SAS had run out of money and was in the process of moving into smaller premises, and had no room to house it. The possibility of cutting off the runestone's inscribed face was investigated (Stevenson 1981: 49), but fortunately was not carried out. The stone was later published (J. Stuart 1822), and remains on display in Princes Street Gardens, Edinburgh.

Connections between Denmark and Scotland decreased during the period of hostilities of the Napoleonic wars, but Scandinavian work still informed Scottish writing: Suhm's date of 70 BC for the

immigration of Odin into Scandinavia (see chapter 2) was quoted by Steuart (1814: 136). Closer contact was resumed when peace came in 1815; in that year a gift of nine stone and bronze antiquities was sent to the SAS from Copenhagen (Stevenson 1981). There was a resurgence of contacts in the 1820s. We saw in chapter 2 that the Royal Society of Northern Antiquaries (KNOS) was founded in Copenhagen in 1825, and it established a short-lived newsletter called *Hermod*. Issue 1 (January 1825) listed the sixty founding members, all Scandinavians; Grímur Thorkelin was one of them. Issue 2 (April 1825) listed a further twenty Scandinavians, and also the first foreign members; the first British member (the eighty-third member overall) was recorded as none other than ‘Walter Scott, Baronet, i Edinborg’. Just two entries after him came another foreigner whom we have already encountered, and who was to play an active role in the later 1840s, namely the misspelled ‘J. Grim [*sic*], *Dr. Philosophiæ*, Bibliotekar i Cassel’ (*Hermod* 2, April 1825: 30).

The curious figure of Alexander Seton re-emerged in the 1820s. He worked actively in both Scottish and Swedish archaeology until his death in 1828. Shortly after despatching the rune stone to Edinburgh in 1787, he had developed what was termed a ‘nervous fever’, and took to wandering restlessly but purposelessly around the Swedish countryside. Suspected of insanity, he was sent back to Britain in 1794 on the grounds that the change of air might do him good; and there he spent the next thirty years. In that time he travelled a lot and undertook a variety of archaeological work, including cleaning lichen off the Ruthwell Cross and transcribing the early Christian inscription on the Catstane near Edinburgh (Selling 1945; Stevenson 1981: 65). He described this work in letters to Swedish archaeologists, which he wrote in rather garbled Swedish, and his drawing of the Catstane inscription was published in volume 3 of Nils Henric Sjöborg’s *Samlingar för Nordens Fornälskare* (1830: 114, fig. 76), reproduced here as fig. 5.1. He also made two papier mâché casts of the inscription; one of these he gave to the Royal Academy in Stockholm (Selling 1945: 11), the other to the SAS in Edinburgh (Stevenson 1981: 65). *The Times* of 28 December 1827 reported the donation by Seton of one of the earlier volumes of Sjöborg’s book to the SAS. Seton also excavated the tumulus at Machrihanish in Argyllshire, his report appearing posthumously

Fig. 76.

Fig. 5.1. Drawing of the Catstane near Edinburgh, made by Alexander Seton and sent to Sweden where it was published. (From Sjöborg 1830: pl. 26, fig. 76).

(Seton 1831).¹ Seton returned to Sweden in 1825, where in the three years until his death in 1828 he excavated frenetically at a variety of sites including the major trading emporium of Birka, outside Stockholm. The Swedish historian Erik Gustaf Geijer (whom we came across in chapter 2) wrote of him in 1826 that ‘the man struck me as somewhat odd, rather like someone who does not properly have *all* his screws in place—he is full of schemes of all possible and maybe also impossible kinds’ (quoted in Selling 1945: 19–20*, original emphasis).

There were also more mainstream connections in this period. At a meeting of the SAS in 1828, Robert Bald described Thomsen’s

¹ Stevenson (1981: 65) states that the Alexander Seton who excavated Machrihanish was the *son* of the rune stone donor; Selling (1945), however, makes it clear that the excavator of Machrihanish and the runestone donor were one and the same man.

museum in Copenhagen. His talk was attended by eighty-three people, an unusually large contingent; he can hardly have avoided describing the museum's arrangement into stone, bronze, and iron, although the text of his talk was never published and does not survive, and the brief minutes that were kept do not mention it. A couple of years later several Danish archaeologists became honorary or corresponding members of the Society (Stevenson 1981: 70). The Society's fortunes declined somewhat in the 1830s but Crown Prince Frederik of Denmark was made an honorary fellow and visited Edinburgh in 1844, bringing a gift of antiquities (Stevenson 1981: 76). At the Society's meeting in February 1846, J. M. Mitchell read a paper on Scandinavian archaeology, but once again there is no information as to what he actually said (Stevenson 1981: 78).

There were thus connections extending back some sixty years when Worsaae arrived in Edinburgh in 1846, just two years after the visit of the Crown Prince and only four months after Mitchell had read his paper, and this undoubtedly facilitated his visit.

WORSAAE'S TRAVELS IN SCOTLAND

Immediately he arrived in Edinburgh, Worsaae realized that he had come to a country that was interested in its own most ancient past. Consequently he felt intellectually much more at home than he had in London. He recorded in his memoirs that:

I soon noticed that there was a different and more free archaeological and historical atmosphere here than in England, where interest in the classical and the foreign were still completely predominant in archaeology, and where for the earlier historical periods the Anglo-Saxons were unjustly praised at the expense of other peoples. The patriotic Scots, on the other hand, who could not forget their country's ancient independence, sought in every way to emphasize and preserve what was distinctly Scottish. Since they thus felt anything but fondness for the Anglo-Saxons, and since they knew full well the many contacts between the Scandinavians and especially the Norwegians and the Scots in northern and western Scotland, I found among them willing ears for an unhindered understanding of the significance of the Danish-Norwegian elements in the British Isles. (Worsaae 1934: 146-7*)

His reaction to the Society's collection of antiquities revealed his frustration with the fact that it was not organized along Three Age System lines, but also emphasized that his chief purpose in visiting Scotland was to find archaeological traces of the Scandinavian presence there:

In contrast to the larger British Museum in London, where the richest archaeological treasures have been collected at immense expense from abroad, but not from England itself, I found in Edinburgh an incomparably smaller, rather badly organized, but for me much more informative museum for national antiquities found in Scotland. Among the typical definitively Scottish antiquities, which just like the English ones showed characteristic differences from those of Scandinavia, I immediately discovered several undoubtedly pure Scandinavian weapons and ornaments, which sure enough had been found in Danish northern England and mainly in the Norwegians' old colonies on the Scottish coasts. (Worsaae 1934: 147*)

Worsaae met most of the Scottish archaeological establishment, though there is little record of their discussions either in Scotland (Stevenson 1981) or in Worsaae's memoirs. As we have seen, various reports on Scandinavian archaeology had been presented at the SAS in previous years, so the Copenhagen museum arrangement into stone, bronze, and iron was familiar in Scotland even before Worsaae's visit (Stevenson 1981: 79). Despite this, Worsaae was not overly impressed with the people he met. Some of them he seems to have found positively quaint; after travelling round Scotland he returned to Edinburgh in October 1846, where:

I had the great pleasure of being introduced to a true antiquarian of the old school, Mr Kirkpatrick Sharpe. He belonged to the type of active but muddled and unscientific dilettantes, who in the development of every science almost always carry out the first steps by collecting the necessary materials for subsequent more serious research. He was a crusty bachelor who wore old-fashioned clothes and stiff boots, and lived alone in a big old house, which from cellar to attic was so crammed with all kinds of antiquities that one could hardly move about in any of the rooms. (Worsaae 1934: 165*)

In evaluating Worsaae's impact in Edinburgh, we must first consider his trip itself, and what he was trying to do. His primary purpose was to locate items of Scandinavian origin and relevance (Briggs 2005);

this was his own main interest, and it was after all what he was being paid to do. His primary focus was therefore not particularly directed at Edinburgh itself, and he spent remarkably little time there. Graham-Campbell (2004) has established that there were not many Viking objects in Edinburgh for him to examine, so he had little incentive to stay long. His published letters to his mother (Worsaae 1934) allow the dates of his travels to be reconstructed reasonably precisely. After leaving London on about 19 June, he reached Edinburgh late in that month after short stays in York and Newcastle. He departed for Glasgow on 6 July, so this first stay in Edinburgh amounted to some ten days at most. He spent about a fortnight at Fairlie House, the residence of the Parker family just outside Glasgow. The Parkers had a yacht, and it turned out that a party of six young women was staying at a neighbouring villa; Worsaae spent a lot of time in their company, noting that he was the only young man in the whole group, and that consequently his English rapidly became completely fluent. The Parkers' yacht took him as far as Portree on the Isle of Skye, which he reached on about 28 July. From then on he travelled by himself, though armed with numerous letters of introduction to the Duke of Sutherland's various agents and factors in the Highlands; he found he did not much care for either oatbread or whisky.

The first week in August Worsaae spent on the Isle of Lewis; throughout his trip he had been particularly interested in brochs, because, as he wrote to his mother, 'they are possibly of Scandinavian origin' (Worsaae 1934: 297*). On Lewis his obsession with brochs was such that a rumour spread among the local population to the effect that Worsaae was the Prince of Denmark, travelling incognito to see whether the brochs could again be put into a state of defence, preparatory to a new Danish invasion of the Highlands. From Lewis he returned to the mainland, spending about a fortnight crossing to Dunrobin Castle, the Duke's residence on the east coast. After a couple of days there, when he made direct contact with the Duke for the first time, he travelled by steamship to Orkney, which he reached about 21 August. He returned to Dunrobin about 4 September, and stayed for about three weeks. During this stay, the Duke provided him with a labour force and encouraged him to excavate a nearby broch. He also rode out to see archaeological sites; after one

twenty mile excursion he returned, famished, to a formal dinner with the Duke and Duchess, describing how after this everyone repaired to the servants' hall and danced reels—'it was the greatest fun to see the mix of people. Girls, servants, the Duke's sons and daughters, sailors from the yachts—all whirling about dancing to the bagpipes' (Worsaae 1934: 310*).

He travelled on to Aberdeen, which he reached on 28 September, meeting in the coach an unnamed British naval officer who turned out to be a member of the KNOS.² He had intended to return to Edinburgh but instead met a Mr Chalmers, and spent no less than three weeks staying with him at his residence of Auldbar Castle near Brechin. Chalmers was an archaeologist with an excellent library, and Worsaae spent much time reading books which, he wrote to his mother, he would otherwise have had to read when he reached Edinburgh. This indicates clearly that he did not seek to maximize the time he spent in the capital. This may have been for financial reasons, because he did not know until he reached Edinburgh that the remainder of the funds for his journey had been forthcoming; but he could probably have found accommodation in Edinburgh with someone in archaeological circles and avoided the expense of lodgings. At all events, he finally arrived back in Edinburgh on 28 October; on the evening of 11 November he arrived in Dublin, so his second stay in Edinburgh can hardly have been longer than his first. His two visits to Edinburgh thus amounted between them to no more than three weeks at most.

Worsaae's trip around Scotland was evidently hugely rewarding. The book he wrote on the antiquities he saw came out in 1851 (Worsaae 1851), and in English translation the year after entitled *An Account of the Danes and Norwegians in England, Scotland and Ireland*. By then he had concluded that the brochs were indeed of Pictish or Celtic rather than Scandinavian origin, because nothing like them was known in Norway, but similar ones were common in areas of the Highlands and Ireland that had not been penetrated by

² The membership list of the Royal Society of Northern Antiquaries for 1846 includes a couple of potential candidates. Captain W. Cameron-Mouat of Bressay, Shetland, is perhaps the most likely, although there is no indication whether his rank was naval or military. Alternatively he might have been Captain George Mansell of London, who is listed as a naval officer.

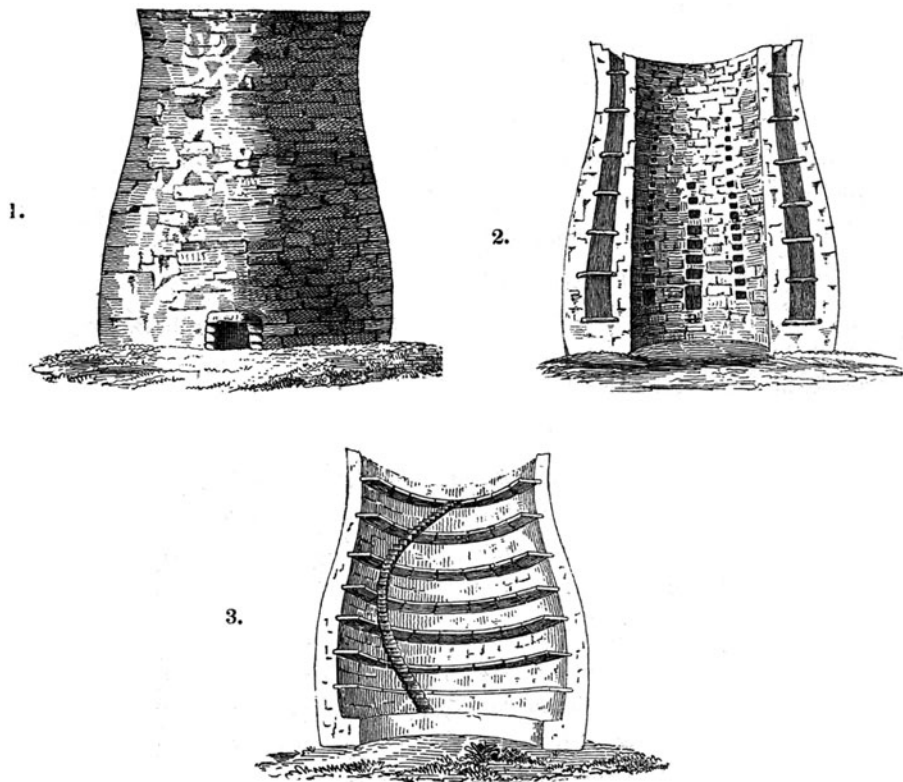


Fig. 5.2. Worsaae's depiction of the Broch of Mousa, Shetland. He never visited the broch, and recognized it to be of Celtic or Pictish origin rather than Scandinavian. Its Scandinavian interest derived from its having been occupied by various Vikings recorded in sagas. (From Worsaae 1851: 297).

the Vikings (Worsaae 1851: 295). But they still interested him because many had been taken over and occupied by the Vikings. He illustrated the broch of Mousa in the Shetland Islands (fig. 5.2), which of course he had not visited. Happy to make use of legendary history when it suited him, he emphasized their Scandinavian connections by quoting mentions in the sagas: Mousa he identified with the 'Mósey' mentioned in various accounts, and he recounted the story of how Björn Brynjulfsön and Thora Roaldsdatter eloped and

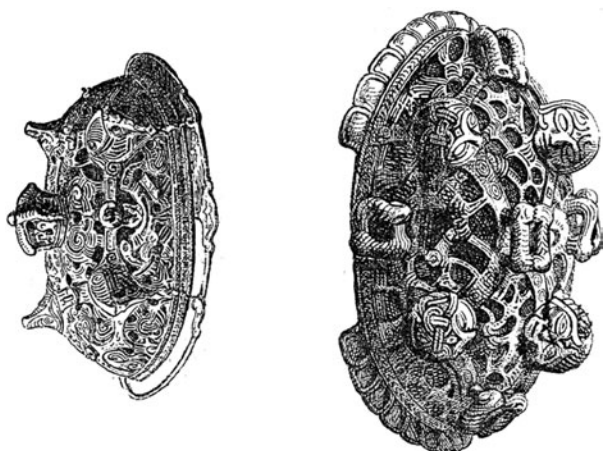


Fig. 5.3. Left: Worsaae's depiction of a bronze bowl-shaped brooch from a Scottish grave; Worsaae identified it as of Viking origin. (From Worsaae 1851: 320). Right: a similar object in the museum in Copenhagen, from Worsaae's first museum catalogue. (From Worsaae 1854: fig. 337).

spent the winter there before fleeing further to Iceland. Two centuries later, Earl Harald Maddadsön besieged the place after his mother took refuge there when eloping with Erlend Junge; but he failed to capture it. Worsaae illustrated almost solely items of Scandinavian interest, including various carved stones and the cathedral of St Magnus in Kirkwall. He also depicted bronze brooches found in funerary contexts, noting that they were identical to examples found in Viking graves in Norway and Sweden (fig. 5.3). He stated (Worsaae 1851: 320) that such brooches were found only in the areas of Scotland settled by the Vikings, never in other areas or in England; but Graham-Campbell (2004: 216) points out that the SAS had acquired one from Yorkshire in 1836, which Worsaae cannot have seen.

There is no particular sign that Worsaae seriously promulgated the Three Age System on his short visits to Edinburgh. He paid much less attention to prehistoric remains than to Viking ones. He wrote to Thomsen from near Cape Wrath on 13 August that 'there are some really interesting things in Edinburgh, but there is not one single man there who has more than a moderate interest in improving the collection' (Worsaae 1934: 304). But had he spent a little longer in

Edinburgh he might have met a man just five years older than himself, a man who had joined the SAS as recently as February that year, and who was in the next few years to do precisely what Worsaae thought would not happen: reorganize the museum along Three Age System lines. That man was Daniel Wilson; but Worsaae and he were never to meet (Ash 1999: 71).

THE DEVELOPMENT OF DANIEL WILSON'S 'IDEA OF PREHISTORY'

Although they did not meet, Worsaae's visit may well had a major effect on Wilson, and encouraged him to undertake the rearrangement of the Society's museum along Three Age System lines in 1848, after Wilson rather abruptly started taking an interest in prehistoric archaeology just a couple of years later. But had they met in 1846 they might have had rather little to say to each other, because Wilson's interests at that time were exclusively devoted to the later historical periods; and as we shall see, another visitor from Scandinavia may have played a more important role in the later development of Wilson's views.

Wilson had for some years moved in the literary and archaeological circles of Edinburgh, and was a protégé of Robert Chambers (Kehoe 1998). The interests of the Chambers brothers ranged widely across literary and historical subjects (W. Chambers 1872), and Robert was a prolific author. It was probably his stimulus that led Wilson to write a two-volume work entitled *Memorials of Edinburgh in the Olden Time*, which appeared in separate parts from 1846—the year of Worsaae's visit—and in book form in 1848 (Ash, Cruft, and Hulse 1999). Chambers' interests however extended much wider than these conventional subjects. He was interested in geology, and undertook trips round the British Isles and Scandinavia examining raised beaches, on which he published a book in 1848 (W. Chambers 1872: 274–6). He was also the anonymous author of a book that was to cause a major controversy through the 1840s and 1850s, *Vestiges of the Natural History of Creation* (R. Chambers 1844), proposing a form of biological evolutionism from primitive to higher forms of

life. It contained a chapter on the early history of mankind, espousing a Prichardian combination of racially distinct features and locally induced variation, coupled with the argument that development proceeded from the barbarous to the civilized; this included development through the various racial types culminating in the Caucasian (R. Chambers 1844: 306). Very few people knew who had written *Vestiges*, and Wilson was not one of them (Secord 1994: xxxviii–xxxix). *Vestiges* made no mention of archaeological evidence, and Wilson did not refer to *Vestiges* in *Prehistoric Annals*. But he cannot have been unaware that Robert Chambers was among the people suspected of authoring *Vestiges*, or that that book championed a radically new means of considering and ordering the past.

For some time after Worsaae's visit, Wilson's interests were, however, dominated by medieval Edinburgh. He was concerned about the destruction of historic buildings, and when the North British Railway Company decided to knock down Trinity College Church and replace it with a shunting yard, he was part of the campaign run by the SAS to save the building. This issue 'dominated Wilson's life in late 1847 and early the following year' (Ash, Cruft, and Hulse 1999: 47); but it was to no avail, and Wilson described how the church was demolished (D. Wilson 1878: II, 10–15). Built in 1460, it was thought to contain the grave of its founder, Mary of Gueldres, the widow of King James II. During the demolition of the church, a grave was duly found on 22 May 1848 (D. Wilson 1878: II, 15), and Wilson made a cast of the skull of the female it contained—his first venture into craniology. Consternation followed on 20 September the same year, when another grave was found, equally likely to be Queen Mary's. Wilson was having dinner with David Laing when the news arrived; Laing in due course accepted the new skeleton as that of Queen Mary, but Wilson believed it to be a later burial and maintained his support for the first find (1878: II, 22–5). Wilson had more fortune in 1849, when his campaign to save John Knox's house from destruction was successful (Ash, Cruft, and Hulse 1999: 48–50).

Wilson became one of the secretaries of the SAS in November 1847 (Ash 1999: 70; Stevenson 1981: 78–9). It is likely that his interest in the archaeology of the earliest times developed only now that he had some responsibility for the museum and had decided to produce a guide to the collections. His *Synopsis of the Museum of the Society*

of *Antiquaries of Scotland* was published in 1849, and marks the rearrangement of the SAS's museum along Three Age System lines (Ash 1986: 44). The first case contained stone artefacts from Britain and Ireland. The second contained the stone tools Crown Prince Frederik of Denmark had brought as a gift in 1844, as well as some from the Americas and the South Seas (Ash 1981: 103). The museum aroused an increasing amount of interest; in 1850 it was visited by over 9,000 people, while on New Year's Day 1851 alone it had 1,330 visitors (Ash 1981: 106). The museum display must owe some of its inspiration to Worsaae's visit, the culmination of a couple of generations of intellectual contact with Scandinavia, as well as to the publications of the Three Age System that were at last beginning to appear in English: the *Ledetraad* volume containing Thomsen's chapter in 1848, and the translation of Worsaae's book the year after (Ellesmere 1848; Worsaae 1849d).

Wilson's major book, *The Archaeology and Prehistoric Annals of Scotland* followed two years later (D. Wilson 1851a). The book is universally and justly considered important for what it contains (see the next section); but it is also remarkable for what it does *not* contain. As we saw above, when considering the most ancient past of Scotland, scholars faced the dilemma of whether to emphasize ancient histories based on the Anglo-Saxon, or the Celtic; or whether indeed to opt for the Scandinavian solution via the Picts. Wilson chose none of these, and launched his book with an attack on the usefulness of ancient history in the Scottish context. The first words of his introduction made this point: 'History which is derived from written materials must necessarily begin only where civilization has advanced to so ripe a state, that the songs of the bard, and the traditions of the priest, have ceased to satisfy the cravings of the human mind for mastery over the past and the future' (D. Wilson 1851a: 1). All the earliest writings inevitably contained much later material, and were consequently 'the wildest mixture of myth and legendary fable' (1851a: 12); sorting out what might be reliable from what was not, was unfeasible—to 'recover the pure thread divested of all its extraneous acquisitions, is the impossible task of the historian' (1851a: 12–13). Some Roman references were useful but they were very sketchy; for a long time after that all was confusion. Even in the era when other nations were seeing the rise of chroniclers, Scotland

remained poorly served, her history subject to 'even more obscurity than that which clouds the dim and fabulous morning of most nations' (1851a: 13). For Scotland, Wilson concluded, reliable history could scarcely be written for any period before the time of King Malcolm Canmore in the late eleventh century.

In making these points Wilson was not necessarily doing anything very new. Ash (1999) notes that two books published during the eighteenth century had done serious damage to the claims of Scottish historical sources to reach very far back. The first was Thomas Innes' *Critical Essay on the Ancient Inhabitants of the Northern Parts of Britain or Scotland* of 1729. This was followed by Lord Hailes' *Annals of Scotland from the Accession of Malcolm Canmore to Robert I of 1776*, which comprised a 'devastating critique of some of the most dearly held historical beliefs of the Scots' (Ash 1999: 66). Claimed early sources were also falling under suspicion in another way. The poems of Ossian were published in the 1760s, and claimed by their composer James Macpherson to be translations of works by a previously unknown bard who had lived in the third century AD. Initially lionized and widely translated into other European languages, it was by the end of the century generally accepted that, whatever their literary merits, they were forgeries and historically useless (Groom 2002). Wilson was thus operating in a historical context that was not able to write much ancient history. He mocked the earlier historians who had claimed that much documentary material had once been available, but had been destroyed or carried off by the pillaging English, particularly the armies of Edward I and Oliver Cromwell:

Edward and Cromwell both contributed a helping hand to the obscurity of Scottish history, in so far as they carried off and destroyed national records which could ill be spared. The apology, however, has been worth far more to maundering manufacturers of history than the lost muniments were ever likely to have proved. Not a few of these irrecoverable national records, so long deplored, it begins to be shrewdly suspected, never existed in the first place. (D. Wilson 1851a: 14)

Wilson thus did not face a robust opposing chronology that had to be vanquished, and, beyond these statements in his introduction, did not need to attack ancient history in the way Worsaae had done in Denmark. The absence of ancient history with any great time depth is

a major difference between Scotland and Denmark, and also (as we shall see) between Scotland and Ireland.

Since ancient history in Scotland could be written off, the early periods were a void. There was, argued Wilson, only one source of evidence that could fill the void: *prehistoric archaeology*. By cutting ancient history down to size, Wilson cleared the arena in which prehistoric archaeology was to operate. He saw archaeology as nothing less than a replacement for ancient history. This is revealed not least by the oxymoronic title of his book: an ‘annal’ is, after all, an entry in a written chronicle, which is by definition historical. A ‘prehistoric annal’ is thus an impossibility—except in the terms envisaged by Wilson. Archaeology might in due course develop the means to crack the code of the archaeological record, and decipher the ‘prehistoric annals’ that material culture comprised:

This point it is at which the modern archaeologist now directs his inquiries, not altogether without the anticipation that these same primitive arts, the product of the beginning of things, may also prove to contain a decipherable alphabet, which may be resolved into definite phonetics, and furnish the key to many inscriptions no less curious and valuable than the parchments of medieval charter-chests, or even the tablet of Abydos and the Rosetta Stone. (D. Wilson 1851a: 16)

Unlike almost all of his peers, Wilson had in fact witnessed and interpreted a long stratigraphic sequence, that at Castlehill in Edinburgh, during construction works in 1850. Under the eighteenth-century buildings were seventeenth-century coins, then the city wall of 1450, and then, under a layer of clay, a ‘mass of decayed animal and vegetable matter’ (D. Wilson 1878: II, 136) which contained a silver coin of Constantine. Below this again were two log coffins which Wilson compared to those from Gristhorpe and Bjolderup, which we have already encountered in chapter 4. Wilson described watching the uncovering of the successive layers as ‘like turning over the leaves of an old chronicle’ (1878: II, 135), and this may have prompted his metaphor of a ‘prehistoric annal’. The beginnings of the decipherment process were of course those provided by the Three Age System; Thomsen’s creation of this was ‘justly esteemed the foundation of archaeology as a science’ (D. Wilson 1851a: 18).

Wilson thus had a breathtakingly clear 'idea of prehistory', something nobody south of the border was to attain for well over a decade (except for Bateman's brief loan of it from Wilson). Consequently it was entirely natural that Wilson was also the first person to use the term 'prehistoric' in the English language—for which he is justly celebrated (e.g. Chippindale 1988; Clermont and Smith 1990; Daniel 1964).

But did he invent the term entirely independently? He always claimed so: in the second edition of the book, he wrote of 'the term *Prehistoric*—introduced, if I mistake not, for the first time in this work' (D. Wilson 1863: xiv, original emphasis); and this has been widely accepted. Clermont and Smith (1990: 98–9), however, point out that the word *préhistorique* was published by the French scholar Gustave d'Eichthal in 1845, but leave open the question of whether Wilson (consciously or otherwise) might have derived it from this source; there is no indication that Wilson knew of d'Eichthal's work, but the possibility cannot be ruled out.

There is, however, another possibility. We saw in chapters 2 and 3 that the adjective *forhistorisk* (Swedish *förhistorisk*) was in increasing use in Copenhagen and Lund from 1833 onwards. Given the general connections between Scottish and Danish archaeology, and in particular Wilson's adoption of the Three Age System, we must consider whether the word itself may also have been derived from Scandinavian precursors. None of the Danish or Swedish instances was, however, translated into English; 'prehistoric' appeared neither in Ellesmere's translation of Thomsen, nor in Worsaae's book (Ellesmere 1848; Worsaae 1849d). Wilson could not read the originals. If he acquired the term or the concept from Scandinavia, it would have to be in conversation with an English-speaking Scandinavian.

Worsaae is the natural first suspect, but he can probably be discounted. He did use the word *forhistorisk* in 1846, the year of his visit to Scotland, in his book on the antiquities of Blekinge (Worsaae 1846a). We know that this book had appeared before his visit, because it was while presenting a copy of it to King Christian VIII that his trip to Scotland was first mooted (see chapter 1). But there is no indication that he took a copy to Britain with him; he did present a copy of his Danish book of 1843 to the SAS (Ash 1981: 101), but this does not contain *forhistorisk*. And as we have seen, Wilson and

Worsaae in any case did not meet, and Wilson's interests had yet to turn to matters prehistoric.

A much more likely candidate is, however, available: the Norwegian historian and philologist Peter Andreas Munch, who visited Edinburgh in 1849. We have already encountered him in chapter 3, as one of Worsaae's opponents in the pamphleteering debate that preceded and accompanied the war of 1848–51. Worsaae was making frequent use of *forhistorisk* in these, which Munch read. Munch's own papers of the period were more focused on philology and etymology than on prehistory, but he did himself use the word *forhistorisk* at least seven times in papers up to 1849. Two of these were in the 1846 articles that initially drew Worsaae's opposition (1846a: 21 n.; 1846b: 237); the other five were in two pamphlets published in Christiania (as Oslo was then known) in the year of his visit to Scotland (Munch 1849a: 25; 1849b: 3, 32 [twice each]).

Wilson and Munch became close friends during the latter's visit. The letters Munch received from Wilson have been published. From one Munch wrote to George Stephenson (a specialist in runic writing then living in Stockholm) it emerges that his stay in Edinburgh lasted from about 24 October 1849 to 2 January 1850 (Indrebø and Kolsrud 1924: 410–12), much longer than Worsaae's brief appearance. Wilson's letters to Munch mentioned 'your visits to our fireside, which both the good wife and myself look back to with no little pleasure' (11 January 1850; Indrebø and Kolsrud 1924: 401). These fireside evenings certainly involved discussions of matters that subsequently appeared in *Prehistoric Annals*; on 11 January 1850, just a few days after Munch left Edinburgh, Wilson wrote to him asking for clarification of his views that the Stones of Stenness were pre-Scandinavian; and he also asked for the Scandinavian folk names for flint artefacts (Indrebø and Kolsrud 1924: 403). He received both; in his book he quoted Munch's letter about Stenness (D. Wilson 1851a: 112 n. 1) and gave the Scandinavian terms without attribution (1851a: 124–5).

Antiquities found in Scotland had often been referred to as 'Danish' since the seventeenth century (Graham-Campbell 2004: 204). Wilson criticized the label 'Danish' as 'one of those convenient words which so often take the place of ideas and save the trouble and inconvenience of reasoning' (Wilson 1851a: xv). His stress on

native manufacture and construction, coupled with his emphasis that the Vikings in Scotland were Norwegian not Danish (1851a: 522), placed him very much on Munch's wavelength. Like Munch, he was thus in opposition to Worsaae, who despite recognizing that the Scottish Vikings were Norwegian, still sometimes slipped into using the adjective 'Danish' to describe Scottish antiquities (Graham-Campbell 2004: 217). On 12 October 1852, Wilson wrote to Munch that 'we must have you back to Scotland; and back you up with Earls and Dukes, that we may show how different a contribution you will give to our History, Antiquities, and Ethnology, from what Worsaae has done, with all their aid' (Indrebø et al. 1955: 92). On 30 January 1851, when the publication of *Prehistoric Annals* was imminent, Wilson wrote to Munch that he had considered dedicating the book to his Norwegian friend, but decided not to because 'I have made so many attacks in it, not only on our own native theories of a Danish origin for our antiquities, but also on some directly traceable to Copenhagen, that I thought it would be a questionable compliment' (Indrebø et al. 1955: 5).

Given the close friendship between the two men and the close alignment of their views, it seems certain that their discussions must have helped to firm up Wilson's 'idea of prehistory', and there is every chance that they may have considered the need for an English equivalent of *forhistorisk*. To Wilson himself must of course go the credit for coining and using the English term; but the coincidence of his long discussions with Munch, himself a routine user of *forhistorisk*, gives grounds for the suspicion that at least the *idea* of the word came from Scandinavia.

THE ARCHAEOLOGY AND PREHISTORIC ANNALS OF SCOTLAND

The magisterial *The Archaeology and Prehistoric Annals of Scotland* was published in 1851 (D. Wilson 1851a). Following strict Three Age System lines, it was divided into Part I, the Primeval or Stone Period; Part II, the Archaic or Bronze Period; Part III, the Teutonic or Iron Period; and Part IV, the Christian Period. In using 'Period' rather

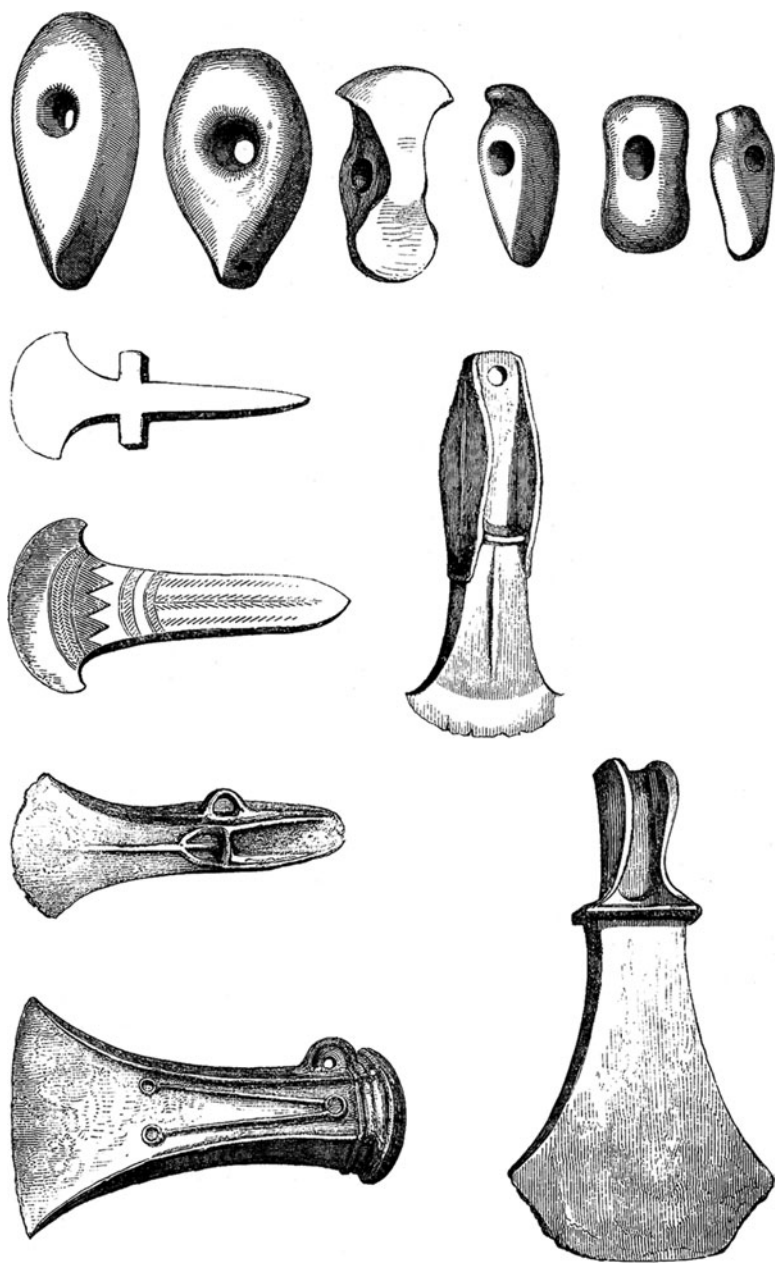


Fig. 5.4. Top row: Wilson's illustration of stone axes and hammers, in the Stone Period section of *Prehistoric Annals*. Rest: bronze axes in the book's section on the Bronze Period. (From Wilson 1851a: 135, 253–7).

than 'Age', Wilson was following the terminology of Worsaae's Irish lectures and later book (Worsaae 1846c; 1849d). Worsaae's book was, however, a rather brief introduction to the Three Age System. Wilson's was the very opposite, running to more than 700 pages; part I on the Stone Period alone contained 170 pages, longer by itself than Worsaae's entire book. The major sections on the Stone, Bronze, and Iron Periods each went through a series of topics such as burials, dwellings, weapons and implements, vessels, personal ornaments and so on. The illustrations reflected this—stone axes were illustrated in chapter VI of Part I, while bronze axes were illustrated in chapter IV of Part II (fig. 5.4). Both of these chapters were entitled 'Weapons and Implements,' each covering the relevant items of just their period. This contrasted greatly with the contemporary English practice of placing all axes in the same illustration, regardless of material (cf. fig. 4.11).

Wilson's 'idea of prehistory' led him to envisage a much longer chronology than did the archaeologists south of the border. A good example of this is his treatment of gold armillae or arm rings. Albert Way had discussed various examples including an ornate spiralliform example from England (fig. 5.5 top) that he believed to be of Anglo-Saxon date (Way 1849a: 51). He also depicted another (fig. 5.5 middle), which he considered unique and of uncertain date: it was not evidently Roman; he was reluctant to call it 'Danish' because of its good quality; and he was averse to calling things 'Celtic' just because they appeared to fit nowhere else (Way 1849a: 54–5). This left few options other than the same post-Roman date and native origin as the English specimen, although Way did not commit himself. This second example, however, came from Largo in Scotland, and hence was to fall under Wilson's purview. He reproduced Way's illustration of it (D. Wilson 1851a: 321), and also showed a similar one from Rannoch (fig. 5.5 bottom), which he dated typologically, regarding it as 'exhibiting unmistakable traces of the imperfectly developed art and mechanical skill of the Archaic [*Bronze*] Period' (1851a: 324)—although noting that its current owner, Lady Menzies, frequently wore it alongside her more modern jewellery. Wilson thus placed both the Scottish armillae in the 'Weapons and Implements' chapter of the Bronze Period section of his book. This indicated a much earlier date than Way suspected; iron, Wilson



Fig. 5.5. Top: Way's illustration of an English armilla he believed to be of Anglo-Saxon date. (From Way 1849a: facing p. 48). Middle: Way's illustration of the armilla from Largo in Scotland, which he hinted was of similar date. (From Way 1849a: facing p. 53, Wilson 1851a: 321). Bottom: Wilson's illustration of the Rannoch armilla, which he dated to the Bronze Period. (From Wilson 1851a: 324).

believed, arrived in Britain at or a little before 100 bc (1851a: 351), so the Bronze Period *ended* about that time. It was much less clear when it had started; numerous classical references to tin led him to suspect that Phoenicians were already visiting Cornwall by the time of King Solomon (1851a: 196), around 1000 bc. And before this, of course, stretched the Stone Period.

In only one chapter did Wilson consider materials of all periods together. This was in the chapter on craniology, which formed the

last chapter in the Stone Period section. He divided human skulls just as sharply into periods as he did any other class of material, however, and by considering them all together was not implying that they showed no variation; on the contrary, he offered in this chapter an outline of the craniological history of the British Isles that was to form the basis for much of the work of the next century. The BAAS had in previous years refused a number of important presentations on craniology (D. Wilson 1851a: xii). In 1850, however, he had presented at the BAAS a paper arguing that two other races had inhabited Scotland before the Celts (D. Wilson 1851b), and he wrote many years later that 'it is amusing now to recall the undisguised incredulity with which a theory was then received which has since met with universal acceptance as a mere truism necessarily involved in greatly more comprehensive assumptions' (D. Wilson 1878: II, 140–1).

Wilson was a religious man and he espoused monogenism, the derivation of all human stocks from one origin, arguing that ethnological history, although it had much further to go, 'appears distinctly to coincide with the Mosaic history of the human race' (1851b: 142). He enthusiastically adopted Prichard's concept of 'Allophylians', using the term repeatedly as a blanket designation for all potential pre-Celtic Europeans. It was a suitable term because it 'suffices to characterize them as distinct from the well ascertained primitive races, without meanwhile assuming any hypothetical origin for them' (1851a: 161). By applying the term, he was again reinforcing the 'idea of prehistory': if he used any ethnic or quasi-ethnic term such as 'Finn' or 'Scythian' or even 'Turanian', such a positive identification would necessarily have implied that the people referred to were within the reach of recorded history; 'Allophylian' was after all more of a negative definition (see chapter 4), simply identifying the population(s) as 'other' with respect to historically known people. Where Wilson differed with Prichard was with regard to the skulls in the Scandinavian graves. Prichard (1843, 1848b), it will be remembered, stated that all the skulls were Celtic, due in part to the incomplete translation available to him of the paper by Eschricht (1837); consequently his Allophylians had no archaeological record and remained strictly theoretical. Wilson accused Prichard of ignoring the conclusions

of the Scandinavians; he was guilty of ‘attaching it may be too slight importance to the strictly archaeological evidence on which they are to some extent based’ (D. Wilson 1851a: 163). Wilson described Nilsson’s earliest brachycephalic skulls as ‘the Allophylian colonists of Scandinavia’ (ibid.).

Wilson set out to demonstrate the existence of not one, but two, races of Allophylians in Scotland prior to the Celts. Once again he drew his inspiration from Scandinavia—and reserved his criticisms for English archaeology. In what he clearly intended as a criticism of the English failure to adopt the Three Age System, he complained of ‘the general misapprehension by men of science in England, of the value of archæological investigations, they have been rendered nearly valueless as a means for the ascertainment of truths relating to primitive ethnology’ (D. Wilson 1851b: 142).

Wilson’s craniological inspiration was drawn from three sources, two of which we have already encountered in chapter 4. First, he quoted John Thurnam’s paper on the Anglo-Saxon barrows and skulls from Lamel-hill; Thurnam did not, however, consider the possibility of pre-Celtic races (Thurnam 1849). Second, he had also undoubtedly read Sven Nilsson’s British Association paper (Nilsson 1848), with its over-complex rendering of Nilsson’s actual views. Wilson could not read any publications in Danish or Swedish, nor of course had he heard Nilsson’s 1847 lectures, so he could not know how small was the sample of crania on which the Scandinavians’ conclusions were actually based. He may indeed have drawn most of his understanding of Nilsson’s 1848 paper from Thurnam (1849), because he everywhere follows the latter’s characteristic mis-spelling of Nilsson’s name as ‘Nillson’. His third and methodologically by far his most important source was however Morton’s book *Crania Americana*. Wilson derived his system of measurements from this source, not from Thurnam, who had presented the only other available set of craniological measurements, the sample of twenty-one skulls from Lamel-hill (Thurnam 1849: 136). The measurements Thurnam gave were not very useful for Wilson’s purposes of racial identification; he listed only the length, breadth, and height of the frontal, parietal, and occipital regions, and the circumference (but not the overall length) of the skull. Thurnam’s measurements were mostly given to the nearest quarter inch, while Wilson’s were to the

nearest twelfth of an inch, and some twelfths were even subdivided. Wilson noted that he was assisted in his measuring by two medical men, and that nearly all measurements had been repeated several times.

Wilson presented two tables of measurements, giving data for thirty-nine skulls from Scotland and two Mexicans from *Crania Americana* for comparison (1851a: 166; 1851b: 146). The tables were for the most part identical, but in his BAAS table Wilson listed four extra measurements including a calculation of the facial angle, an early measure developed by the Dutch anatomist Pieter Camper (see Prichard 1848b: 111–13). These were not tabulated in *Prehistoric Annals*, but the table in that work had a measure of cranial capacity. The first measurements in both of Wilson's tables were overall length ('longitudinal diameter') and parietal diameter. These are the measurements he would have required to calculate the cephalic index so commonly used by later craniologists, but he does not appear anywhere actually to have made the calculation. He apparently determined whether a skull was dolichocephalic or brachycephalic on the basis of its appearance; only once did he resort to considering the individual measurements, in a case he considered doubtful (D. Wilson 1851a: 174). With regard at least to the specimens he illustrated (fig. 5.6), the differences between the main classes were clear to the eye.

Wilson identified nine skulls as primitive dolichocephalic or kumbecephalic, six as brachycephalic, one as Roman, eleven as Celtic, and twelve as medieval. His term 'kumbecephalic' he defined as meaning 'boat-shaped', and applied to the primitive dolichocephalic form (1851a: 169); and as we saw in chapter 4, this was the term Bateman picked up and used the year after. Discussing the nine kumbecephalic skulls, Wilson commented:

The whole of these, more or less, nearly agree with the lengthened oval form described by Professor Nillson [*sic*] as the second race of the Scandinavian tumuli... Though they approach in form to a superior type, falling under the first or Dolicho-kephalic class of Professor Retzius' arrangement, their capacity is generally small, and their development, for the most part, poor; so that there is nothing in their cranial characteristics inconsistent with such evidence as seems to assign to them the rude arts and extremely limited knowledge of the British Stone Period. (D. Wilson 1851a: 169)

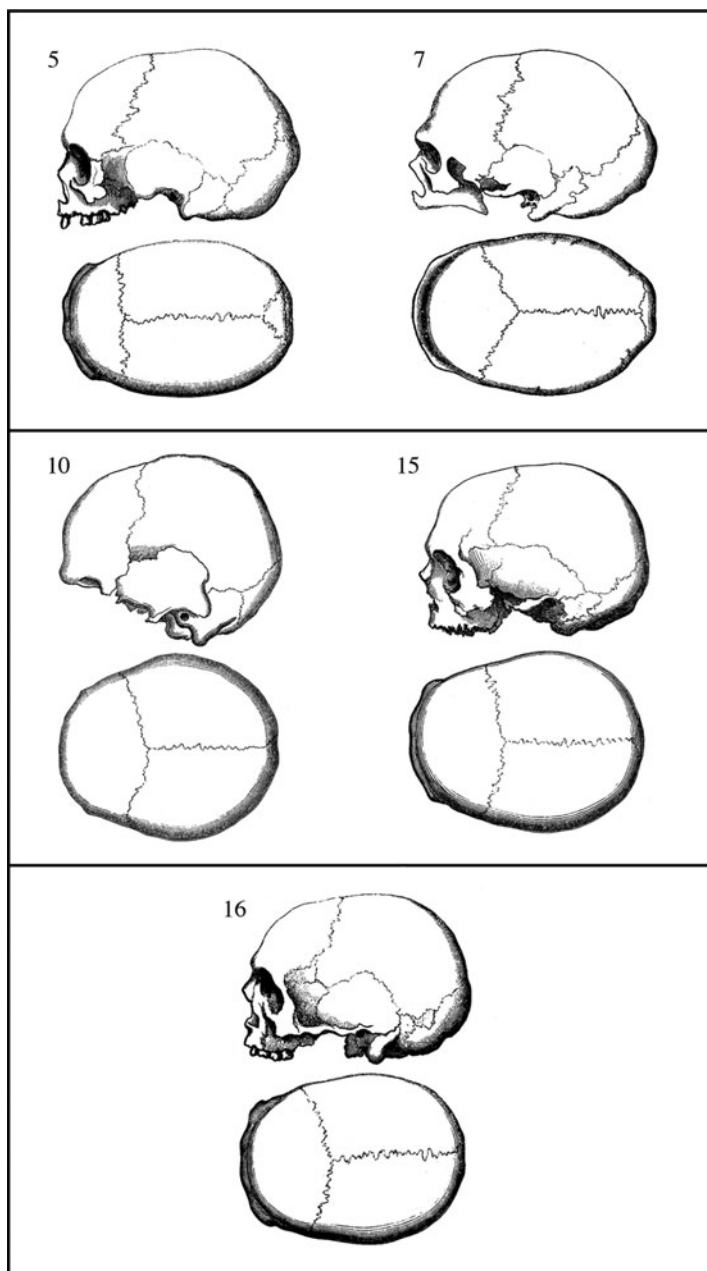


Fig. 5.6. Wilson's drawings of skulls. Top: kumbecephalic skulls nos. 5 and 7, from Cockenzie and Nether Urquart respectively. Middle: brachycephalic skulls nos. 10 and 15, from Montrose and Linlithgow respectively. Bottom: skull no. 16, from Newstead, associated with Roman pottery but argued to be Celtic. (From Wilson 1851a: 168–72).

The six brachycephalic ones that followed were completely different. They were:

of the British Brachy-kephalic type; square and compact in form, broad and short, but well balanced, and with a good frontal development... They correspond very nearly to the Brachy-kephalic crania of the supposed primeval race of Scandinavia, described by Professor Nillson [*sic*] as short, with prominent parietal tubers, and broad and flattened occiput. In frontal development, however, they are decidedly superior to the previous class of crania. (D. Wilson 1851a: 170–1)

His next eleven skulls 'afford a fair average criterion of the Celtic type' (D. Wilson 1851a: 172). He illustrated two of the kumbecephalic and two of the brachycephalic skulls, but none of the Celtic ones; instead he showed the one skull associated with Roman pottery (fig. 5.6), which, however, he felt was more likely to be that of a native Celt than of a Roman legionary—though he did not say why (1851a: 172).

The main outcome of all this was that, in placing his dolichocephalic or kumbecephalic skulls before the brachycephalic ones, Wilson *reversed* the order of skull types that had been found in Scandinavia, where the brachycephalic form was the oldest. He noted this but was able to offer no explanation, saying that more data were required to confirm what he had put forward (1851a: 171). To see how reliable his sequence was, we must examine how he dated the individual skulls—and only a minority were at all secure.

The kumbecephalic group provided the largest number of reasonably well-dated examples, six of the nine being referred to the Stone Period either by grave type or artefact association. Only one (no. 1 in Wilson's table) was dated by *both* grave type and the presence of flint flakes; the latter had already been ascribed to the Stone Period in a previous chapter (D. Wilson 1851a: 121). Another, his no. 2, was from a stone cist previously ascribed to the Stone Period (1851a: 72). Skulls nos. 3–6 also came from such stone cists, but these had not previously been discussed (1851a: 168). The final three skulls were of uncertain attribution. Skull no. 7 came from a group of cists, some of which had early items, but others had objects of iron; Wilson evidently did not know which cist had contained the skull, which he stated was 'selected here as another example of the same class of crania' (1851a: 168). Skull no. 8 *might* have been recovered in

1782 from a Stone Period chamber; while no. 9 had no information except a find location. Skulls nos. 5 and 7 were illustrated (see fig. 5.6).

The smaller brachycephalic group presented more uncertainties. Skull no. 10 was found with some clay urns. This was not adequate to date them—Wilson did not discuss ceramics in his Stone Period section, but he stated later that this was not because he believed ceramics to be absent, but simply because there was no way they could yet be typologically divided between periods (D. Wilson 1851a: 280). Skull nos. 11–13 were found in bogs with no associated finds (1851a: 170). The best dated was no. 14, coming from a cist which also contained a pot that contained several bronze rings (1851a: 170–1). Skull no. 15 also came from a cist—‘no relics were found in the cist, but some time prior to its discovery a bronze celt and spear-head were turned up in its immediate vicinity’ (1851a: 171). Illustrations were provided of nos. 10 and 15 (see fig. 5.6).

The Celtic group was the worst dated of the lot. None of the skulls had archaeological contexts that provided any useful dating evidence. They all came from locations associated with historical events, except for a group of five from Iona which ‘no doubt’ came from the investigation of a cemetery on the island carried out in 1833 (D. Wilson 1851a: 173). This may be why he chose not to illustrate any of these, but selected instead the one skull (no. 16) associated with Roman pottery—the skull of which he felt that ‘it is, perhaps, more probable’ that it belonged to a native Celt than to a Roman soldier (1851a: 172).

Wilson’s skull sequence rested on two lynchpins. The first was the reasonably secure dating of as many as six kumbecephalic skulls to the Stone Period. His ascription of megalithic chambers to this period seems to have given him a degree of confidence with regard to these. The second was the similarity of his third group of skulls to the ‘Celtic’ type defined by the Scandinavians: ‘the proportions of these Scottish Celtic crania entirely agree with the assumed type already referred to, as recognized by the ablest ethnologists. Professors Nillson [*sic*] and Retzius, and Dr Thurnam, all concur in describing the type of the old Celtic cranium as intermediate to the true Dolicho-kephalic and Brachy-kephalic forms’ (D. Wilson 1851a: 174). By identifying this group as Celts, Wilson was obliged to

place them after the other two types. But this was the only reason he had to place them in third chronological position, because none had any good dating evidence. But once this had been assumed, there was only one possible place for the brachycephalic skulls: in the Bronze Period, between the Stone Period and the Celts—even though only one had any direct association with bronze.

Had Wilson not made the *a priori* division of the ancient past into the Stone, Bronze, and Iron Periods, he would not have been able to generate a chronology based on the skulls alone. In the absence of the multivariate statistics that later generations would have deployed, he relied almost entirely on his visual impression when he allocated a skull to a category. His drawings showed a clear distinction between kumbecephalic and brachycephalic (fig. 5.6)—but to a considerable extent he illustrated his most characteristic examples. When a plot is made of skull length and parietal breadth, the raw materials of the cephalic index, it becomes apparent that nos. 5 and 7 are two of the most extreme kumbecephalic skulls (being towards the lower right of the scatter), while no. 10 is one of the most extreme brachycephalic ones; only no. 15 is more central (fig. 5.7). Nevertheless, on this measure the three main groups of skulls do plot out differently, and Wilson's sequence was to be confirmed by later workers in England, especially John Thurnam and George Rolleston (as we shall see in chapter 7), on the basis of larger samples. The sequence had, however, already been proposed by the Irish researcher William Wilde, as we shall see in the next chapter. But Wilson nowhere referred to Wilde; the Irish sample was tiny, and Wilde's chronological arguments (as we shall see in chapter 6) must have struck Wilson as bizarre in the extreme and quite unusable.

Prehistoric Annals laid down a comprehensive archaeological sequence for Scotland, encompassing not just the materials artefacts were made of, but a variety of other things too. In this respect it did for Scotland much the same as Thomsen had done for Denmark fifteen years before—and added the crania to the scheme, which Thomsen had not himself done. The completeness of Wilson's treatment, coupled with the absence of an opposing chronology for Scotland based either on ancient history or on ethnology, meant that *Prehistoric Annals* had a fairly clear run, and was never seriously opposed on its home ground.

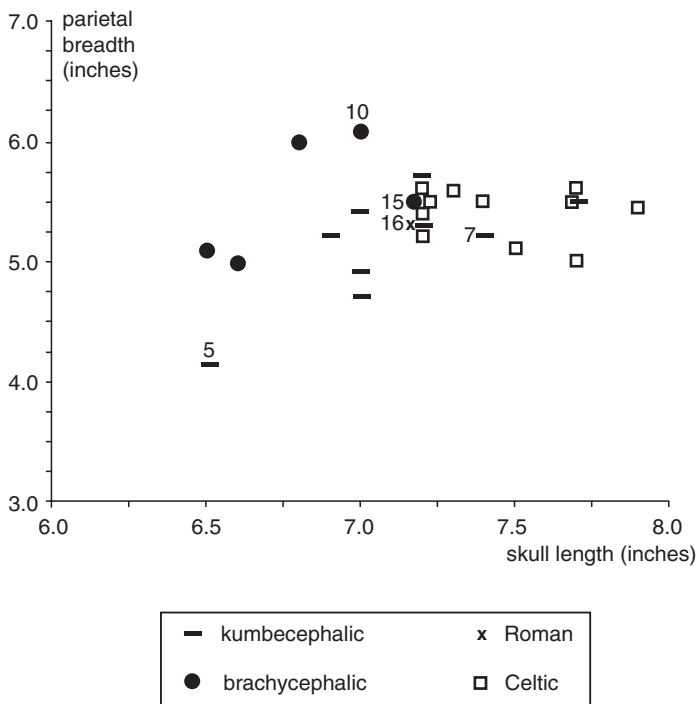


Fig. 5.7. Chart plotting skull length and parietal breadth of the kumbecephalic, brachycephalic, Roman, and Celtic skulls measured by Wilson. Numbers are those in Wilson's table (see the caption to his fig. 34). Measurements are in inches, but the twelfths given by Wilson are converted to tenths because of the limitations of the plotting programme. (Data from Wilson 1851a: 166; 1851b: 146).

SCOTTISH ARCHAEOLOGY FROM WILSON TO GREENWELL

Wilson's energy had another major beneficial effect on Scottish archaeology: the establishment of its first regular periodical. The SAS had previously published *Archaeologica Scotica*, but to call this an occasional publication would be to impute to it a sense of urgency that it entirely lacked. The first volume appeared in 1792, twelve years

after the SAS was founded. The second appeared thirty years later, in 1822, and the third in 1831, this gap of nine years being the shortest it was ever to achieve; the 1831 number was the one that contained Alexander Seton's (1831) publication of the Machrihanish tumulus. The fourth volume appeared in 1857, while 1890 saw the appearance of the last; perhaps, given its track record, one should add: 'so far'.

The *Proceedings of the Society of Antiquaries of Scotland* (PSAS) was a very different animal. Wilson was a prime mover behind its establishment (Ash 1999: 72). The first three volumes were triennial, covering 1851–4, 1854–7, and 1857–60 respectively. From then until 1878 it appeared biennially; and from 1878 it has appeared annually. In the first volume, Wilson as secretary of the SAS was able to announce that the Crown had finally, after many years of urging, agreed to take over the SAS's museum and use it as the basis for a new National Museum of Scotland (Wilson 1854). But when we track the impact of Wilson's triple legacy—*Prehistoric Annals*, the National Museum, and PSAS—we must do so in the absence of Wilson himself. Despite the success of the book, he was unable to obtain employment in any Scottish university or museum, although he was given an honorary degree by St Andrews (Ash 1999: 78). Never a wealthy man, he was therefore obliged to take a chair in history and English literature at University College, Toronto, in 1853; and henceforth his contribution was to be largely outside Scotland. His theoretical impact in Canada, including the publication of his two-volume *Prehistoric Man. Researches into the History of Civilization in the Old and the New World* (D. Wilson 1862) has been explored by Trigger (1999).

Although the first volume of PSAS contained the address given by Wilson in 1851 (see above), by the time this first issue appeared he was therefore no longer in Scotland. *Prehistoric Annals* had, however, been enthusiastically received (Ash 1999: 78), and PSAS contains hardly any statements implying hostility towards the Three Age System. Henry Rhind was one of the few people to publish in both English and Scottish journals. We saw in chapter 4 how in English publications he both expressed his opposition to the Three Age System (Rhind 1856), and also directly criticized Wilson's espousal of the Allophylian concept (Rhind 1853). This latter criticism was in an article describing his excavations of a Scottish broch, the results of which he also presented to a Scottish audience in the first volume of

PSAS (Rhind 1854). In this article, however, he ventured no criticism of either the Three Age System or the Allophylian hypothesis; the article was simply a detailed description of the site and finds. It would appear that he did not wish to take on the Three Age System in the Scottish archaeological context. The third volume of *PSAS* did contain a short summary of a paper given by Rhind, by then more concerned with Egyptological matters, on the use of bronze and iron, in which some implied criticisms appeared (Rhind 1859). However, the phraseology was oblique to the point of incomprehensibility. There was, Rhind stated, a bronze culture in the distant past, on which 'an iron culture has been, as it were, superimposed'. He then immediately countered this statement by adding that 'we look in vain among actual vestiges for proofs of any such hypothetical schemes' such as iron coming later than other metals. The relative chronology of neither bronze nor iron was fixed. Archaeologists could therefore stop searching for such information, because people had used both bronze and iron 'from a most remote antiquity'. Nevertheless, 'we do know that in regions where iron was subsequently employed for the great purposes of practical life, bronze had previously occupied the prominent position' (Rhind 1859: 465). One wonders whether the original talk made more sense to its Scottish audience than the summary does to modern readers.

Apart from Rhind's papers, most of the contributions in the first decade of *PSAS* were on medieval subjects, offering rather little scope for comment about the efficacy of the Three Age System. At the end of the decade, one of the SAS's vice-presidents, Professor J. Y. Simpson, gave an address to the Society in which he praised among other things the Danish system of ordering periods, with stone coming first (Simpson 1862: 10–11); the idea that a pre-Celtic aboriginal or Turanian population had been present throughout Europe (1862: 12); Wilson's 'masterly' *Prehistoric Annals* (Simpson 1862: 14); and the suggestion that current stone-using peoples around the world, including those of New Caledonia in the Pacific, might serve as models for 'this old Caledonia of ours, during even the so-called Stone Age' (1862: 31). This long review did not give any suggestion that there was much dissent. A cautionary note did appear in volume 5; John Alexander Smith referred to 'the so-called ages of stone, of bronze, of iron' as 'artificial and merely assumed periods of unmeasured time',

although he did not contest the point that metals were rarer in earlier times (Smith 1864: 96). In the same issue was a report on a cranium. The author, William Turner, did present a calculation of its cephalic index, and concluded that the skull was brachycephalic (Turner 1864: 280). The previous year had seen the publication of the second edition of *Prehistoric Annals* (D. Wilson 1863), written in Toronto. Wilson's sample of kumbecephalic skulls had increased to twenty-two, and of brachycephalic to twenty-four, both lists now including some English examples (1863: 267, 275). Turner fitted the skull he was studying into Wilson's scheme, noting again that the British sequence differed from that established on the mainland (Turner 1864: 281–3).

In the mid 1860s we encounter for the first time a man who was to play a major part in later developments in England: William Greenwell, a minor canon in Durham cathedral. Greenwell excavated very large numbers of burial mounds in various parts of Britain; his base in Durham was closer to Edinburgh than to London, but far enough from either to avoid him becoming very closely identified with any group or centre. He was a member of both the SAS in Edinburgh and the Antiquaries in London, but he avoided many of the political problems of London archaeology by never joining either the Institute or the Association (J. C. Hodgson 1918), although he was briefly an honorary corresponding member of the Institute when he published a paper in *Arch. J.* (see chapter 7). His work was characterized by three things: first, the detailed observation and reporting of the burial mounds he published; second, attention to stratigraphy and the detection of different burial episodes in the same mound, all viewed against the backdrop of the Three Age System; and third, a wide knowledge of artefacts from across Britain and Ireland, which enabled him to draw far-reaching connections. All these three aspects are evident in the major paper he published on the burial mounds of Crinan Loch, near Kilmartin in Argyllshire, and each will be examined in turn. Here is an example of his detailed reporting of one of the Kilmartin mounds:

The cairn is 110 feet in diameter, and 13½ feet high. The excavation was commenced on the south-west side, when, about 8 feet from the outside, some stones were found standing upright and apart. This proved to be a

portion of one of a double circle of stones which was enclosed within the cairn. The inner of these parallel circles was 27 feet in diameter, the outer one being 37 feet, the two thus standing about 5 feet apart, and the outer circle was about 16 feet from the centre of the cairn. The stones which composed these circles were about 3 feet high and 2 feet wide, and stood from 3 to 5 feet apart, except for a space towards the centre of the cairn, where, in both circles, four stones were placed close together; whilst another portion, a few feet distant from these four stones, had the space between the two upright stones filled in by a wall of smaller stones placed flat. In the centre, within the circles, was a cist made of four slabs of schist set on edge, with a cover of the same stone. The cist, which lay N.E. by S.W., was 3 feet 5 inches long, 2 feet 4 inches wide, and 21 inches deep. It was half filled with river gravel, and contained an urn covered by the gravel, and a necklace of jet beads placed above the urn; all trace of the body, which had, there is no doubt, been an unburnt one, had disappeared. (Greenwell 1866: 339–40).

This level of detail was hitherto unparalleled; Greenwell did not publish a plan of the mound, but he scarcely needed to because his observations and measurements were so precise that the reader could draw one for himself.

A nearby cairn at Largie gives an example of his attention to stratigraphy. After the usual detailed description of the mound and its features, he described the megalithic central chamber as ‘one of the most instructive places of sepulture I have ever seen’ (Greenwell 1866: 342). This central chamber was subdivided into four compartments, the two southern ones containing much material. Inside one compartment he encountered a small cist, clearly secondary, which contained nothing, but which he believed was the source of some unburnt human bones scattered around. Beneath this was a dark layer containing burnt human bones and stone tools. In the next compartment he found three pots ‘of the type which is always found with unburnt bodies’ (Greenwell 1866: 345), so he referred them to a secondary inhumation, overlying a layer of burnt human bones similar to that in the other compartment. From this he concluded that the earliest mode of burial in large megalithic chambers in this part of Scotland comprised cremations, ‘though I do not doubt that there was a still earlier time than this of burning, during which the body was interred unburnt’ (Greenwell 1866: 346). The secondary burials were similar to those in flat cist graves elsewhere, which were

dated to the Bronze Age (Greenwell 1866: 346 n. 1). He thus recognized secondary burials; dated them to the Bronze Age by grave type and ceramic association even though they contained no bronze items; and realized that his primary cremations nevertheless post-dated an earlier phase of inhumation burial. This highly developed chronological awareness rested on his acceptance of the Three Age System.

This paper also revealed Greenwell's third major attribute, his wide knowledge of the archaeological record and his ability to draw comparisons with items from elsewhere. He did this on a number of occasions in this paper, but stressed particularly that the pottery in these southwest Scottish graves was very similar to that on the coast of Ireland just opposite, and that some Scottish stone tools were made on raw material imported from Ireland. This showed that 'a constant intercourse was kept up between the two shores' (Greenwell 1866: 350).

Greenwell's paper was a major tour de force which raised the examination and interpretation of Scottish burial mounds to a previously unattained level of quality. If there had been little opposition in *PSAS* before, there was none after Greenwell's paper. This same volume saw the first use of a Three Age System period name in an article title, by Jamieson discussing 'remains of the Stone Period in the Buchan district of Aberdeenshire' (Jamieson 1866). The next volume saw the first two uses of the word 'prehistoric' in a similar context (Laing 1868; Traill 1868), and also witnessed John Alexander Smith, the man who had voiced some opposition to the Three Age System in 1864, come fully on board in an article about bronze sickles (Smith 1868).

The only apparently dissenting note came from George Petrie (not to be confused with the Irishman of the same name, whom we shall encounter in the next chapter). Petrie lived in Orkney and was an active archaeologist who was involved in among other things the recovery of the Viking-age hoard from Skail (Graham-Campbell 1984). His paper on the recently exposed houses at Skara Brae suggested that the settlement might be quite late, despite the fact that the implements consisted exclusively of stone and bone items. The ceramics and domestic architecture argued that the people were capable of better things, and that it was isolation rather than ignorance that prevented them from using metals (Petrie, of Orkney, 1868: 217). But Petrie was not a dissenter and in general accepted the Three

Age System. Two years previously he had argued that the broch at Oxtro was overlain by a series of cist graves that dated from the 'bronze age' (Petrie, of Orkney, 1866: 200); finds of metals in brochs were rare and probably later intrusions. This implied that he believed that brochs dated to the Stone Age, though he did not use that term.

The uniformity of Scottish archaeologists reveals the completeness of their adoption of the Three Age System. Much of this was due to Daniel Wilson, but in addition the academic community was, as shown above, ready to listen to what he propounded. The impression presented in this chapter of Scottish archaeology in these years is one of self-reliance, of a community operating to its own agenda—not one that was overawed by London. This impression differs somewhat from that presented by Kehoe (1998), who casts the Edinburgh community rather more as outsiders, men of humble background in contrast to the upper gentry of London. The Londoners were men of the 'X club' dining circle like Lubbock, Darwin, and Huxley; men who, while reverential of the aristocracy above them (Kehoe 1998: 21) gave evolution the imprimatur of their own high social status (Kehoe 1998: 42), thus ensuring that the books of Lubbock and Darwin enjoyed greater success and acceptance than those of Wilson and Chambers. There may be some truth in this, but this may also be one of the instances mentioned in chapter 1 where the social context of the issue has been *over-stressed*. It is worth recalling that Wilson's achievements took place in the early 1850s, before the X club had emerged. Englishmen like Thomas Wright and Albert Way, also of high social class, took an opposing view because they interpreted the evidence in a different way. Chambers' book on evolution was simply nothing like as good as Darwin's; Wilson's on archaeology concentrated on Scotland and did not discuss the massive implications of the newly discovered antiquity of man, while Lubbock's *Pre-historic Times* (1865) was world-wide and up to date. And with regard to reverence for social rank, it was Wilson who dedicated *Prehistoric Annals* to an aristocrat; neither Darwin, nor Huxley, nor Lubbock ever did so with any of their books. It was Wilson who accepted a knighthood; neither Darwin nor Huxley did, and Lubbock as an hereditary baronet could scarcely avoid his—not that he wished to. Perhaps 'outsider status' was something more keenly felt in later twentieth-century Edinburgh than it had been 150 years earlier.

Ireland: Realm of the Four Masters

On 9 January 1843, Richard Griffith addressed the Royal Irish Academy (RIA) about some antiquities found in the River Shannon. The river was being dredged to render it navigable, and the artefacts were discovered during the deepening of the old ford at Keelogue. Griffith was the chairman of the Commissioners carrying out the work, and his expertise was in engineering rather than ancient history. He stated that the finds came from a layer of gravel; in its upper part were many bronze swords and spears, while a foot lower were numerous stone axes. Due to the rapidity of the river's flow there was very little aggradation, so despite the small gap the bronze objects were substantially later than the stone ones. The river formed the border between the ancient kingdoms of Connaught and Leinster. The objects had apparently been lost in two battles for the ford that had taken place at widely differing dates; stressing that he was no expert himself, Mr Griffith wondered whether ancient Irish history might contain records of battles at this spot (Griffith 1844).

This was probably the earliest non-funerary stratigraphic support for the Three Age System ever published, but it did not signal the acceptance of the Three Age System. Just as telling as Griffith's stratigraphic observation was his immediate recourse to ancient history for an explanation; for, as we shall see, ancient history provided the dominant framework for the ancient Irish past until the end of the nineteenth century. The Irish had far more early manuscript sources than the Scots or the English, although wars and invasions had reduced them; the Welsh scholar Edward Lhwyd wrote from Sligo on 12 March 1700 to his colleague Henry Rowlands that 'the Irish have many more ancient manuscripts than we in

Wales; but since the late revolutions they are much lessened. I now and then pick up some very old parchment manuscripts; but they are hard to come by, and they that do anything understand them, value them as their lives' (in Rowlands 1766: 315).

In the seventeenth century various Irish scholars brought together the historical accounts available to them. Geoffrey Keating (Seathrún Céitinn, in Irish) wrote the influential *Foras Feasa ar Éirinn* or 'History of Ireland' in c.1634, and an English translation was printed in 1723 (Waddell 2005). The *Annals of the Four Masters*, named after the four historians who compiled them, were composed about the same time, but were only fully translated into English and published by John O'Donovan in 1848–51 (University College Cork 2002); these annals ended in AD 1616 and purportedly reached back over 4,000 years. Both made use of the medieval *Leabhar Gabhála* or 'Book of Invasions', which gave the sequence of ancient inhabitants of Ireland: Partholon and his followers arrived eight generations after the Noachian Flood, and were followed by the Nemidians, the Formorians, the Firbolg, the Tuatha Dé Danaan, and finally the Milesians.¹ From the later eighteenth century the origins of these various peoples were mainly traced by philological studies, leading to claims that the Irish were descendants of peoples such as the Etruscans, Phoenicians, Persians, and Indians.

This was the disciplinary context in which Worsaae found himself. The Irish situation was very different to that of England or Scotland: while there was an interest in antiquities, ancient history provided the overarching scheme and chronology, and there was no need for one based on the antiquities themselves. As we shall see, even the veracity of Griffith's account was questioned in a lecture given by Eugene O'Curry in 1860 (O'Curry 1873a: II, 271), and a decade later another man involved in the Shannon works stated that the numerous finds from the river provided no information about the relative ages of stone and bronze artefacts; exact observations of their positions were rarely made, and the workmen (who were rewarded when they found antiquities) regularly manufactured fakes (Long 1870).

¹ Spellings of these early peoples vary. Here I follow O'Curry (1873a) unless in quotation.

Much had happened in the generation after Griffith's presentation. Worsaae had come and gone, meeting the two leading Irish archaeologists of the time, George Petrie (1790–1866)² and William Wilde (1815–1876). There had been a huge upsurge in the translation and publication of the ancient historical sources, most notably the *Annals of the Four Masters*. John Waddell's *Foundation Myths. The Beginnings of Irish Archaeology* (Waddell 2005) provides many important insights into these events as a whole. Consideration here will be more limited, directed specifically towards the factors affecting the reception of the Three Age System. When the Three Age System was finally adopted, it was not as a 'top-down' imposition from the centre in Dublin, but rather as a 'bottom-up' movement that emerged from the provinces.

THE GOVERNOR, THE GENERAL, AND THE PHOENICIAN COLONIES

The ancient historical structure that Worsaae was to encounter on his arrival in Ireland took shape during the second half of the eighteenth century. This structure was an aspect of Irish nationalist feelings that were emerging at this time, and continued to be so through most of the nineteenth century. It is thus paradoxical that its two early leading proponents were senior English colonialists, Governor Thomas Pownall and General Charles Vallancey; and that their most articulate critic was an Irish cleric, Edward Ledwich.

Thomas Pownall (1722–1805) was Lieutenant Governor of New Jersey in 1755–60, after which he returned to Britain, being known thereafter as 'Governor' Pownall. In America he became interested in the processes of colonization, in particular the clash between colonizers and colonized. When he took an interest in antiquities, it was natural that he should take a colonial perspective (Orme 1974). In the Irish context Pownall's main contribution was a major paper on New Grange (Pownall 1773). Showing an awareness of the Four Stage Theory (see chapters 3 and 5), Pownall regarded hunter-gatherers,

² Listed in the References under Petrie, George (of Ireland).

whom he termed 'woodland-men', as being replaced by farmers, or 'land-workers' (1773: 241; see Orme 1974). The Celts were the woodland-men of Europe; they were taught the merits of religion and agriculture by colonizers 'in the same manner as our East India Company is at this day advancing subordinate entrepots and settlements for trade, from their fixed posts and ports in Bengal and on the Malabar and Coromandel coasts' (Pownall 1773: 243). Who were these colonizers? Pownall envisaged two streams, a maritime one moving through the Mediterranean and round to the British Isles, and a terrestrial one moving over land into central and northern Europe. With regard to the first, Pownall did what many others were to do after him: scour the ancient sources for the names of suitable peoples, and then amalgamate them all into one:

We meet these people in divers places under various appellations; as Edomites, Erythræans, Phœnicians, Pœni, all signifying the same thing, as also Tyrrhenians and Tyrians and Etruscans. They were also called (from their original gentile name) Iberians; sometimes from the names or appellations of the leaders of their colonies, they were called Cadmæans, Heraclides, and so forth; the name of Ercol, Arcles, or Hercules, being common to many of these leaders. (Pownall 1773: 242)

Pownall's second group, originating west of the Black Sea, were variously known as Tihtans, Teütönes, Teütschs, Belgæ, Bolg, or Volg; they 'became, from their abundant population, a hive, from whence many successive swarms came forth, and colonized through the middle and north-western parts of Europe' (Pownall 1773: 248). Warming to his beehive metaphor, Pownall described the Vikings as 'swarms of the same people' (1773: 249), and the Anglo-Saxons as 'different swarms from the same hive' (1773: 250). As we shall see, this metaphor was to recur unexpectedly on the other side of the world, evidence that Pownall's work had been read there.

From this historical perspective, Pownall interpreted New Grange as a monument built by newly civilized Celts, after they had been contacted by the colonizing people. He was able to provide clear evidence in support of this, because inside New Grange he located an inscription on a flat stone forming the north wall of the left side chamber (fig. 6.1). It did not appear to be Gaelic; Pownall concluded that it was Phœnician, and (since it consisted only of numerals) that



Fig. 6.1. Inscription inside the chambered tomb at New Grange, identified by Pownall as Phoenician. (From Pownall 1773: facing p. 258).

it had been part of a larger inscription originally placed elsewhere, probably 'some marine or naval monument erected at the mouth of the Boyne, by some of these Eastern people, to whom the ports of Ireland were well known' (Pownall 1773: 260). When this original structure fell into ruin, the stones of which it was constructed were reused to build New Grange. Consequently, 'the peculiar and secreted situation of this stone became a peculiar means of its being a *singular instance of the preservation of the only eastern or Phœnician inscription found in these countries*' (1773: 260, original emphasis).

Pownall thus found archaeological evidence in support of his colonial hypothesis. His contemporary Charles Vallancey (1725–1812) was to develop the colonial argument using linguistic evidence, and later in his career was to trace the origins of the colonizers much further to the east than Pownall had envisaged. Vallancey was a military engineer who rose to the rank of Major-General, commanding all the Royal Engineers in Ireland. He had been stationed at Gibraltar as a junior officer in 1750–62, after which he was posted to Ireland. During his time in Gibraltar he had come into contact with Jewish scholars, and from them had learnt Hebrew and Chaldean. Within days of arriving in Ireland he began noticing similarities between these languages and the Irish he heard being

spoken around him, and Vallancey's interest in the origins of the Irish was born (Lennon 2005; Nevin 1993).

Vallancey established an archaeological periodical entitled *Collectanea de Rebus Hibernicis*,³ which first appeared in 1770 and in which he himself published extensively; he was also a founder-member of the Royal Irish Academy (RIA) (McDowell 1985), and published articles in the *Transactions of the Royal Irish Academy* (TRIA). His methodology was largely etymological, involving the search for words in oriental languages that resembled Irish ones. His perspective was definitively biblical: 'scripture, is certainly the only standard of all antient [*sic*] history, and the touchstone by which the truth of it may be tried' (Vallancey 1784: ii). From this monogenist perspective the question was not whether languages *might* be related—they necessarily *had* to be; the job of the etymologist was simply to tease out the connections. For Vallancey, the Irish were the descendants of Javan the son of Japhet; they moved into western Europe four centuries before the arrival of the descendants of Gomer, another of Japhet's sons. These Gomerites, 'that mixture of Scythians, Phœnicians, and Ægyptians, known by the Greeks by the name of Pelasgoi, who gave the name of Bruttan, to Britain' (Vallancey 1784: 58), were the ancestors of the Welsh, Britons, Gauls etc., and they pushed the Javanites into Ireland. The Irish were thus quite different to the Welsh.

Vallancey's methodology was neatly summarized in an article he wrote on a relatively recent colonization, one clearly attested by historical sources (Vallancey 1788a). This concerned an immigration

³ Citing *Collectanea de Rebus Hibernicis* presents problems. Vallancey wrote much of the contents himself, though various other people contributed articles and letters; parts were sometimes reissued at later dates, apparently not always being identified as second editions. This may account for oddities such as the fact that the copy of volume 2 in the National Library of Scotland dates part 5 to 1786, while parts 6 and 7 are dated five years *earlier*, to 1781; yet the pages are numbered consecutively. Some papers have title pages giving the title and author; others do not, and in such cases I assume that Vallancey himself was the author. Should a title page become detached, there is no way to identify the author. This is presumably what led John Waddell (2005: 80, 90 n. 1, 264) to identify Vallancey as the author of a paper entitled 'Druidism revived' in *Collectanea* 2(7); the abovementioned copy in the National Library of Scotland, however, has a title page to this article identifying the author as William Beauford (Beauford 1781). The dates, titles etc. I quote here are simply the ones on the copies I have examined; an analysis of the publishing history of *Collectanea* is much needed.

of English soldiers into Ireland in 1167–9. They had helped Dermot king of Leinster to suppress a rebellion; as a reward he gave them lands in Wexford, where their descendants were still to be found in Vallancey's day, having preserved their manners, customs, and language (Vallancey 1788a: 21). As proof of this, Vallancey presented a long list of archaic English words that were still in use among them (1788a: 28–35). For the ancient colonizations he used the same methodology. In 1786 he produced his *Vindication of the Ancient History of Ireland*, publishing the preface both as a separate short book in London (1786a) and also as the introduction to the *Vindication* itself, which formed a number of *Collectanea* (1786b). He started by asserting the fundamental veracity of the ancient annals: 'the Irish manuscripts contain a more perfect account of the emigrations of the Armenian-Scythians, Persians, &c. from the banks of the Caspian and Euxine seas, to the islands of the Mediterranean, to Africa, to Spain, and to the Britannic Isles, than any history hitherto known' (Vallancey 1786a: 3). Since the account they gave meshed perfectly with other sources, it was evident that the annals were historically reliable, not tenth-century fabrications (Vallancey 1786a: 4). The outline derived largely from that given by Geoffrey Keating (see the start of this chapter), and Vallancey quoted Keating to the effect that Parthalon arrived in Ireland twenty-two years before the birth of Abraham (Vallancey 1786b: 24). Having established this, Vallancey then sought etymological support for the scenario; here is an example of his style in a discussion of the name 'Ararat':

And here I must observe, that *Arrarat* is a Scythian name for the mountain of the ship, for *Art*, or *Aorth*, or *Arth*, is a ship, and *Ar* a mountain: in the Egyptian language *erhot*, as *gin-erhot*, navigatio, in Irish *Arthgim* navigare; for *gin* in Egyptick, and *gim* in Irish, is the verb *facere*. I am led to this derivation, seeing the Hebrew etymologists have gone so much out of the way for an explanation of Ararat, viz *maledictio tremoris*; aut ex Hebræo et Syro *maledictio*, sive *lux currentis*: and we learn from *Haitho*, the Armenian, that the name of the mountain in their language is *Aurth*, which perfectly corresponds with the Irish *Aorth*, or *Arthrac* a Ship. So *Ao hor* and *Eathar* from *Eatharac* a ship, from *thora* pellis & *ac* water: hence the Phoenician Hercules was named *Melic-artus*, or *Melec-Aorth*, the King of the Ship, or the Sailor of the Ship. (Vallancey 1786a: 35–6, original emphasis and spelling)

If this drawing of links between Hebrew, Egyptian, and Irish appears bizarre to the modern reader, it appeared natural to Vallancey. He and most of his contemporaries had nothing like the modern concept of exclusive language families. In the biblical model (shared, as we shall see, by Sir William Jones), all languages had to belong to the same family, so connections between them were inevitable.

Although etymology was his principal interest, Vallancey did also consider monuments and artefacts, and was a member of the Antiquities Committee of the RIA (McDowell 1985: 13). In a foretaste of things to come in the nineteenth century, he considered the famous Irish round towers to be of oriental origin, built by 'these same Phœnicians or Pelasgi' (Vallancey 1784: xi); two years later he identified them as Persian fire towers (Vallancey 1786b: 338–9). In an extended consideration of bronze artefacts he displayed some confusion about metallurgy, stating that 'the weapons of the antient [*sic*] Irish were all of brass or copper, mixed with iron and zinck' (Vallancey 1784: 50). He linked bronze axes firmly to his ancient historical structure, stating that they were made by the Javanite ancestors of the Irish before the Gomerites arrived in western Europe; this was demonstrated by the fact that, although the axes were found all over Europe, the moulds from which they were cast were found in Ireland, showing that they were made there (Vallancey 1784: 56–9). But these axes were mainly a vehicle for further etymological discussion; he described one as:

The brasshead of a Tuagh catha, a general name for the war axe, from the Chaldee tuach to strike, whence the Greek thuein, the French tuer, to wound, to kill, and the Arabic tawur, a battle-axe or halbert; the Irish cath a battle, skirmish, compounded with arbhar, a host, forms *catharbharr*, commonly written catharb, as if contracted of cath and treab, a tribe, but it is undoubtedly the Syriac and Phœnician catharba; turma mixtionis, is a bad translation of this word by Bochart; hence the caterva of the Romans. Persic kaw, warlike; Khesh, war; Arabic ketal or katal a soldier; whence the Irish proper name Cathal, by which they translate Carolus, quasi Cathareolas, expert in war. (Vallancey 1784: 62)

Some much more recent material culture also supported the oriental argument. A gravestone described by Waddell (2005: 95) as 'an unremarkable sixteenth-century grave slab' from Lusk, near Dublin, was identified by Vallancey (1788b) as showing ancient Egyptian

influence. This was because there were two hands carved by the side of the Christ figure (fig. 6.2), and 'no symbol was more in use with the Ægyptians and with the ancient Irish than the *hand*' (Vallancey 1788b: 59, original emphasis); and he went on to trace connections with Hebrew, Arabic, and Persian.

Vallancey's scenario remained dominant through the later eighteenth century (McDowell 1985: 5; Waddell 2005: 79), though not accepted by everyone. William Beauford reconsidered the inscription found by Pownall in New Grange (fig. 6.1). He concluded that it was actually in Irish, written in the ogham script, and deciphered it as reading *oenguh* or *oemguth*, meaning 'the sepulchre of the hero' (Beauford 1781: 212); while Phoenicians were probably the first travellers to reach the British Isles, he did not consider that any firm traces of them in the form of coins or monuments had been found (Beauford 1787: 141–2). Vallancey's most severe critic was however Edward Ledwich (1739–1823). As early as 1781 he wrote in *Collectanea* that Ireland's annals owed more to patriotic zeal than to ancient reality, so that 'intemperate zeal led to the fabrication of fictitious annals, and the wild delusions of romantic history' (Ledwich 1781b: 83). He dismissed Vallancey's oriental origin for the round towers, stating that the ancient Irish were incapable of building in stone; the towers were likely of Viking origin, although he admitted that 'whether there are any round towers in Denmark or Norway is not positively decided' (Ledwich 1781a: 133). A few years later he took up the matter of the druids, the British and Irish priesthood encountered by the Romans. Druids had a special place in the oriental hypothesis: Pownall for example considered druids to be Phoenician missionaries (Pownall 1773: 243). Pownall's article had been published in London, in the *Antiquaries' Archaeologia*; in an article specifically intended to refute Pownall and published in the next volume of the same journal, Ledwich (1785: 304) dismissed the entire colonization theory as 'literary pharmacy' and 'groundless hypothesis or historical Romance'. He pointed out that few classical authors even mentioned the druids; after considering what little material they presented, he concluded that 'if any traces of an enlightened and polished people can be found in, or are fairly deducible from it, the discoverer without envy or rivalry may enjoy the reward of his sagacity' (Ledwich 1785: 318). His own view was that the wisdom claimed by (or for) the druids was no more than

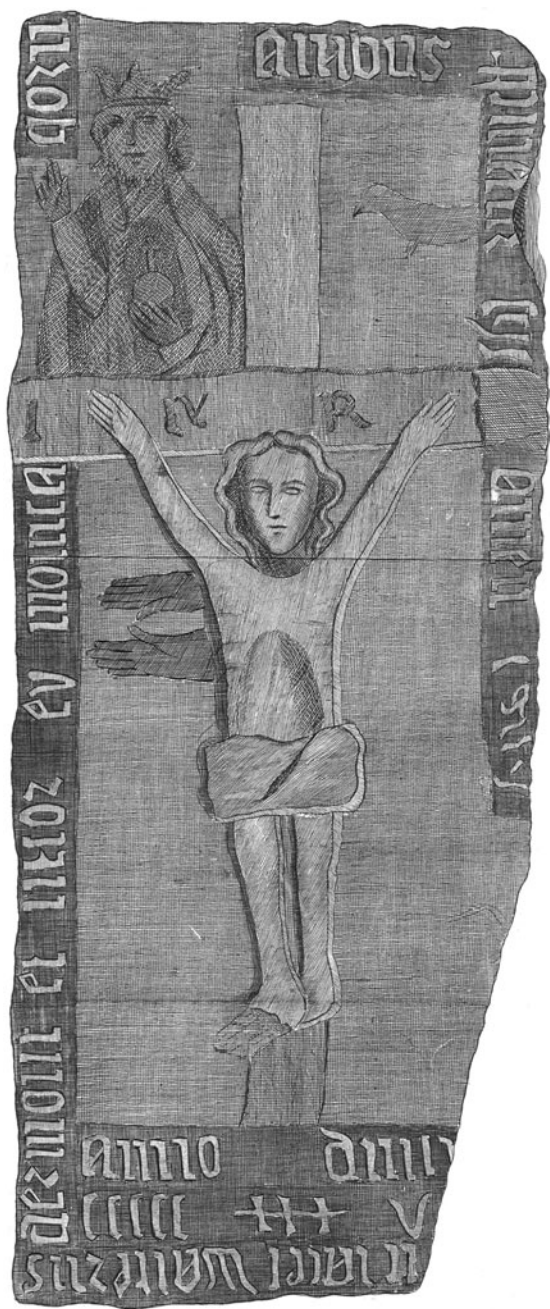


Fig. 6.2. Sixteenth-century gravestone from Lusk, Co. Dublin. The hands claimed by Vallancey to show Egyptian influence are visible to the left of the Christ figure. (From Vallancey 1788b: antiquities section, facing p. 57).

‘the charlatanerie of barbarian priests’ (Ledwich 1785: 322). His most trenchant criticism of the Irish annals he reserved however for publication in Ireland. It was natural, he argued, that ‘a nation emerging from incivility and ignorance’ should use mythical ancient history as ‘the only evidence of extinct national honour’, but as the nation improved, ‘these phantoms vanish’ (Ledwich 1792: 21). The annals were composed in medieval times; the arrival of cultured Arabs in Spain brought the art of romantic fiction to Atlantic Europe, and the Irish writers caught from them this ‘pleasing contagion’ (Ledwich 1792: 23). Ledwich recounted a story of St Patrick’s conversion in the fifth century resulting in 30,000 people starting immediately on a pilgrimage to Jerusalem; ‘here is a palpable forgery...calculated to countenance the crusades, and determines the date of this fiction to the twelfth century’ (1792: 27). An ancient conquest of Spain featured in Keating’s *History of Ireland* dated from still later times, being nothing but a garbled version of its fifteenth century reconquest from the Moors (Ledwich 1792: 28). In conclusion:

Such then seems to me the origins and grounds of Irish romantic history, a subject of little curiosity and less value, yet necessary to be thoroughly examined to be for ever exploded. In this enlightened age it can require no apology for exposing this wild chaos of absurdity and fable; as it stands at present it reflects no honour on our native country, nor can its annihilation in the least injure it. (Ledwich 1792: 30)

Ledwich’s criticisms were, however, not destined to have as much impact as they might have, because another paper published in 1792 was to provide unexpected support for the oriental hypothesis. Vallancey’s historical canvas already stretched from Ireland to Persia, and now it was to extend still further east to include India; because the new paper was published by Francis Wilford, a protégé of Sir William Jones in Calcutta.

A PASSAGE FROM INDIA

We saw in chapter 1 that Sir William Jones published his discovery of the Indo-European linguistic connections in 1786 (Jones 1786

[1806]); and that given his monogenist and biblical perspective he expected Chinese and the Aztec and Inca languages also to be linked (Jones 1792 [1807]a). Vallancey had met Jones before the latter's departure for India, and the two remained in touch by letter thereafter (Vallancey 1807: 14). Vallancey's 1786 *Vindication* contained a short statement suggesting that he knew the gist of Jones's Indo-European idea before it was published: 'another proof of an Oriental colony in Ireland, may be drawn from the great affinity of the old Irish with the Sanscrit or Hindostan language, particularly in theological terms; a strong proof, in our opinion, of the Bramins deriving their origin from the Tuatha Dadann of Irish history, being a mixture of the Southern Scythians with the Dedanites of Chaldæa' (Vallancey 1786b: 365); and a copy of this book reached Jones in Calcutta a year after its publication in Dublin (Vallancey 1807: 15). For his part, Jones certainly read *Vindication*, although he was rather dismissive of it in letters to friends (Trautmann 1997: 97–8). We may be sure Jones had also read Pownall's (1773) article on New Grange and Phoenician colonization, although he did not cite it by name, because when discussing the spread of people from Phoenicia into Greece and Italy, he mentioned that 'a swarm from the same hive' moved north into Scandinavia (Jones 1792 [1807]a: 491). This reiteration of Pownall's apian metaphor (see above), and in precisely the same context, cannot have been by chance. Wilford sent Vallancey a packet of papers through an intermediary in 1796 (Vallancey 1797: 8), and Vallancey later cited a publication of Wilford's (Vallancey 1807: 24). Thus Jones and Wilford in Calcutta were in touch with Vallancey in Dublin, and they all knew each other's work. Their shared biblical perspective led them to expect a monogenic derivation of peoples, and Jones demonstrated the truth of this with his Indo-European connections. Wilford's papers on the ancient history and geographical knowledge of the ancient Indians caused the rest of the expected edifice to fall neatly into place.

Wilford's results were little short of momentous. He had learned Sanskrit, and was translating documents acquired for him by the 'pandit' or Indian expert he employed. He extracted his material from the 'Puranas', the sacred Sanskrit books, which he knew to contain 'historical poems' or 'legendary tales' (Wilford 1792 [1807]: 295)—but was struck by the parallels he found with western

history and geography. He cited the account of a patriarch named Satyavrāta, who was miraculously preserved from a universal flood. Satyavrāta had three sons, named Sharma, C'harma, and Jyāpeti, between whom he divided the earth after the flood receded; Jyāpeti was given the lands north of the Himalayas, Sharma those to the south, while C'harma was cursed. This was clearly a version of the biblical account of Noah and the deluge, and Noah's sons Shem, Ham, and Japhet, and if this was not enough, the Puranas even stated that the descendants of Sharma reached a river Wilford identified as the Nile, where they built a pyramid dedicated to 'PADMA'-*dévi*, or the Goddess residing on the Lotos' (Wilford 1792 [1807]: 313). There were mentions of a country called *Mísra* or *Misrena*, very similar to *Misr*, or Egypt. Only one conclusion could be drawn: 'the word *Misr*, which the *Arabs* apply to *Egypt*, and to its metropolis, seems clearly derived from the *Sanscrit*;... of *Misr* the dual and plural forms in *Hebrew* are *Misraïm* and *Misrím*, and the second of them is often applied in Scripture to the people of Egypt' (Wilford 1792 [1807]: 337, original emphases). Many points of etymology and geographical description supported this; the River *Cáلی*, on which *Mísra* lay, could only be the Nile.

The implications were remarkable: ancient Indian writers knew Old Testament history, and the geography of lands far to the west. But from a monogenic and biblical perspective it of course made perfect sense that they should, and Sir William Jones inserted a paper immediately after Wilford's, endorsing its results and pointing to even more parallels in Wilford's documents (which Jones had gone through with Wilford and the pandit). The Greek legends of Cepheus and Cassiopeia, and of Perseus and Andromeda, were paralleled in the Indian stories of the similarly-named Capéya and Cásyapi, and Párasica and Antarmadá, respectively (Jones 1792 [1807]b: 467). And there was more to come. In a subsequent paper Wilford identified a place referred to as *Mócsha*, pronounced *Moca* or *Mucta*, as Mecca, stating that Islam had replaced an earlier Indian religion in the area (Wilford 1795 [1807]: 369–71); and he mentioned Hindu colonies as far away as Astrakhan in southern Russia, where they venerated the Volga under the name of *Súrya muc' hí-Ganga* (1795 [1807]: 380).

And even this was just the beginning. In 1796 Mr Gore Ouseley (a member of the Asiatic Society, which a couple of years earlier had

dropped the 'k' from 'Asiatick') carried a packet of papers from Wilford to Vallancey. These included unpublished texts from the Puranas *which showed that the ancient Indian writers knew the British Isles*. Vallancey published this information the next year, in a book entitled *The Ancient History of Ireland, proved from the Sanscrit Books of the Bramins of India* (Vallancey 1797). Vallancey stated that Wilford's sources indicated that the British Isles were called *Tricatachel*, or 'mountain with three peaks'; one of these peaks was called *Sveta-Dwip*, or 'white island', seemingly corresponding to the ancient name for England, 'Albion', which had the same meaning; the second was called *Suvarna-Dwip* or *Suvarna-Cuta*, synonymous with *Su-Cuta* or *S'cuta*, hence 'Scotia'; and the third was called *Suvarneya*, similar to 'Hibernia' and said to be the abode of *Pitris*, 'the fathers' (Vallancey 1797: 9–12). These correspondences were so exact that Vallancey was left in no doubt: the ancient Indians had had links with Ireland. He identified *Pitris* as St Patrick (1797: 12), and many geographical names were shared between the two places; thus the Puranas referred to the Indus as the *Soor* or *Shoor*, and 'everyone knows that this is the name of that beautiful river, which flows by the City of Waterford' (1797: 24). This was a massive vindication of Vallancey's position, providing support from an independent historical tradition that demonstrated ancient connections even further east than he had previously maintained.

But back in Calcutta, Wilford had not yet published the original documents; and when he set to work to do so, it all went horribly, disastrously wrong. Checking the original documents, he noticed that wherever the key name *S'wetam* or *S'weta-dwīpa*, the term for 'Britain', appeared in the text, the writing looked different and the paper was discoloured. It seemed that his texts had been altered; holding the documents up to the light, he could see that the original names had been painted out, and *S'weta-dwīpa* written over them. He was 'thunderstruck' (Wilford 1805 [1808]: 248), as well he might be. He reassured himself that he had not yet published his paper on the western islands (apparently giving no thought to Vallancey, who had used the information in his 1797 book)—but was then struck by a horrible thought. He checked back to the documents on which he had based his earlier paper about Egypt, and discovered that *in these too the key geographical names had been*

altered. 'I shall not trouble the Society with a description of what I felt, and of my distress at this discovery', he wrote (1805 [1808]: 248). Only one person could be responsible. Wilford confronted his pandit, and the two men had a blazing row. It turned out that instead of hiring the assistants and copyists Wilford had paid him to employ, the pandit had embezzled the money and for years had supplied Wilford with fraudulent material. He had systematically doctored such genuinely old manuscripts as he had supplied, in the way that Wilford had spotted; he had also inserted new pages into many of them, and in addition had personally composed entirely new documents in the Puranic style, amounting to no fewer than 12,000 lines of literature—and then passed them off as originals. Wilford had discussed biblical stories and Greek and Roman mythology at length with the pandit, who had simply rewritten them in traditional Indian style, and given them back to him. There was no Indian version of the story of Noah and his sons, or of the Greek myths. Sir William Jones had died in 1794 without knowing about any of this; Wilford noted sadly that 'I shall ever lament that I was the cause of SIR WILLIAM JONES being thus misled like myself' (1805 [1808]: 262).

The unfortunate Wilford was obliged to publish a humiliating retraction (op. cit.: 248–66). But perhaps the most remarkable aspect of this entire curious story was the fact that, despite realizing that he had been hoodwinked and making this full admission, Wilford *nevertheless continued to believe that the Puranas described Britain and Ireland*. Some documents the pandit had supplied had after all been genuine, and in these 'many of the legends were very correct, except in the name of the country, which he generally altered into that of either *Egypt* or *S'wétam*' (1805 [1808]: 251). These were still useful, 'because they always contain much truth, and the learned, therefore, have not been misled in their general conclusions from my essay on *Egypt*; though it would be dangerous for any one to use detached passages, and apply them to any particular purpose' (Wilford 1805 [1808]: 262). Furthermore, 'with regard to the *British Isles*, I soon found that the grand outlines were perfectly correct; even more so than those of my essay on *Egypt* and *Ethiopia*' (Wilford 1805 [1808]: 263). Having been hoodwinked by the pandit, Wilford now proceeded to hoodwink himself:

The White Island, in the West, is the holy land of the Hindus. It is of course a sort of fairy land, which, as might be expected from their well known disposition, they have not failed to store with wonderful mountains, places of worship, and holy streams. It would be highly imprudent to attempt to ascertain their present names and situation; though I have occasionally broken through this rule, and may have been seduced, by a strange similarity of names and other circumstances, within the fascinating attraction of conjectural etymology. (Wilford 1805 [1808]: 263)

Wilford's self-deception reached a climax in his final paper on the subject (Wilford 1808 [1812]). He unabashedly used the names the pandit had provided for him, such as *Tri-cûta*, or 'Three-peak-land', and *Swétam* (1808 [1812]: 11). He drew a map of the northwestern quarter of the world based on descriptions in the Puranas (fig. 6.3) 'and the only additions I have presumed to make, are, first, a rough delineation of the western shores of *Europe*; and secondly, the polar circle' (1808 [1812]: 12). Having inserted these additions, Wilford then proceeded to amaze himself at how accurately they depicted Europe, Iceland, and even America (see fig. 6.3):

The shape and general outlines of the western shores, in the accompanying map, bear no small affinities with those of *Europe*, which they were intended to represent. There we may trace the *Bay of Biscay*, the *German Sea*, and the entrance into the *Baltic*. But, above all, the greatest resemblance is in the arrangement of the *British Isles*, *Iceland*, and the adjacent shores of *America*: and this surely cannot be merely accidental. The islands of *St'hula*, or *Thule*, now *Ferro*, *Chan'dica*, the *Shetland Isles*, *Indradwipa*, or the *Orkneys*, are placed beyond the *British Isles*; and I have arranged them in the manner they are in the map, on the supposition that they really answer to the above islands. (Wilford 1808 [1812]: 13–14).

He resorted again to comparative etymology, using the pandit's geographical nomenclature with no apparent pang of conscience: 'MUCTI-DWÍPA is also another name for the *White Island*... and signifies the blessed island' (Wilford 1808 [1812]: 21), while '*Hira-n'ya* and *Su-varn'eya* are obviously the same with *Erin*, and *Juvernía*, or *Ireland*... the third peak-land, or *Scotland*, is called *Aya-cûta*, or the *Iron peak or island*' (Wilford 1808 [1812]: 22).

Wilford's disaster should have had major repercussions in Ireland, but it did not. The aged Vallancey produced his last book in 1807 (he was 82 that year). He evidently never knew of Wilford's 1805

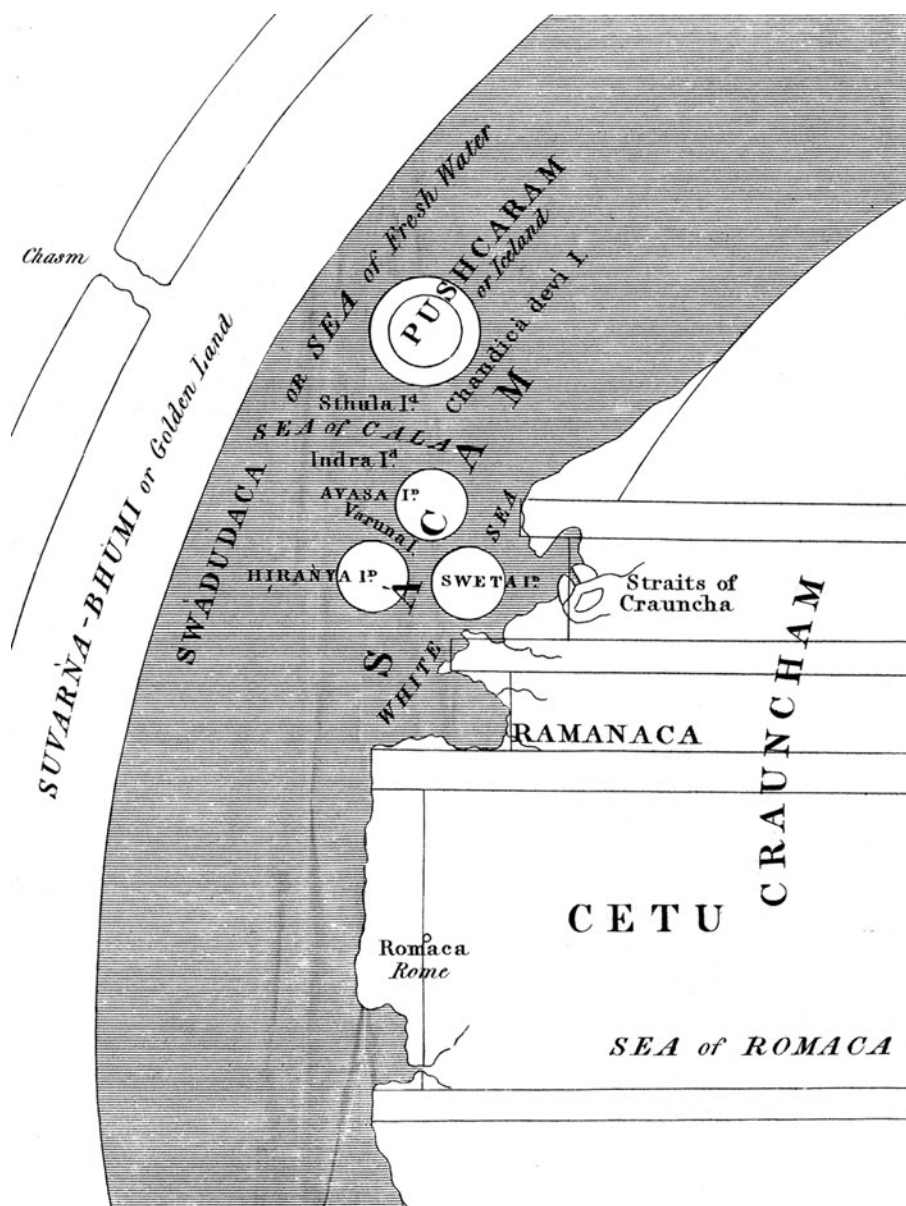


Fig. 6.3. Map drawn by Francis Wilford showing the purported knowledge of northwest Europe demonstrated in ancient Indian geographical texts. (From Wilford 1808[1812], facing p. 31).

admission that he had been defrauded by his pandit, because his book was entitled *An Essay on the Primitive Inhabitants of Great Britain and Ireland. Proving from History, Language, and Mythology, that they were Persians or Indoscythæ, composed of Scythians, Chaldeans, and Indians*. He dedicated it to among others the ‘Learned Society of Calcutta’. He continued to believe that there was an ancient Indian story of Noah: stating that the Irish had ancient mentions of two persons called *Menu*, one of whom was the parent of mankind, the other the survivor of a universal deluge, he added that ‘the first is *Menu* or Adam of the Brahmins; the last, *Menu* or Noah of the Brahmins; names that reflect light upon the researches of Sir Wm. Jones, and the learned society of Calcutta; at the same time, they establish the authenticity of the ancient Irish history’ (Vallancey 1807: ix). He quoted Jones on many other occasions, and also Wilford’s statement that there were Hindus on the Volga (see above). He concluded that Ireland’s ancient inhabitants were Indo-Scythians or ancient Persians. Their architectural legacy was the famous round towers: ‘they made no stone buildings, the fire tower excepted, which was copied from the most ancient pagodas of India’ (Vallancey 1807: 147).

The opening decades of the nineteenth century saw a lessening of activity in Irish historical and archaeological circles (McDowell 1985: 22; Waddell 2005: 96). There was little discussion of Vallancey’s scenario, which thus remained largely intact by default. When there was a renewed upsurge of interest in the 1820s much of the writing was, as we shall see, in continuation of Vallancey’s work. Despite opposition from George Petrie and his coterie, the oriental and Indian connection continued to be prominent through most of the nineteenth century. Of those discussed below, only the eccentric Henry O’Brien even mentioned what had befallen Wilford—and as we shall see, he did so only to say that Wilford and his pandit had actually been right all along.

WORSAAE AND IRISH ARCHAEOLOGY

The archaeological world encountered by Worsaae on his arrival in Ireland was very different from those of England or Scotland. In

Ireland, archaeology did not have a separate society of its own, but remained very much part of the RIA; Worsaae may have considered a separate archaeological institution desirable, because in the second of two talks he gave to the RIA he noted that 'the archaeologists have not yet their general meetings like the naturalists' (Worsaae 1846c: 344). At its foundation in 1785, the RIA had established three committees to advise on the publication of papers: Science; Polite Literature (mainly classics and philosophy); and Antiquities. At this time there were insufficient numbers of men of learning in Ireland to justify the establishment of independent societies for each (McDowell 1985: 13). Vallancey was a member of the Antiquities Committee. The RIA's *Transactions* (*TRIA*), which appeared from 1787, contained separately paginated sections from each of the three committees. Scientific contributions were numerically predominant from the start, but the few papers on antiquities included important contributions from Vallancey, Ledwich, and Beauford (see above). In 1790 two rooms were made available to house the collection of artefacts, and a gift of Danish antiquities was received in 1815 (Mitchell 1985: 107).

In the early decades of the nineteenth century, the RIA lost its early impetus. The Polite Literature Committee met on 14 May 1810, but did not do so again until 11 February 1839 (McDowell 1985: 22). Antiquities saw something of a resurgence in the 1820s under the influence of Sir William Betham, the gentleman whom we have already encountered in chapter 4 giving an unexpected paper on Phoenician colonizations at the Canterbury meeting of the Association. An Englishman, he came to Dublin in 1805 and was appointed to the heraldic office of Ulster King of Arms in 1820. He was a strong supporter of Vallancey's oriental hypothesis, and has indeed been described as 'almost a reincarnation of Vallancey' (Mitchell 1985: 99). As we shall see, the *TRIA* began publishing papers in this vein once again during the 1820s. George Petrie provided further revitalization from the 1830s, and the Antiquities Committee was reformed in 1836 (Briggs 1999: 351). The collection of antiquities had been run down, and many items including those from Denmark were lost in building works in 1834, and in a fire in 1837 (Mitchell 1985: 107). Petrie's purchase of a small private collection for the RIA in 1837 marked its rebirth (Stokes 1868: 79), but we shall see that a decade

later Worsaae was not impressed with it. Archaeology remained a relatively minor part of the RIA's activities, and Worsaae wrote to his mother that before giving his first talk, he had to sit through 'a whole lot of mathematical presentations etc' (Worsaae 1934: 326*). The *TRIA* and the RIA's new *Proceedings* (*PRIA*, published from 1836) reflected this, appearing intermittently and containing only a minority of papers on archaeology. Thus although the RIA encompassed archaeology, neither in disciplinary emphasis nor in publication intensity was it a counterpart to the London-based Antiquaries, Institute, and Association, or the SAS in Scotland.

Worsaae arrived in Dublin on the evening of 11 November 1846, and departed on 8 February 1847. His posthumously published memoirs ended with his departure from Scotland, so for his own account we are restricted just to the letters he wrote home (Worsaae 1934). Charles Kirkpatrick Sharpe, the crusty old gentleman Worsaae had met in Edinburgh, thought little of Scots Highlanders and less of the Irish; he warned Worsaae that the Irish outdid the Highlanders in both idleness and pride, and would seek to mislead him with tales of their ancient glory; 'beware of "Paddy"' was his final comment (Worsaae 1934: 166*). But Worsaae thoroughly enjoyed the Irish, reporting to his mother that he was out most nights and liked the banter: he would tease people about how few Scandinavians it had taken to conquer Ireland, and they would retaliate by talking about the defeat of the Danes at the Battle of Clontarf in 1014 (Worsaae 1934: 340); 'the Irish are incomparably more lively and more fun than the Scots' (Worsaae 1934: 343*).

Worsaae's stay in Dublin was his longest in any of the capitals he visited. He visited Tara, New Grange, two round towers, and the Clontarf battlefield, but these involved overnight stays at most. The famine was ravaging the countryside, and this was not a good time for extended trips in the hinterland. Dublin itself was not much affected by the famine, and it has been said that Worsaae was hardly aware of the situation (Henry 1995: xiv), but this impression is based only on the letters to his mother and to Thomsen (Worsaae 1934). One other letter shows that Worsaae was in fact highly aware of the famine; this he wrote to Jonas Collin, a government official in Copenhagen, on 28 January 1847 (Clément 1930: 15–18). This important letter, which discussed the political situation at some length

and showed how the factions were seeking to use the past to legitimize their cause, is translated in Appendix 5.

Worsaae lost no time in contacting people and examining collections of antiquities; in a letter to Thomsen written just twenty-four hours after his arrival, he stated that he had *already* visited George Petrie and had examined his personal collection, and had also looked through the RIA's collection. The former was small, the latter rather a mess, stored in a single room and all mixed up. Once again he paid particular attention to antiquities of Scandinavian origin (Worsaae 1934: 318). On 8 January he wrote to Thomsen that he had acquired a bowl-shaped brooch found with a skeleton just outside Dublin (Worsaae 1934: 335). This may have been from Kilmainham, where railway construction had uncovered several graves; in 1851 he illustrated some Scandinavian swords from this site, with an Irish one for comparison, and two bowl-shaped brooches (fig. 6.4). He added that he might be able to obtain more from the RIA's collection, but that he would take only a few poor items; he did not want to commit Thomsen to an expensive exchange, and the RIA's collection was in any case too poor to stand the loss of good items (Worsaae 1934: 335).

Worsaae's two presentations to the RIA⁴ were apparently the only formal talks he gave on his entire trip, despite his later statement to the contrary (see chapter 4). The first was delivered impromptu at the behest of Petrie, who was responsible for the RIA's collection and wanted the members to hear about the museum in Copenhagen. It was relatively brief, largely describing Thomsen's work and including a short mention of the Three Age System; it described Thomsen's 1831 pamphlet on what to do with antiquities (Worsaae 1846c: 313). His second talk was much more of a formal statement. It is the one commonly quoted as evidence for his propagation of the Three Age

⁴ The title page of the offprint of the *Proceedings of the Royal Irish Academy* containing the text of Worsaae's two talks states that they were delivered on 30 November and 7 December, while the heading in the text gives the date of the second as 14 December. In the *Proceedings* itself, the date is also given as 14 December, although 7 December has been quoted at least since 1851 (D. Wilson 1851a: 17 n. 2). We can however be sure that 14 December is the correct date, because in a letter dated 'Christmas Eve 1846' Worsaae wrote to Thomsen that he had delivered his second talk 'last Monday week' (Worsaae 1934: 330*). Christmas Eve in 1846 fell on a Thursday, so the Monday of the previous week would indeed have been 14 December.

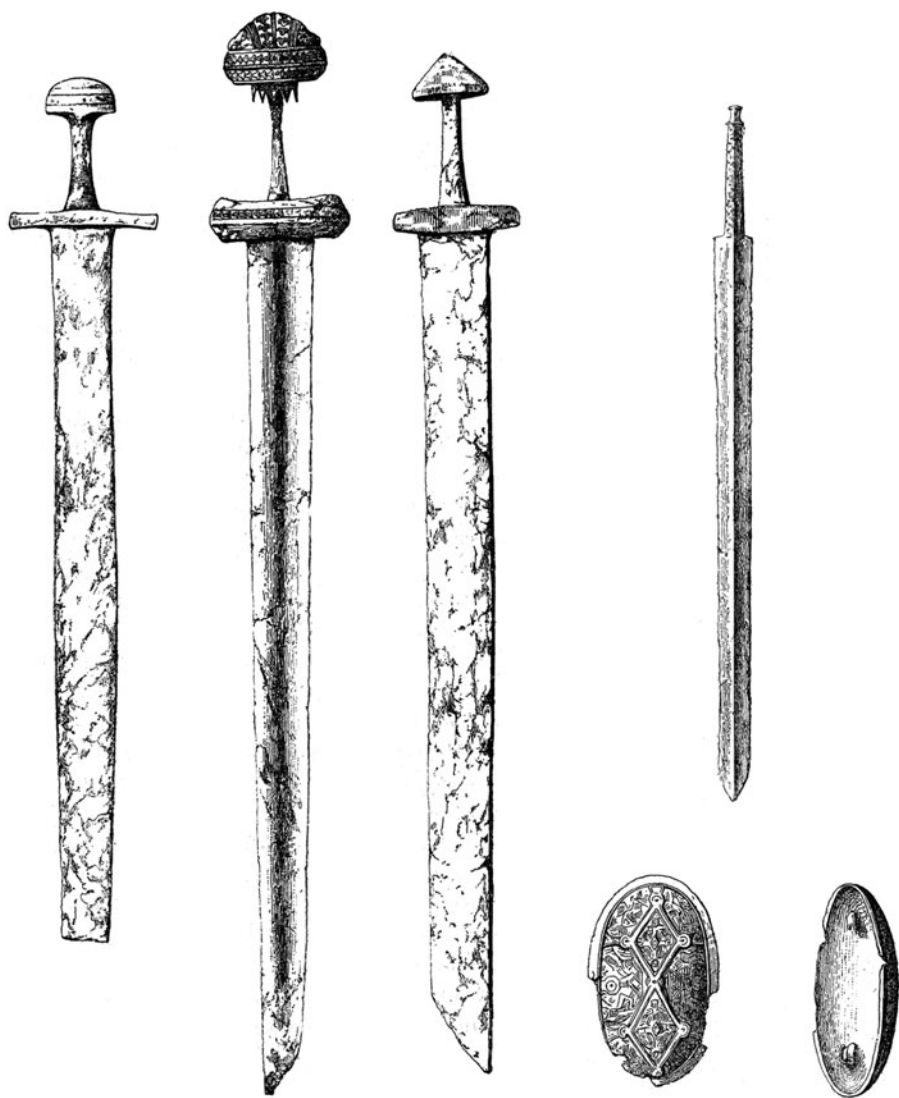


Fig. 6.4. Worsaae's illustrations of three swords and two bowl-shaped brooches of Scandinavian origin, found at Kilmainham near Dublin. The smaller sword is of Irish origin, added for comparison. There is no indication of scale on any of Worsaae's figures, but he did state that the Irish sword was much smaller than the Scandinavian ones. (From Worsaae 1851: 406, 407, 408).

System, and he did indeed present it at some length. Worsaae wrote to his mother that he had not written out the text in English before delivery, and in writing it up for publication he was considerably aided by newspaper reports of what he had said (Worsaae 1934: 330). The publication used 'Period' rather than 'Age' to refer to the main epochs; hence the use of 'Stone Period', 'Bronze Period' etc. by Daniel Wilson (1851a) and subsequently Bateman (1852) (chapters 4 and 5). Worsaae's coverage was fairly straightforward; worthy of note was his statement that the Stone Period was before the reach of all history, being 'arrived at by the study of antiquities alone' (Worsaae 1846c: 329)—very much reiterating his exchanges with Munch and Grimm at this time (see chapter 3).

One important fact about Worsaae's second presentation is often overlooked: the relative weighting of its sections. His discussion of the Three Age System in fact amounted to fewer than five pages, but he devoted over twice as much to *praising ancient history*, in clear contrast to the hatchet job he was doing at home (chapter 3). Pointing out that both the Scandinavian and the Irish literatures were written in their respective native tongues rather than Latin, he discussed saga descriptions of captured Irish princesses, and annal references to Viking raids (Worsaae 1846c: 343).⁵ He was evidently genuinely impressed by the ancient Irish literature, and in highlighting its Viking references he was not endorsing the suggestion that sources such as the *Annals of the Four Masters* could shed light on events centuries or millennia earlier, still less the wilder theories then

⁵ The text of Worsaae's two talks, along with a translation of the letters he wrote and received while in Dublin, and sections of the English edition of the book he wrote about the Scandinavian archaeological traces he observed in Britain, have been published by Henry (1995). The text of the talks published in the *Proceedings of the Royal Irish Academy* refers several times to 'brass', changed to 'bronze' in the separate offprint; Henry follows the journal in this respect. The otherwise admirable translation of Worsaae's letters contains one error worthy of correction. In a letter to Thomsen dated 12 November 1846, in a discussion of bronze artefacts, we find in the translation: 'The lance points have unique kinds of lobes which we don't find on ours; the Celtic ones are generally more rounded' (Henry 1995: 1–2). Worsaae however actually wrote '*Celterne er gjerne mere runde*' (Worsaae 1934: 318), and this means 'the *celts* are generally more rounded'. Worsaae was not describing corpulent Bronze Age Irishmen; he was using 'celt' to mean 'axe', a usage normal in both English and Danish at the time. Had he wished to attribute an artefact class to Celtic people he would have used the word adjectivally, '*de celtiske*'.

current. Sir William Betham, the ‘reincarnation of Vallancey’ mentioned above, had published a book entitled *Etruria Celtica* (Betham 1842), in which he argued that the Irish language was of Etruscan and Phoenician origin. Carl Christian Rafn, the secretary of the KNOS, was considering publishing some Irish annals in Copenhagen, and wanted Worsaae to contact Betham; Worsaae however dismissed him contemptuously in a letter to Thomsen written on 8 January:

I will not be caught up in fantasies; Rafn wants me to consult Sir William Betham, who has written the famous Etruscan-Irish books!!! He is in truth the last person I want to consult . . . Do warn Rafn about Sir W. Betham; that man has damaged the reputation of the KNOS here a great deal. Rafn seems formerly to have regarded him as an authority, which forced men like Petrie out of the KNOS! (Worsaae 1934: 338*)

As we shall see, the feud between Betham and Petrie was a major factor in Irish archaeology in the 1830s and 1840s, and the KNOS membership lists for the 1840s indeed reveal that Betham was a member, while Petrie was not. Worsaae however made no public criticism of Ireland’s ancient history. There are three possible reasons for this. First, he may genuinely have believed that it might indeed have the chronological reach it claimed; but given his approach at home this seems unlikely. Second, he may have felt that he could not criticize his hosts; he was actually staying with James Henthorn Todd, a major figure involved in the translation and publication of annals, and, perhaps mindful of Kirkpatrick Sharpe’s warning, he may have concluded that it was easier to let it go. Third, and perhaps most important, any such attack would have had to include Petrie himself among its targets. Petrie was the most significant man in Dublin interested in the archaeological record, but his enthusiasm for the ancient annals was (as we shall see) leading him to hang archaeological monuments onto the ancient historical chronology in precisely the way Worsaae was contesting at home.⁶

The movement that ultimately led to the Royal Society of Antiquaries of Ireland in 1890 was founded in 1849. It was the antithesis

⁶ I am grateful to Stephen Briggs for the information that Worsaae stayed with Todd, and that Rafn was interested in the possibility of publishing Irish annals in Copenhagen.

of the organizations based in capital cities. Its emergence was like that of a successful guerrilla movement: foundation in hard times in a province far from the capital, under a couple of visionary leaders; spread into adjoining provinces; growth to take over the hinterland; the establishment of a fifth column inside the capital; and finally the capture of the capital and establishment within it. Such was the history of the organization founded as the Kilkenny Archaeological Society in 1849, as its successive names show:

The Kilkenny Archaeological Society (1849–54)

The Kilkenny and Southeast of Ireland Archaeological Society (1854–68)

The Historical and Archaeological Association of Ireland (1868–69)

The Royal Historical and Archaeological Association of Ireland (1869–90)

The Royal Society of Antiquaries of Ireland (since 1890)

The impetus came from two men we have already encountered in chapter 4 as founder-members of the Institute in 1846: James Graves, a Kilkenny curate, and his cousin John Prim, editor of the *Kilkenny Moderator*. By 1849 the population of Kilkenny had fallen by 2,000 due to the famine, but the Society attracted a membership of 149 in that year, mostly from Kilkenny itself. In the early 1850s it appointed local secretaries in surrounding counties (Ireland 1982). It grew apace, acquiring Royal patronage in 1869, but in 1870 half its membership still came from the Kilkenny area. By 1874 membership had widened significantly: in 1850 it had had only 1 member in Dublin, but by 1874 this had grown to 107, with 37 more in London (McEwan 2003: 50–7, table 3). Its meetings were held in Kilkenny until the 1880s, when some began to be held in Dublin, and by that time some of its officers also lived in the capital. The Society officially moved to Dublin in 1890, when it changed its name for the last time; in the next decade its Dublin membership tripled, while its Kilkenny membership halved (Ireland 1982). In a final parallel with guerrilla movements, this led to alienation from its original heartland, and a new Kilkenny Archaeological Society was founded in 1946. Graves and Prim edited the successive periodicals, whose names tracked that of the society, until Prim's death in 1875.

But during Worsaae's visit all this lay in the future. It must therefore have appeared to him that Petrie represented the best hope for Irish archaeology. However, in the dozen years after Worsaae's visit, Irish archaeology mirrored the trend in England and hardened its position *against* the Three Age System. The first issue of the Kilkenny journal did, it is true, contain a response by its editors to Worsaae's (1846c: 313) description of the pamphlet issued to the general public in Denmark; entitled 'Hints and queries intended to promote the preservation of antiquities, and the collection and arrangement of information on the subject of local history and traditions', it was clearly intended to do the same job (Graves and Prim 1851). In the same issue there was however a paper by Prim on megalithic graves, stating that the Danish scheme might not work in Ireland:

Mr Worsaae states that in Denmark they [*the graves*] are ascertained to belong exclusively to the Stone-period, before the custom of burning dead bodies was introduced, and that consequently the entire skeletons are there always to be found in them. In Ireland our researches have not yet been conducted with sufficient accuracy or system to enable us to come to any positive conclusion on this subject; but in more than one instance bones of men and animals subjected to the action of fire, and baked clay urns containing ashes, are said to have been found in Giants' Graves. (Prim 1851: 16)

Papers on prehistory were generally rare, and in the second half of the 1850s pretty well ceased altogether. On the few occasions authors mentioned chronology in the 1850s, it was to link material to ancient history; for example when John Windele (1853) discussed some cist graves his aim was to determine whether they contained the bodies of Milesians or Fenians.

One other local society briefly published a journal in the 1850s. The Ulster Archaeological Society, founded in 1853, produced nine volumes of the *Ulster Journal of Archaeology* between 1854 and 1862, after which the journal foundered. Here too, the little attention given to the Three Age System was hostile. The society's founder noted that stone tools were so common in Ireland that they must have been used until quite recently, so while the 'theory lately promulgated by Danish archaeologists' might work elsewhere, it was 'not supported (so far as Ireland is concerned) by the experience of archaeologists in

this district' (MacAdam 1855: 234). Another writer stated that in the River Bann, being dredged like the Shannon, stone axes lay *above* a series of bronze finds and were thus younger than them (O'Lavery 1857).

Publications emanating from Dublin also showed little interest. William Wakeman's book *Archæologia Hibernica*, published the year after Worsaae left, simply classified everything pre-Christian as 'pagan' (Wakeman 1848). The only hint of the Three Age System was a mention of a bronze pin and two iron knives found inside the megalith of Dowth. Wakeman apparently found this anomalous, accounting for it by stating that 'with respect to instruments of iron being found in a monument of so early a date, we may observe, that in the Annals of Ulster there occurs a record of this mound . . . having been searched by the Northmen of Dublin as early as A.D. 862' (Wakeman 1848: 33). These were evidently the people who had introduced the metal implements—which implied that Dowth was constructed in a pre-metallic era. William Wilde's *The Beauties of the Boyne and the Blackwater* appeared in 1849 and 1850. This book will be discussed fully below; suffice it here to mention that Wilde also discussed the Dowth finds (Wilde 1850: 209), but apparently *without* finding them anomalous. Edward Clibborn, the curator of the RIA's museum from 1844, was an open opponent of the Three Age System (Mitchell 1985: 116, 120), and the *PRIA* reflects a similar trend. Griffith's (1844) paper on the Shannon finds, discussed at the start of this chapter, did not in fact initiate any chronological discussion. The succeeding issues contained a diminishing number of notices of finds, and only one discussion of chronology: John Kemble's article *attacking* the Three Age System (Kemble 1857). He opened by stating clearly that 'I hold very different opinions from my friend Dr. Worsaae; and that from the conviction that the adoption of his opinions, and the pushing them to their legitimate consequences, would betray us into grave historical errors, I feel it my duty on this occasion to protest as publicly against them as he himself gave utterance to them' (Kemble 1857: 463). While there was a time when metals were not used, as the Shannon finds showed (Kemble 1857: 464), bronze and iron were everywhere employed contemporaneously, while stone continued in use until the seventeenth century (1857: 467). Any or all materials might therefore occur in funerary

contexts; consequently 'it is impossible to apply with strictness the canon of Copenhagen to the characterizing the graves according to their different periods' (Kemble 1857: 467).

The two major archaeologists in mid-nineteenth-century Ireland were George Petrie and William Wilde. Neither was a professional archaeologist. Apart from some years in the Ordnance Survey, Petrie was a landscape painter and illustrator. Wilde was a noted eye surgeon, and also played a major role in the Irish census, for which he was knighted. They never collaborated except (briefly and unsuccessfully) on the crannog of Lagore; Briggs (1999) regards their personalities as incompatible, characterizing Petrie as 'of sensitive and romantic personality . . . , slow and careful in bringing his personal tasks to completion', Wilde as 'a prolific polymath, efficient and incisive in his rapid syntheses of any subject matter that came before him' (Briggs 1999: 352). Neither was to bring the Three Age System to Ireland.

GEORGE PETRIE AND THE ROUND TOWERS

Petrie's scientific approach has been contrasted to that of Sir William Betham: 'by mid-1838, the Royal Irish Academy had become the arena in which two paradigms were fighting a duel to the death: old-fashioned, entrenched, genteel muddle-headed amateurishness as championed by Sir William Betham; newfangled, scientific, pedantic and intolerant factualism as championed by George Petrie' (Leerssen 1996: 128). This section examines how Petrie dealt with one major problem: the date of the Irish round towers (fig. 6.5). This was hugely controversial, and it was said at the time that 'a ready method of testing the sanity or insanity of an Irish antiquary is to ask him his opinion as to the round towers' (Stokes 1868: 143).

Petrie collected his data on the round towers while working for the Ordnance Survey of Ireland. The Survey, which he joined in 1828, intended to record ancient monuments, and Petrie was in charge of the antiquarian section. His staff included the Gaelic-speaking historians John O'Donovan (later publisher of the *Annals of the Four Masters*) and Eugene O'Curry, the draughtsman William

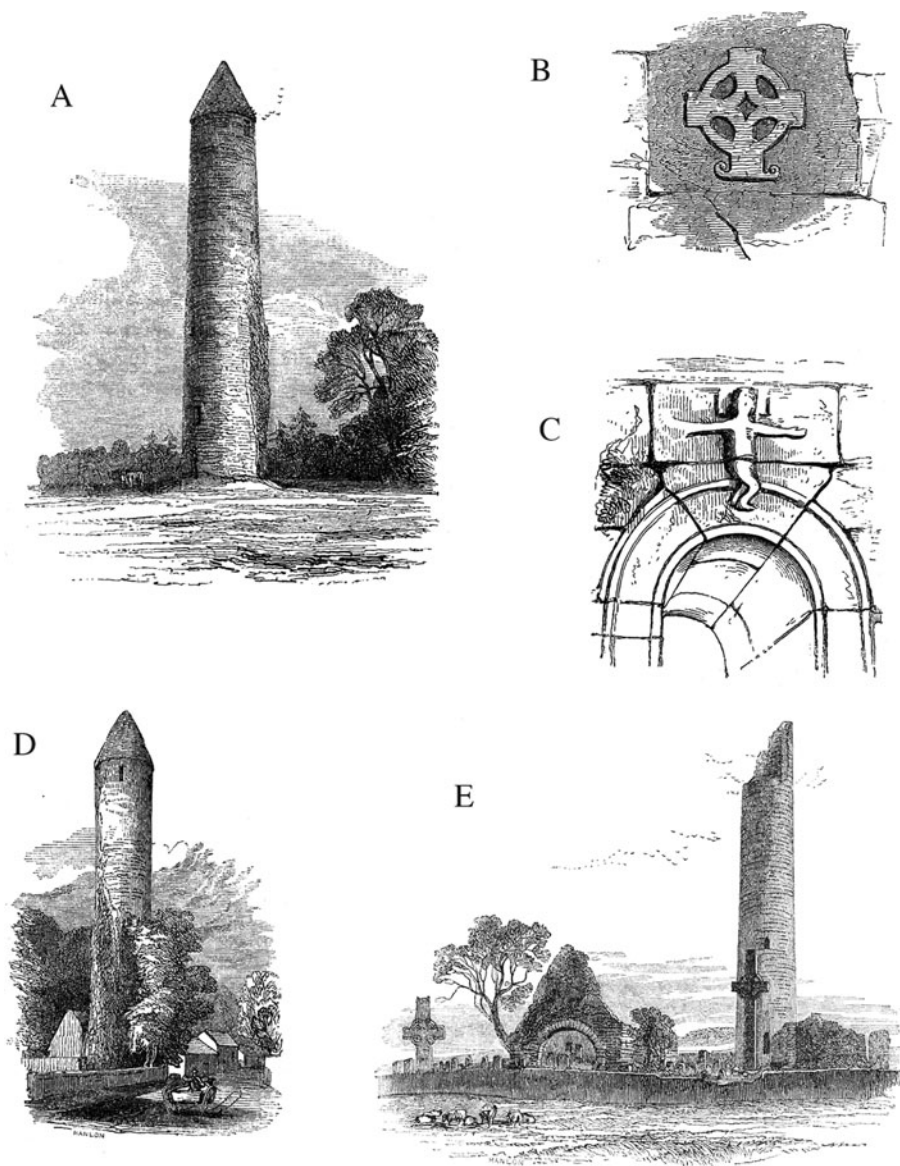


Fig. 6.5. Round towers and their Christian symbolism. A. Antrim tower; B. cross over the doorway at Antrim tower; C. sculpture of crucifixion over the doorway at Donoughmore tower; D. Clondalkin tower; E. general view of Monasterboice. (From Wakeman 1848: 99, 101, 102, 103, 105).

Wakeman (who later wrote *Archæologia Hibernica*), and George du Noyer. Each county map was to be accompanied by an antiquarian memoir, but only one was ever produced, a large volume covering Londonderry. It became clear that at this rate the project would be hugely expensive and would never be finished, so in 1839 this part of the Survey was wound up, and the staff discharged; Petrie resumed his earlier career as a painter (Mitchell 1985: 101–4; Stokes 1868; Waddell 2005: 97–103). Much work had been done, and Petrie's publication of *Tara* (discussed in the next section) was originally intended as part of the memoir for County Meath (Petrie 1839). Petrie, a Protestant, was elected to the RIA in 1828 and joined its Council in 1829. O'Curry and O'Donovan were both Catholics; O'Curry was given a Chair when the Catholic University was founded in 1854, while O'Donovan had a Chair in Cork but lived mostly in Dublin. The three comprised the Dublin historical elite; Petrie and O'Donovan were among the founders of the Irish Archaeological Association in 1840, which published historical documents; O'Curry was also a leading member. O'Donovan was also a founder of the Celtic Society (as was Wilde). These two societies amalgamated in 1853 to form the Irish Archaeological and Celtic Society; the same year saw the foundation of the Ossianic Society. These historical societies did little to popularize the study of history, and their publications were available only to their members (McEwan 2003).

Petrie's publication on the round towers (Petrie 1845) was partly a response to suggestions that the towers were of ancient and exotic origin and purpose. This was first proposed by Charles Vallancey, who we saw above considered that they were Persian fire towers (Vallancey 1786b: 338–9), copied from the most ancient Indian pagodas (Vallancey 1807: 147). Subsequent writers accepted this. Charles O'Connor's⁷ *Rerum Hibernicarum Scriptores Veteres* of 1814 also espoused an oriental origin but made the towers astronomical observatories (Leerssen 1996). From the 1820s the *TRIA* saw several papers arguing in the same direction. J. T. Flaherty (1825) argued

⁷ Not to be confused with another Charles O'Connor (1710–1791), who wrote *Dissertations on the Ancient History of Ireland* (published 1753, second edition 1766) and also published in *Collectanea de Rebus Hibernicis* (Waddell 2005: 66).

that sacred fires would be difficult to keep alight in the Irish weather, so the roofed round towers were erected for this purpose. He quoted *Asiatic Researches* but did not argue for an Indian origin; for him, the proof that they were Phoenician or Canaanite was etymological: ‘the very name of Canaanite is still preserved in the Irish *Ceannaidhe*, pronounced *Canee*, a merchant. What else than Phoenician could be understood by the *Feine*, and the *Fenius Fear-siodh*, or “man of wisdom,” so much insisted upon in our remotest annals?’ (Flaherty 1825: 112, original emphasis). He disliked Ledwich’s assault on the veracity of the annals (see above) and criticized it strongly:

I feel bound to ascribe this sceptic spirit to a total ignorance of the early and genuine annals of the country and of the language in which they are conveyed. This prime defect, in the qualifications of a national historian, has been universally acknowledged by the unbelievers themselves. It is not difficult, however, to disprove their negatives: positive arguments they do not, and cannot advance. (Flaherty 1825: 101–2)

Three years later Miss Louisa Beaufort (1828: 131) accepted Pownall’s argument that the New Grange inscription (fig. 6.1) was Phoenician, but she regarded the towers as coming from further east. She quoted Sir William Ouseley’s descriptions of Indian parallels, and wondered whether a tribe called the *Malesir* in the Malabar area might show a link to the Irish St Molaise (Beaufort 1828: 152). She favoured a Zoroastrian origin, and stated that the Psalter of Tara, dating from AD 79, mentioned the sacred fire at Thlachtga tower in Munster (Beaufort 1828: 206). John D’Alton accepted that Persian missionaries had introduced fire worship. Some Irish towers were built on hexagonal bases, like the Indian ones (D’Alton 1830: 134 n.), and he quoted the ancient annals as saying that fifty-seven towers fell in an earthquake in AD 448, so they were certainly in existence by then (1830: 136). A few Christian symbols carved on the structures did not trouble him; ‘they are a very small exception indeed, and these unusual features are evidently the additions of much later days’ (D’Alton 1830: 137).

Petrie stopped the RIA publishing such works by restructuring its publications. The new *PRIA*, instituted in 1836, were a record of the RIA’s meetings, containing abstracts of papers read, and some shorter pieces such as Griffith’s paper on the Shannon finds. This left the

TRIA just for major papers, and Petrie was behind the setting up of a Committee of Publications to referee manuscripts (Leerssen 1996: 137). In 1836–8 Sir William Betham read four papers to the RIA championing Etruscan and Phoenician origins of Irish (30 November 1836; 13 February and 24 April 1837; and 22 January 1838). Abstracts appeared in the *TRIA*, and reveal that they were entirely in the orientalist tradition; the second for example was on a Phoenician secret society called the Cabiri, which had links with Ireland; Betham added rather mysteriously that ‘such members of the Academy as were Freemasons must be struck by analogies which he could not more clearly explain’ (Betham 1840a: 36). The Committee, however, would not accept the papers for publication in the *TRIA*; hence their independent appearance as *Etruria Celtica* in 1842—the book Worsaae derided in his letter to Thomsen. We saw in chapter 4 that James Cowles Prichard had demonstrated the Indo-European nature of the Celtic languages, including Irish Gaelic, in 1831. In contrast to the earlier universalist vision of Sir William Jones and Charles Vallancey, Indo-European was increasingly becoming an *exclusive* group of related languages; Betham had failed to grasp that Gaelic could therefore *not* be related to Semitic languages like Phoenician (or indeed to Etruscan, of unknown affiliation despite his claimed decipherment of inscriptions), so that his ‘highly civilized Phoenician-Celtic invaders’ (Betham 1842: I, 16) were a nonsense.

George Petrie’s definitive publication on the round towers resulted from his own suggestion that the RIA offer a prize for the best essay on the subject. The prize was offered in 1832, and Petrie duly won it, claiming the gold medal and £50 (Stokes 1868: 146); but his delay in publishing the essay (it would not appear until 1845) allowed Sir William Betham to ‘get his retaliation in first’—and also unleashed upon the world Henry O’Brien’s book *The Round Towers of Ireland, or the Mysteries of Freemasonry, of Sabaism, and of Budhism, now for the First Time Unveiled*. O’Brien’s essay was awarded a £20 consolation prize at the instigation of John D’Alton (Leerssen 1996: 112–14; Stokes 1868), a member of the RIA’s Council who (as we have seen) favoured the orientalist argument. O’Brien’s bizarre book combined a hectoring tone with rambling argumentation, and flashes of bitterness at not winning the RIA’s prize outright: ‘my sole object was to expose the *flimsiness* of that subterfuge, by which the Royal Irish

Academy, or rather their council! had hoped that they could blindfold the public, as well as they had succeeded in sequestering my prize!’ (O’Brien 1834: 381, original emphasis). What was the date of the towers? They were as old as the pyramids (O’Brien 1834: 412)—‘but the Royal Irish Academy say *no*’ (1834: 374, original emphasis). Where had their builders come from? Egypt (1834: 77). And Persia, as the similarity of the names ‘Erin’ and ‘Iran’ showed (O’Brien 1834: 127–8). And Etruria; ‘but who, let me ask, were those Etrurians? none other, most undoubtedly, than the Pelasgi or Tyrseni, another branch of our Tuath-de-danaan ancestors...’ (O’Brien 1834: 85). But the Indian connection was paramount: ‘let it be known that the “Round Towers” of Ireland were temples constructed by the early Indian colonists of the country’ (O’Brien 1834: 91, original emphasis). Worship of Shiva explained their shape:

But what was the form under which this *deity* was recognized?... The eastern votaries..., that their vivid imagination might be still more enlivened by the very *form* of the *temple* in which they addressed their vows, actually constructed their architecture after the model of the *membrum virile*, which, obscenity apart, is the divinely-formed and indispensable medium selected by God himself for human propagation and sexual prolificacy. (O’Brien 1834: 101, original emphases)

Thus the round towers were ancient Indian phallic symbols. This gained support from the Gaelic word *budh*, meaning ‘penis’, a clear connection to Buddhism (O’Brien 1834: 103). It also explained an otherwise obscure term for the round towers given in the *Annals of the Four Masters*:

Fidh-Nemphed; which, as it has heretofore puzzled all the world to develop, I shall unfold to the reader with an almost miraculous result. *Fidh*, then... is the plural of *Budh*, i.e. Lingam...; and *Nemphed* is an adjective, signifying *divine* or *consecrated*...; so that *Fidh-Nemphed*, taken together, will import the *Consecrated Lingams*, or the *Budhist Consecrations*. (O’Brien 1834: 104–5, original emphases)

O’Brien quoted Wilford repeatedly (e.g. 1834: 92, 220 n., 243–4, 248 n., 259–61, 325; and, misnaming him ‘Wilfrid’, 1834: 154). He derived his sexual theory from Wilford, who it will be remembered stated that Indian colonists of the Nile Valley had built a pyramid there in veneration of the goddess Padma-devi. O’Brien translated

her name as '*the deity of desire*' (1834: 93, original emphasis), and transferred the principal to the round towers, which because of their shape inevitably involved a change in the sex of the god worshipped. He was fully aware of Wilford's debacle with the pandit, but argued that it simply was not relevant to this issue:

the '*Pundit*' could have had no motive, either of interest or of vanity, such as influenced his *transcriptions*, here to mislead his victim. It was the mere utterance of a casual opinion, without reference to any deduction. Besides it was not the statement of the knave at all, but that of a number of religious men of letters, who all agreed in the ascription above laid down. (O'Brien 1834: 92, original emphases)

For O'Brien, Wilford had simply not gone far enough with his Irish-Indian links; recognising that Wilford's embarrassment 'has afforded scope for some jests at his expense' (1834: 325), O'Brien continued that

had he [Wilford] but known, however, the coincidence of *our monuments* with those *mysteries* which the Puranas record, how they mutually support and dovetail into each other, he could not only have laughed to scorn the traducers of his services, but fixed his fame upon a pinnacle of literary pride, which no *undergrowth* of envy could have subverted. (O'Brien 1834: 325, original emphases).

The fundamental proof of this was Wilford's mention of *Mucti-dwīpa* as another name for the sacred 'White Island' (see above); he had not seen the connection with Ireland, but it was crystal clear to the increasingly emphatic O'Brien:

The most *mysterious* and *religiously-occult* name given to *Ireland*, in the days of its pristine glory, was *Muc-Inis*.

This word has three interpretations—firstly, the *Boar Island*—secondly, the *White Island*—and thirdly, the *Sacred*, or rather the *Divine*, and *Consecrated Island of God*.

Is it necessary that I should say one syllable more to authenticate the Puranas, and identify this *hallowed* spot with the *paradise* of their encomiums? No: I shall not affront your understanding by so supposing. The explanation of this *single* term has, more effectually than could a *ship load of folios*, set to flight the hobgoblins of ignorance and of scepticism, and reared the castle of truth on the ruins of prostrated error. (O'Brien 1834: 327, original emphases)

On this basis O'Brien convinced himself that Wilford's pandit had been telling the truth all along: 'avaunt, then, *evermore* to the humbug of *back-reckoning*, and the charge of *imposture* upon the Brahmins! I flatter myself, I have laid an *extinguisher*, for ever, upon that pretext' (1834: 339–40, original emphases).

Such was the ultimate expression of Vallancey's legacy, but O'Brien's hectoring did not endear him to everyone. After a hostile reviewer in the *Dublin Penny Journal* suggested that the 'B.A.' after O'Brien's name might stand for 'Big Ass' (Leerssen 1996: 121–2), he became insane and died 'from a disordered brain' (Betham 1842: II, 192).

That a book like this should be awarded a prize by a respectable academic organization strikes the modern reader as somewhat odd, but it was (as described) just the latest in a long line of publications in the orientalist vein. More was to come: John Windele (whom we encountered above wondering whether his cist graves contained Milesians or Fenians) uncovered a skeleton in the base of one tower, and concluded that the towers were therefore *funerary* (Windele 1842). Betham adopted this conclusion in *Etruria Celtica*, arguing that it invalidated Petrie's still unpublished book:

the delay of the appearance of [*Petrie's book*] has rendered it an abortion confuted while yet unpublished; if it ever appears, it may be useful as a statistic account of the present appearance of the round towers, but for showing the object for which they were erected, it will be altogether worthless. (Betham 1842: II, 229)

When Petrie's book finally appeared in 1845 it amounted to over five hundred printed pages. It was cogent and to the point. Petrie summarized his conclusions right at the start. The towers were of Christian origin, constructed between the fifth and thirteenth centuries, for four reasons: they were always associated with ancient ecclesiastical foundations; their architecture was identical to that of the adjacent churches; the architecture was of Christian type, the Christian symbols being integral not added; and they incorporated features never found in older pagan structures. They were constructed as belfries and storage keeps, but might be used as beacons or watch-towers when occasion demanded; they were suitable for these purposes, and the annals contained numerous references that they were indeed so used (Petrie 1845: 2–3).

Before going into detail on the towers themselves, Petrie spent over one hundred pages demolishing the arguments of the orientalist. He challenged Vallancey's etymological arguments, concluding that 'it is a difficult and rather unpleasant task to follow a writer so rambling in his reasonings and so obscure in his style' (Petrie 1845: 21). O'Connor's 1814 book, states Leerssen (1996: 76), 'was never mentioned but in awe' by other historians, but Petrie had no respect for it; he checked O'Connor's quotations from the (then unpublished) *Annals of the Four Masters* by consulting an original manuscript and confirming his opinion with John O'Donovan, after which he accused O'Connor of falsification (Petrie 1845: 52). Miss Beaufort's sources he examined one by one, and found inaccurate: the Psalter of Tara no longer existed, and the secondary source Miss Beaufort used actually mentioned a palace, not a tower, at Thlachtga; and the towers seen by travellers in India and elsewhere in Asia all differed from the Irish examples (Petrie 1845: 36–9). Petrie dismissed D'Alton's claim about the earthquake in AD 448 by pointing out that this section of the annal was a verbatim quote describing not Ireland, but Constantinople, lifted from the late Roman chronicler Marcellinus, whose work had been published in Paris in 1654 (Petrie 1845: 46). Windele's claims of funerary use he demolished by citing various of Windele's and Betham's descriptions, showing that they were self-contradictory and unreliable. Petrie made much less response to O'Brien; only in the last paragraph of this section did he state that

the only remaining hypothesis of those referring the Round Towers to a pagan use, namely, their having been PHALLI, or PRIAPEIA TEMPLA, is happily so absurd, and at the same time so utterly unsupported by authority or evidence worthy of refutation, that I gladly pass it by without further notice. (Petrie 1845: 109, original capitalization)

Now we see one reason why Petrie's book was so long delayed: he checked a very large number of the original sources quoted by the orientalist. The comprehensive rebuttal this enabled him to make, coupled with his detailed case for the medieval date of the towers, was accepted by his Dublin colleagues including Wakeman (1848: 100–1), Wilde (1850: 146–7), and O'Curry (1873a: III, 52). Pownall's 'Phoenician' inscription at New Grange (fig. 6.1) was depicted by

both Wilde and Wakeman. Wilde (1850: 199) was unconvinced that it was an inscription at all, while Wakeman thought it might have been 'forged by one of the many dishonest Irish antiquaries of the last century' (Wakeman 1848: 26–7). But this acceptance by the Dublin elite did not signal the end of the debate. Just four years after Petrie's publication, Francis Crawford published an article in *TRIA* arguing very much along Bethamite lines. He accepted the Indo-European grouping but evidently did not understand its exclusive implications, arguing that there were extensive links between especially the Celtic branch of Indo-European and Hebrew (Crawford 1849). Petrie's publication filter did not keep this paper out of the *TRIA*, perhaps because it was in the Polite Literature section. From the 1860s the oriental hypothesis saw a resurgence. Sir Henry O'Neill (1863), Marcus Keane (1867) and Ulick Bourke (1876) all argued in major books that the towers were built by settlers from Asia before 1000 BC. O'Neill devoted an entire chapter to 'Doctor Petrie's mistakes', in which he dismissed the Christian nature and date of the carvings (1863: 91–5) and architecture (1863: 97–102). He argued for a pagan and oriental origin, stating that 'we have many proofs in support of O'Brien's theory' (O'Neill 1863: 111), quoting again the existence of parallels in India, and adding that the towers were as old as the pyramids (1863: 112). The rambling Bourke, a cleric, apparently took exception to O'Brien's phallic arguments and did not quote them. Keane, however, quoted O'Brien repeatedly and accepted most of his arguments; he stated that Ireland's earliest inhabitants were Cuthites or Scythians, displaced from India after the collapse of their empire, which 'I believe to have been from the time of Nimrod to that of Abraham' (Keane 1867: 204), at which time phallic worship extended from Babylon to India (1867: 208). Keane knew so little of the sources that he conflated Sir William Jones and Francis Wilford into one authority, 'Sir William Wilford' (1867: 225), not a slip of the pen because this name also appeared in his index. And it should not be thought that these were the views of outsiders: O'Neill, Keane, and Bourke were all members of the RIA. This tradition indeed continues; it is perhaps no surprise to learn that O'Brien's book has been reprinted several times, most recently under the title *The Round Towers of Atlantis*.

PETRIE, O'CURRY, AND THE THREE AGE SYSTEM

George Petrie, Eugene O'Curry, and John O'Donovan have been described as 'the rescue team of Irish antiquarianism, the men who set the investigation of Gaelic antiquity on a new, scientific and critical footing' (Leerssen 1996: 102); and it has been suggested that Petrie instigated the use of the Three Age System in Ireland (Mitchell 1985: 114–16; Raftery 1972: 156). This section will, however, show that Petrie did no such thing. Although he did not accept the extremes of the orientalist position, his archaeology was dominated by the ancient historical chronology of O'Curry, who strenuously *opposed* the Three Age System.

By 1860, Ireland had an ancient historical chronology that outdid even that of Peter Frederik Suhm (see chapter 2). In lectures delivered in 1855–6 and published posthumously, O'Curry (1873b) described numerous manuscript sources. Both the *Annals of the Four Masters*, and the *Leabhar Gabhála* or 'Book of Invasions', used the chronology of the Septuagint (the Greek translation of the Old Testament), which according to O'Curry dated the biblical Creation to 5199 bc (1873b: 172), younger than the 5509 bc adopted by Joseph Scaliger, but older than Ussher's 4004 bc (which was based on the Hebrew version) (see chapter 1). Lectures in 1860 (O'Curry 1873a: II, 232–5) summed up the sequence of colonizers of Ireland, and their dates. Eight generations after the Noachian Flood, Parthalon and his followers dispossessed the earliest colonists, who had come from the Caucasus; then came the Nemidians, a Japhetic people, who were displaced by Formorian sea rovers. The exiled Nemidians split into two peoples, both of whom returned to Ireland; the Firbolg came first, followed by the Tuatha Dé Danaan; the latter defeated the Firbolg at the first battle of Moytura in 1890 bc, or according to another reckoning 1272 bc. In the second battle of Moytura soon after, the Tuatha Dé Danaan defeated the remaining Firbolg and more Formorian sea rovers. In his earlier lectures O'Curry (1873b: 250) mentioned that 'many ancient Cyclopean graves and monuments' still marked this battlefield; we will consider these shortly. Finally came the Milesians, who arrived in 1694 bc (O'Curry 1873a: II, 3). The chronology might not be certain in all details, but its reach

was truly stupendous. It was accepted (with some caveats) by Thomas Wright, the English opponent of the Three Age System whom we met in chapter 4; his *History of Ireland* (Wright 1855) depicted on its title page a precisely datable event from the annals, the meeting of King Cormac the Hairy and his bride Eithne the Fair in AD 254 (fig. 6.6).

This was the context in which George Petrie operated. He actually focused rather little on archaeology, and was much more interested in matters historical and epigraphic. He acquired a manuscript of the *Annals of the Four Masters* for the RIA (Petrie 1831). The Roman typeface was seen as an English colonial imposition, so it was unsuitable for the publication of the Gaelic text of the *Annals*; Petrie used his artistic skills to design a new typeface, the 'Petrie A', which O'Donovan used in the publication (S. Deane 1997: 106–8). The establishment of the Catholic University in 1854 caused a difficulty, because Petrie's typeface was used by the Dublin University Press, based at Trinity College, a Protestant institution. In 1857 John Henry Newman, the Rector of the Catholic University, therefore commissioned Petrie to design another typeface. This Petrie did; it became known as the 'Newman typeface' (Staunton 2005: 92–3), and was used for the Gaelic sections of among others O'Curry's books (fig. 6.7). With regard to his archaeology, Petrie showed something of the tendency already identified in England: he was more willing to employ the Three Age System when dealing with artefacts than with graves—but even regarding artefacts, this was minimal. He was commissioned to write a catalogue of the RIA's collection in 1853 (T. G. Wilson 1942: 226), and set to work with his customary slowness. The BAAS proposed holding its 1857 meeting in Dublin, and the catalogue needed urgent completion to be available for this; but by 1856 Petrie had minutely described 363 stone axes, divided them into twenty-eight classes—and nothing else. The work was taken away from him and entrusted to William Wilde; what he made of it, we shall see below. Petrie's own collection was organized along Three Age System lines, but his manuscript catalogue remained unfinished at his death (Stokes 1868: 82–4).

When dealing with standing monuments Petrie was totally under the sway of ancient history. In a paper read to the RIA in 1834 but published only in 1972, he described the architecture of defended



Fig. 6.6. A precisely dated event from the annals: King Cormac the Hairy casts his eye on Eithne the Fair, AD 254. (From Wright 1855: title page).

cuan o lochain cecinir.

Temair toga na tulae,
 Fota Eriu innoiaae,
 Aroadaair Chormaie mic Airt,
 Mic Cuinn Ceocathair comhair.
 Cormac ba cunrail a mai,
 Ba rai, ba fili, ba flait,
 Ba rir breitein fer fene,
 Ba cara ba coigele.
 Cormac na clai caegaro cae,
 [Do r] ilaro Saltair Tempach,
 Ir in tSaltair rin aa,
 Anur uech funn pencura.
 Ir in tSaltair rin aobey,
 Seet n-airon Eireno inbri;
 Cois nris na coigero doirni,
 Ri Eirenn ir a hoirni.
 Ir inti aa de gae leir
 Ina nris cae n coigro;
 Ina nris n Tempa air
 Do Rir gae cuigro ceolais.
 Coimneoe comaimreao cae,
 Ceo nris na maile doirith,
 Cricao ceo coigro f [ocruaich],
 Ota tmaigro co tnom tuait.

Fig. 6.7. An example of the typeface designed by George Petrie in 1857 for the Catholic University, known as the 'Newman typeface'. The Gaelic poem is by Cuan O'Lochain (d.1024) and describes Tara. (From O'Curry 1873b: 496).

sites such as raths, duns etc. He demolished the suggestion that they were built by the Danes—but his proof was that such places were mentioned at much earlier dates in the *Annals of the Four Masters*; the earliest he quoted was AM 2859 (Petrie 1972: 234), which in O'Curry's Septuagint chronology was 2350 BC. His major work on the royal site of Tara (Petrie 1839) was fundamentally an attempt to use the site to prove the annals correct. Petrie spent 100 pages (27–128) establishing the historical chronology; this started with Tara's establishment by Slainge, first king of the Firbolgs, and ended with its abandonment in AD 563 after the reigns of 142

pagan monarchs (9 Firbolg, 9 Tuatha Dé Danaan, 123 Milesian, and 1 usurper), and 9 Christian ones (1839: 28). Then, 'taking the preceding ancient documents as a guide, the remains on the Hill have been identified with the descriptions given of them' (Petrie 1839: 149). Twenty enclosures and other earthworks were ascribed to particular kings in this way (Petrie 1839: 149–51). Petrie's conclusion was that 'it will be seen that the veracity of these ancient documents has thus been proved, beyond the possibility of rational doubt, by many of the principal monuments which they describe, and which still remain' (p. 151). The only objects Petrie described were two torcs found in 1810, in the 'immediate vicinity' (1839: 181) of standing stones marking the graves of three druids called Mael, Blocc, and Bluicni (1839: 178). Petrie hesitated to state categorically that the torques had belonged to these druids, although he hinted that they might: 'though the name of the original wearer of the Tara Torques is, perhaps, now lost beyond the possibility of recovery, yet the certainty of their locality invests them with a high degree of antiquarian interest' (1839: 184).

In his considerations of funerary monuments, Petrie's interest was to place them into the ancient historical chronology. O'Curry's 'Cyclopean graves' on the field of the second battle of Moytura (see above) are actually the Carrowmore megalithic cemetery, and Petrie identified them categorically as the graves of the fallen Firbolg. The different ranks of the fallen determined the type of funerary monument they received, and a cairn marked the spot where the Firbolg king Eochy was killed (Petrie 1840a). In his book on the round towers, he identified New Grange and the other Boyne megaliths as the graves of the Tuatha Dé Danaan kings; another cemetery at Relecnabreena was of later date and contained Milesians, including their last king, Dathi, struck by lightning in AD 406 (Petrie 1845: 103). A contretemps between Petrie and Betham over a cromlech discovered in Phoenix Park in Dublin in 1838 is also revealing. Betham invoked Indian connections; on 9 April 1838 he read a letter from an acquaintance describing a similar one 'accidentally stumbled upon in the course of a tiger hunt' near Bombay (1840b: 152). Petrie followed this on 28 May the same year, stating that the monument belonged to 'the earliest period of society in Ireland' (Petrie 1840b: 190); he did not, however, name this period, but was more concerned to translate

the mound's name, *Cnoc-maraidhe*, which he rendered as 'hill of the mariners' (1840b: 190). This name hints at immigrant seafarers; although he opposed the orientalist arguments, groups like the Firbolg and Tuatha Dé Danaan had to come from somewhere. Petrie rarely speculated where this might be; but in his essay on military architecture (which he never published during his lifetime) he stated that New Grange was so similar to the Treasury of Atreus in Greece that New Grange must be of Greek origin (1972: 259, 263), and he added that the most impressive defensive sites in Ireland 'were the work of a Greek or Phoenician colony' (1972: 264). This suggests that his views on colonization were not in fact vastly different from Betham's, although their views of the level of civilization of the colonists differed greatly; and at least Petrie's Greeks were Indo-European, and this fitted with his understanding of the Indo-European language group as an exclusive unit (in contrast to Betham, see above).

It will by now be clear how strange Worsaae must have found Irish archaeology in 1846–7. Whatever the merits of Petrie's work on the round towers, his datings of funerary monuments were more outlandish even than those Worsaae was demolishing at home. The historian O'Curry would have none of the Three Age System. In his lectures of 1860, he pointed out that descriptions of both battles of Moytura indicated that metal tools were in use (O'Curry 1873a: II, 240–7); some stone items might be much younger, since early sources made no mention of bows and arrows, and barbed flint arrowheads were too skilfully made to be very ancient (1873a: II, 272). He went on to assault the Three Age System. After quoting Worsaae's book at length, he concluded:

Such are the fundamental dogmas laid down by the northern antiquaries; dogmas laid down, I must say, without any reference whatever to historic or traditionary authority... [*They have been*] received with seeming satisfaction and perfect faith by, I believe, the far greater part of the antiquarians of Europe, but perhaps less generally in Dublin than anywhere else. (O'Curry 1873a: II, 267)

He did not accept that stone tools were earlier than metal ones anywhere in northern Europe (O'Curry 1873a: II, 265–6), and he simply did not believe Griffith's stratigraphical observations regarding

the Shannon artefacts. Griffith had not stated how much time might be required for the accumulation of the sediment that separated the stone and bronze artefacts, and subsequent reports indicated that they were all mixed together anyway (O'Curry 1873a: II, 269–71). O'Curry was also derisive of the relatively shallow chronology some were advocating (though he did not say who); supposing that the Keelogue bronzes dated from the third century AD and the stone weapons 600 years earlier, 'I should then be glad to know whether these two periods—both of them remote enough in our views—would in any way approximate to Sir Richard Griffiths' [*sic*] notion of "a very remote age", and of "a period still remote from us"' (1873a: II, 269).

W. K. Sullivan was the posthumous editor of O'Curry's lectures, and he took up the entire first volume (over six hundred pages) with his introduction. He championed O'Curry's stance against the Three Age System, pointing out that new sciences usually go through a period of 'crude hypotheses and hasty generalizations', and that 'Prehistoric Archaeology was not an exception to this rule' (in O'Curry 1873a: I, ccccvii). In his view, O'Curry was bringing sound scholarship to bear on the issue: 'the ignorance of even the most elementary archaeological and historical facts, and the want of critical judgement displayed by many ethnologists and geologists who had first entered the domain of the archaeologist, led sober investigators like O'Curry to look with suspicion upon their classification of prehistoric antiquities according to the three "ages"' (in O'Curry 1873a: I, ccccviii). For Sullivan the chief early historical fact was the spread of the Aryans, a concept that meshed easily with notions of colonists like the Tuatha Dé Danaan and the Milesians etc.: 'it is now a recognized fact in science that from the Indus to the Atlantic Ocean, and thence across the American continent to the shores of the Pacific, the descendants of one primitive, blue-eyed, fair-haired race, divided into several branches and speaking dialects of what was once a common language, held sway' (in O'Curry 1873a: I, iv). The historian's task was to use philology and early literature to determine the nature and geographical origin of the Aryans. Since these lines of evidence indicated that the Aryans had all possessed metals when they started to diverge, the notion of successive Stone, Bronze, and Iron Ages, if valid at all, could only apply to the original Aryan

homeland—wherever that might be (in O'Curry 1873a: I, ccccviii–ccccix).

Sullivan's archaeological perspective (such as it was) probably derived mostly from the work of Sir William Wilde, the most active and productive writer in mid-century Dublin; it is to him we now turn.

THE ELUSIVE SIR WILLIAM WILDE

William Wilde has sometimes been regarded as a pioneer of the Three Age System (Morse 1999: 5; Morse 2005: 104; Raftery 1972: 156), though others have not seen him as such (Waddell 1998; Waddell 2005: 134). During the 1840s he aligned himself with the Petrie–O'Curry–O'Donovan axis. This emerges from his writings on ethnology. In 1844 he wrote a paper in the *Dublin Literary Journal*, quoting Sir William Betham several times, at one point stating that 'I am not sufficiently versed in the Gaelic to assert that it is not Punic' (Wilde 1844: 234). He reused this article as chapter 9 of his *The Boyne and the Blackwater* (Wilde 1850); much of it was verbatim, but the fading fortunes of Betham are shown by all reference to him being cut (except one footnote); the equivocal passage quoted above was amended to the categorical statement 'the Gaelic is not the Punic tongue' (Wilde 1850: 222). In this chapter Wilde however failed to link archaeology and ancient history, to the long-lasting detriment of Irish archaeology.

One of Wilde's interests was crannogs, lake islands built up to form fortified settlements; in 1857 he published an early cross-sectional view (fig. 6.8). The first to be systematically examined was Lagore, the RIA requesting Petrie and Wilde to collaborate (Briggs 1999). Wilde's paper on the animal bones appeared rapidly (Wilde 1840a), but Petrie's never materialized; Wilde mentioned bronze and iron objects but did not discuss chronology. This was the same crannog described some years later in *Arch. J.* by Talbot, who as we saw in chapter 4 stated that it was of Iron Age date, and earlier than the Anglo-Norman invasion (Talbot 1849: 108). Wilde, however, never considered crannogs very ancient; he gave a list of references to them

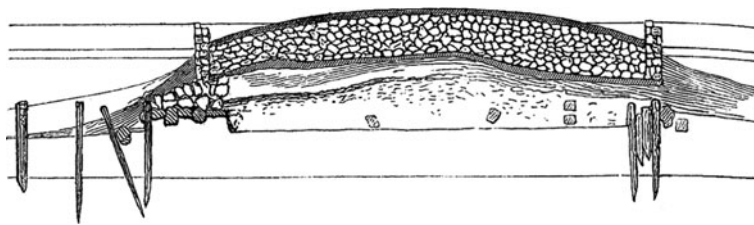


Fig. 6.8. Wilde's cross-section of the crannog at Ardakillin Lough, showing the wooden piles, retaining walls and rubble fill capping the original island. The three upper horizontal lines show (from the top) the maximum and normal lake levels in winter, and in summer. (From Wilde 1857: fig. 152).

in the annals, most of which were medieval, the last indicating occupation as late as AD 1610 (Wilde 1857: 230–3). He reiterated this in a paper read in 1859, stating that the Irish crannogs were much younger than the Swiss lake dwellings, because many of the Swiss examples contained stone implements (Wilde 1861a).

Did this latter statement mean that Wilde had, partially at least, adopted the Three Age System? His chronological views are hard to pin down. In 1850 he quoted a recent find from Redbay, which contained a stone axe, two bronze axes, and two early ninth-century Saxon coins. He regarded all the items as contemporary, concluding therefore that stone and bronze axes were used contemporaneously in the Saxon period (Wilde 1850: 239). He also mentioned a 'well-authenticated' case of bronze and iron weapons being found together at Edenderry, which he regarded as 'a circumstance of considerable interest' (Wilde 1850: 38) but without saying why, beyond observing that 'we have reason to believe bronze weapons were used to a very late period indeed' (Wilde 1850: 40).

We saw above that the task of cataloguing the RIA's collection was taken from Petrie and given to Wilde as the date of the BAAS meeting approached. Wilde had only four months; on 24 August 1857, just two days before the BAAS was due to convene, he presented the first volume of 246 pages to the RIA (T. G. Wilson 1942: 228), a truly remarkable achievement. Whether from haste, indecision, or conviction, he opted not for a chronological scheme, but for one based on *materials*. This first volume dealt with antiquities of stone, earthen,

and vegetable materials. He wrote that 'in the present state of antiquarian knowledge, a chronological classification could not be fully carried out' (Wilde 1857: 1); he admitted the possibility of a pre-metallic era, but limited himself to saying that 'all primitive nations throughout the world . . . must, in the absence of a knowledge of the harder metals, such as copper, bronze, or iron, have employed weapons and tools of flint and stone' (Wilde 1857: 5). This oblique phraseology implied that an absence of metals might be geographically as much as chronologically conditioned. This enabled him to avoid stating categorically whether there had actually been a pre-metallic period in Ireland, or just somewhere else like Sullivan's Aryan homeland. The catalogue did not suggest that he was a secret convert to the Three Age System. The stone section (fig. 6.9) covered everything from flint arrowheads (Wilde 1857: 19–24) to medieval ecclesiastical items (Wilde 1857: 132), the earthen materials section everything from cinerary urns (Wilde 1857: 169–94) to clay tobacco pipes (Wilde 1857: 159–60). The second volume of the catalogue appeared in 1861, covering animal materials and bronze items (Wilde 1861b). The bronze section (fig. 6.9) covered bronze celts (Wilde 1861b: 364–7) and post-medieval tobacco pipes and keys (Wilde 1861b: 546–50). The third volume, covering gold, dealt with everything from lunulae (Wilde 1862: 10–19) to ecclesiastical rings (Wilde 1862: 81–3). Wilde never reached iron. He was quite prepared to quote Danish parallels from Worsaae's (1854) catalogue of the Copenhagen museum, while scrupulously avoiding any chronological conclusion (e.g. Wilde 1857: 17).

Wilde rarely discussed chronological matters in the catalogues, and when he did his views remained rather intangible:

Some northern archaeologists hold that the metal implements were introduced by a new and totally different race from those that worked only in stone. This may be true in Scandinavia . . . , but it certainly is not applicable to the metallurgical art in Ireland, where the earliest implements of both [*copper and bronze*] are of the rudest forms, and evidently copies of the stone articles of the same class . . . Furthermore, as we pass northward, from Denmark to Norway and the top of Sweden, the amount of bronze gradually lessens, and in the former country is replaced by iron . . . It would appear that the stone period was longer, and the metal one shorter and later in all these countries than in the British Isles, and Ireland in particular. (Wilde 1861: 371–2)



Fig. 6.9. Items from Wilde's catalogues of stone and bronze objects. Top row, left to right: flint knife; stone axe; stone mould for casting bronze axe; gravestone with Ogham inscription; chalice. Bottom row, left to right: bronze axe; bronze tobacco pipe; bronze key. (From Wilde 1857: figs. 7, 52, 73, 105, 109; and Wilde 1861b: figs. 250, 425, 431).

He clearly regarded bronze and iron as contemporary in Scandinavia, their differential occurrence being determined by geography rather than chronology; he admitted a Stone Period in Scandinavia, but what did he really think about a Stone Period in Ireland? This question is thrown into sharp focus when we consider O'Curry's attack on Griffith's Shannon stratigraphy (see above); because O'Curry stated that the person who announced to the RIA on 8 February 1858 that

Griffith's stone-bronze sequence was in error *was none other than Wilde himself*:

Dr. Wilde in addition said that he had lately waited on Sir Richard Griffiths [*sic*] and others of the Shannon commissioners, to ascertain if there had been any error in their communication to the Academy as to the facts of the case; that is, whether there was really but one foot of loose stuff between the bronze and the stone implements; and they all stated positively that there was not, and more, that there was no evidence whatever to show that they were not found mixed up together. I may remark however, that this was the first time that Sir Richard Griffith's and Mr. Kemble's error was publicly announced in the Academy. (O'Curry 1873a: II, 271)

But Wilde's role in this remains characteristically intangible; he was certainly present at the relevant meeting of the RIA and was recorded as donating to the collection 'a pair of pampootees⁸ from the Island of Aran,' but there is no record of any discussion of the Shannon finds by him, O'Curry, or anyone else (*PRIA* 7: 15–20).

One can hardly avoid the impression that Wilde did not really know what to think about a Stone Period, and quite deliberately avoided committing himself so as to avoid coming into conflict with ancient history. This conclusion is reinforced by a consideration of his only major attempt to construct a chronological system: his discussion of ethnology. As mentioned above, this appeared twice (Wilde 1844, 1850), the second time with modifications as chapter 9 of *The Beauties of the Boyne and the Blackwater*, a charming historical travelogue in which the crania stand out like a proverbial sore thumb. Wilde used a quasi-Three Age System term only once in his entire book: he had had Eschricht's (1837) paper translated (Wilde 1844: 247), and while discussing Scandinavia mentioned 'the bronze or metallic period' (Wilde 1850: 227). Even this inconclusive term was nowhere applied to Ireland.

Wilde was seeking in this chapter to do something quite complex: to reconcile the ancient historical chronology with grave types and skull forms; but the attempt collapsed in confusion. To understand

⁸ Traditional sandals of the Aran Islands, made of untanned cowhide or sealskin.

what happened, we must follow his argument closely. He first enumerated the five successive waves of pre-Christian colonists from the ancient historical account: Parthalon, the Nemidians, the Firbolg, the Tuatha Dé Danaan, and the Milesians (Wilde 1850: 218–9). He was not concerned with the first two. He termed the Firbolg ‘aborigines’ (p. 218) and stated that they had been widespread throughout Europe. He revealed knowledge of some ethnological writing, equating the Firbolg with pre-Celtic Finns and Basques of Rasmus Rask, the Danish philologist whom we encountered in chapter 2 (Wilde 1850: 220), though he did not mention Prichard or use the term ‘Allophylian’. He repeated Petrie’s conclusion that the Firbolgs defeated at the battle of Moytura were the people buried in the Carrowmore tombs (Wilde 1850: 222). The subsequent Tuatha Dé Danaan he regarded as Celtic, and they were workers of bronze (*ibid.*). He had earlier described New Grange as ‘one of the oldest Celtic monuments in the world’ (Wilde 1850: 190), implying that he accepted Petrie’s view that New Grange was a Tuatha Dé Danaan tomb. The Milesians were also Celtic, and the Vikings probably brought the working of iron to Ireland around AD 900 (Wilde 1850: 223).

This ancient historical scenario, with its incidental stone–bronze–iron sequence, collapsed in self-contradictory disarray when Wilde tried to link it to archaeology and skull form. He listed four types of burial, in chronological order. First was the ‘dome-roofed chamber’ of New Grange type, which Wilde termed the ‘Great Pyramid of the west’ (1850: 224). Wilde had travelled to Egypt in 1837–8 and had seen the chamber inside the Step Pyramid at Saqqara; its corbelled ceiling he believed was older than that in the Great Pyramid, and he likened it specifically to New Grange (Wilde 1840b: I, 377–8). But he had previously accepted that New Grange contained Tuatha Dé Danaan people, yet these came *after* the Firbolg in the ancient historical scheme. The second burial type was the cromlech (Wilde 1850: 228); he did not name the people who built them, but described them as ‘aborigines’ (1850: 229), the term he applied to the Firbolg; but here they came *after* the Tuatha Dé Danaan. From the cromlechs he had skulls to examine (there were none from New Grange); they were long-headed, like Eschricht’s second Danish group, but the Irish ones were never found with metals—nor were

other skulls from the heathen period (Wilde 1850: 229). This must include the Tuatha Dé Danaan, but he had earlier said that the Tuatha Dé Danaan were workers of bronze. The third burial type was the small stone chamber (Wilde 1850: 231). The skulls they contained were round, indicating 'the highest forms of the Indo-European variety of the Caucasian race'; once again Wilde doubted that they were buried with metals (*ibid.*). But if these were Indo-Europeans, what did that make the Tuatha Dé Danaan, whom he had earlier termed 'Celtic'? The fourth burial type was the urn containing a cremation—but these could be found inside the first three burial types as well (Wilde 1850: 232–3). The chapter never resolved any of these contradictions but dissolved into anecdote; for example, the skeleton of a Knight Templar from Larne had 'an Irish physiognomy, and a Firbolg form of head' (Wilde 1850: 237).

Wilde's chronological attempt was a disaster that he would never repair. A decade later he was still assigning skull types to Firbolg and Tuatha Dé Danaan (Wilde 1861c). The deaths of O'Donovan in 1861, O'Curry in 1862, and Petrie in 1866, might have left the way clear for Wilde to influence events. He was at the height of his career in 1864, when he was knighted at the age of 49, but within a year he was disgraced. A pioneer of anaesthesia in Ireland, he was accused by Mary Josephine Travers of having taken advantage of her when she was under anaesthetic in 1854, when she was 19. This was the start of a ten-year affair between them; Miss Travers only resorted to law when Wilde sought to end it, perhaps the reason why the jury awarded her, not the £2000 damages she had sought, but one farthing. Following his public disgrace and humiliation, Wilde withdrew ever more into ancient history. He bought a house on the field of the first battle of Moytura in western Ireland. The house belonged to his aged and increasingly eccentric aunt, a Miss Fynne, whom Wilde as a medical man was able to remove by certifying her a lunatic (T. G. Wilson 1942: 245–61). He spent much time there analysing the battle, identifying various features including the burial mound of the attendant of Eochy son of Erc, king of the Firbolgs, who saved his master from an attack by three Tuatha Dé Danaan warriors but lost his own life in the process (Wilde 1869: 23). He had given his son, the famous playwright, names redolent of Ossianic history: Oscar Fingal O'Flaherty Wilde. At the time of his death in 1876 he was arranging

the building of a monument to the Four Masters, which was constructed after he died. He was buried as he wished, on the field of Moytura (T. G. Wilson 1942: 250ff.).

We last encountered William Greenwell in chapter 5, publishing Scottish burials. In 1867 he visited Ireland and wrote to John Evans that New Grange impressed him greatly, though he did not care for Wilde. He also summarized his impressions of Irish archaeology:

A great deal might be done in Ireland by systematic investigation, but they pay far more attention to the so called historical rubbish, the Four Masters *et hoc quas omne* than to facts, & like some blessed Christians they try to accommodate a true revelation to a dubious if not a false one. And when you get to talk to any of them about the country, its past or present state, they at once become mad. (quoted in Joan Evans 1943: 124)

Worsaae might have had similar feelings twenty years earlier, but if so (as we have seen) he kept them to himself. But between their two visits the movement had started that was to bring the Three Age System to Dublin twenty years after Greenwell's visit.

OUT OF KILKENNY: THE THREE AGE SYSTEM IN IRELAND

The RIA was not to be the organization that brought the Three Age System to Ireland. We saw above that some of its members continued to espouse the orientalist position into the 1870s, and neither the *TRIA* nor the *PRIA* suggest that there was much emphasis on archaeology at this time. Volume 10 of *PRIA* (which contained Wilde's paper on the Battle of Moytura) appeared in 1869. The next (volume 1 of the second series, covering polite literature and antiquities) did not appear until a full decade later. This contained a paper stating that pottery and burial monuments in County Tyrone related to a chief called Bodb dearg, who had lived 3,685 years ago—or 1825 BC, calculated from the date in 1870 when the paper was read (Sigerson 1879). The only mention of Three Age System chronology occurred as a confused afterthought to a paper on a long barrow and crannog in Fermanagh:

the fact of a stone celt being found in the crannoge shows, according to pre-historic archaeologists, that these early lake dwellers belonged to the neolithic period. If this is their place, according to the recognised order of these things, I think the building of the monument may be referred to that dark mysterious period known by the term 'palaeolithic'. (Plunkett 1879: 328)

And another decade was to go by before the second volume of series two appeared. In *TRIA* there was similarly little interest in prehistory. In volume 22 the antiquities section was dated 1873, suggesting that its late appearance held up the polite literature and science sections, dated 1867 and 1871 respectively; this issue contained the announcement that antiquities and polite literature were to be combined into one section, but even together they produced far fewer papers than the science section. The one paper on prehistory in this last gasp of the antiquities section was on megaliths, taking an entirely Bethamite position in arguing that Indian megaliths revealed the origin of the European ones; examples at Shahpoor and elsewhere were so similar that they 'agree in establishing the identity of the great Aryan Nomadic tribes of the East Celts or Scythians being Druids, with those of the West' (M. Taylor 1873: 362). After this, volumes 25 (1875) and 26 (1879) contained only scientific papers; volume 27 (1886) contained just polite literature and antiquities, but not one dealt with a prehistoric subject.

We left the Kilkenny society in 1860, when its interest in matters prehistoric was also at a low ebb. In historical matters the society did not favour the orientalist position, preferring the annalist views of the Dublin elite. James Graves, one of the society's founders, had been a protégé of John O'Donovan (Ní Ghrádaigh 2006: 98), and in the first issue of the journal had demonstrated that a supposed ancient Pelasgic or Phoenician inscription was actually modern, and in Irish (Graves 1851). This lack of interest continued through the 1860s, in which decade the few comments on chronology were hostile to the Three Age System. Some hint of Steenstrup's stratigraphic work in Danish bogs had reached Edward Benn, who wrote that he had never heard of such an thing in Ireland; in any case, it was a subject for naturalists rather than archaeologists (Benn 1866: 299).

A year later George Morant discussed a crannog at Ballyhoe. He stated that he had found artefacts of all kinds including flint axes and arrowheads, bronze pins, iron implements, and even lead bullets.

He concluded that that all these items were in use at the same time, and in the recent past, as the bullets testified—‘a period far later than “prehistoric” antiquarians would lead us to believe’ (Morant 1867: 10). He made the very revealing remark that these finds ‘incline me to believe that the now generally accepted divisions of the ages of stone, bronze, and iron, are not borne out in this instance’ (Morant 1867: 9). ‘Generally accepted’ by whom? As we have seen, the doctrine was not being handed down from Dublin; nor was it evident in earlier volumes of the Kilkenny journal. Morant was surely referring to developments *outside* Ireland, particularly in England. The membership of the Kilkenny society was becoming increasingly international in these years (see above), and had links with London from the start. James Graves and John Prim were, as we saw in chapter 4, founder-members of the Institute in 1846; the Institute had a system of local secretaries in various parts of Britain and Ireland, and their man in Kilkenny was none other than James Graves. As early as 1846 he had reported on the destruction of a megalith (*Arch. J.* 3: 155–7); in 1849 the early days of the Kilkenny society were well written up (*Arch. J.* 6: 95, 317–18, 436); and in 1856 the first three issues of the Kilkenny journal were praised (*Arch. J.* 13: 305–13). As membership became increasingly international, links with other societies such as the Cambrian Archaeological Association were established (Ireland 1982: 73–4). Graves corresponded with Albert Way about manuscripts, and Way donated artefacts to the Kilkenny museum (Ní Ghrádaigh 2006: 117–18). Through these channels the Kilkenny society must have been aware that support for the Three Age System in England was growing (see the next chapter).

The only serious mentions of a Three Age System period in the Kilkenny journals in the 1860s came in an exchange between two men who did indeed have outside connections: George du Noyer, who worked for the Geological Survey of Ireland and whom we have already seen publishing a paper in *Arch. J.* (du Noyer 1847); and William Lukis, another member of the Channel Island archaeological family, whose father Frederick and brother John we met in chapter 4, who worked extensively in France and published at this time in the Association’s *JBAA* (W. C. Lukis 1866a). The exchange concerned whether dolmens were originally covered by mounds, or were free-standing; the chronological mentions were incidental. Du Noyer

commented on recent arguments that dolmens might be medieval, not prehistoric at all; he apparently accepted these, but added that dolmens were impossible to date (du Noyer 1866a: 482). Lukis' rejoinder stated that, although not all contemporary, they belonged to the 'Age of Stone' (W. C. Lukis 1866b: 495), envisaging a much earlier date than du Noyer. In his reply, du Noyer changed tack completely, airily stating that he had 'long thought... they belong to the "Stone Age"' (du Noyer 1866b: 497), and that he intended soon to publish a paper on this; but he never did.

Things gathered pace in the 1870s. William Wakeman, author of *Archæologia Hibernica* (Wakeman 1848), had lost his job as a draughtsman when Petrie's section of the Ordnance Survey was closed (see above). He became an art teacher, but retained a strong interest in archaeology (particularly crannogs), and drew many illustrations for the Kilkenny journal as well as Wilde's museum catalogues (Briggs 1999). He was to play a major role in introducing the Three Age System, supported by James Graves, the surviving member of the duumvirate that had founded the Kilkenny society in 1849 (John Prim died in 1875). Two papers published by Wakeman in 1871 revealed how his perspectives were changing—and how contacts with England were involved in this. He first discussed the crannog at Ballydoolough, which was rich in ceramics. At Graves' suggestion, he sent some sherds to Albert Way in London, who replied that he doubted they were of any great antiquity. But Wakeman was unconvinced by Way's diagnosis even though crannogs were not supposed to be very old:

Aware as I was that crannog fortresses had been in use in Ireland all through the middle ages, even down to the sixteenth century, I hesitated to believe that this kind of ware was necessarily of any very high degree of antiquity. However, upon comparing the markings most commonly found upon the vessels, with those shown upon some very beautiful sepulchral urns, discovered in the immediate vicinity of Ballydoolough and Lough Eyes, I find that the very same kind of punch and the identical pattern which were used upon the one were likewise used upon the other. (Wakeman 1871a: 367)

Typology thus dated the Ballydoolough crannog to a far earlier period. Wakeman's other paper discussed a burial mound, and was the first in the Kilkenny journal (by then the *Journal of the Royal*

Historical and Archaeological Association of Ireland) to use the term 'pre-historic' in its title. He placed the burial in 'the so-called "stone age"' (1871b: 582) and quoted both William Greenwell and John Thurnam as authorities (1871b: 590).

But in the 1870s the more traditional members of the society were still fitting archaeology into the ancient historical chronology. Maurice Lenihan made a confused attempt to date a bronze shield:

at the risk of being contradicted, I argue on the Celtic, or, if you choose, Tuatha de Danaanic, or possibly Danish origin of the shield, by disjunctive syllogism, as I believe the logicians call it, in this way.

It is not Saxon, for the Saxon shield was spiked in the centre, like a German helmet.

It is not Norman, for the Norman shield was kite-shaped.

It is therefore Celtic, if not Danish, for does not Ossian mention round shields, whilst all the ancient Irish bards and writers sing and speak of the Celtic shield as round, such as the fine specimen now under consideration. (Lenihan 1873: 121)

And in 1878 Dillon Kelly discussed a burial mound with two chambers, identifying the skeleton in one as a Tuatha Dé Danaan, that in the other as a Firbolg (Kelly 1878: 180). Wakeman (1882a) would have none of this, arguing that skulls of various shapes could occur at any time—as had been shown in England. The only reliable way to diagnose the date of a burial was through the associated artefacts (Wakeman 1882a: 188). The first two mentions of the Bronze Age appear in this same journal issue, Wakeman mentioning 'the age of bronze culture' (1882b: 259), Richard Caulfield stating that some copper artefacts formed 'the link between the Stone and Bronze period' (1882: 342).

The middle 1880s saw the full and final acceptance of the Three Age System. Dealing with the finds from a crannog at Lisnacrogghera, Wakeman demonstrated his clear understanding that a group of unsystematically collected artefacts need *not* all be contemporary:

It might be supposed that from a critical examination of the form and material presented by antiquities which have been discovered in connexion with nearly every crannog hitherto either wholly or partially explored, archaeologists would be able to assign at least a probable or approximate date to the structures with which the remains occurred. Such an idea cannot,

however, for one moment be sustained, as the yield of objects not unfrequently comprises articles composed respectively of flint, stone, bone, bronze, glass, wood, amber, and iron, which might have been lost or deposited at different times, and have belonged to various ages and races of people. (Wakeman 1884: 376)

1886 saw the publication of the aged James Graves's last paper (he died that year); a short note on axes, it mentioned the Palaeolithic and Neolithic, and stated that copper was transitional between stone and bronze (Graves 1886). It was also the year of publication of two much larger works, which established the Three Age System as the dominant chronological scheme in Irish archaeology. Both were by W. G. Wood-Martin. The first was the beginning of his multi-part *The Rude Stone Monuments of Ireland*, appearing in the same journal issue as Graves's last paper. It started with a clear statement of the Three Age System, which it relied on throughout (Wood-Martin 1886a: 471–2). The second was *The Lake Dwellings of Ireland*, his major book on crannogs. Wood-Martin criticized Wilde's view that the crannogs were largely medieval, saying that this had held back systematic investigation; Wilde's suggestions that crannogs contained no stone and few bronze weapons 'have since been abundantly confuted' (Wood-Martin 1886b: 26). Later in the book he gave a lengthy exposition on the Stone, Bronze, and Iron Ages, and their respective artefacts (Wood-Martin 1886b: 55–73).

At the same time the chronology of the annals came under fire from archaeology. James Graves dismissed the ancient historical claims of the *Annals of the Four Masters* in a comment on Wakeman's paper on the Lisnacrogghera crannog (see above). An iron spearhead hafted with bronze rivets suggested to Graves that Lisnacrogghera dated from the overlap between the two eras. This put him in mind of 'the *ninth century legend* of the armourers of the Tuatha dé Danann, when preparing for battle with the Formorians at the northern Moytura... without assuming that the bronze-iron age should be thrown back to the *mythic period* of the Tuatha dé Danann and the Formorians' (Graves 1884: 406–7, added emphases). Thus the annals might provide information relevant to the time they were written down; but not to the mythical times to which they purported to reach back. Wood-Martin was more outspoken in his next book, *Pagan Ireland*, published in 1895. He devoted a chapter to

questioning the early records (Wood-Martin 1895: 26–59), and then utterly dismissed the annals as a source of history:

The mythical stories by Geoffrey of Monmouth, and other scribes of that school, relative to the colonization and history of England, have long been consigned to the literary waste-paper basket; and why should the extravagant legends related of Ireland be treated with more leniency? To transmit, by oral tradition, a chain of events, extending back in an unbroken order to the Creation, would be an impossibility. (Wood-Martin 1895: 61).

Archaeology was the way to examine the ancient past: ‘the present school of archaeology is pre-eminently that of the spade; the spade is a great solver of problems, and destroyer of fantastical theories (Wood-Martin 1895: 64). Waddell (2005: 143–65) shows that while some continued to argue for the veracity of the annals into the twentieth century, most scholars by then ignored them as historical sources.

By 1885 a great deal of water had flowed down the Shannon since Richard Griffith had found his stratified artefacts in 1843, some of it very murky indeed. In 1846 the youthful Worsaae had found a chronology dominated by ancient history, some of it supported by the writings of an Indian forger. George Petrie might appear to a historian to represent a ‘newfangled, scientific, pedantic and intolerant factualism’ (Leerssen 1996: 128), but to a prehistoric archaeologist he comes across as ineffectual, disabled by his faith in the Tuatha Dé Danaan. Sir William Wilde arguably never even reached Petrie’s level of scientific competence. Dublin was never to provide the archaeological lead; and it was only when the Kilkenny society was poised to capture the capital that the Three Age System became dominant and the chronology of the annals overthrown. And we should not over-estimate the effect of Worsaae, because the Three Age System finally won through not in the year of his visit—but in the year of his death.

Fighting it Through: England 1860–1880

On 29 July 1858, a stone tool was found among the bones of extinct mammals in Brixham Cave. More soon appeared; they were undeniably contemporary with the bones, and the antiquity of humankind was established. A carefully planned series of publications in 1859 ensured that most of the archaeological world accepted this conclusion very rapidly, and historians of archaeology have rightly identified this episode as one of the most crucial developments the discipline has ever seen. Darwin's *Origin of Species* was also published in 1859, and evolution and human antiquity between them created a huge revolution in our understanding of ourselves.

Histories of the archaeology of the rest of the nineteenth century correctly devote much attention to developments in the Palaeolithic, and to Near Eastern archaeology (Grayson 1983; Trigger 1989; van Riper 1993). These were the growth areas of the discipline. Palaeolithic archaeology was elucidating the new 'deep time' of the human species, by working out the sequence of industries in the 'Drift' (glacial moraine) and the caves, and the implications of human evolution. Near Eastern archaeology was deciphering long-forgotten scripts and excavating the ruins of cities hitherto known only from the Bible or the *Iliad*. Less consideration has been given to other areas of archaeology, in particular the study of the later pre-Roman periods in England, and this has left the impression that little remains to be said in this area (but see Daniel 1950: 79–84). In England, however, the debate about the adoption of the Three Age System was to continue for another twenty years, and that is the topic this chapter will address.

The discovery of human antiquity outflanked the short chronology until then espoused by English archaeologists. Thomas Wright

wrote rather plaintively that until recently, archaeologists had considered that the pre-Roman occupation of Britain amounted to 'a few generations, at most', and that they had been content with the biblical chronology of 'somewhat more than six thousand years' (Wright 1866a: 176). This very short chronology made unnecessary any subdivision into periods. Now these archaeologists found themselves jostled by an altogether alien group of new men, who dealt in huge (though unspecified) depths of time. For these people the Three Age System provided a vital series of intermediate periods bridging the gap between the people of the drift and the caverns, and the people of the classical world. In the first major synthesis of the evidence for the deep antiquity of humans, Sir Charles Lyell introduced the Three Age System purely to play this (now) necessary role (1863: 9–11). Lyell was a geologist, but it was not just geology that was using elements of the Three Age System in this way. In an essay on ethnology and human evolution published in 1865, Thomas Henry Huxley examined people at various stages of civilization, noting that people such as the Eskimo, low down on the scale, did not know the use of metals (Huxley 1865 [1896: 228]). A few years later, Charles Darwin himself signalled his incidental acceptance by noting that the most ancient people used stone tools (Darwin 1871: I, 237), and he accepted that there had been a Bronze Age in Europe (1871: I, 160). The progressivist anthropology being developed by Edward Burnet Tylor required the Three Age System even more specifically. In 1865 Tylor clearly accepted the entire sequence (1865: 191 ff.), and it formed a key element of his major work *Primitive Culture* (Tylor 1871: I, 52 ff.). Sir John Lubbock was the foremost member of this group who wrote specifically about archaeology, his *Pre-historic Times* appearing in 1865 (Lubbock 1865).

Thus people in various disciplinary areas adjacent to traditional archaeology found the Three Age System necessary to their chronological structures, although peripheral to their main areas of work. This had not been the case before 1860. For these men, the later pre-Roman period was often rather a minor interest. This meant that even Lubbock had insufficient archaeological expertise to defeat Thomas Wright in debates about the Three Age System. The outcome depended on a small number of archaeologists who were both willing to adopt the Three Age System, and had the expertise derived

from their own excavations to fight it through. William Greenwell was the most able and important of these, supported most prominently by Augustus Henry Lane Fox (who assumed the surname Pitt-Rivers in 1880). John Evans, though not an excavator, was the scheme's main proponent at the Antiquaries. Craniology was dominated in the 1860s by John Thurnam and William Barnard Davis, both of whom we have already encountered. Both continued to oppose any notion of pre-Celtic peoples, and Thurnam achieved the remarkable feat of squaring the Three Age System and a succession of skull types with a chronology as short as Wright's.

THE MEN AND INSTITUTIONS OF THE NEW SCIENCE

A small group of geologists, palaeontologists, and archaeologists made up the interdisciplinary team that established human antiquity, whose papers in 1859 have been described as 'a coordinated onslaught' (Grayson 1983: 188). The main developments in the wider field of archaeology in the period 1860–80 similarly resulted from the activities of a small group of men. John Evans was one of these; he was the principal archaeologist involved with the Brixham Cave work, and a leading figure in the Antiquaries. Sir John Lubbock emerged as the chief popularizer of both human antiquity and the Three Age System, the first edition of his *Pre-historic Times* appearing in 1865. Augustus Wollaston Franks, the British Museum man we have already encountered in chapter 4, was another major figure, and the three most prominent British excavators were mentioned above: Greenwell, Lane Fox, and the craniologist Thurnam. These men formed a mutual support network, acting in concert when the need arose. In the early 1860s most of them joined the Ethnological Society of London, the society founded in 1843 by James Cowles Prichard (see chapter 4), and this brought them into contact with archaeologists such as Henry Christy, excavator (with Lartet) of Palaeolithic caves in France, as well as the evolutionist Thomas Henry Huxley and the pioneer anthropologist Sir Edward Tylor (Chapman 1989; O'Connor forthcoming; Stocking 1987: 238–73). There were many non-academic connections as

well, Lubbock for example being a neighbour and close friend of Charles Darwin.

This group was not exclusive, however. It had close connections with the major traditional archaeologists whom we have already encountered in chapter 4, men who were of major importance but who are often overlooked in discussions of this period. Albert Way, a leading member of the Antiquaries and leading light in the Institute, was the uncle of Lane Fox's wife, and he arranged Lane Fox's entry into both societies; Lubbock soon followed him into both, and these two plus Evans and Franks supported each others' bids for office. None however joined Thomas Wright's Association (Chapman 1989: 27). Way was to give up his opposition to the Three Age System in the later 1860s, albeit in a rather low-key way (see below). Thomas Wright never did so, and as soon as the 1859 papers on human antiquity appeared, he opposed the claims in a high-profile exchange of letters in the *Athenaeum* with John Evans and A. C. Ramsay. Wright was, however, secretary of the Ethnological Society from 1857 to 1869 (Anon. 1878), and encouraged Christy, Lane Fox, and Evans to join. They were soon followed by Lubbock, who was elected president in 1863 (Stocking 1987: 249). Wright and Lubbock thus held office in the same society at the same time.

John Evans was the archaeologist who took human antiquity to the Antiquaries. His first major paper, part of the 'co-ordinated onslaught' pushing human antiquity, was read on 2 June 1859 and appeared in *Archaeologia* the next year (Evans 1860). With regard to Three Age System terminology, he was careful not to belabour his audience with the suggestion that the Danes and the Scots had been right all along. He quoted Wilson only once (Evans 1860: 288 n.) and Worsaae not at all; but his use of 'period' rather than 'age' makes it clear that his terminology stemmed from them. He introduced the terminology quite gently, as if it was his own invention, when discussing the time which 'for convenience sake, I will call the Stone period' (Evans 1860: 289). He was careful thereafter to refer to it as 'the *so-called* Stone period' (Evans 1860: 291, 293, added emphasis). This made it look more like a harmless and provisional home-grown product than an outside imposition. Another aspect of Evans may also have eased its passage; historically best known for his two major papers on human antiquity (Evans 1860, 1863) and for his book on

stone tools (Evans 1872), he was in fact much more of a traditional member of the Antiquaries than is usually realized. This emerges from a consideration of his general activities at the Antiquaries. The vast majority of his contributions, both before and after 2 June 1859, were about ‘conventional’ topics; even his presentations of prehistoric items were usually couched in traditional rather than confrontational terms. This enabled Evans to make the case in a way that someone like Lubbock, a newcomer with no track record in conventional archaeology, would scarcely have been able to do. In the following list of his activities at the Antiquaries in the thirty-five years from 1850 to 1885 as recorded in *PSAL*, his two major papers on the Palaeolithic (on 2 June 1859 and 16 May 1861) almost disappear among the seals, coins, and gold rings:

- 17 Jan. 1850, ancient sepulchral urn (*PSAL* first series 2:42)
- 24 Jan. 1850, British gold coin (*PSAL* first series 2:43)
- 11 Dec. 1851, Roman finds from Boxmoor (*PSAL* first series 2:191–2)
- 26 Feb. 1852, bronze sword from Hawridge (*PSAL* first series 2:215)
- 3 Feb. 1853, Roman villas at Boxmoor (*PSAL* first series 2:295)
- 10 Feb. 1853, proclamation of James II (*PSAL* first series 2:296)
- 23 Mar. 1854, Sir William More’s wedding (*PSAL* first series 3:79)
- 10 Jan. 1856, furniture at Loseley (*PSAL* first series 3:246)
- 3 Apr. 1856, fifteenth-century seal (*PSAL* first series 3:266)
- 8 Jan. 1857, letters of Queen of Bohemia (*PSAL* first series 4:21)
- 5 Feb. 1857, Tudor & Stewart proclamations (*PSAL* first series 4:28–9)
- 12 Feb. 1857, letters of Queen of Bohemia (*PSAL* first series 4:35)
- 18 Mar. 1858, will of Thomas Key (*PSAL* first series 4:180–3)
- 19 May 1859, medieval gold cross (*PSAL* first series 4:326)
- 2 June 1859, flints in the drift (*PSAL* first series 4:329–33)
- 19 Jan. 1860, flint flakes from Reigate (*PSAL* second series 1:69–77)
- 2 Feb. 1860, brass rubbing (*PSAL* second series 1:87)
- 15 Nov. 1860, bronze fibula (*PSAL* second series 1:224)
- 16 May 1861, flints in the drift (*PSAL* second series 1:398)
- 13 Feb. 1862, Irish bronze spearhead (*PSAL* second series 2: 65–6)
- 18 June 1863, stone hammerheads (*PSAL* second series 2:285)

- 28 Jan. 1864, middens and Somme gravels (*PSAL* second series 2:341–5)
- 11 Feb. 1864, his book on coins (*PSAL* second series 2:349)
- 19 Jan. 1865, medieval gold ring (*PSAL* second series 3:38)
- 19 Jan. 1865, stone flakes from Bengal (*PSAL* second series 3:38–44)
- 23 Nov. 1865, Grand Pressigny flint (*PSAL* second series 3:166)
- 31 May 1866, bronze hoard from Saxony (*PSAL* second series 3:328–36)
- 24 Jan. 1867, flints from Lough Neagh (*PSAL* second series 3:412)
- 23 Dec. 1869, fifteenth-century gold ring (*PSAL* second series 4:412)
- 18 May 1871, flints in the drift (*PSAL* second series 5:165–70)
- 30 Nov. 1871, Neolithic flint exhibition (*PSAL* second series 5:224–9)
- 8 Feb. 1872, Anglo-Saxon iron knife (*PSAL* second series 5:258)
- 13 June 1872, Egyptian flint (*PSAL* second series 5:331)
- 23 Jan. 1873, Bronze Age (*PSAL* second series 5:392–412)
- 30 Jan. 1873, Kentish bronzes (*PSAL* second series 5:424–5)
- 30 Jan. 1873, French bronzes (*PSAL* second series 5:432–3)
- 13 Feb. 1873, Cornish flints (*PSAL* second series 5:440–1)
- 13 Mar. 1873, cylindrical boxwood sundials (*PSAL* second series 5:471)
- 30 Apr. 1874, nominated Vice-President (*PSAL* second series 6:213)
- 25 Jan. 1877, seventeenth-century shoehorn (*PSAL* second series 7:121)
- 22 Mar. 1877, thanks Schliemann for talk (*PSAL* second series 7:178–9)
- 17 May 1877, seventeenth-century brass plate (*PSAL* second series 7:229)
- 24 Jan. 1878, flints from Sowerby (*PSAL* second series 7:327–8)
- 28 Mar. 1878, tomb of Edward Langley (*PSAL* second series 7:354)
- 5 Dec. 1878, bronze hoard from Berkshire (*PSAL* second series 7:480–5)
- 30 Jan. 1879, seal of Joanna, Queen of Sicily (*PSAL* second series 8:34–9)
- 25 Nov. 1880, prehistoric congress in Lisbon (*PSAL* second series 8:419)

- 19 May 1881, Indian spindle whorls (*PSAL* second series 8:537–8)
- 16 Mar. 1882, medieval gold ring from Sussex (*PSAL* second series 9:112)
- 24 Apr. 1882, bronze hoard from Wilburton Fen (*PSAL* second series 9:127)
- 19 Feb. 1885, seventeenth-century roundels (*PSAL* second series 10:207–16)
- 11 June 1885, Roman military decoration (*PSAL* second series 10:326)
- 11 June 1885, Greek coins and English medals (*PSAL* second series 10:324)

The Three Age System thus crept into the Antiquaries via the gap between deep human antiquity and the Roman conquest. After Evans's first presentation, the next naming of a Three Age System period in *PSAL* appears to have been in French, a letter from the Swiss-lake-dwelling expert Frederic Troyon read on 12 January 1860 mentioning 'l'âge de la pierre' (*PSAL* second series, 1: 55). A week later Evans described flints from Reigate, of a type found 'all through the stone and bronze periods' (*PSAL* second series, 1: 76), and on 17 May 1860 Franks mentioned the Swiss 'Stone period' (*PSAL* second series, 1: 163).

After this references became more sparse, until the meeting of 28 January 1864. At this meeting (*PSAL* second series, 1: 327–46) a letter was read from R. G. Haliburton of Nova Scotia, describing the finding of a shell midden in that province; Haliburton stated that he had speculated on parallels between stone tools of North America and Europe as far back as 1854, 'when the stone, bronze, and iron ages had not been determined' (p. 330). John Evans, Augustus Wollaston Franks, and Henry Christy (the latter a Fellow since 1862) took the chance to dominate the meeting with contributions from themselves, and from several invited non-members. John Lubbock (who would not join until 1866) started the discussion by stating that since there had been neither a Bronze nor an Iron Age in North America, Haliburton's shell midden might be considerably younger than the Danish examples with which he was familiar, which contained no bronze. Franks pointed out that the use of native copper in North America need not be coeval with the European Bronze Age. Lubbock then responded to a query from the Scot Robert

Chambers (Daniel Wilson's mentor, who we met in chapter 5) with the suggestion that Scottish shell middens might also be younger than the Danish ones, since some of them contained bronze objects. Christy argued that the Danish shell middens were in fact intermediate between the 'drift' and 'surface' periods, using the latter term to denote all subsequent time. Hugh Falconer, the palaeontologist involved in the Brixham Cave investigations, stated that caves he had examined in Sicily were older than those Christy had examined in the Dordogne. Evans then stated that Christy believed Le Moustier to be older than the other Dordogne caves, and that it might be intermediate between them and the 'drift'. Franks mentioned a bone needle from Aurignac, and Christy concluded the discussion by drawing a distinction between the ages of the 'drift', the caves, and his 'surface' period—though even the latter was 'to a very great extent pre-historic' (p. 345).

This was the heaviest dose of deep chronology and Three Age System terminology yet inflicted on the Antiquaries. The presence of all these men, several not even Fellows, must have been planned in advance with this purpose in mind. The 28 January 1864 meeting marked a major threshold for both human antiquity and the Three Age System; both were now clearly mainstream issues. The discussion of the former took the latter entirely for granted; any Fellow who accepted the evidence for human antiquity could hardly avoid accepting the subsequent periods as well. This is not to state that the Antiquaries thereafter became a major forum for the propagation of the Three Age System. Business continued very much as usual, with the medieval period completely dominating the proceedings; but Three Age System periods were routinely mentioned in a minority of presentations. This percolated slowly through into *Archaeologia*. Evans published two more papers in this journal in the 1860s, on flints from France and Ireland respectively. The former dated to 'the Stone period of central France' (Evans 1866: 388), while the latter might be relatively recent, albeit from 'an age long prior to that of iron' (Evans 1867: 407). The Bronze and Iron Ages featured less prominently until 1869, when two papers by Lane Fox and one by Thurnam appeared (see below).

Most of the membership of the Antiquaries however remained disinterested and apathetic, despite the continuing efforts of Evans

(Briggs forthcoming). It never became a base from which the Three Age System could spread throughout English archaeology. We have seen that a number of the key figures joined the Institute under the aegis of Albert Way, and in 1865–6 it looked briefly as if they might have captured that organization—but it was not to be. William Greenwell, briefly an honorary corresponding member but never a full one, published a major paper in *Arch. J.* espousing the Three Age System (Greenwell 1865a). The next year, the Institute established a section dealing with ‘primeval antiquities’ and elected John Lubbock as its president, and his presidential address again stressed it (Lubbock 1866). 1864 saw a new German invasion of Schleswig and Holstein, this one successfully wresting both provinces from Danish sovereignty. Worsaae again used archaeology for nationalist purposes, and published a lengthy paper in the same issue of *Arch. J.* arguing that the archaeological record showed that Schleswig was thoroughly Danish; this paper was entirely structured on Three Age System lines (Worsaae 1866). But this impetus was not followed up; Lubbock’s primeval section never convened again after its initial meeting (Briggs forthcoming), and 1867 saw a paper by the Archbishop of York comprehensively *repudiating* the Three Age System (York 1867). This debate will be analysed fully below; but the Institute was not to be the organizational base of the men championing the Three Age System.

The Ethnological Society proved to be a more useful vehicle, despite the apparently curious anomaly that Thomas Wright was its secretary throughout the 1860s. That this society was able to accommodate both Wright and Lubbock came about as the result of complex events. As founded by James Cowles Prichard, it was based on two key theoretical elements: monogenism, and human diffusion. Wright was one of the archaeologists who sheltered under both (see chapter 4), while Darwinian evolutionists like Lubbock were not in principal hostile to either; biological evolution was after all a form of monogenism. When the Ethnological Society split in 1863 (the year Lubbock was president) it therefore did so along a very different fault line. The breakaway section, led by James Hunt, tended to be hostile to Darwinian evolution and favoured a *polygenic* origin of humans. This group formed the rival Anthropological Society of London, the two organizations remaining separate until reunification in 1871 as the Anthropological Institute of

Great Britain and Ireland (Burrow 1963; Chapman 1989; Stocking 1987: 238–73). This split between the ‘Ethnologicals’ and the ‘Anthropologicals’, as they became known, gave the prehistoric archaeologists the chance they needed. The Ethnologicals’ journals published a number of papers on topics as diverse as the prehistoric archaeology of India (M. Taylor 1869), Sven Nilsson’s views on Stonehenge (Nilsson 1866), and Lubbock’s only venture into excavation, a Welsh megalith (Lubbock 1870). These archaeological papers were always in a minority compared to the more mainstream ethnological ones, but were in this way provided with a venue they would not otherwise have had. The archaeologists remained a force to be reckoned with after reunification in 1871; disturbed that former Anthropologicals were taking over the new Anthropological Institute in late 1872, they launched a coup at the meeting of 7 January 1873, and succeeded in getting George Busk, a sympathetic former Ethnological, elected president. The vice-presidents included Evans, Lubbock, and Lane Fox; Franks was on the council. Greenwell did not wish to hold office, but had come south from Durham to vote in the crucial ballot, and a few days later he attended a talk by Evans at the Antiquaries (O’Connor forthcoming); what he said on that occasion, we shall see below.

This convoluted piece of history explains why prehistoric archaeology was mostly being published in an ethnological journal during the 1860s; and also why two of the most significant papers *opposing* the Three Age System appeared in that same journal (Crawford 1866; Wright 1866a). But before we examine this debate, we need to understand what the principal English field archaeologists were doing at the same time; because this casts light on how Lubbock might have won the debate—but failed to do so.

FIELDWORKERS: GREENWELL, LANE FOX, AND THURNAM

We have previously encountered William Greenwell’s fieldwork skills in Scotland, where he teased apart the multiple burial episodes in the burial mound at Largie (Greenwell 1866). He was carrying out

similar work in England in the 1860s, and emerges as the most significant excavator of this period.

Greenwell's major paper in *Arch. J.* (Greenwell 1865a) was based upon the Three Age System. He castigated poor excavations undertaken in an 'ignorant and greedy spirit of mere curiosity-hunting' (1865a: 241), arguing (in terms of which Thomsen would have approved) that 'the urn, the dagger, and the arrow-head, possess a very trifling interest, and give us, comparatively, little information, unless we know the circumstances of their deposition, and the objects with which they were associated' (ibid.). He first considered long barrows, which he assigned to the earliest period, when people did not have metals. While an absence of metals in the long barrows was not of itself proof that metal was unknown, 'I am inclined to attribute them [*the long barrows*] to a stone-using race, which was supplanted or intruded upon by one acquainted with bronze, and whose burial places remain in the round barrows described in this memoir' (Greenwell 1865a: 108). He concurred with Thurnam that skull shape supported this. All skulls so far known were dolichocephalic, which indicated that a particular race was involved in the construction of the long barrows (ibid.). He next considered round barrows, unravelling the individual burial mounds to see what he could learn from them. In one barrow on Wykeham Moor, he found a bronze dagger, a flint knife, and a small ceramic vessel (fig. 7.1). The bronze and flint objects were found together two feet southeast of the centre of the mound, in such circumstances that 'there can be no doubt that both the bronze dagger and the flint knife had belonged to the person who was buried in this house [*barrow*]; and we have, therefore, a valuable illustration of the contemporaneous use of bronze and stone' (Greenwell 1865a: 243). The archaeological record thus indicated that the rigid separation that Thomas Wright assumed the Three Age System to involve (see chapter 4) was simply not an issue; contemporaneous use of flint and bronze could be accommodated perfectly well within the overall chronological structure. The pot from this barrow, however, came from a cist twenty-five feet from the southeast edge of the mound; since the mound was ninety-eight feet in diameter (Greenwell 1865a: 241) the reader could calculate that the cist and the central find were twenty-two feet apart, and

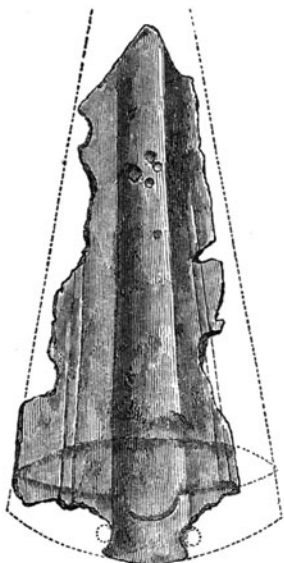


Fig 6.—Bronze dagger; length of orig. 8 inches.

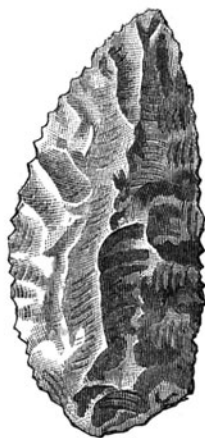


Fig. 7.—Flint knife; length of orig. $4\frac{1}{2}$ inches.



Fig. 5.—Height of orig. $4\frac{1}{2}$ inches, width at the mouth 5 inches.

Urn, with relics of bronze and flint, found in a barrow on Wykeham Moor.

Fig. 7.1. Greenwell's illustration of objects from a barrow in Yorkshire. The bronze dagger and flint knife were found together in the centre of the mound, and must have been contemporary; the pot was found in a peripheral cist and need not be contemporary. (From Greenwell 1865a: figs. 5–7).

Greenwell made no claim that the pot was contemporary with the other items.

Greenwell considered the round barrows to belong to the Bronze Age, because they contained no trace of iron; although iron was more perishable than bronze, if it had commonly been included in the burials some traces would have been found. Iron was in common use when Caesar invaded Britain, and the bronzes in the barrows were in any case dissimilar to Roman items. This meant that they were considerably earlier than the Roman period, although in this paper Greenwell ventured only that they dated from ‘many centuries before the Christian era’ (1865a: 256). In a paper on barrows in Northumberland published that same year (Greenwell 1865b) he went further. Many round barrows did not contain bronze objects, but the other items they contained nevertheless referred them to the Bronze Age:

I have never observed any difference in the mode of burial in round barrows where only flint has been found, from that in those where bronze exists, nor do the urns or flint implements differ in the two cases; precisely the same kind of urn and the same shaped flint knife or arrow head is found in a barrow where a bronze dagger or pin occurs, and in an adjoining one where not a vestige of metal appears. I cannot, therefore, see any reason to suppose that some of the round barrows were made before bronze was known, whilst others are of a date after its introduction. (Greenwell 1865b: 200)

And this time he tentatively suggested an absolute date:

Taking then for granted that they belong to a period before the Romans set foot in Britain, to what date may we carry them back? With our present evidence no satisfactory answer can be given, but I should hesitate very much in suggesting a later period than B.C. 1000, for the earliest of the round barrows. Iron was in use when Cæsar landed, and was so common for certain purposes, that we cannot view its introduction as having taken place but at some considerable distance from that time; but nearly all the tumuli are of a date before the use of iron, of which metal very few traces have been found in them, and we are therefore obliged to carry back the latest of them to a period which dates many centuries before the time of Cæsar. This makes the date B.C. 1000 certainly not too early for the oldest of the round barrows. (Greenwell 1865b: 204–5)

And before this, of course, stretched the Stone Age with its long barrows. This chronological depth was vastly greater than Thomas

Wright's 'few generations, at most' for the time depth of the pre-Roman occupation (see the start of this chapter). In exploring the greater time depths that the discovery of human antiquity made inevitable, Greenwell broke radically with previous English work. He was (apart from Thomas Bateman's brief foray in 1852) the first archaeologist of the later prehistoric periods of England to develop and present a clear 'idea of prehistory'.

After voting for the Ethnologicals' coup at the Anthropological Institute in 1873 (see above), Greenwell attended a meeting of the Antiquaries on 23 January that year. Like the one in January 1864 (see above), this appears to have been a pre-planned performance involving three key players, organized with a particular purpose in mind: this time the reinforcement of the Bronze Age as a period, and the suggestion that it was subdivisible on archaeological grounds. John Evans was still plugging away at getting the Three Age System fully accepted at the Antiquaries, and he spoke about the Bronze Age in connection with a display of artefacts he had organized (Briggs forthcoming). His paper was printed in full in *PSAL* (Evans 1873); he still found conciliatory phraseology necessary:

Now the question of course arises—what is it we are to understand by the Bronze Period? *I think* that the division into periods which has been *more or less in use* amongst antiquaries for a long series of years, and which has in later times been adopted mainly in consequence of the influence of the Danish school of archæologists, *may now be regarded as fairly established*. (Evans 1873: 392–3, added emphases)

After presenting an outline of the Three Age System, Evans considered the bronzes type by type. Axes he subdivided, describing first the simplest flat form that he believed was copied from stone originals; 'the next stage from the flat celt is the flanged celt' (Evans 1873: 397); palstaves he considered as 'the more perfect form of these flanged celts' (1873: 398); socketed axes were described last. This terminology neatly left open the question of whether these categories were chronological, or just typological. He next considered daggers, starting with what he termed the 'knife-dagger' which was 'probably the more ancient' (Evans 1873: 403). Apart from this, he only mentioned chronology near the end of his presentation. Hoards had been found which could be dated to the later part of the Bronze Age, because 'palstaves and socketed celts

are present, which appear to be late forms of these implements' (Evans 1873: 411). After Evans sat down, Lane Fox immediately rose to say that he believed the axes did indeed form a chronological sequence, and then added that William Greenwell was in the audience; since he was the leading British authority, said Lane Fox, 'he will be able to tell us whether it is a fact that in the earliest Bronze Period none but the simpler forms are found' (in Evans 1873: 413).

In what was clearly not an off-the-cuff presentation, Greenwell made two cogent points. The first involved the importance of high-quality excavation, and the detection of intrusive items:

of course there are implements found in barrows not associated with interments, and thus barrows afford evidence which is not quite satisfactory. For example, we may find a shilling of Elizabeth or of Victoria in a barrow deposited long subsequent to the time of its erection. It is only, therefore, when we find implements in actual contact with the body, and there is conclusive evidence that no disturbance has taken place since the mound was thrown up, that we can say that we have conclusive evidence as regards the identity in point of time of the barrow, and the implements contained in it. (quoted in Evans 1873: 414)

It was only a dozen years since Thomas Bateman's wife had lost her gold cameo ring during the backfilling of a barrow, causing Bateman to wonder whether it might lead a future re-excavator into error (see chapter 4); but the gulf between the conceptual frameworks of Bateman and Greenwell was huge. And this was not all Greenwell had to say—his second point was to stress the internal chronological subdivision of the Bronze Age. He identified a suite of four implements commonly found in the early barrows: the simple flat axe and the knife-dagger mentioned by Evans; the awl; and the drill; 'besides these, I do not know of any implements that have been discovered in actual contact with the body' (Evans 1873: 414). Other items were thus no more part of the original burials than the Elizabethan or Victorian shillings. Such other items were the ones Evans had noted came from hoards, in which 'you find the sword, the spear-head, the socketed celt, the palstave, the gouge, the chisel, and other articles, but you never find these little drills, and awls, the thin knife dagger, or the plain axe. From these premisses I argue that these different sets of implements belong to quite two different periods in the use of bronze' (Evans 1873).

This passing of the topic from Evans to Lane Fox to Greenwell neatly coaxed the subdivision of the Bronze Age into the open via a series of elementary but incremental concepts that all but the most hidebound Antiquary had to accept: typological variability; then contextual difference; and finally date. But what happened next had probably *not* been anticipated: Sir Henry Howorth rose to argue that the bronzes were likely to be of Phoenician origin (Evans 1873: 415–18), an argument he had published five years earlier, although he had distanced himself from the ‘pretensions of over-patriotic Irishmen’ (Howorth 1868: 88). Augustus Wollaston Franks, who after years at the British Museum knew English antiquities far better than Howorth did, shot this down by observing that no characteristic Phoenician items were found in England, and that Evans’s arguments for the indigenous origin of the bronzes were correct (quoted in Evans 1873: 418). Evans concluded by thanking his supporters—and again criticizing Howorth (Evans 1873: 418–19).

Augustus Henry Lane Fox was the second major excavator of the day. Lane Fox was introduced to Greenwell in 1867 by Albert Way (Chapman 1989: 33), and after visiting Greenwell’s excavations in Yorkshire that year he began work on hillforts in Sussex (Thompson 1977: 47). Their likely defensive nature attracted his interest due to his military background, and in the first of two papers published in 1869 he concluded that they were well placed for defensive purpose and would have housed large garrisons. He mentioned local tribes recorded as fighting against the Romans, without explicitly stating that it was they who had constructed the hillforts (Lane Fox 1869a: 51). His second paper discussed excavations he had undertaken at two of them, Cissbury and Highdown. A fragment of bronze led him to believe Highdown was Bronze Age (1869b: 57, 75), but it was his excavations at Cissbury that were to prove of greater interest.

Inside Cissbury he had observed large numbers of flints on the surface, and his purpose was to see whether they would also be found during excavation. They were; some fifty pits were visible on the surface as shallow depressions in the terrain, and he opened about thirty. They were just a few feet deep, and in them he found many flint tools and unworked blocks, but only one fragment of a polished axe. He noted that polishing was Lubbock’s criterion for distinguishing Palaeolithic from Neolithic. Many of the unpolished items struck

him as so primitive in appearance that they were apparently Palaeolithic, so 'Cissbury has produced specimens of nearly every type known to have been found amongst flint implements from the drift, and cave, up to the surface period' (Lane Fox 1869b: 68–9). He concluded that the pits had been dug in order to obtain the high quality flint that characterized the region. Were the pits contemporary with the defences? Almost all of them were inside the hillfort, although one or two were outside. The latter caused him a little doubt: 'it would therefore appear just possible, though not probable, that the pits might have existed on the hill before the intrenchment, and the ramparts have been thrown round them in after years' (Lane Fox 1869b: 73). A few years later he was to be thankful he had added this caveat. In this first excavation of Cissbury he dug a trench across the ramparts. He found many flints in the bottom of the ditches, which he thought 'appears to me to afford strong presumptive evidence' (Lane Fox 1869b: 74) that the ramparts and the flint pits were indeed coeval, the one built to protect the other.

Greenwell was instrumental in causing Lane Fox to re-evaluate his conclusions. In 1870 Greenwell presented the results of his excavations at the classic flint-mining site of Grimes Graves, where there were numerous surface depressions like those at Cissbury, but no hillfort. The pit Greenwell dug turned out to be a spectacular thirty-nine feet deep, and at the bottom galleries led away to the sides, following the seams of flint and connecting up with the bottoms of adjacent vertical shafts like the one he had excavated. As at Cissbury, there were very large numbers of flint fragments in the surrounding area, but Greenwell recognized that their apparently primitive appearance was because they were waste products from the preparation of blocks of raw flint, not primary artefacts. There was no need to assume they were Palaeolithic, something also supported by the animal bones, which were identified as predominantly domestic cattle by the palaeontologist W. Boyd-Dawkins. Greenwell considered the possibility that the site might date from a later period, when flint was used alongside metals, but there was no evidence to suggest this. In conclusion:

We may regard these workings, then, as belonging to the neolithic age, when metal was unknown, but when the grinding and polishing of stone was understood. The palaeolithic age, when flint was the most extensively used

in the same district, cannot have been that of the working of these pits; for, apart from the fact that nearly all the flint implements have been made from surface flints, and these generally not belonging to flint of the quality obtained at Grimes Graves, the greater part of the animal-remains found in the pit do not belong to the fauna of the drift, nor were any bones of the most characteristic animals of that period discovered there. (Greenwell 1870: 434)

This gave Lane Fox pause for thought. He walked over Cissbury in the company of John Evans, and they concluded that there was so much flint all over the landscape that the quantity inside the hillfort was not relevant to the date of its construction. Greenwell had dug into one of the depressions in Cissbury in 1868, going down just a few feet; but two other excavators had subsequently dug pits that turned out to be shafts thirty-nine and twenty feet deep respectively. Lane Fox re-excavated one of his own earlier pits and the one Greenwell had examined, and now realized that neither had been bottomed the first time: both were much deeper, and had lateral galleries at the bottom like Grimes Graves (Lane Fox 1876: 365). Lane Fox also now observed faint traces of many more shafts *outside* the hillfort than he had previously noticed; he counted thirty-nine (1876: 368). This raised again the question of whether the shafts and the defences were contemporary; now stressing the provisional nature of his earlier conclusion (1876: 366), he decided to excavate a trench across the ramparts in the area where they intersected the line of shafts. Lane Fox was able to demonstrate conclusively that the shafts had been backfilled *before* the ramparts were constructed. The Neolithic mine shafts were of a much earlier date than the ramparts, the date of which he did not conclusively establish. He depicted this with military precision, in what are probably the first diagrams of vertically cut sections in English archaeology (fig. 7.2).

John Thurnam was the third major excavator of the 1860s, concentrating like Greenwell on barrows. We last encountered him taking an interest in craniology, and ascribing the pottery he recovered from the West Kennet long barrow to the traditional category 'British or Celtic type' (Thurnam 1860: 418), but attempting no chronological attribution (see chapter 4). With regard to this pottery, Greenwell was later to point out that it was typologically of Bronze Age date and therefore intrusive (1877: 508), something Thurnam had not understood in 1860. But by 1864 Thurnam had espoused the

Three Age System, stating at the Antiquaries on 16 June that year that round barrows were of Bronze Age date and largely contained cremation burials (Thurnam 1864). A member of both the Ethnologicals and the Anthropologicals, he continued his earlier focus on skulls in a major paper in the latter's *Memoirs* (Thurnam 1865). This was the first paper in which he concluded that skull type *did* indeed change through time, something craniologists had been unable to establish until they had an independent archaeological chronology against which to calibrate the skulls (see chapter 4). Once Thurnam had adopted the notion that long barrows were older than round barrows, the skulls fell into place: 'a sort of axiom has, I think, now been established to this effect: *Long barrows, long skulls; Round barrows, round or short skulls*' (Thurnam 1865: 158, original emphasis).

Thurnam had long been collaborating with the other craniologist we have already encountered, Joseph Barnard Davis. The upshot was the monumental two-volume *Crania Britannica*, parts of which began appearing in the 1850s but which was published in its entirety in 1865 (Davis and Thurnam 1865). The two men did not completely agree, and each signed individual chapters and descriptions of skulls. Neither, however, accepted the existence of a pre-Celtic 'Turanian' or 'Allophylan' population. Davis considered that the finds of dolichocephalic skulls in the earliest barrows disproved any such pan-European population, because the earliest skulls in Scandinavia were brachycephalic (Davis and Thurnam 1865: I, 20); he continued to stress the variability present *within* populations, using terms like 'acro-cephalic' and 'platy-cephalic' as he had previously done (see chapter 4). Thurnam considered it hard to define what a Celtic skull was, and thought that 'Iberian' intrusions into Britain and Ireland might have involved fairly small numbers in southwestern regions, and post-date the main Celtic immigration (Davis and Thurnam 1865: I, 55–8).

The chronology of both men was very short, entirely within the scope of Thomas Wright's 'few generations, at most' of pre-Roman occupation. All thirty-five prehistoric skulls figured in *Crania Britannica* were placed within tribal regions delimited by the Roman geographer Ptolemy in AD 120; and it was clear that both men thought it highly likely that most or all did in fact derive from

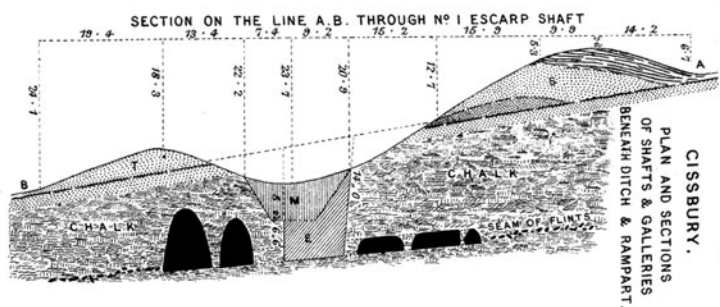
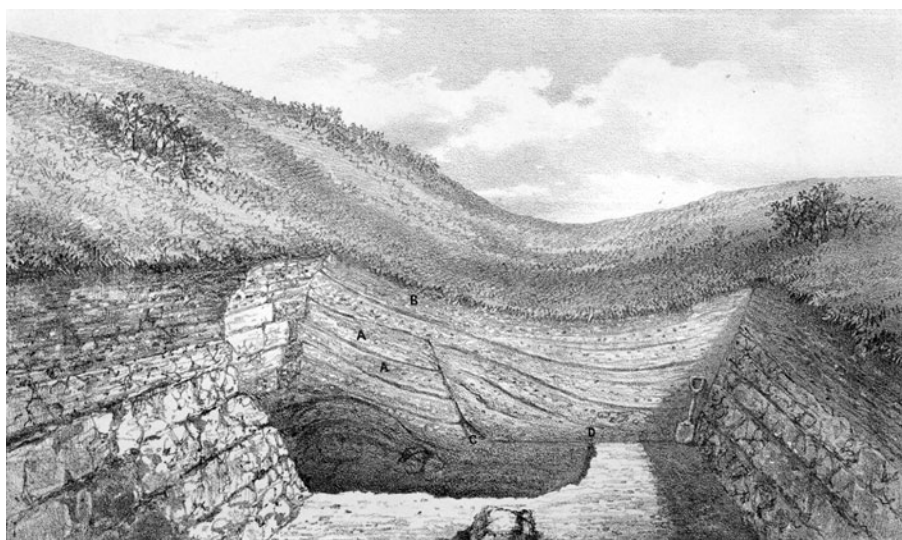
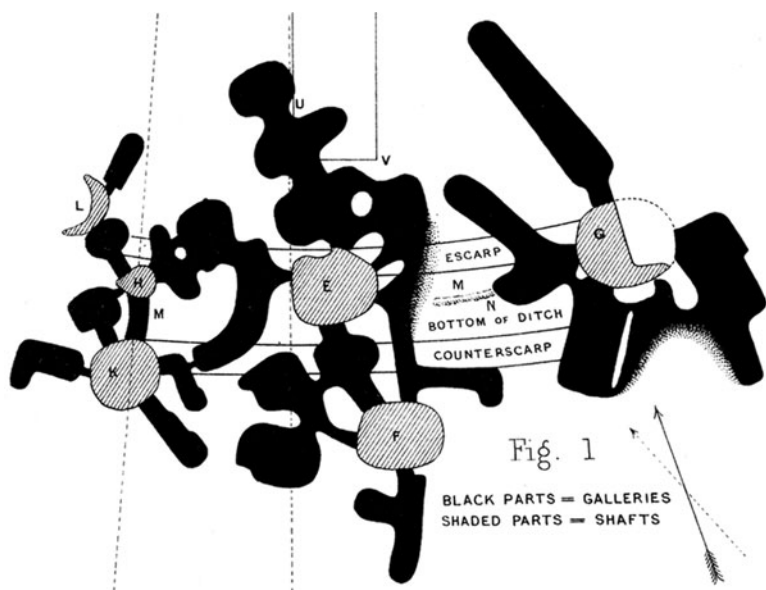


Fig. 7.2. (top) Lane Fox's depictions of the stratigraphy at Cissbury. Top: view with inner rampart to the left; B–C–D is the original cut of the overlying ditch; A is the original backfill at the top of the Neolithic shaft, cut through by the later rampart ditch; the excavated shaft extends below the floor of the ditch. (bottom) section drawing with the inner rampart to the right, showing the ditch M cutting into the backfilled shaft E, the black signatures marking the lateral galleries extending from the bottom of the shaft. (opposite) plan view marking the line of the ditch, the placing of the shafts (H, K, E, F, and G), and the lateral galleries (black). (From Lane Fox 1876: pls. 15 and 16).



those very tribes. Thurnam considered that flint was used right down to the Roman invasion, while bronze swords and axes continued to be used well into the Roman period (Davis and Thurnam 1865: I, 90–2). He contested Wright's dating of the Gristhorpe log coffin and finds to the early Roman period (see chapter 4); but his redating hardly moved it: 'it is of course impossible to fix a date for the interment, though it is probably not earlier than two or three centuries before, or later than the first century of, our era' (Davis and Thurnam 1865: II, entry on Gristhorpe, not paginated).

Thurnam was able to maintain this short chronology because *he did not accept human antiquity*. Had he done so, he would have had to stretch out the pre-Roman periods substantially. But he rejected the association of flint tools with bones of extinct animals at Brixham Cave because people would not have lived in the cave while such dangerous animals were resident; and the French cave evidence was unconvincing because the flints were similar to those in British barrows, which were always associated with modern animal species—so the French material had to be mixed (Davis and Thurnam 1865: I: 50–1). This allowed him to retain a very traditional perspective, despite accepting Daniel Wil-

son's cranial sequence (which Davis never did). Wilson, it will be recalled, argued that both his early long-headed or 'kumbecephalic' and his subsequent round-headed peoples were pre-Celtic Allophylians; the Celts were his third group, comprising a new series of long-headers (see chapter 5). Thurnam remained absolutely opposed to this, believing that the round skulls in the round barrows represented the people who occupied Britain when the Romans arrived (Thurnam 1865: 126). These people were therefore undeniably Celtic, not Allophylian. The shallowness of his chronology emerges from his treatment of the long barrows, which he regarded as earlier than the round ones. Thurnam had excavated mainly in Wiltshire and Gloucestershire, where long barrows were relatively common. This was the district recorded by the Romans as occupied by a tribe called the Dobuni, who were probably the more primitive people of the interior of Britain mentioned by Caesar (Thurnam 1865: 120). The Dobuni were certainly Celtic, and their long barrows contained no metals. Thus while the long barrows belonged to 'a more ancient people' than the round barrows (p. 120), in Thurnam's view there was actually little or no real chronological difference between them.

Thurnam's chronology was thus distinctly ancient historical in scale, and hardly very ancient at that, still tied to the identification of historical peoples. Greenwell, however, considered the cranio-logical pursuit of the Celts to be a waste of time because the very concept 'Celt' could not be defined. Discussing the round-headers of the round barrows in the same year *Crania Britannica* was published, he asked:

Were they Keltæ? An answer must first be given to a primary question—What is the keltic type? Now many difficulties, at present, stand in the way of an answer to this, which, until they are solved, make vain, it seems to me, all attempts at a solution of the secondary question. Merely to allude, in conclusion, to one difficulty. The skull, from the round barrows, is eminently brachycephalic; the skull of the modern Irishman or Scotch highlander, whom we commonly term 'Celts,' is *not* brachycephalic, and the skull, like the race, never essentially alters—its features are unchangeable. (Greenwell 1865b: 205)

Thurnam's later major papers (Thurnam 1869, 1871) concentrated much more on archaeological materials he had excavated in Wiltshire

and Gloucestershire. He did, however, restate his belief that the Dobuni constructed the long barrows. He added the extra historical conclusion that the earthen form (which had a more southerly distribution) was older, dating before the arrival of the Belgic tribes; those with stone chambers (with a more northerly distribution) dated from after the Belgic encroachment in the south (Thurnam 1869: 235–6). He was never to lengthen his chronology, dying in September 1873 on the day he finished proof-reading his paper in the late-appearing 1871 volume of *Archaeologia*. In this he argued against both the early date of the Bronze Age, and its subdivision:

those who claim a higher antiquity for our round barrows than I am able to accord to them found their arguments in part on the absence in them of socketed celts and spear-heads, as well as of leaf-shaped swords. Celts of the socketed and palstave type from the surface of tumuli may be mere waifs and prove nothing. (Thurnam 1871: 446)

But nobody had argued that these items were found on the surface of barrows; they occurred in the entirely different context of hoards. Thurnam, however, continued to insist that the socketed axe, while perhaps a later development, first appeared in the ‘late Celtic’ period, and that they ‘seem to have been used far down into the Roman period’ (1871: 446). Thurnam thus tried to cram the Three Age System into a very short historical time frame; something only possible because he rejected deep human antiquity.

The importance of William Greenwell in all this is greater than has previously been realized. As we shall see in the next section, this was also true in the major debate about the Three Age System that took place in the later 1860s; and it might have been greater still, had Lubbock paid more attention to Greenwell.

THE WRIGHT–LUBBOCK DEBATE, AND THE ROLE OF GREENWELL

The debate between Thomas Wright and John Lubbock sheds light on the unsettled nature of archaeology in the 1860s. Wright had

previously invested a great deal in the rejection of the Three Age System (see chapter 4); although the pre-Roman past was a relatively minor part of his work, he knew a lot about the archaeological record. He was not the only major opposing figure; John Crawfurd, a leading member of the Ethnologicals, argued that the situation would vary in different parts of the world, but iron would usually come *before* bronze (Crawfurd 1866). Lubbock in *Pre-historic Times* (Lubbock 1865) was probably trying more to impress his peers than convert opponents of the old school. Some of its chapters had earlier appeared as separate articles in the journal *Natural History Review*, and the book comes across as a series of separate essays rather than a coherent work discussing topics in chronological order. Thus its first two chapters dealt with the Bronze Age, subsequent ones with tumuli, Swiss lake dwellings, Danish shell middens, and North American archaeology; cave men and human antiquity only appear after all these, not in first place as chronological cohesion would require. Additionally, Lubbock's knowledge of the archaeological record was inferior to Wright's.

In two papers published the year after Lubbock's book, Thomas Wright rejected the Three Age System as an inept and ill-informed attempt at deepening the past. 'I fear that far too much of prehistoric archæology, as it has been hitherto presented to us, rests only upon a want of knowledge of what is historic' (Wright 1866a: 195). While he might accept that there was a period when people only used stone tools, he utterly rejected an age of bronze—'I do not believe in the existence of such a period in Western or Northern Europe' (Wright 1866b: 73). The Three Age System had not been accepted by English archaeologists, but now geologists were trying to force it on them. Human antiquity, he stated:

has given rise to a new school of investigators, who have gone to work, I think far too hastily, to fill up the great vacuum thus left in man's history, not only with all the monuments the character of which is undecided, but also with multitudes of those of which the science of archæology would easily teach them the correct appropriation. This new field of inquiry has been called præhistoric archæology, and its advocates have seized upon the theory of periods of the northern antiquaries, and carried it almost to absurdity. (Wright 1866b: 81)

But another man had also read *Pre-historic Times*. William Greenwell wrote to Lubbock from Durham on 12 August 1865 commenting on the book, anticipating potential objections, and showing how Lubbock could improve his case. Lubbock adopted some, but only some, of Greenwell's recommendations. Had he adopted them all, he might have won the debate, but in the end the result was inconclusive. Lubbock might have felt that he ended ahead of Wright on points, but neither man landed a knock-out blow. This section will examine two key areas of the debate: whether bronze swords were Roman or prehistoric; and whether barrows showed a trend from inhumation to cremation in prehistory. In the first, Lubbock adopted Greenwell's suggestions, but in the second he did not.

Thomas Wright had long argued that bronze swords were of Roman age. Bronze objects were often found on Roman sites or along Roman roads. Classical authors stated that the Britons defeated by the Romans had long swords, but the Roman legions were equipped with short stabbing swords much more like the bronze ones. French archaeologists had found bronze swords associated with Roman coins (Wright 1852: 74–7; 1861a: 74–6). The swords were identical in all areas of Europe where they were found; different peoples lived in these different areas, so the uniformity of the swords suggested a common origin for them all. This had to be Rome, since at no other time did sufficiently close connections exist between these far-flung regions. Swords were also traded outside the Empire, where they were treated differently: in the Roman parts of Britain they were apparently lost by accident, whereas in Scotland, Ireland, Scandinavia, and Hungary they were grave goods. The reason for this was straightforward: the Romans (and by implication their Romanized subjects) did not bury weapons with their dead, while the other Celts and Teutons did (Wright 1861b: 14–17). Wright drew a parallel with his own times: 'they were made in the Roman empire, and sold to the barbarians, just as now, at Birmingham and in others of our great manufactories, articles are made for exportation to suit the tastes of the Indian of America or the Negro of Africa' (Wright 1861b: 15–16). Buried hoards of fragmentary bronze objects, the working stock of itinerant smiths, were also widespread, suggesting the Roman period for the same reasons; and at Heilly, in France, bronze swords had been found together with Roman coins (Wright 1861b: 16–19).

To bolster his claim for a Bronze Age, Lubbock had to detach the bronze swords from the Roman era and place them deep in prehistory. In the opening pages of *Pre-historic Times* he argued that bronze and iron weapons were not found together, nor were bronze swords found with Roman coins. Wright had only given one example of bronze swords on a Roman site, the fort of Ardoch in Scotland. Lubbock quoted at length from one of Wright's footnotes (Wright 1861b: 104–6) to the effect that Roman coins were found scattered about the sites of Roman cities; from this he concluded that, had the Romans used bronze swords, these too should be found frequently on such sites (Lubbock 1865: 8–12). He added that swords in different parts of Europe were not identical, as Wright had claimed, but showed regional variations; in any case, they were numerous in places like Ireland and Denmark, which the Romans had never conquered. Swords were found near Roman roads, but these roads often followed the course of older lines of communication so this provided no evidence of date (Lubbock 1865: 33–5).

There is a sense of over-confidence about Lubbock's treatment of Wright. The warm reception his friends gave *Pre-historic Times* may have encouraged him to believe the battle was won, because in his address to the Primeval Section of the Institute the next year he gave a conventional description of the Three Age System, mentioning only briefly Wright's 'untenable' Roman date for the swords (Lubbock 1866: 203). The cogency of Wright's response probably surprised him.

Wright had little difficulty in countering Lubbock's arguments. Bronze swords, he wrote in the *Ethnologicals' Transactions* (1866a), were indeed found together with iron ones, for example being dredged up together from the Thames. With regard to Roman artefacts, he reiterated the associations with bronze swords in France and at Ardoch, and saw no reason to dispute their accuracy (Wright 1866a: 183–6). Bronze swords were rare on Roman town sites, but Lubbock's long quote from Wright achieved nothing because Wright made the telling point that iron swords were not found in Roman towns either; consequently 'I am sorry to be obliged to say that this remark only shows that my friend, in common with the advocates of this system of periods generally, is but imperfectly acquainted with the archaeological conditions of the question' (1866a: 181). Wright further pointed out that his discussion of bronzes near Roman roads

had not been about swords at all, as Lubbock had stated, but about the hoards of his travelling smiths. In times when travel was both slow and dangerous, it was natural that such travellers would now and again be obliged to bury their stock. Of the suggestion that the roads were pre-Roman he was dismissive: ‘Sir John Lubbock seeks to explain the position of these finds by supposing that the Roman roads were laid upon older British roads, but this is an objection to which I cannot listen until he brings me the slightest substantial evidence that such was the case’ (Wright 1866a: 193). In a paper in *JBAA* he stated that the rarity of associations of bronze swords and Roman coins outside the Empire was ‘hardly worth discussing’ (Wright 1866b: 80) because there was no monetary economy outside the Empire; what use would these people therefore have for coins? He stressed again the uniformity of the bronze swords, illustrating four (fig. 7.3) from the Somme, Lake Neuchatel, Sweden, and another location in Scandinavia to make this point. Roman depictions of all kinds, from coins to the Arch of Constantine, showed the same kind of sword; he depicted a coin, and some sculpted examples from Roman sepulchral monuments in Algeria (fig. 7.3).

This was a fairly effective refutation of Lubbock. However, Greenwell’s letter to Lubbock (written before Wright’s refutation appeared) stressed that ‘Wright’s view as to the absence of remains of pre Roman times is based on no authentic facts, as also is his attribution of bronze implements & weapons to Roman influence’ (Greenwell to Lubbock, 12 August 1865, British Library, London, Add MS 49641/84–7)—Lubbock had after all not checked the original sources. Greenwell also used his experience of firm contexts to point out that ‘with regard to bronze the finding it near Roman roads or camps proves nothing, any more than the finding of money of George III would do’ (*ibid.*). It may well have been these comments that stimulated Lubbock, in an article written jointly with his brother Frederic, to check Wright’s points in greater detail, and to be more critical of his looser contextual claims (Lubbock and Lubbock 1867). They checked the original publication of Ardoch (R. Stuart 1852), where Roman swords were illustrated in the same figure as the fort (fig. 7.4), and established that Stuart nowhere stated that the swords actually came from the fort (Lubbock and Lubbock 1867: 106). They checked the French claims of associations of Roman coins and

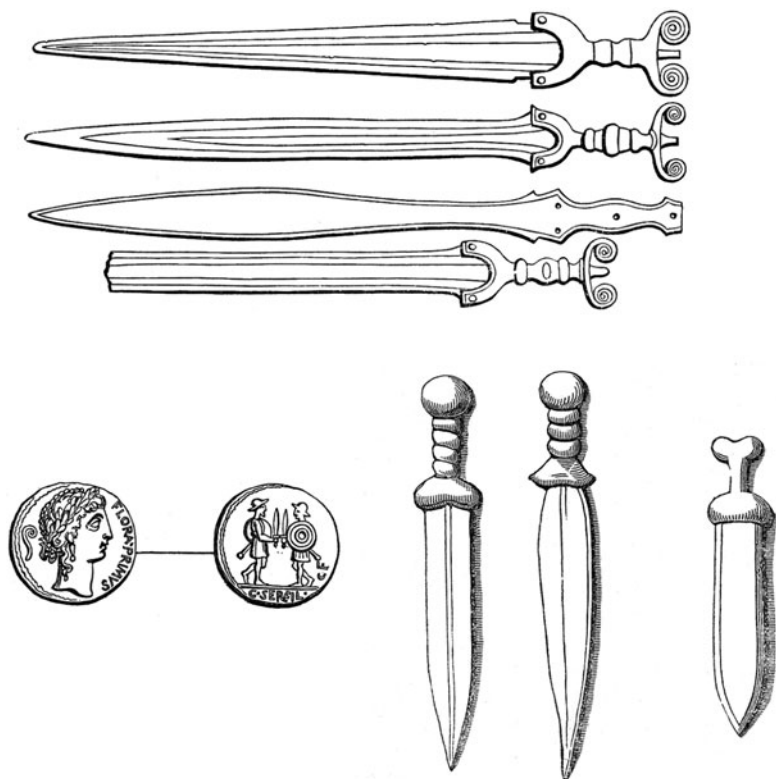
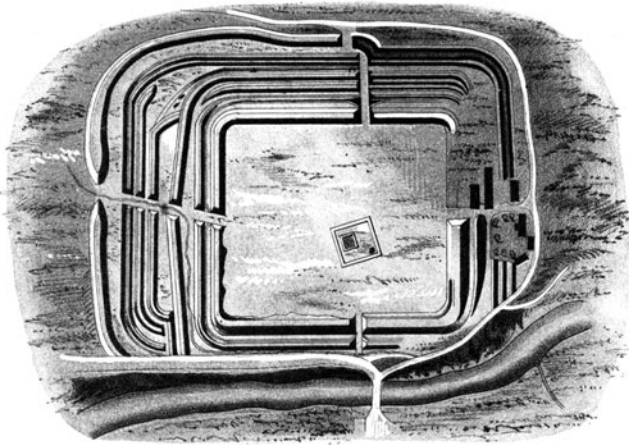


Fig. 7.3. Thomas Wright's illustrations designed to show that bronze swords were Roman. Top: bronze swords from various places in western Europe; Bottom: a Roman coin and funerary carvings from a Roman site in Algeria showing swords of the same kind. (From Wright 1866b: 75, 76, 77).

bronze swords, concluding that excavation quality had been poor and no precise records kept (Lubbock and Lubbock 1867: 109). With regards to Wright's artefacts from the Thames, they used a variation of Greenwell's point about intrusive coins to demolish the value of the association: 'a thousand years hence coins of Queen Victoria will be found in the same place, with bronze weapons and stone implements, but this will not prove that bronze weapons, stone implements, and sovereigns were all in use at the same period' (Lubbock and Lubbock 1867: 106).

2



ROMAN STATION AT ARDOCH, AS IT APPEARED IN 1755.

1

ARTHUR'S OON.



5



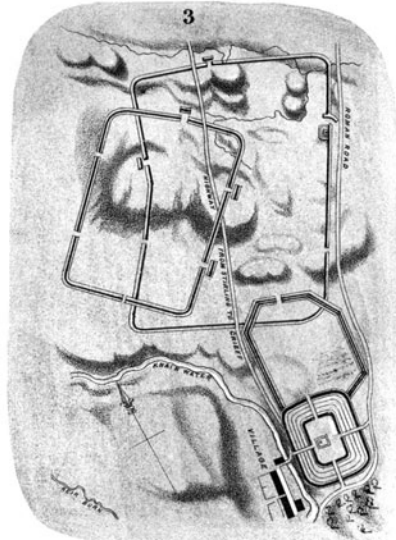
INSCRIPTION FOUND AT ARDOCH.

4



BRONZE WEAPONS.

3



CAMPS AT ARDOCH, PARTIALLY RESTORED.

Fig. 7.4. Bronze swords illustrated in the same figure as the Roman fort at Ardoch. (From R. Stuart 1852: plate 5).

All this amounted to a considerable sharpening of Lubbock's argument. Greenwell may not have been the only person encouraging Lubbock in this way, but his influence does appear to have enabled him to score some important points. Had Lubbock followed Greenwell in the other area of debate considered here, Wright's position might have crumbled a decade earlier than it did.

This second field of debate concerned how archaeological classification should be undertaken. It pitted the ideologies of the two approaches more clearly against each other than the swords did. Lubbock sought to identify a quasi-geological 'stage' of cremation burial in England, using this to support the existence of a Bronze Age. Wright in contrast argued that a 'horizontal' classification was the only appropriate way to proceed:

the proper, and the only correct, arrangement of a museum of antiquities is, no doubt, the *ethnological* one. Relics of antiquity should be classed according to the peoples and tribes to whom they are known or believed to have belonged, and to the localities in which they are found, and then only have they any intelligible meaning. (Wright 1861b: 11)

This was largely determined by their differing chronologies: Lubbock's long chronology required intermediate stages to support it, while Wright's short chronology had no room for them, and almost everything had to be Roman or later. This included Britain's most impressive monument, Stonehenge. Wright tentatively ascribed it to the Roman period (1854: 304; 1861a: 81–2). Lubbock, committed to a Bronze Age, took a different view because Stonehenge was surrounded by barrows: 'if, then, we could determine the date of these tumuli, we should be justified, I think, in referring the Great Temple itself to the same period' (Lubbock 1865: 52). Sir Richard Colt Hoare had examined 151 of these barrows, of which 'the great majority contained interments by cremation, in the manner usual during the Bronze age' (Lubbock 1865: 52), even though only 39 of them actually contained bronze items. But the weakness of this dating became clear, when Lubbock in a later chapter returned to the barrows. If cremation was a test of the Bronze Age, the barrows were Bronze Age; but the clincher was that so many of them lay around Stonehenge and must therefore be of the same date as the temple, so he was inclined to date them to the Bronze Age (Lubbock

1865: 102). Lubbock was thus dating Stonehenge by means of the barrows, and the barrows by means of Stonehenge, hardly a very robust approach. Thus if he could establish cremation as a Bronze Age trait, his argument would be considerably strengthened.

Lubbock sought to establish that Bronze Age cremation followed Neolithic inhumation by a statistical analysis of the only two major samples of barrows then available: Thomas Bateman's from northern England, and Sir Richard Colt Hoare's from the South (Lubbock 1865: 101–2). Fig. 7.5 shows his summary tables. First he presented data from 297 burials described by Bateman (fig. 7.5 A), followed by data from 267 described by Hoare (fig. 7.5 B). He concluded that in Colt Hoare's sample almost all Bronze Age graves were cremations, but that in Bateman's northern sample the reverse was the case. Then he combined the Bateman and Hoare datasets into a single table (fig. 7.5 C) and considered them as a whole. He concluded that extended inhumation was evidently characteristic of the Iron Age, but:

As regards the habit of burning the dead, the evidence is less conclusive. Out of a hundred cases, indeed, of graves characterized by the presence of bronze, the corpse appears to have been buried in a contracted posture nineteen times only, in an extended position only seven times. It is evident, therefore, that during the Bronze age the dead were generally burnt . . . There can be no doubt that in the Neolithic Stone age it was *usual* to bury the corpse in a sitting, or contracted posture; and, in short, it appears probable, although far from being satisfactorily established, that in Western Europe this attitude is characteristic of the Stone age, cremation of that of Bronze . . . At the same time, it must be admitted that the evidence is very far from conclusive. (Lubbock 1865: 103)

One does not need to be a statistical genius to see that, despite his caveats, Lubbock was using Colt Hoare's larger sample of barrows with bronze to swamp Bateman's smaller one, to obscure regional variation and create his Bronze Age cremation stage. But what his figures actually showed was that in Bateman's northern sample, ten out of thirty-seven burials with bronze (27 per cent) were cremations; while in Hoare's southern sample forty-nine out of sixty-three such burials (78 per cent) were cremations. This clearly demonstrated regional variation, something that fitted Wright's tribal perspective much better. Greenwell warned Lubbock in no uncertain terms:

A. Lubbock's table of 297 burials listed by Bateman

IMPLEMENTS.	CORPSE.				TOTAL.
	CONTRACTED.	BURNT.	EXTENDED.	POSITION UNCERTAIN.	
None . .	27	63	3	7	100
Stone . .	53	48	2	31	134
Bronze .	15	10	5	7	37
Iron . .	2	3	14	7	26
Total .	97	124	24	52	297

B. Lubbock's table of 267 burials listed by Hoare

IMPLEMENTS.	CORPSE.				TOTAL.
	CONTRACTED.	BURNT.	EXTENDED.	POSITION UNCERTAIN.	
None . .	9	160	3	12	184
Stone . .	2	5	1	1	9
Bronze .	4	49	2	8	63
Iron . .	—	—	7	4	11
Total .	15	214	13	25	267

C. Lubbock's table combining the two above

IMPLEMENTS.	CORPSE.				TOTAL.
	CONTRACTED.	BURNT.	EXTENDED.	POSITION UNCERTAIN.	
None . .	36	223	6	19	284
Stone . .	55	53	3	32	143
Bronze .	19	59	7	15	100
Iron . .	2	3	21	11	37
Total .	112	338	37	77	564

Fig. 7.5. Lubbock's summary tables of published burial data. A: 297 burials described by Bateman; B: 267 burials described by Colt Hoare; C: the Bateman and Colt Hoare datasets combined. (From Lubbock 1865: 101–2).

I should not say that during the bronze age the majority were burnt, the burial in stone cists, with a very slight, if any mound, is exceedingly frequent, & these, I have no doubt, are, as a rule, subsequent to burials after cremation, with these burials we find bronze daggers not uncommonly. We have numerous skulls (markedly brachy-cephalic) of the bronze period, usually found with vessels of the drinking cup style. (Greenwell to Lubbock, 12 August 1865, British Library, London, Add MS 49641/84–7)

But Lubbock did not take Greenwell's advice, and the second edition of *Pre-historic Times* (Lubbock 1869) went to press with the identical tables and discussion. Greenwell wrote to John Evans on 7 December 1868 expressing his disappointment:

Lubbock is in the press, I fear the Bateman tables will not be much improved, he says 'I made a visit to Mr Bateman's on purpose to examine the points you allude to, but it would take a very long time to find all the things even if it were possible to do so'. This is a great pity, for the tables, as they stand, are by no means satisfactory, and are misleading in fact. (Ashmolean Museum, University of Oxford, JE/ B/ 1/7)

Greenwell gave Lubbock details of over one hundred of his own barrow excavations; Lubbock duly listed these in his third edition (1872: 146–50), but he *did not change his summary tables*, which remained exactly as in fig. 7.5. John Thurnam (1871: 310) produced a table incorporating Greenwell's results, and a southern sample from the Dorset excavator Charles Warne (1866). His percentage calculations clearly gave Lubbock the *coup de grâce* by supporting the regional pattern, with far more cremation in the South than the North (fig. 7.6).

Lubbock in fact never rectified the situation, and the identical tables and discussion appeared in every edition of *Pre-historic Times* through to the last (Avebury 1913). Perhaps he was losing some of his interest in archaeology as his joint career of banking and parliamentary membership claimed more of his time. At all events, after 1867 his debate with Wright languished. Wright himself reprinted his 1866 paper from the *Ethnologicals' Transactions* (Wright 1866a) in the third edition of his *The Celt, the Roman, and the Saxon* (Wright 1875: 1–22) without responding to any of the criticisms of Lubbock and Lubbock (1867). Lubbock no doubt felt he had been victorious—but a reader of *Arch. J.* and *JBAA* in the decade after 1866 would have concluded that Wright had won the exchange.

	Numbers			Proportions		
	unburnt	burnt	total	unburnt	burnt	total
Wiltshire (Hoare)	82	272	354	23.2	76.8	100
Dorsetshire (Warne)	21	91	112	18.7	81.3	100
Derby, Staffs. and Yorks (Bateman)	150	121	271	55.4	44.6	100
Yorkshire, &c. (Greenwell)	58	53	111	52.2	47.8	100

Fig. 7.6. Thurnam's table listing four Bronze Age datasets, those of Bateman and Greenwell from northern England, and those of Hoare and Warne from southern England. (From Thurnam 1871: 310).

AFTER THE DEBATE: RESISTANCE 1867–1877

1866 was a remarkable year for *Arch. J.*, seeing the publication of the major pro-Three Age System papers by Lubbock and Worsaae mentioned above. The year after was just as remarkable, in view of an outspoken counterblast from no lesser a personage than the Archbishop of York (William Thomson). An aspect of the debate in these years was that those opposing the Three Age System used ever stronger language:

Many people think to this day of a museum of antiquities as a collection of stones and potsherds, ticketed into dignity by falsehoods which no man can prove and no man expose, and divide collectors into two classes—those who deceive themselves, and those who would deceive other people... The theory of three periods, the Stone Age, the Bronze Age, and the Iron Age, has been carried too far; and in assigning a place to any weapon or other implement, people often forget that long after bronze and iron had been discovered, stone might continue to be used among the poorer and less civilized, whilst in our own country it is very probable that the iron instrument preceded the composite metal bronze which was in use on the continent. (York 1867: 86–7)

In regarding the contemporaneous use of stone and metal tools as disproof of the Three Age System, the archbishop was following the earlier arguments of Thomas Wright (1852: vii), ignoring authors as

disparate as Worsaae, Lubbock, and Greenwell who had all stressed that contemporaneous use was part of the scheme (see fig. 7.1).

We shall return to the *Arch. J.* shortly. Thomas Wright's *JBAA* meanwhile showed no signs of adopting the Three Age System. William Lukis, the Channel Islander whom we last encountered debating the age of Irish megaliths with George du Noyer (chapter 6), countered the suggestion that long barrows were older than round barrows by suggesting that long barrows were in fact composed of several round ones arranged linearly. Thus round barrows were necessarily *older* than long ones, which invalidated the craniological scheme of Thurnam and Greenwell (W. C. Lukis 1866a). Some years later, Roberts followed Wright's tribal party line when considering urn burials; his major problem was whether his finds contained the remains of the Trinobantes or some other tribe; they were definitely pre-Roman because of the absence of metals (Roberts 1871). Even in 1877, the year of Thomas Wright's death, Wise's discussion of megaliths such as New Grange and Maes Howe dated them merely as 'Celtic' (Wise 1877), while William Lukis discussed the megaliths of Cornwall without any mention of chronology at all (W. C. Lukis 1877).

Wright himself was, however, showing signs of unravelling in the 1870s. As part of his espousal of a short chronology in his debate with Lubbock, he had criticized the early dates ascribed to the Swiss lake dwellings, putting this down to 'a want of knowledge' (Wright 1866b: 83) on the part of the eminent Ferdinand Keller. To prove this he depicted two of Keller's claimed Bronze Age pots (fig. 7.7), stating that they were clearly post-Roman and should be termed 'Frankish' or 'Alemannic', and depicted typologically similar Anglo-Saxon pottery from England in support of this. But when he produced the third edition of *The Celt, the Roman, and the Saxon* in 1875 (complete with his reprinted 1866 article), he included a frontispiece labelled 'Roman British Pottery'. This included one of Keller's pots as well as the group he had earlier called Anglo-Saxon (fig. 7.7).

Opposition to the Three Age System acquired a curious 'backwoods' feel in the 1870s. The Cornishman William Copeland Borlase produced a book on the burial customs of his native county, which he started by assaulting the Three Age System and particularly Worsaae. Claiming that bronze axes had been found with Roman coins, he concluded:

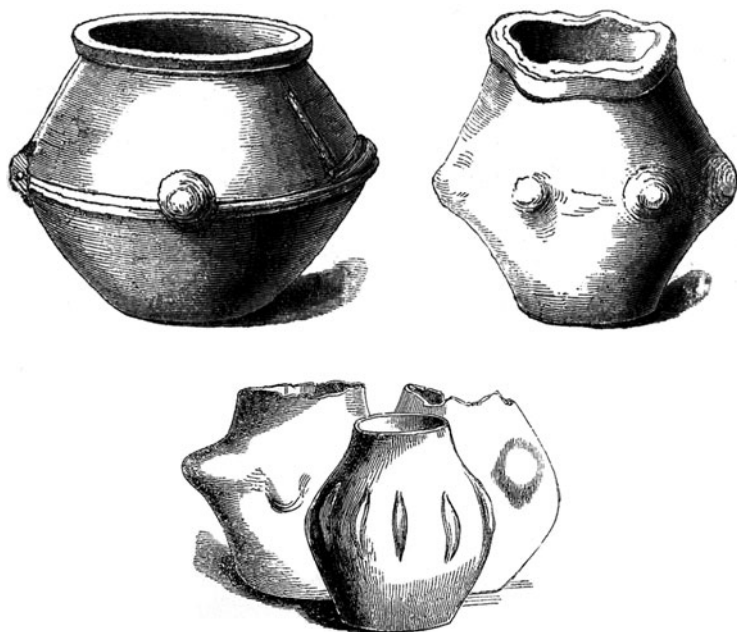
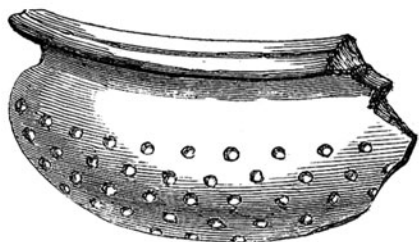


Fig. 7.7. Pottery illustrated by Thomas Wright. (*top*) two pots from Switzerland claimed by Keller to be Bronze Age, regarded by Wright as post-Roman. (*bottom*) Anglo-Saxon pottery from a cemetery near Derby. (*opposite*) the frontispiece from the third edition of Wright's *The Celt, the Roman, and the Saxon*. (From Wright 1866b: 83, 84; and Wright 1875: frontispiece).

Here then is a difficulty for Mr. Worsaae. What if a few of his countrymen (antiquaries instead of pirates) would once more turn their prows to the now friendly shores of Danmonia? Would the solid entrenchments (of fact) be proof against them this time? The truth is that the whole theory of these periods, applicable to certain localities perhaps, or useful for purposes of mild generalization, breaks down directly it is considered universally inclusive, or is applied at random to individual instances. (W. C. Borlase 1872: 5–6)

Clearly considering himself a scientist working from observation rather than preconceived theory, he continued that:



Roman British Pottery.

[*the archaeologist*] must ever be ready to be taken by surprise, and never be astonished to find a pet notion rudely dashed to the ground by a stroke of the pickaxe, or a turn of the shovel. Never should he be ready to sacrifice a fact, merely because it is hard to explain, upon the altar of a much more indefensible theory; and should he, in exploring remains which he considers pre-historic, chance to light upon any object which would bring them within the pale of history, he should, before discarding it, ask himself fairly the question: 'Why should not this monument belong to the same period to which I know this relic must be assigned?'. (W. C. Borlase 1872: 6)

He certainly practised what he preached. In the barrow on Morvah Hill he found a coin of the Roman emperor Constantine alongside stone tools; this is his discussion of the date of the burial:

What is the most natural inference then? That the coins must be thrown out of the question, because of the flint chip? or the whole structure referred at once to the Stone Age, thousands of years B.C., because it is encircled by large stones, or because the pottery is rude, and its ornamentation not curvilinear? Is it not rather the only fair course to admit at once that this interment, although possessing every characteristic of the so-called Stone Age, was placed here not earlier than the end of the third century, A.D.; that is, at the time when the coin was struck. (W. C. Borlase 1872: 263–4).

To which a Worsaae or a Greenwell might have responded that recent improvements in excavation technique had evidently passed Mr Borlase by. But Borlase was publishing in mainstream journals as well; the year after his book, he had papers in both *Archaeologia* and *Arch. J.* In the first he likened a find to similar ones Greenwell had presented, but without making any use of Greenwell's chronology (Borlase 1873a). In the second he identified some hut circles as the oldest traces of habitation in Cornwall, stating that coins dated them to the late Roman period (Borlase 1873b: 346–7). Borlase's mainstream status was reaffirmed by an anonymous reviewer in *Arch. J.*, who singled out the dating chapter of his book for particular praise, stating that it proved that virtually all Cornish archaeology dated from the third century AD or later (*Arch. J.* 33, 1876, 97–8).

For sheer outspoken ignorance it would be hard to beat James Fergusson, an architect who wrote a book entitled *Rude Stone Monuments* (Fergusson 1872). Displaying absolutely no knowledge of what the Danes had actually done to produce the Three Age System, he lamented that:

If the Danes, instead of breaking up their ‘finds’ and distributing them in cases according to a pre-conceived system, had kept and published a careful record of the places where the contents of their museums had been found, and in what juxtaposition, we should not probably be in our current difficulty. Under the circumstances, it is perhaps fortunate that we had no central museum, but that our antiquaries have published careful narratives of their proceedings. (Fergusson 1872: 10)

What would Thomsen have said to that? Fergusson went on to liken the Three Age System to the wild druidical theories of the eighteenth-century archaeologist William Stukeley; it had all been downhill from then on, as he explained in his choicest prose:

Stukeley, however, cut the vessel adrift from the moorings of common sense, and she has since been a derelict tossed about by the winds and waves of every passing fancy, till recently, when an attempt has been made to tow the wreck into the misty haven of prehistoric antiquity. If ever she reaches that nebulous region, she may as well be broken up in despair, as she can be of no further use for human purposes. (Fergusson 1872: 15–16)

Not everyone was caught up in this maelstrom of invective, however. The Dorsetshire squire Charles Warne sailed serenely through, excavating barrows with his friends and untroubled by questions of chronology. He had been excavating barrows in his native county since the earliest days of the Association, a paper by him appearing in the publication of the Winchester congress alongside those of Thomas Bateman (Warne 1846); and like some latter-day Bateman, he was content to continue producing huge lists of the barrows he had dug. His *The Celtic Tumuli of Dorset* contained only one tiny chronological hint, an absence of metals from one site suggesting it was old (Warne 1866: 62). Five years later he seems to have felt the need to add a professional veneer on his next book; Dr Wake Smart provided this in the form of an introduction presenting the Three Age System and suggesting that most of the barrows Warne considered were Bronze Age (in Warne 1872: xv). This seems to have absolved Warne of any need even to hint at chronology in the main text of the book, for none is to be found.

We now return to the Institute, and the curious figure of Albert Way, whom we encountered as an opponent of the Three Age System in chapter 4. Way was in the habit of rewriting or commenting on

articles that appeared in *Arch. J.* He did this twice in the same issue in which the Archbishop of York launched his counter-attack (see above). Both concerned material from North Wales excavated by the Hon. William Stanley, in whose stately home Way sometimes stayed. One was on graves and urns, and is described as 'from notices communicated by' Stanley rather than merely 'by' him; it would appear that Way modified or rewrote it (Stanley 1867a). We saw above (fig. 4.14) how in an earlier paper Way (1849b) had accepted a funerary urn as that of Bronwen, a person historically dated to the first century AD. Stanley described and illustrated it once more, with a slightly modified caption, still stating it was Bronwen's urn (Stanley 1867a: 18). In the other case Way allowed Stanley (1867b) to present his excavations of hut circles, following this with an article of his own on the finds (Way 1867). Stanley's idea of chronology involved three periods, the first with primitive grain crushers, the second with pottery, stone, and bronze, the third with artwork, nets, and weapons like those of the Eskimo; his huts dated to the first (Stanley 1867b: 241–2). Way's paper contained no mention of any date except the statement that a jet necklace was not Roman (Way 1867: 263–4). Two years later Way wrote an article about bronzes from Devonshire; a find from Plymstock he noted was 'the most valuable and instructive examples of relics of their class hitherto brought to light in Devonshire' (Way 1869: 346), but without saying why or making any attempt to date them; this was to have an important outcome, so the Plymstock finds are shown in fig. 7.8. Way continued to ignore periodization in his last major prehistoric article in *Arch. J.*, on Welsh cromlechs (Way 1871).

This ignoring of the Three Age System casts Way in a very traditional light as far as his operations within the Institute were concerned. But something very strange was taking place outside that organization. Stanley and Way were publishing their Welsh material not just in *Arch. J.* but also in an expanded version in *Archaeologia Cambrensis* (Stanley and Way 1868a), and reproduced this in a book (Stanley and Way 1868b). This book contained Stanley's paper on burials and urns, a verbatim reprinting of his paper on the huts, and an enlarged version of Way's commentary on their finds (Stanley 1867a and 1867b; Way 1867). Remarkably, in this book the section on graves and urns is *based upon the Three Age System*. Stanley had no

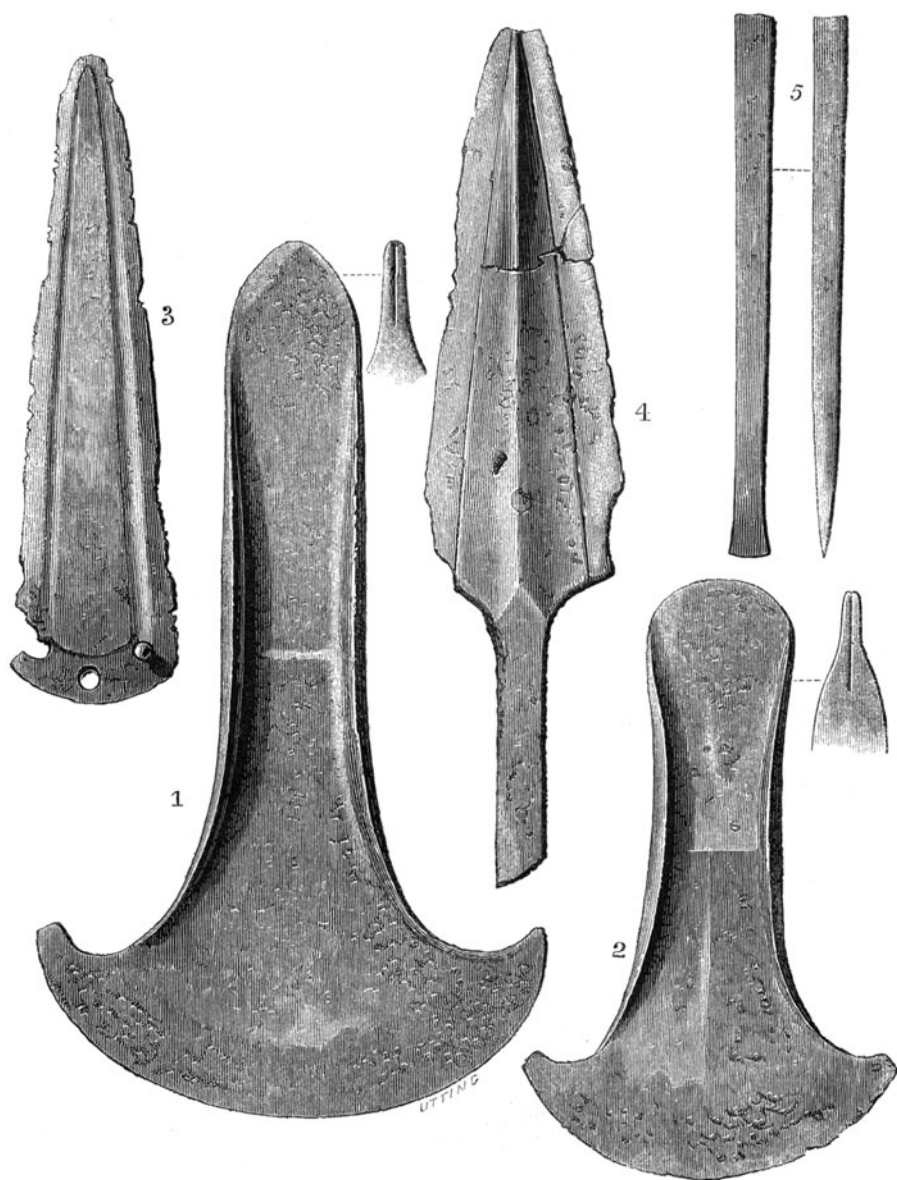


Fig. 7.8. Bronze items found at Plymstock in Devon. (From Way 1869: facing p. 346).

chronological sense at all (see above), so this change must have been due to Way himself. Urns were divided up chronologically; for example, incense cups (type II) were dated to 'the "Neolithic" or Later Stone Age' (Stanley and Way 1868b: 72). Lubbock's 1866 address to the Institute was cited as 'a valuable and lucid summary of the results of modern research in regard to the succession of periods' (Stanley and Way 1868b: 76). Greenwell's date of 1000 BC for the transition from the Neolithic to the Bronze Age was adopted, though he was not quoted by name:

to this later stone period, extending, according to the conclusions of archaeologists of reliable authority, to a thousand years, approximately, before our era, the most ancient interments seem to belong... To the Bronze Age, commencing possibly some thousand years before our era, the more skilfully fabricated urns are doubtless, for the most part, to be assigned. (Stanley and Way 1868b: 76)

Bronwen's urn was again discussed, with results completely different to those presented by Stanley in *Arch. J.* only the year before:

It will be obvious to anyone conversant with the facts, so largely augmented by recent researches into British burials, that the relics with which so interesting a tradition has been associated must be assigned to a much earlier period than the days of Bronwen the Fair. The introduction of the use of bronze may indeed be stated, approximately, as having occurred about a thousand years before our era; it may be inferred that some considerable period would elapse before its extension to the distant shores of Mona. (Stanley and Way 1868b: 23)

The urn was illustrated as in Stanley's article of the previous year, complete with its new caption which neatly separated Bronwen from the urn (fig. 7.9). Examination of the incinerated contents of the urn had revealed a fragment of uncremated skull and sherds of another vessel in the ashes, clearly an interpolation but probably derived from an *earlier* inhumation, because (on the authority of Franks) the sherds were typologically older than 'Bronwen's urn' itself (Stanley and Way 1868b: 21–3).

Albert Way had evidently changed his mind about the Three Age System. This also emerges from the letter he wrote to William Wakeman in Ireland, dated 28 August 1870. It will be recalled that Wakeman sent Way some pottery from the Ballydoolough

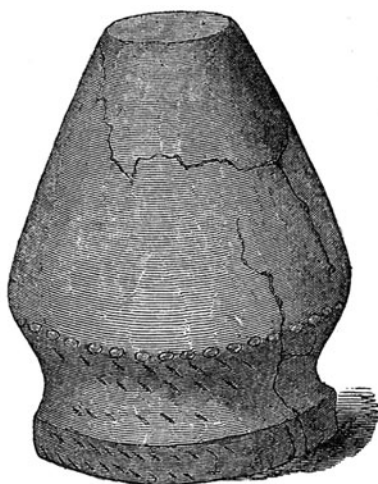


Fig. 6.—Urn, as supposed, of Bronwen, daughter of Llyr. Date of her death, about A.D. 50. Height, 12 inches; diameter, at the mouth, 9 ins. British Museum.

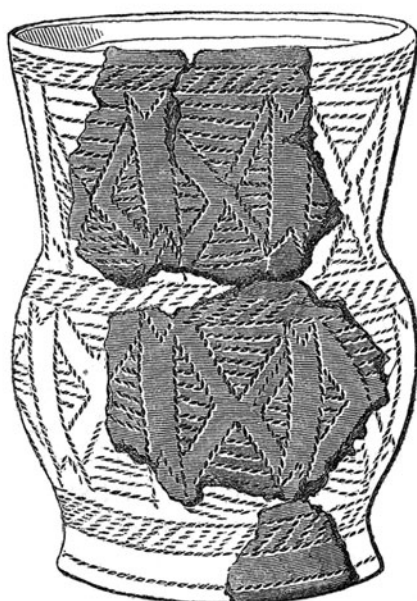


Fig. 7.—Drinking-cup. Fragments found with Bronwen's Urn.

Fig. 7.9. Top: the second illustration of Bronwen's urn, with its revised caption (cf. fig. 4.14). Bottom: sherds of a pot found in the ashes inside Bronwen's urn, which Way recognized as indicating a secondary burial. (From Stanley and Way 1868b: figs. 6 and 7). Size of original urn given in the caption.

crannog, but did not accept Way's diagnosis of the material as late (chapter 6). The wording of Way's letter is however revealing: 'I should not, however, ascribe these remarkable wares to a very early age; for instance, to that in which the use of bronze was prevalent' (in Wakeman 1871a: 366–7); Way clearly accepted the Bronze Age even if he did not think these sherds belonged to it. But why did he not signal this in any of his papers in *Arch. J.*? Perhaps the orthodoxy he had established in the Institute, reinforced by the Archbishop of York in 1867 after the deviation in the papers of Greenwell, Lubbock, and Worsaae in 1865–6, made it impossible to recant at so late a stage. At this remove, Way remains an enigma; but the reticence he displayed in his 1869 article on bronzes was to have an outcome: it led *Arch. J.*'s reviewer of Greenwell's *British Barrows* into a major error.

LAST GASP: THE HOSTILE REVIEW OF GREENWELL'S BRITISH BARROWS

As the 1870s drew on, two things were happening. The first was that the Institute and the Association became rudderless with the deaths of their respective leading figures, Albert Way in 1874 and Thomas Wright in 1877. Opposition to the Three Age System began to fade in both organizations, and mentions of the scheme appeared in both their journals. At the Institute's meeting in 1878, John Evans gave a paper on the antiquities of Northamptonshire, which was organized around the Three Age System (Evans 1878). In *JBAA* two years later, Prigg (1880: 60) assigned a group of bronzes to 'near the close of the bronze age'. Secondly, high-quality archaeological fieldwork was continuing to produce solid results. In Greenwell's case this culminated in the publication of *British Barrows* in 1877. This has been described as 'the dullest book ever written' (Marsden 1974: 99) and it certainly cannot be considered light reading; but it had a major job to do. It reported on 378 burials from the Yorkshire Wolds alone (Greenwell 1877: 19) and also covered barrows from other parts of the country. Previous barrow studies had always been regional, but Greenwell's title was not just nicely alliterative: it reflected reality. It included over 400 pages of descriptions of barrow excavations, but more

importantly these were *preceded* by 132 pages of precisely the kind of theory deplored by Borlase, Fergusson, and their kind; there was also a long section by George Rolleston on human skulls. The book was organized explicitly around the Three Age System, but the discussion ranged much further than the artefacts. Greenwell concluded that the economy was based on domestic animals (1877: 109–10) and cultivated cereals (1877: 114–15), and he was the first person ever to identify a dairy economy on the basis of the ages of the cattle jaws in a zooarchaeological assemblage; this he did at Grimes Graves (Greenwell 1870: 431; Greenwell 1877: 115–16). He was even prepared to draw conclusions about Bronze Age social structure (1877: 111–13).

The long and hostile review of *British Barrows* that appeared in *Arch. J.* marked the last serious attempt in mainstream British archaeology to oppose the Three Age System. Although technically anonymous, the reviewer can be identified as C. Sprengel Greaves QC (1801–1881), a gentleman whom we have not previously encountered. The identification can be made because of three mentions in the review of past work by the reviewer. Discussing a barrow he stated that ‘*we conjectured* that the barrow had been formed’; a point ‘*is explained* in a former volume’; and ‘*we, however, procured* some jet from Whitby’ (Greaves 1879a: 187, 187, 298, added emphases). The references at these points to *Arch. J.* 18: 69, 33: 393, and 29: 283 respectively are all to contributions by Greaves. As his legal career came to an end he took an interest in archaeology; although he had written a one-page note on the formation of barrows (Greaves 1876), his contributions to *Arch. J.* reveal that he was mostly interested in the medieval period:

- 1861, barrow at Bradley (*Arch. J.* 18: 69)
- 1863, medieval cemetery in Oxford (*Arch. J.* 20: 191)
- 1865, Greek inscription from Sestos (*Arch. J.* 22: 171–3)
- 1865, excavation at Ilium Novum (*Arch. J.* 22: 337)
- 1866, medieval punishment of lepers (*Arch. J.* 23: 68–9, 73)
- 1867, on the accuracy of rural traditions (*Arch. J.* 24: 77)
- 1871, incense cups in burial in the Troad (*Arch. J.* 28: 68)
- 1871, small vase in burial in the Troad (*Arch. J.* 28: 164)
- 1872, words on the death of Canon Rock (*Arch. J.* 29: 186)
- 1872, comb with runic inscription (*Arch. J.* 29: 280–6, 291)

- 1874, burial of Ranulph Earl of Chester (*Arch. J.* 31: 295–6)
- 1874, extract of Camden's *Britannia* (*Arch. J.* 31: 302)
- 1874, brasses in Mugginton Church (*Arch. J.* 31: 375–82)
- 1874, Etruscan inscription (*Arch. J.* 31: 386)
- 1876, brasses in Morley Church (*Arch. J.* 33: 290–3, 401)
- 1876, stained glass from Long Melford (*Arch. J.* 33: 397)
- 1876, barrow formation (Greaves 1876)
- 1877, on the stature of the ancient Greeks (*Arch. J.* 34: 190)
- 1878, rubbings of medieval gravestone (*Arch. J.* 35: 90)
- 1878, preservation of posts in sand and clay (*Arch. J.* 35: 299)
- 1879, *British Barrows* review (Greaves 1879a)
- 1879, cannibalism in England (Greaves 1879b)
- 1880, medieval document from Chelmerton (*Arch. J.* 37: 330)

Greaves' only substantive paper was on cannibalism, appearing in the same year as his review of Greenwell (Greaves 1879b). His writings (including his review of *British Barrows*) are littered with references to the classics; even his note on barrow formation was a speculation on whether a description in Tacitus accounted for a feature he had seen in excavation. Greaves was thus only minimally equipped to review *British Barrows* at all, and as we shall see, he hardly bothered to read it before writing his review. He could not grasp chronology that did not involve written sources; he contested Greenwell's (1877: 130) statement that the Iron Age might have begun around 250 bc because Roman traders in Caesar's day could not know what was going on so early, and in any case only knew the south coast. 'This consideration leads us to doubt the Canon's position... It seems impossible to us to fix any date, even approximately' (Greaves 1879a: 194). We may speculate whether Greaves was the last man in British archaeology to have absolutely no 'idea of prehistory'.

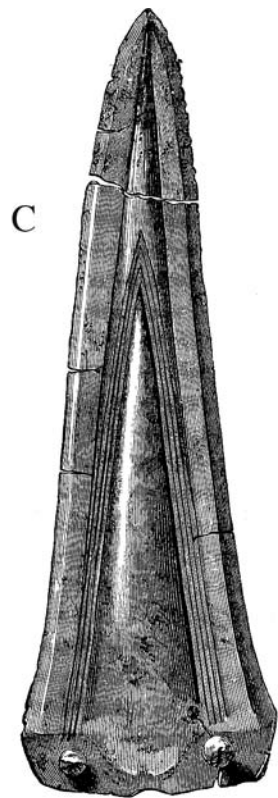
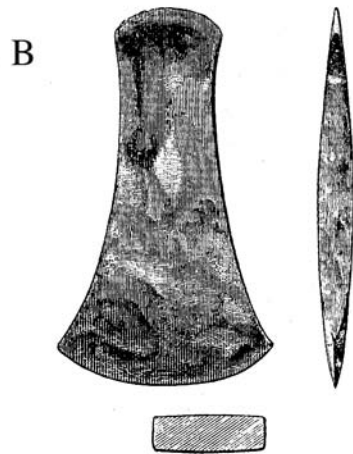
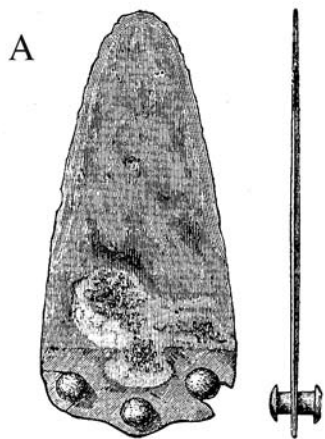
This final section will consider three criticisms raised by Greaves in his review, all of which, however, highlight more the ineptitude and carelessness of the review than any shortcomings in *British Barrows*. The first criticism concerned Greenwell's conclusion that long barrows were older than round ones. A basic barrow erected over a burial would be round, stated Greaves; things like stone chambers, or the lengthening of the mound into the form of a long barrow, were very clearly later innovations and additions to the original round

barrow (Greaves 1879a: 196). We saw above that W. C. Lukis (1866a) had argued that a long barrow was really an extended accretion of round barrows; Llewellyn Jewitt argued the same in his two books (1870: 6; 1877: 2–3), and the craniologist Joseph Barnard Davis (1857: 43) had hinted at the same thing. Greaves would have done well to quote these authorities in support of his position, but he apparently did not know of them. In fact Greenwell had demonstrated that this argument was false; as in his previous writings he published no excavation plans, but described his sites so precisely that a reader could draw his own. This is part of what he wrote about the Willerby Wold long barrow:

It is 132 ft. long, 50 ft. wide at the east, and 40 ft. at the west end, where the height is 5½ ft., the opposite end being 7 ft. high. A shallow trench was observed along both sides of the mound, but it was not continued round the ends... The mound was composed of chalk-rubble, flints and earth... Along the centre line of the barrow, and commencing at the east end, where the action of fire had been strongest, was a deposit of calcined chalk and flint, 3½ ft. wide and about 4 ft. high, resting upon the natural surface. The evidence of burning became gradually less towards the west... As was found to be the case in the Scamridge long barrow... the mesial deposit of chalk and flint in this mound was perfectly distinct from the general material of the barrow. (Greenwell 1877: 488)

Greenwell here clearly demonstrated the constructional unity of these barrows; the chalk spine was a primary feature constructed before the rest of the long barrow was placed on top. Had Greaves bothered to read this description he could have been left in no doubt about this.

Greaves' second criticism was of Greenwell's cranial chronology. He singled out a statement that round barrows contained both brachycephalic and dolichocephalic skulls, indicating people of two different stocks. Greaves disputed this, stating that 'to us it seems clear that there were not two stocks, but one people, some of whom had the one kind of skull, some the other' (Greaves 1879a: 293) because a man would have paid no attention to skull shape when choosing a wife. Greenwell did make the statement alluded to (Greenwell 1877: 126–7) but in the context of a comparison with other parts of Britain. Had Greaves read Greenwell's book thoroughly, he would have discovered the more comprehensive



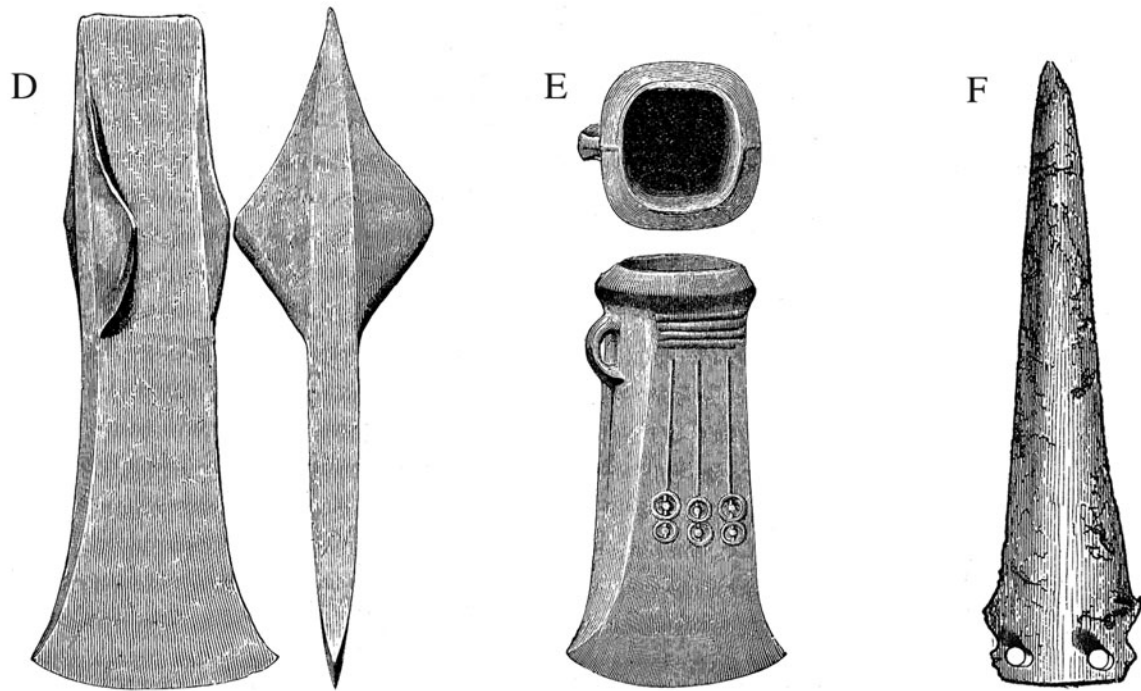


Fig. 7.10. Selection of Greenwell's illustrations of bronze artefacts. A: simple knife-dagger; B: simple flat axe; C: more robust knife-dagger; D: flanged axe; E: socketed axe. A, B and C are early forms, D and E later forms. (From Greenwell 1877: figs. 37, 38, 41, 42, and 43). F is Worsaae's illustration of a knife-dagger from Denmark. (From Worsaae 1866: fig. 3).

discussion of skulls a few pages earlier, which pre-empted his criticism. The long barrows, the earliest burials, contained just long-headed people, but the round barrows were different:

The round barrows, then, contain two very distinct forms of skull, a long and a round one, together with less characteristic forms which may be supposed to have belonged to people who were descended from inter-marriages between persons whose heads were of the two different types in question... [*the earlier long-headed people were*] probably intruded upon and conquered by the more powerfully made round-headed folk, who, as is nearly always found to be the case, would in course of time become intermixed with them, and with whom in the end they would become identified as one people. (Greenwell 1877: 122)

Greaves' third criticism was about Greenwell's subdivision of the Bronze Age—and it was here that Albert Way's previously noted reticence caused him problems. We saw above that Greenwell advocated a two-phase Bronze Age as early as 1873, with simple knife-daggers and axes representing the early phase found as grave goods; while swords, spear heads, and flanged and socketed axes represented the later phase, mainly known from hoards. In *British Barrows* he illustrated the simple early forms (figs. 7.10 A and B), and a more complex and robust form that he had not found in any barrows, but which had turned up in some Wiltshire barrows (fig. 7.10 C); he also depicted the later forms of flanged and socketed axes (fig. 7.10 D and E). Greaves contested this dating, arguing that the Plymstock find published by Way (1869) proved Greenwell wrong:

[*the simple knife-daggers and plain axes*] were produced by great skill, and the bronze was of excellent quality... Nor does it seem reasonable to suppose that such instruments would have ceased to be made when bronze was more generally manufactured; and in this case we have an instance of the great danger of trusting to the mere absence of a thing as proof that it did not exist; for at Plymstock, Devon, there were found... sixteen bronze celts, a two edged weapon, a mortice chisel and three of these bronze knives. This is quite sufficient to show that these knives were made in the best bronze period; and it completely upsets Canon Greenwell's conclusion from the presence of these small instruments in the round barrows that they 'belong to a period before bronze was in common use'. (Greaves 1879a: 191)

Plymstock did indeed have such daggers (fig. 7.8 no. 3), which were very similar to the specimen Greenwell illustrated (fig. 7.10 C). But what Greaves had failed to notice was that Greenwell had discussed the dating of the Plymstock find, albeit in a footnote, demolishing any notion that the hoard might be late: ‘a hoard of bronze articles . . . was discovered at Plymstock, Devon. There were no paalstabs [*palstaves*] of the later form, no socketed celts, spear-heads, nor swords, but knife-daggers, an early, though not perhaps the earliest, form of axe, a narrow chisel &c’ (Greenwell 1877: 46–7 n. 1). Thus the Plymstock hoard was uniformly early, containing no late types; the slight flanges on the axes (fig. 7.8 nos. 1 and 2) implied they were a little later than the plain form (fig. 7.10 B), but certainly not as late as the fully flanged late type (fig. 7.10 D). Had Albert Way come out in support of the Three Age System in his *Arch. J.* papers, this was an embarrassment Greaves might have been spared; for Worsaae (1866: fig. 3) had illustrated a classic knife-dagger (fig. 7.10 F), ascribing it to the Early Bronze Age, and since this paper too appeared in *Arch. J.* Way cannot have been in ignorance of it.

Greaves was one of the last of his kind, and he died in 1881. It was not in Greenwell’s nature to attempt any kind of reply. Greaves in any case had shot himself so comprehensively in the foot through the ineptitudes of his review that a reply was scarcely needed. By failing so completely to make any impression on Greenwell’s book, Greaves had in fact done the Three Age System a considerable service: he had shown that *British Barrows* had rendered it unassailable.

Aftermath

We have followed the story of the Three Age System a very long way indeed. We saw how it emerged in Copenhagen and Lund, how it was received there, and how Worsaae fought to establish it there. We then saw how it came to Britain, and followed in Worsaae's footsteps from London to Edinburgh to Dublin and back to London again. In each of the three capitals in which it was considered, accepted or rejected, the academic context was quite different from the others. In London the archaeologists were safe sheltering under the dominant ethnological paradigm, and for some time saw no reason to venture out from beneath it. In Edinburgh the Four Stage Theory and long links with Denmark made the Scandinavian story much easier to swallow rapidly. In Dublin the historical elite was so blinded by the glory of their ancient history that there was no place for the archaeological theory, and it had to be carried into the capital by an originally provincial archaeological movement. Back in London, the safe ethnological chronology was jolted out of alignment by the discovery of human antiquity, and alongside this—and on the back of high-quality archaeological excavation—the Three Age System finally won the day.

Some aspects of the story have long been well known. The roles of C. J. Thomsen, of J. J. A. Worsaae, of Daniel Wilson, and of John Lubbock have all received much exposure in discussions of the history of archaeology. But in following this story we have also sometimes looked beneath stones that have seldom if ever previously been lifted in this connection, at least in the Anglophone literature. It has for example rarely been understood that Thomsen's 'idea of prehistory' was not simply forging back into hitherto uncharted

chronological territory, but was to begin with leaning on the elaborate ancient historical structure of Peter Frederik Suhm. It was only when Christian Molbech kicked away this structure in the 1830s that the Three Age System had to stand on its own. Fortunately it was rapidly supported by three other chronologies employing physical evidence, and they acted as supports in its very earliest days of independence. When Worsaae hastened ancient history into its grave in the 1840s, the Three Age System was therefore able to stand on its own four feet (archaeology, economy, ecology, and craniology). This meant it was robust enough to fight off and defeat the nationalist claims of Jacob Grimm at the end of the decade, in parallel with the Danish military victory. Academic victories are no defence against tanks, however, and had Worsaae been alive when Hitler's army rolled over the border in 1940, he might well have considered his victory meaningless.

The extent of the resistance to the Three Age System in London is something rarely appreciated. Men like Thomas Wright, Albert Way, and their colleagues were not blinkered diehards. They were the renaissance men of the Victorian era, masters of a multiplicity of academic strands to a degree that modern archaeologists can only stand in awe of. Their chronological views comprised a complex network of associations and assumptions some created by themselves, many derived from neighbouring disciplines. That history has proved them wrong in no way belittles their achievements; it places them instead in a certain perspective, from which with early twenty-first century hindsight we may see in retrospect how they went wrong. But whether that will stop us from making the same type of mistake, from committing some minor part of our careers to an orthodoxy that later explodes our reputations, one has reason to doubt. Had one asked Thomas Wright what he would be remembered for, he would almost certainly not have chosen anything to do with prehistoric archaeology. And had one asked Thomas Bateman, it is unlikely that he would have understood the question. The person it would have been most interesting to ask would have been the fickle and intangible Albert Way, whose life leading the Institute seems in its last years to have been a double one.

The Scottish story in contrast appears uncluttered, a clean-cut event appropriate to this efficient and clear-sighted northern nation.

The nationalist context of Scottish developments has sometimes been overstated, but the fellow-feeling between Daniel Wilson and Peter Andreas Munch was undoubtedly important. Paradoxically, it was Munch's nationalist feelings directed against the Danes, rather than Worsaae's directed against the Germans, which were ultimately to have the greater formative influence. Scottish archaeologists can today bask in the reflected glow of Wilson's 'idea of prehistory', and indeed his first Anglophone use of the very word; it does not diminish Wilson's achievement and stature to suggest that his conversations with Munch may partly have stimulated both. Nor are Scottish archaeological credentials in any way threatened by the fact that the first man to publish prehistoric stratigraphy north of the border came from (just) south of it; William Greenwell was pushing no agenda other than the strictly archaeological.

The Irish situation in contrast was the most long drawn out of any. One suspects that, after Edinburgh, Worsaae might have felt himself on another planet when he arrived in Dublin. The high profile of people like the bumbling Sir William Betham or the insane Henry O'Brien apparently muted his normally critical outspokenness. Having perforce put his trust in George Petrie, Worsaae could not have foreseen that he would never deliver the Three Age System to Dublin. Still less could he have foreseen that it would be a provincial society that would ultimately come through and bring it to the capital. Of all the names considered in this book that have emerged as crucial to the success of the Three Age System, those of James Graves and William Wakeman are perhaps the least sung; but had they not laid the groundwork for W. G. Wood-Martin, there is no telling the state in which Irish archaeology might have found itself at the turn of the twentieth century.

Back in London after human antiquity burst upon the scene, it has often been assumed that Sir John Lubbock had an easy time of it in assuming the victor's crown, in order to see both human antiquity and the Three Age System through to ultimate triumph. The weaknesses in his argument, however, laid him open to Wright's effective riposte. English archaeologists mostly know William Greenwell as the man who wrote the huge but rather boring book about hundreds of barrows. Few appreciate the degree of resistance to the Three Age System that remained into the 1870s, and thus how important *British*

Barrows was in quelling it. Fewer still appreciate how Greenwell's excavation technique gave him the wherewithal to publish so incisively, and to provide Lubbock with (some of) the answers to feed back to Wright. And none will have heard of C. Sprengel Greaves, the unfortunate retired lawyer whose misfortune it was, briefly and falteringly, to carry the dying torch of Way and Wright after their deaths. Whatever one may think of the intractable Wright and the intangible Way, it is hard to avoid feeling some sympathy for the unexpectedly exposed Greaves.

What was at stake in all the arguments considered here was: who should speak for the most ancient past. As archaeologists, we all live to this day with the outcomes of the various debates that were fought through as described in this book. In Copenhagen, archaeology has a national identity so secure and all-encompassing that Danish archaeologists sometimes find it curious that any prehistorian should choose to work abroad. But the robust chronological structure that archaeology began to develop in the 1830s and 1840s has since become the envy of Europe, and prehistorians from Britain have always flocked to become members of the KNOS. Sir Walter Scott, member no. 83, was the first of many, many more; the author of this book is member no. 6927. In Scotland the situation was somewhat similar, and the SAS has had no problem including prehistory since Daniel Wilson first used the word in 1851 (and again, this author is a member). In Ireland the historical trajectory was the complete opposite, but it led to the same result: the establishment of the Royal Society of Antiquaries of Ireland very late in the day (1890) led to it becoming, in the absence of any major opposition from the Dublin ancient historical old guard, a truly national society encompassing all periods. But in England the complex nature of academic and institutional history meant that archaeology's back broke around 1880; and it has never mended. After token discussions of prehistoric archaeology and the Three Age System around that time, the Institute and the Association both withdrew from prehistory and retreated to the Roman and medieval periods, where to this day they very largely remain—resisting (so far) any attempts at reunification. The Antiquaries have to a great extent done the same thing. Prehistory in England has consequently remained institutionally more separate from Romano-medieval archaeology than it has in

Copenhagen, Edinburgh, or Dublin; and with institutional has gone academic separation. In England, prehistory has ever since had closer intellectual ties with anthropology (the name tactically adopted by the Ethnologicals when they re-absorbed the Anthropologicals in 1871) than it has with Roman or medieval archaeology. Arguably this has, however, had a beneficial effect; it is open to question how far the Childean archaeology of the 1920s and 1930s, the 'New Archaeology' of the 1960s and 1970s, or the 'Post-processual Archaeology' of the 1980s and 1990s would have developed without this anthropological connection. It was not until the internationalization of a provincial society, under the auspices of Grahame Clark, created the 'Prehistoric Society' in 1935 that English prehistorians even had a national institutional home.

The device around which the story of this book has been told is the journey undertaken by J. J. A. Worsaae in 1846–7; but over a century and a half later we may reasonably ask how influential he really was at this time. He later claimed to be the catalyst that introduced the Three Age System to Britain, and histories of archaeology have often reiterated this. But it emerges in these pages that neither his stratigraphic excavations nor his 'Viking raid' on Britain actually achieved very much in the short term. Contrary to the myth that has subsequently grown up around him, he hardly ever excavated multi-period sites, or used their stratigraphy to argue for the Three Age System. In London and Dublin his chronological arguments were pretty much ignored for the time being. In Edinburgh the Three Age System was fought through by Daniel Wilson, a man whom Worsaae never met, and who may have derived more stimulation from Worsaae's opponent Peter Andreas Munch. But Worsaae's importance in demolishing ancient history and creating modern archaeology in 1840s Scandinavia cannot be overestimated, though this contribution has largely remained largely unknown to Anglophone historians of archaeology. The solid base this created enabled Scandinavian archaeology to continue to provide new stimulation for English archaeology throughout the nineteenth century and beyond, quite a lot of it through the person of Worsaae himself.

The story of the adoption of the Three Age System is ultimately a human one, the story indeed of a rather small number of people scattered across northwestern Europe. Social forces no doubt played

their part, but as emphasized in chapter 1, the small number of players in any one place at any one time added a major stochastic element. Had J. J. A. Worsaae, or Thomas Wright, or Daniel Wilson, or George Petrie, or various others, behaved in a way different to the way they did, the effects might have been incalculable. May this warn us against making the assumption that the direction of scientific progress is either rational; or straightforward; or inevitable.

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Appendices

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Translation of Münter's Proposal of 1807

The following is the full text of Frederik Münter's proposal to the Danish Ministry of Home Affairs, in response to their request for his advice on the preservation of antiquities. It was written on 31 March 1807. It was published twice in Danish by Hermansen (1931: 299–302; 1949: 73–7) and is here translated from those sources.

While the injunction laid upon me by the Royal Danish Ministry of Home Affairs on 24 March, to present a proposal on how a commission might be established with the important purpose of preserving the antiquities that still survive from ancient times, is an honour and flatters me—as it must be for me, whose dearest pastime in the hours my official duties allow is the study of history and times past; so also am I fearful of fulfilling this injunction, because I am fully aware of the difficulties that would attend the execution of so broad a purpose, because so much would depend on the many individuals whose duty it would be to occupy themselves with these things, and who would not all be inspired by the same spirit, the same energy, and the same knowledge. But I nevertheless hope that the ideas which I hereby have the honour to present to the Ministry of Home Affairs will, on closer scrutiny, be found to be of sufficient use that they may be implemented, and, if they do not fully achieve the intended purpose, will at least serve to preserve something from destruction which would otherwise be irrevocably lost.

Before one can seriously consider the preservation of the remains from antiquity that are still to be found in the King's realms, it is necessary to know what they are. I include in these not just the so-called giants' graves, stone dolmens, stone emplacements, runestones, and what is found of stone and of precious and base metals in the graves; but also remains from the Middle Ages, such as inscribed and uninscribed gravestones, rune staves, armour and weapons, household equipment and jewellery, holy vessels from the churches, depictions of saints, coats of arms in wood and stone of the noble families, and finally documents of all kinds, all this up to the beginning of the sixteenth century. Some bits of information on these, especially the monuments of the first type, which had already attracted the interest of O. Worm¹ and which he discussed in his *Monumentis Danicis*, and also on the gravestones of the Middle

¹ Ole Worm, 1588–1654, leading archaeologist and the first to compile a systematic list of monuments.

Ages, can be found in the Danish Atlases of Resen, Pontoppidan, and Hofman, and there is also much Danish material in the drawings of Abildgaard² in the Royal Library; but this information is not always accurate or sufficiently complete; nor does it cover all classes of early remains, and much of what existed in the days of Worm, of Resen, and even of Pontoppidan, Hofman, and Abildgaard, is now destroyed or lost.

It therefore appears that the first thing must be the initiation of a (so far as possible) complete audit of national antiquities still surviving. To facilitate this, all knowledgeable men in both kingdoms³ and the Duchies⁴ must be called upon in such a way that it is made clear that their involvement in this purpose will earn His Majesty's approval. The clergy in particular must be asked to provide information, each for his own parish, about what is to be found there, and both the bishops and the deans must be encouraged to take an interest in the matter, to collect reports, and send them in. It is particularly desirable that a time be set by when the reports must be received, so that they may be made use of; this would at the very least demonstrate the government's earnest desire to see this interesting project brought to completion; and if it later becomes necessary, the time may be extended. But correspondence like these reports, and the perhaps frequent questions they will raise that need closer examination, and the comparisons with the earlier reports that they will certainly require, appear to make it necessary that there should be a central place into which everything comes, from which everything proceeds, and which finally can present to the Ministry the results of all the investigations set in train. Thus a commission, which consists of a few expert men in Copenhagen, and a few other corresponding members in Norway and the provinces, seems to be best suited to act as such a central place; and since it is presumed that only such men will be chosen for this work as have a true zeal for and love of historical and antiquarian studies, they will assuredly strive to further the project in so far as circumstances allow. This commission's work must thus consist of: first, to produce a kind of outline, according to which the clergy and other officials must submit their reports: next, to communicate with these people and their rural representatives in all those instances where more information is required: to receive the drawings or antiquities that have to be sent in: to make sure that reports have really been received from all deaneries and bishoprics: and finally, when everything has been collected, to extract the results from it all, and to produce suggestions on the following main points:

- I. which monuments from ancient times that are found scattered about the fields, and which due to their size and extent cannot be moved, should be preserved by Royal Decree for later times; and which, on the other hand,

² Seventeenth- and eighteenth-century archaeologists; Resen died in 1688, Pontoppidan's dates are 1698–1764, Hofman's are 1713–1793, and Abildgaard's are 1718–1791.

³ Denmark and Norway.

⁴ Schleswig, Holstein, and Lauenburg.

because they are of lesser importance, every landowner or land user may be allowed to make use of in any way they wish;

- II. what arrangements can be made for such remains from the Middle Ages as are found in churches or other public buildings, to preserve them from destruction;
- III. how the public can best be educated about the value of the antiquities that are dug up from the ground on a daily basis, and which are most often destroyed, just because people do not know that they might be of any use;
- IV. how at least cost to the state a national museum (which now, since the matter has been raised by Professor Nyerup, seems to have become a public wish) could be established for the antiquities found in all His Majesty's dominions, whether they are already in the Royal collections, or at a later date might become part of them;
- V. and how this museum could be of benefit to the public.

The expense involved in such a commission would, I suspect, be insignificant. Since the correspondence involves official business, the commission might hope for free postage; and the clerk that its secretary would require would easily be satisfied with a small annual salary. Nor would a few trips here and there, which might become desirable or necessary, involve any great expense, particularly if the commission's corresponding members in Norway and the provinces were carefully chosen, and were men who were interested in the project. If too great expense were incurred, the commission would only need to be allowed to apply to the supervisors of the Royal fund for public use.

I have thus, so far as I can see, fulfilled the Ministry's first injunction to me. I recognize with the greatest gratitude this proof of graciousness and confidence in me that this estimable Board has shown, by also asking me to suggest the men I would regard as the most suitable to join such a commission: but I hope that I may be forgiven by the Ministry if I do not venture to fulfill this suggestion. Those men both inside and outside Copenhagen who love and study history and the antiquarian sciences are not unknown, so such an informed and expert Board should know perfectly well who are the leaders among them; and what a mere scientist so seldom has the chance to observe, namely the suitability and interest in the project of other scientists, the members of the Ministry can pass judgement on better than anyone else. I allow myself to utter just one wish, that the Court High Steward, Chamberlain Hauch, himself a prominent scientist, and whose official position equips him so well for involvement in this matter, should together with one of the gentlemen of the Ministry's Board, be among the commissioners that the Ministry put forward for His Majesty's most gracious approval.

Translation of Thomsen's Letters of 1825 to J. G. G. Büsching

This appendix presents two letters written by C. J. Thomsen to the German archaeologist J. G. G. Büsching in 1825, in response to a book called *Abriss der deutschen Alterthumskunde* which Büsching had published in 1824. The letters were written in German, in which language they were published in 1930, and from which they were translated into Danish and published by Hermansen (1934:101–5). The translations below are made from Hermansen's Danish text. The letters themselves were apparently destroyed during World War II (Street-Jensen 1988: 28, n. 7).

19 February 1825

I have read this little book, which contains important material, several times; I find it contains much that is good, but also much which does not at all correspond with our experience in Scandinavia. Things seem to be simpler here, in that we can see very little with any Roman influence, and none with any Tartar. I would not be telling you the truth, and this is a hundred times more worthwhile than all compliments, if I told you I shared your opinion in general. To put artefacts into their proper context I consider it most important to pay attention to the chronological sequence, and I believe that the old idea of first stone, then copper, and finally iron, appears to be ever more firmly established as far as Scandinavia is concerned. If only you were here, I could clarify our opinions for you with hundreds and still more hundreds of examples; it will be more difficult for me to succeed by letter; but in a way I owe it to you and will try.

It appears clear to us, that in an early period the whole of the northern part of Europe: Scandinavia, most of Germany, France, and England, was occupied by very primitive peoples, who all resembled one another a great deal. It is certain that they resembled the savages of North America in many respects. They were warlike, lived in the forests, did not know—or only just—the use of metals, were divided into groups, and were in part injured, in part defeated, and in part forced out into other more marginal areas. To this first epoch, in which I regard it as foolish to think about towns, writing, or any manufacturing skills, we refer the graves in which we find stone objects not as symbols, but as actual weapons and tools. Since items are found in France, England, and northern Germany which correspond precisely with these, I am in no doubt that the same

circumstances applied there, the only difference being that it ended there at an earlier date. This is the reason why we find relatively far more antiquities from this epoch in Scandinavia than in the other countries mentioned... When did this period end? In Scandinavia our oldest legends give us a clue that we should not despise. For it is recorded that Odin, who came from the East with the Asas, settled in the countries that lay closest, in particular in Sweden and at Uppsala. He waged war, and drove out or conquered the earlier inhabitants; they are described as giants and dwarves, who lived in caves. Probably the Finns, who were forced into northern Norway, and most of the serfs, were also part of them. It is also said that Odin brought with him the knowledge of metals, and knew how to cast spells, especially with the runes he had invented. Since we now assume that Odin and his hoard came to Scandinavia around the time of the birth of Christ, I think it is likely that the older situation and type disappeared in the course of a couple of centuries, and that we can begin the second epoch around AD 200. The first epoch must have ended much earlier particularly in France.

How long the second epoch lasted, and how it differed from the first and the third, are inevitable questions that force themselves upon us. I believe that the northern German artefacts give us a fortunate indication and information. With my own eyes I have convinced myself that weapons and tools of this period are completely and absolutely the same as those found here in Scandinavia. What characterizes them is shape and decoration, the latter very precise and in its own particular style, I call them circle- or wave-motifs, like the various ones I sketch here.¹ In this period we know neither iron nor silver in Scandinavia, or only very little and just in the transitional period. Everything is made of bronze or gold. Dominant among the types are: swords without a guard, celts, the so-called palstaves, stone items used symbolically and made of material far too soft and decorative—and also often too small—to have served as weapons or tools. At the height of this period metalworking had already been taken to a considerable level of skill, and both the shapes and the decorations were fundamentally more aesthetic than in the subsequent period, definitely not so cluttered as later. According to our earliest sources there was writing at this time, but only very rarely and as a means of doing magic, and used by a few priests. They knew how to build ships, but according to the depictions we have of them on rock surfaces and various items, they were fairly long and low rowing boats. In Germany this period ended under the twin influences of attacks from the East by the Slavs, and Frankish dominance from the West. I do not think this really got going before the fifth or sixth centuries, and this style thus ended first in Germany and maybe one or two centuries later here in Scandinavia. In Scandinavia this was the so-called 'snake style'. I have not found among the heathen artefacts of Germany one single piece of it, common as it is with us. As representatives of this style I will mention to you the cast of an animal head which I sent you long ago (which

¹ Hermansen states that Thomsen drew spirals, concentric circles, and wave motifs in his letter at this point.

probably came from the upper part of a standard), the gold bracteates with snakes, and—if you were here—hundreds of others. In this period skillfully braided and woven objects also appeared, steel and iron became common, also silver, the first replaced bronze, swords were equipped with guards, writing was adopted, runes were understood by every educated person, large ships were expertly built which were capable of carrying a large number of people, and major voyages were already undertaken, and people were in many ways well above those of North America; I will mention just the three major advances: 1. the skilful working of metals, 2. the script, 3. the building of large ships. The snake style belongs to this last period and also continues over into Christian times, as we can see very clearly through many antiquities which we can date, and which are all executed in this style, e.g. the remains of valuables in the grave of the still half-pagan Queen Thyra, most runestones, the earliest decoration of buildings etc. etc. From the last category it appears to me that traces of this style are also found in Germany, England, and France. I will not argue that they came from the North to the South, but something which resembles the Scandinavian is certainly there. Maybe all of it originally derives from one source, only with the difference that in Scandinavia, where there were still pagans, one finds such decorated items in graves, while in the more southerly countries it only appears on Christian objects, for example column capitals, decorations on doorways, tenth- and eleventh-century manuscripts etc. etc.

The foregoing unfortunately insufficiently worked out views will, albeit vaguely, have shown you how our collection of objects from the heathen period are divided into three great epochs or periods, which also have their own subdivisions. Since we have observed many times that objects that cross between periods are rare, while the periods themselves are clear to see, far fewer difficult questions arise than one would think. For example, the graves reveal that iron was used earlier and more commonly in Norway than it was here. Several things suggest to me that iron was more costly than bronze in the transitional period. We have found it sparingly used at this time, for example axes of bronze with edges of steel, and arrowheads of bronze where iron swords occur. This last is not yet certain, but if it is confirmed it will support my opinion.

But, honoured sir, you have surely long been tired by reading these often interrupted and therefore imperceptible developments. Come and see—I hope that I will be more easily understood.

1 March 1825:

I have just now finished reading a very well-written review in the *Göttingische Gelehrten Anzeigen* for 1 May last year of a book which I must lay hands on immediately. I had already heard of it but not seen it yet. It is a response to a prize offered in Holland about the so-called ‘giants’ graves’ by Nicolaus Westendorp, of which there are two earlier editions, the most recent from 1822, but not here yet!! To my not inconsiderable surprise, I see that this learned man shares my views with regard to the earliest antiquities. The learned reviewer expresses it in such a way that I can see clearly, yes very clearly, that I must fear

that you believe I have copied from this gentleman. Although my observations, which have led to the same conclusion, are not so well worked out and provided with academic citations, and are in general only fragmentary and friendly communications to you in private, I cannot immediately tell you that I agree with it. I always get the *Göttingische Gelehrten Anzeigen* through a periodical-reading club, but we have now established a new institute here in order to be more up to date. The reviewer does not seem completely to support the idea and is himself a researcher in linguistics, which appears to be the weakest part of Westendorp's book, and is more inclined towards those who do not ascribe primitiveness to the oldest antiquities. Through Westendorp's assurances it has been incorrectly posited that the pots in the giants' graves are more decorative than later ones. I do not agree, and everything I have from graves of this period, when stone was still used for real weapons, is—at least so far as I know it—primitive, decorated to be sure now and again with a few stokes, or with rims more or less accurately made by hand, but there is no question of the potter's wheel or of Mr. Westendorp's types, and it is certain that he has seen more than was really there.—Even in my second period ceramic manufacture in Scandinavia was only of medium quality, but it seems that the better off used urns of bronze, even of gold. And I have often found wheel-made urns finished with a glaze, with objects of the third period; and I believe that glass urns also belong to this period. I cannot agree with Mr Westendorp when he assigns the giants' graves only to the first period, and I think that the determination that stone objects were really used as weapons is better. Nor shall I defend his 'Celtic people', but rather his 'Celtdom'. One may definitely take things a long way in France and England, but it is certainly unnecessary to go as far as this gentleman has. As far as Scandinavia is concerned, the beginning of the Roman period about the time of the birth of Christ is sufficient. I can also without any difficulty point to three or four antiquities of this kind for each one that can be mentioned in the southerly countries.—The clearest evidence for and against Skule Thorlacius is found in our museum, which at the time he wrote only existed on paper. It is clear that stone items were first used as weapons from the fact that we have found many broken ones than have subsequently been repolished. This would not have happened with symbolic items. Furthermore, we have also found an instance of an ox with a wedge driven halfway into its skull. It is just as clear that stone objects were used in later times as symbols or for holy worship, because the items are far too small and even made of amber, which could not possibly serve for weapons or tools. The stone knives in large measure belong to this class; they are all polished and are often found together with other polished items. . . . Please excuse my hasty and illegible writing. I would not have you think that I was a plagiarist, the similarity between my views and those in the reviewed book could really give that impression.

Translation of Eschricht's (1837) Paper on Crania

This is a full translation of: Eschricht, D. F. (1837), 'Om hovedskallerne og beenradene i vore gamle gravhøie', *Dansk Folkeblad*, 3, 28–9 (15 September): 109–16. His paper was accompanied by two illustrations, reproduced in fig. 3.3. Eschricht's mention of *forhistorisk*, 'prehistoric', occurs in the fifth paragraph. Eschricht misinterpreted the date of the single skull he believed to derive from the Bronze Age. He mentioned the original publication of the site by Magnussen (1827). Magnussen stated that the skeleton was accompanied by objects of both silver and glass; the skeleton must thus date from the Iron Age, not the Bronze Age, as Eschricht would have realized had he read the relevant parts of Thomsen (1836). Thomsen's quoted communication about the find does not make any mention of this; presumably he did not realize the use Eschricht was going to make of the skeleton, or that its date had been misunderstood. The measurements Eschricht gives for the femur and tibia of the 'Bronze Age' skeleton are impossible; no man with a femur as long as $20\frac{3}{4}$ inches (54.3 cm) could possibly have a tibia as short as $11\frac{1}{2}$ inches (30.1 cm). In the original publication, Magnussen (1827:154) gives the lengths as 21 inches and $16\frac{1}{2}$ inches respectively, which suggests that Eschricht's length for the tibia is a misprint, and should have read $16\frac{1}{2}$ inches.

On the Skulls and Skeletons in our Ancient Burial Mounds

There can hardly be any Danes unaware of the *kæmpehøie* or *jettehøie*¹ that are so common in this country, or that they are ancient burial mounds. The quantity in which they still exist, despite countless having been razed over the years, immediately shows that they do not derive from a short period of time, and history correctly states that the custom of raising a mound over the dead was nearly universal in Scandinavia right up to the introduction of Christianity.

As a result of the different periods from which these burial mounds derive, their internal organization and contents also differ. For a very long time the corpses were burnt, and only the burnt bones deposited in clay vessels. It is, however, beyond any doubt that this practice was neither the earliest, nor was it ever universal; since we often find in the same grave both skeletons, and burnt

¹ Both terms mean 'giants' graves'.

bones in vessels. It also seems that in each of the different periods to which the burial mounds belong, that it was the custom to inter along with the remains of the deceased his weapons, tools, jewellery, and also various items of religious significance; but in the later parts of the heathen period all these objects were of bronze (copper with added tin), sometimes of gold, more rarely of silver or iron; in the earliest period in contrast the jewellery was mainly of amber, the weapons and tools of stone, more rarely of bone, never of metal.

Since it must be regarded as overwhelmingly likely that the Germanic or Gothic tribes, who already over ten centuries ago entered Sweden, Norway, and Denmark, and who are ancestral to the modern Swedes, Norwegians, and Danes, already knew the use of iron, it follows from this that the oldest burial mounds are in large measure at least a couple of millennia old, and date from a period which is older not only than the history of our country, but furthermore is older than all the legends and myths that have been preserved until our day. It is therefore evident that they are the remains of people who occupied Denmark before the Danes.

What people was this? The oldest legends mention giants, trolls, alfs, banes etc. as the hereditary enemies of the Asas (i.e. the Goths), and it is most probable that these terms refer to the country's earlier inhabitants, and that by one or more of these names must be understood that earlier group whose untiring efforts made up for their lack of metal.

It is natural that we could wish to know these former inhabitants of the country from a different perspective than through the inimical descriptions that remain to us in the legends of the conquerors. It would be particularly interesting to find out what relationship they had to the peoples that lived in neighbouring countries at the same time, and since history here gives us almost no guidance, it is mainly from the burial mounds themselves that we must obtain our information. In fact the detailed examination of these mounds that in the most recent times has been undertaken here in Denmark, as well as the thorough comparison between our burial places and those of the adjacent countries, has already produced important results. As a result, interest in the matter has already become widespread among the people, and if everyone would do their bit, knowledge of the country's circumstances in that prehistoric time may yet reach a much higher level of certainty than would seem possible at first glance. The intention of this article is to draw attention to a source of evidence which has so far been almost completely ignored, despite that fact that it might become absolutely the most productive, if only it were used with sufficient care.

We want to know which people built these age-old burial mounds. We can get information about this from the arrangement and contents of the mounds; but would it not be just as important if we could get a clear picture of the size, shape, hair- and skin-colour of our oldest countrymen? It is true that one cannot always tell to which nation a person belongs just by looking at him; but here we are not asking about nations as closely related as e.g. the present Swedes, Danes, or English, or even so close as any of these with the French or the Italians, although one would probably admit that the differences between these nations can be

recognized in almost all individual people. We are asking here whether these oldest inhabitants were *Goths* or *Germans*, or whether they were *Celts* or *Huns*, or perhaps even *Lapps* or *Finns*—peoples who even now are so different from each other, and who at that time, before there was any peaceful concourse between them, must have been even more so.

It will thus definitely be conceded that a reliable picture of the appearance and entire physical make-up these inhabitants, dead and buried for millennia, would be an important contribution to the earliest history of our country. But it has now been shown that, just as one can form quite a good impression of the way of life of these people by examining what remains of their tools and weapons, so one can also form a much better impression of their appearance *by closely examining what remains of their bodies that died millennia ago*. If I am successful in demonstrating this in what follows, then it must surely also be admitted that we have hitherto been greatly mistaken in paying so little attention to the skulls and skeletons found in the burial mounds.

Of all the items found in the burial mounds, probably none have been less carefully treated than these. From the many mounds that have already been opened, and in which sometimes twenty skeletons have been found in a single mound, there is so far not one complete skeleton in any of our national collections. It is of course true that the excavation of skeletons often requires a great deal of care, particularly if the mound consists not of sand but of humus, and sometimes only the head can still be moved. But the reason probably mainly lies in the fact that nobody thought that these human bones could have any importance.

The following circumstances provided the present opportunity to undertake these investigations of this kind. Mr Hage, a merchant from Stege, arranged in the summer of 1836 for the excavation of two burial mounds in the vicinity of the town, one larger and one smaller, lying close together. The internal organization and contents, which were virtually identical in the two, showed that they belonged to the mounds of the earliest period. Access was gained to a narrow passage via a tight opening on the southern side of each mound, and from there into a space in the middle of the mound, the funerary chamber itself. Both the passages and the funerary chamber were built of large unshaped boulders. In the small mound the latter was 14 alen [8.8 m] long, 3½ alen [2.2 m] wide, and 2½ alen [1.6 m] high, but a number of the covering stones were blown up and taken away some years ago by the farmers, as a result of which the chamber had largely collapsed. No skeletons were found inside, but there were many stone weapons, pots or funerary vessels of clay, and also a large number of pieces of amber jewellery.

In the large mound the passage was 10 alen [6.3 m] long, and its entrance very low, made of boulders. The innermost space or funerary chamber was 16 alen [10 m] long, 4½ alen [2.8 m] wide and 2½ alen [1.6 m] high. The walls were constructed of large rectangular stones, the spaces between them carefully filled with flags of split sandstone. The weapons, tools and jewellery found in this mound were exactly the same as in the other, with the addition of a small tool

made of bone, the purpose of which is not easy to discern. It comprises a square-sectioned bone rod about 2 inches [5 cm] in length and 2 lines [4 mm] across, on one end of which is a bone ring of the same thickness. But what is more important to us is that *several*, perhaps 9 or 10, *human skeletons* were found inside. Unfortunately several accidents befell these during excavation. In part they were disturbed when some of the covering stones collapsed, and in part, which one would not have expected, they became objects of greed for ignorant people. When the excavation was finished, only three heads were initially removed, and the rest of the bones allowed to remain until the next day. But that night some bone collectors found their way into the grave chamber; and Mr Hage, under whose auspices the excavation had taken place, next morning was outraged to recognize remains from the funerary chamber among a group of bones that were brought to him for sale. He arranged for them to be retrieved, but inevitably this could only be done in a very imperfect manner. We must thus restrict ourselves to the three crania preserved to begin with, so far as their shape and appearance allow the drawing of wider conclusions.

All 3 *crania* are of adults, since their wisdom teeth have already erupted. One head, of which is presented a drawing at a somewhat reduced scale [*fig. 3.3 top*], was of a man about 26 years of age, with a particularly powerful musculature. This can be seen from the wear on the teeth, which is e.g. still minor on the wisdom teeth, and from the unusually pronounced irregularities at those places on the face wherever the person's muscles were. Such irregularities show unmistakably the very powerful movements of these muscles over a number of years. On the other two crania these irregularities are less developed, and the facial form less characteristic, but on the other hand the crania were relatively spacious and more spherical. These undoubtedly come from younger individuals, perhaps females. But despite these crania being of adults, they are all remarkably small; since measurement revealed that their circumference was about 16 inches [39.5 cm]. Heads so small are rarely encountered among modern Danes. Although it is really just the face that can be described as small. The cranium is relatively spacious, particularly when one considers its remarkably round shape. These individual features, which will not escape anyone who just compares the drawing shown here with other skulls, allow us to derive very important conclusions.

Among all the peoples of the globe three variants are particularly evident, so that three human races are especially clearly discernible; they are the *Caucasian*, the *Mongol*, and the *Ethiopic* (the Negro race). Among their distinguishing points those that involve the shape of the cranium are among the clearest; among Caucasians this approaches a spherical shape; among Negroes it is longer and narrower; and among Mongols it is more broad and flat. The face is also more long drawn out among Negroes, with a projecting mouth; among Mongols it is low but very broad, while among Caucasians it is smaller in proportion to the cranium than in the other races, so that neither the mouth nor the sides of the face project.

It is already clear from this that the three crania at the very least belonged to neither Negroes nor Mongols. There was in any case no reason at all to suspect the former, but the latter could have been considered possible. Because Lapps and Finns, peoples related to the Mongols, did occupy neighbouring Sweden and Norway; because in particular the Huns, a pure Mongol people, pushed forward toward Scandinavia along the southern shore of the Baltic Sea, it was entirely possible that an individual horde of them could have reached the Danish islands, even though history does not specifically say so. A few archaeologists have even suspected that *Eskimo* tribes could have been the original inhabitants of the country. But if these suspicions have any basis, they can at any rate not apply to the people who built the mounds described here; since the heads described here have none of the characteristics that typify the crania of Eskimos, Lapps or Finns, to say nothing of the more typical Mongolian tribes.

All these criteria would have gained a great deal if we had also been able to undertake investigations of the rest of the body, e.g. the size of the hands and the feet in relation to the rest of the body; since among the Mongols and the Eskimos they are remarkably small, and in general the size of the various parts can be much better judged when one places them alongside the other parts of the skeleton. But despite the fact that we must restrict ourselves to just these three heads, we can still go much further in our conclusions.

In these heads the characteristics of the *Caucasian* race are not just clearly present, they are for the most part extraordinarily prominent. The cranium, although in and of itself not large, is thus large in proportion to the face, and the *facial angle* (or the angle formed on the maxilla between two lines, one of which extends downwards from the most prominent part of the forehead, the other forward from the auditory canal) comes quite close to being a right angle (it is 80°); particularly in the two heads in which the facial muscles were less developed. Nor is the cranium strongly developed in any direction, neither in length, breadth or height, but is strikingly close to being a sphere. In this respect we conclude that these three crania most resemble those peoples *whom history suggests are in psychological terms the best favoured*; since the crania of the ancient Greeks were also distinguished not by their size, but rather by their harmonious development in all directions, or by their spherical form; this is also universally accepted as the most beautiful and the noblest. In some respects these heads seem to bear some resemblance to those of other tribes of the Caucasian race. Of all the crania with which I have had the opportunity to compare them, the similarity, especially with regard to the upward projection of the nasal bone, is greatest in those of 2 Hindus (people of the Indian nation), which Dr Cantor has sent to the University's anatomical museum from Calcutta. Only this can be said with certainty, that *these heads belonged to individuals of a noble tribe of the Caucasian race*.

We will attempt to go yet another step forward. We will try to find out what the appearance of the people from whom these heads were taken actually was. Just as the facial region is very small, it is also certain that *the entire body was not above middle height*. Measurement of the rest of the bones found in the same

grave appears to demonstrate that the ten skeletons present for the most part belonged to people *who were neither above nor below middle height*. That this investigation is not completely decisive is due to the fact that the bones of all ten skeletons became mixed up, and many went missing, so that it has not been possible to reassemble one single complete skeleton from them.

It has been shown that the facial bones are remarkably small: *thus the face itself would have been very small*. In contrast all the insertions of the facial muscles are very marked: *the facial features would thus have been very strong in life*. The orbits are very small, low and deep set under the brow ridges: *their eyes too were thus small and deep set with strongly projecting brows*. The nasal bone has a striking upward projection, so that a deep narrow pit is formed between them and the brow ridges: *they therefore had* (not a flat snub nose like Mongols and Finnlapps) *but in contrast a strongly curved hook nose*. The small face with its lively features, the small eyes placed deeply under the eyebrows, and the large hooked nose are characters that together suggest a *dark colour of skin, eyes, and hair*.

Just as observations of the skeleton allow the drawing of a number of conclusions about the appearance of the body during life, so also they permit a few conclusions to be drawn about *the people's way of life*. The incisor teeth are sharp, not completely worn down as they are e.g. among the Greenlanders and Eskimos. This shows that these our oldest countrymen *did not use their teeth or nourish themselves in the manner of the arctic peoples*, a conclusion which a well-known natural scientist has believed could be put forward based on an examination of the types of stone tool. The teeth in all three heads are very worn. None has any caries, but most are quite well covered by calculus.

The head illustrated here [*fig. 3.3 bottom*], which is the one on which the muscle insertions are most characteristic, and which evidently belonged to a man in the prime of life, has received a very powerful blow on the right temple. It *appears* to have happened during life, and to have been caused by a narrow edged weapon similar in shape to a chisel, which is also roughly like the similar stone weapons that are very common in the graves. However, it is not possible to say with complete certainty that the injury did not occur during or after the excavation.

But—all these conclusions are drawn from three heads found in one and the same mound. Could it not be possible that these burial mounds were family graves (*ancestors' graves* rather than *giants' graves*), and that these individuals were closely related, so that the characteristics put forward here might be representative of them alone and not the entire people?

We have already seen that the crania are not enough, but that for a complete investigation we require all the bones, and that they be treated as carefully as possible; we see now that it is not enough to have one, or two, or three skeletons, at least when they all come from the same place, but that in contrast one can basically never have too many.

When my investigations had proceeded thus far, I had to go to the museums in which the skeletons from the burial mounds are housed, in particular to the National Museum, which has become so important for the earliest history of our country. It was in fact there that I found the greatest quantity of material,

although nothing in proportion to the other riches of this museum. Every excavation has proceeded more or less like the excavation described here on Møn. It has been very rare that one has gone to the inconvenience of preserving the skeletons. At most, the crania have been placed on one side, and even this rarely with the requisite attention. The museum has no complete skeletons from funerary mounds, but only skulls, and these only from two mounds; that is, some from a mound at Udby Mark near Stege, thus unfortunately from the same locality from which these 3 heads come, and some from the so-called Maglehøj, near Hellested, in Zealand. The first mound was 100 alen [63 m] in circumference and 5 alen [3.1 m] high; the narrow passage extended to the East. In the central stone chamber were found twenty human skeletons, and the cranium and some bones of a dog; and besides a large number of antiquities of stone traces of decayed pieces of amber, but not the slightest trace of metal. The so-called Maglehøj also had a narrow passage extending to the East. The contents were like the other one (no traces of metal). Only three crania and some bones were found, which were '*regarded as human bones*', thus not examined by anyone with any relevant knowledge. Beside these crania I have so far seen only two others from burial mounds. One is in the private collection of the conservator, Mr Ibsen, and was found in a mound in Jutland; the other is in the University's anatomical museum, but all that is known about it is that it was found in a burial mound, with no information as to the location of the find or of who donated it to the museum.

However inadequate these materials might be said to be, they are, however, not completely insufficient for the present provisional investigation. In all of them the characteristics noted above are so clear that none of them could be confused with a head of another people. This goes in particular for the small face, the foreshortened rear of the head, the round cranium, or more correctly for these three characteristics taken together. In contrast the nasal bone is in none of them so strongly upwardly projecting as in those from the mound on Møn excavated in 1836, and it must in general be admitted that these three heads are in fact the ones in which all the characteristic criteria are the most strongly marked.

From this it appears possible to conclude with certainty that these mounds containing stone objects and skeletons *were all erected by one and the same people*, whose characteristics were described above; but *that each mound most likely only contains a single family*. The latter conclusion is also supported by the fact that, among the heads from the mounds near Stege, there was one of a child aged 8 (tooth replacement was just starting), and some seem to have belonged to females (the bones which most clearly reveal the sex have never been preserved). The head preserved in the university museum is particularly interesting because *there is still some hair upon it, and this is dark brown*, which makes the assumption that those people had a dark skin and hair colour almost certain, since it is most unlikely that this darker colour should be a result of its age.

The direction this study should now take is plain to see. I have compared the heads with those of Greenlanders, Finnlapps, and Kalmuks, with the Slavs, and

with a large part of the other Caucasian peoples; but I have so far unfortunately been unable to compare them with the heads of true Celts; nor with heads from similar burial mounds abroad, and only then, but with certainty, will it be determined whether it was one and the same people that occupied Scandinavia, England, northern Germany, and France during the Stone-Age; and furthermore whether these people were Celts or not. The investigation is thus still only half finished; but it was in any case not the purpose of this communication to present the results of a completed study. The intention was more to show how much remains to be done, and how much more could still be found out, if only the topic attains a wider interest, and if everyone deals carefully with whatever remains from ancient times chance may place in his hands.

It is in fact possible that the investigation is far further from completion than now appears. It has been demonstrated that these ancient people lived here in the country before *the true Danes*; but it does not follow from this that their period immediately preceded that of the latter. While we can be sure that our ancestors, *the Goths*, already knew the use of iron when they came into Scandinavia, so is it—as simply a consideration of the National Museum will show—also clearly demonstrated, that between the Stone Age and the immigrations of the Goths was a long period which has for a long time been referred to by archaeologists as the *Copper* or *Bronze Age*.

This raises the question: was the people that lived here in Denmark during this Copper Age the same as the one that lived here during the Stone Age, or was it a different one? Many circumstances seem to argue for the latter; particularly the great difference in the types of tools and weapons, and above all perhaps the very different mode of burial. But here we will only discuss what can be learned in this respect by comparing *the skeletons* from these different time periods, the more so because this method of investigation, by answering this question, must be regarded as especially reliable, provided that the materials for the comparison are not far too few. Unfortunately it is true that what I have so far had the opportunity to examine from the Copper Age amounts to just one single cranium with the associated femur and tibia, and it is true that it is impossible to derive conclusions of general value from single observation. But this cranium, illustrated here at a somewhat reduced scale [*fig. 3.3 bottom*], is so strikingly different in shape from those in the stone chambers, from those of modern Danes, and indeed from the crania of all known peoples, that the presumption that a Scandinavian people belonging to the Copper Age, fundamentally different from that of the Stone Age and also from our own ancestors the Goths, already becomes very probable.

The find from which this cranium comes is described in *Antiquariske Annaler* 4, p. 152 [*Magnusen 1827*]. In the summer of 1821 another skeleton was excavated from a site where two had previously already been found, close to a gravel quarry outside the garden of Sanderupgaard on Fyn. It was surrounded by several items, some of precious metals, some of brass. Next to the feet, which lay to the NE, was a fairly large metal cauldron; by the head, which lay to the SW, was a smaller one; round one of the fingers were two spiralliform gold rings, and

a silver buckle apparently lay near the chest. As well as the much-damaged skull, the National Museum has one femur and one tibia of this skeleton, which clearly belongs to a much later part of the heathen period than the stone chambers. If the depiction of this cranium is compared with that shown earlier, even someone with no expertise at all will easily be able to see its peculiarities.

The crania of the stone chambers are distinguished by their spherical shape and their equal development in all directions, so that they are nearly as high as they are long from front to back; this one is in contrast elongated, flattened, and compressed, and the height is almost only half as much as the length. In the former crania the forehead was high and the occipital especially short; in this one the forehead is very low, and the occipital so unusually broad and long, that any anatomist that only had the rearmost part of the occipital bone before him might doubt whether it actually came from a human skeleton. In the former ones there were strong traces of the effects of the muscles, as there are on this one; but in the former they derived particularly from the muscles that determine the facial features, while here they are especially from the chewing muscles, and even more from the neck muscles, for the attachment of which the occipital has a protruding bone ridge; I have never seen one equal to this, or seen it described in any human cranium. This skeleton is also rather remarkable with regard to size. The femur is $20\frac{3}{4}$ inches [*54.3 cm*] in length, the tibia $11\frac{1}{2}$ inches [*30.1 cm*]. This results, following the usual relations between the bones of a person, an overall height of 75 inches [*196 cm*], a height which is rarely attained by modern Danes, and seems to have been just as uncommon among most of the peoples known from ancient times.

It is possible that chance has meant that an unusually tall man has become the only representative of his time period, and that his skeleton has been preserved until our day with definite indications of the time period; but we are also forced to consider the possibility that such physical height and such an unusual skull shape might have been quite common at the time. Which of these opinions is the correct one is something that it is most important for the history of our country to find out; and in order to find it out, we have at the moment no other means than to recommend that everyone into whose hands come by chance the skeletons and crania from that period should take all the care of them that the importance of the matter appears to require.

Chancery Councillor *Thomsen* adds in his written communication to me about this head: 'I have myself visited the findspot, which ought definitely to be more closely examined, since to all appearances a kind of cemetery from the heathen period has been encountered. The reason it was necessary to postpone the investigation is that the graves are not in a field, but go in under a road. I have not, however, abandoned the hope that it will be possible to investigate the cemetery, and that this site will produce more antiquities and skeletons.'

May this hope soon be fulfilled, and produce fruitful results!

Prichard's Discussion of the Three Age System in 1841

This appendix presents an extended caption of three drawings of skulls presented by Prichard (1841b: xvii–xxii). It is believed to be the first mention of the Three Age System to have found its way into the English language (Morse 1999, 2005). Plate 3 and the description was apparently a late addition to Prichard's volume; some of the text is a partial translation of Eschricht (1837—see appendix 3 above for a full translation). Eschricht's skull appears alone in Prichard (1841b: facing p. 204), while the Finn and the Lapp described by Prichard under fig. 2 appear facing p. 304 of the same work. Prichard did not obtain these other skulls from Eschricht, nor do they appear in one plate as shown in fig. 4.6 of this book until 1843 (Prichard 1843: facing p. 206)—both points contra Morse (2005: caption to plate 9). In the following, the sections Prichard quotes from the translation of Eschricht are italicized, to distinguish them from Prichard's own comments.

PLATE 2. Figs. 1, 2, represent skulls of the two principal varieties of the Iotun or Great Finnish race. Fig. 1, is a Lapponic skull from Blumenbach's decades. Fig. 2, is a skull of an Esthonian Finn, from the work of Dr. Alexander Hueck, published at Dorpat in 1838.

These skulls are described and their differences pointed out in a section of this volume, relating to the physical characters of the Finnish nations.

PLATE 3. Contains a drawing taken from the cast of a skull in the Museum of the Royal College of Surgeons. The cast was presented to the College by Professor Eschricht of Copenhagen, together with a learned and interesting memoir on the sepulchral remains of ancient races in Denmark and the neighbouring countries, published in the '*Danske [sic] Folkeblad*.' The cast is that of a cranium discovered in a barrow in the isle of Moen, which appears from Professor Eschricht's account to be a good specimen of a great number of skulls found in similar situations. The memoir which accompanied it gives much curious information on the subject of the sepulchral remains dispersed over the north of Europe. The comparison of these remains with the numerous relics of a like description spread through the British isles [*sic*], and with the contents of innumerable tumuli existing in the north of Russia, and particularly along the banks of the great rivers of Siberia, may hereafter throw an important light on the ancient

history and ethnography of all these regions. Professor Eschricht's memoir communicates some interesting facts, which may suggest the topics of future inquiry.¹

Over many parts of Denmark are scattered earthen mounds, which are termed in the country 'Jettehoie' or Giants' tombs. They are regarded as relics of the olden time. Their vast number proves, says the author, that they were not raised during one age, and history records that the custom of erecting mounds over the dead prevailed in the north of Europe for many centuries previous to the introduction of the Christian religion. That these monuments belonged to different ages is further evinced, by the difference of their structure, and of the relics of ancient art which have been discovered in them. For a long period of time it seems to have been customary to deposit in these graves burnt bodies, or merely collections of burnt bones in earthen vessels: but this was not the oldest custom, nor was it universally prevalent: we sometimes find bones in earthen vessels in the same graves with entire skeletons. With the dead it was usual to bury various articles, such as his weapons, working tools, ornaments, and some religious tokens, probably amulets, fetisses or talismans. In the later pagan times such things were of bronze, sometimes of gold, seldom of silver or iron: in the more ancient times the ornaments were generally of amber, and the weapons and implements of stone or bone; seldom, perhaps never, of metal. This circumstance furnishes the ground for distinguishing the sepulchral remains of the northern land as belonging to different chronological eras.

Now, as we are obliged to admit that iron was known to the nations of Gothic or German race, who inhabited Sweden, Denmark, and Norway, from an early age, and who were the ancestors of the present Swedes and Danes, we must refer the existence of the earliest class of these remains to a period ending two thousand years ago, and reaching back not only beyond authentic historical memorials, but even beyond the earliest traditions. It is evident that they belonged to a people older than the Danes. Who were this people? The early traditions speak of giants, elves, the hereditary enemies of the Goths, and it is highly probable that under these names were designated that ancient race whose indefatigable industry supplied the want of metal. As history gives little information, a research into the contents of the sepulchral mounds themselves seems to be the only resource for elucidating this question.

Though many of these graves have been opened, and in some not fewer than twenty skeletons have been discovered, there is yet not one entire skeleton in any museum in Denmark.

In the summer of 1836, M. Hage of Stege, in the isle of Moen, ordered two mounds to be opened, which were situated close together, near Byen: the style and contents of these barrows prove that they belonged to the oldest period of similar remains. An opening in the southern end of each mound affords an entrance to

¹ A footnote at this point reads: I am indebted for the opportunity of consulting this memoir to the kindness of Professor Owen. The drawing has been taken by the permission of the Council of the Royal College of Surgeons, in whose Museum the cast is deposited.

a narrow passage, which leads into a chamber in the centre of the mound; the passages, as well as the chamber, are formed by means of rough stones of a flat shape. The sepulchral chambers are fourteen or sixteen ells long, between four and five broad, and two ells and a half high. From this account it would appear that these oldest 'Jettehoie' or 'Gravhoie,' in the Danish islands, bear a close resemblance to our long sepulchral barrows in Britain. Some of them contain, as it seems, ten or even twenty human skeletons. Three skulls were procured by Professor Eschricht from one of the tumuli above mentioned. They are described and figured in the memoir, and the cast sent to the Museum was taken from one of them. Professor Eschricht afterwards compared these skulls, and the relics of art found in the same barrows, with several extensive collections of similar remains in the Danish museums, particularly with the contents of sepulchral mounds near Hellested in Sjælland. The result seems to be, that the shape of the skulls is very similar in all the tombs which belong to the first age, or that of stone implements. In these tumuli there are numerous ornaments of amber, weapons of stone and of bone, but no relics that indicate the knowledge of metals among the people who deposited them. These tumuli are very numerous, and extensively spread, showing that the tribe to which they belonged were for ages the sole inhabitants of the northern countries. In a series of barrows different from those described ornaments, such as rings of gold, sometimes of copper or of bronze, make their appearance; and these belong evidently to a much later period of Paganism. A third age succeeds, which is that of iron instruments and weapons: the people whose relics are found in these last are supposed to have been the ancestors of the Danes, namely, of the Lutic, Gothic or Germano-Scandinavian race.

We still want more precise information, as to the osteological character of the skeletons found in these different series of tumuli, and the memoir contains no account of these which belong to the two latest periods. On the remains found in tumuli of the earliest class some interesting remarks are to be found in Professor Eschricht's Memoir, but these are scarcely sufficient to satisfy all doubts as to the important ethnological question, to what people they belonged. The author supposes they were 'a Caucasian race.' He draws this inference from the spherical form of the head and its considerable developement [*sic*], and from the shape of the nasal bones, which, as he says, are arched, indicating a prominent or aquiline nose. On the other hand, he mentions characters which belong to the Finnish nations rather than to Indo-Europeans. He says that the orbits of the eyes were small and deeply set under the eye-brows, so that the eye must have been deeply set with strong prominent eye-brows: there is a considerable depression of the nasal bones between the orbits; these are characteristics of the Finnish race, as may be seen in a preceding page of this volume, which contains an account of the osteology of an Esthonian Finn. A still stronger feature of resemblance to some of the Lappish, Finnish, and many kindred races, is the lateral projection of the zygoma, giving to the skull much of that pyramidal form, which is so remarkable a feature of the Turanian nations. This will be perceived by the reader, on inspecting the annexed engraving, which was taken from the cast, though it is

not perceptible in the profile or in the front view—neither of them affording aspects of the skull which are satisfactory—given in the ‘Danske [*sic*] Folkeblad.’ It would be rash to conclude from these characters that the skulls in question belonged to a Finnish people, though that race is known, as we have seen, to have approached in ancient times the borders of Denmark. We might rather look upon the Cimbric or Celtic inhabitants of Northern Europe, as does Professor Eschricht, as the erectors and occupants of these ancient tombs. Some remains found in Britain give reason to suspect, that the Celtic inhabitants of this country had in early times something of the Mongolian or Turanian form of the head. However this may have been, we recognize in both countries remains belonging to two successive periods; I mean these of the stone and of the copper age, in the phraseology adopted by Professor Eschricht.²

The comparison of the sepulchral remains found in Denmark, and spread in great abundance through some parts of Holland, and over Sweden and Norway, with those of our own country, would open a field of most interesting research. It is evident, from the preceding observations, that the ‘Jettehoie,’ or oldest sepulchral mounds of Denmark, are very similar in construction, and contain relics of a similar kind, with the greater part of our long barrows, and perhaps with most of the old sepulchral mounds spread through the south of England, and in various parts of Wales and Ireland. In most of the mounds examined by the late Sir R. C. Hoare, the remains of ancient art were similar to those above described: they belonged to a people in a corresponding state of society, probably to the same people. Implements and weapons of stone belong to each: only amber is not found, as far as I know, in British barrows, that material having been abundant only near the Baltic; ornaments of bone seem to have held the place of amber. Only in a few barrows, according to Sir R. C. Hoare, are ornaments of gold found—weapons of brass and golden rings have been more frequently seen in Ireland. These relics of copper or brazen ornaments are evidently of a later date than that long series of ages which raised the great majority of the numerous mounds and barrows which are spread both in the British isles and in the northern regions of Europe, but all the barrows, where implements of iron are still entirely wanting, probably belonged to a period anterior to the entrance of the German nations. It is on the whole probable that they were raised by Celtic tribes, of which the Cimbri were the last remains on the northern continent. For the Celts were long ignorant of the use of iron, if we may draw an inference from the British barrows. It is true that the Britons used iron in Caesar’s time for some purposes, namely, iron rings for money, and probably the scythes of chariots were of iron,—for what else could be used, unless it were brass? But the use of iron may have been confined to the Belgae in South Britain, who introduced it from Gaul. It must have been unknown during many ages to the

² A footnote at this point reads: The three heads described are very small: though they appear to have belonged to adults, the circumference measures only about sixteen inches. Heads so small, as the author observes, are seldom seen among the modern Danes. This however may be an individual rather than a national character.

Britons, as we have inferred from the contents of the barrows, which were the old British sepulchres.

It is much to be regretted that there is no national collection of the sepulchral remains of our ancestors. Ample resources yet exist for enriching such a collection, were it but commenced; but these resources are diminishing every day. Great numbers of skeletons have been found, and the bones scattered, within my knowledge, during the last few years. In Ireland the Royal Academy have set a laudable example in the care directed to such pursuits, and much may be expected from the enlightened zeal and activity of Dr Wilde and other members. How much might the Society of Antiquaries have effected, if their attention had been directed to these researches!

Translation of Worsaae's Letter of 1847 on Irish Politics

This presents a translation of Worsaae's letter from Ireland to Jonas Collin, giving his assessment of the current political situation in Ireland. The Danish text is published by Clément (1930: 15–18).

Dublin, 28 January 1847

Most esteemed Councillor of State!

On my return to Edinburgh from the Scottish Highlands I had the pleasure of receiving the news that His Majesty the King had most graciously allowed me the increased travel support that I had sought, and that the money had, due to the assistance of Your Excellency, already been paid to my brother. I cannot sufficiently assure the Councillor of State of my extreme gratitude for the assistance you have always extended to me. I am fully aware that I have received a sum which is by our standards considerable, for the trip on which I have now been away for nearly eight months, and I do not think that anyone would have been able to support themselves in England for so long, and travel about so much as I have, had I not been equipped with extraordinarily good references.

I have now been in Ireland for over two and a half months, and I have truly had a most interesting stay. I have found a remarkably large number of antiquities, both from the ancient Celtic period and from the time when the Norwegians and the Danes had large possessions here.

In Ireland, as in Scotland, all monuments of unknown origin are ascribed to the Danes. It is almost flattering for a Dane to hear how triumphantly the 'Young Irish' describe the Battle of Clontarf, where Brian Boru beat the Danes. It was also on the field at Clontarf that O'Connell a few years ago wanted to hold the great popular gathering or 'monster meeting', which was forbidden by the government. It was his intention to have proposed that, just as in his day Brian Boru had expelled the Danes, so now should they also expel the English.

The way the Battle of Clontarf has been brought into political questions and movements has naturally had the consequence that the historical truth has become diluted, and that the Danish forays to Ireland in general are now mostly viewed in a completely wrong light. I took it upon myself to discuss this in some lectures I gave to the Royal Irish Academy, and particularly due to this I have been fortunate to open the way for a closer collaboration between Irish and Scandinavian historians and archaeologists, which will definitely lead

to much new information coming to light. The ancient Irish annals and manuscripts contain a great deal of information about the Scandinavians' wars in Ireland.

I have made as many excursions here on the east coast of Ireland as circumstances allow. But it is of course in no way a good time for trips into the country. The famine is spreading wider and wider, and is accompanied by typhus which has already killed many. A short while ago a couple of hundred starving peasants began looting bakers' shops on the edges of Dublin; other than that we have not seen much of the famine here. The only thing is that the streets are filled with an astonishingly large number of beggars and ragged folk.

I do not need to tell you, Councillor of State, how exceedingly interesting it is for a foreigner to visit such a faction-ridden country in such stirring times, the more so because so far the country has, due to its distant location, been very little known. I used to think, as most people at home think, that Daniel O'Connell, or as he is jokingly called here, 'King Dan', was the leading figure among the liberals, and that it was he who worked in particular to raise the people to freedom and independence. But I must confess that I now have a completely different opinion. It cannot be denied that O'Connell has a quite remarkable degree of power in Ireland, he appoints God knows how many members of parliament and controls a great number of the high civic appointments about the country. But he only has this power because of the ignorance of the people and the influence of the Catholic priests; he is the mouthpiece of the Jesuits and the rest of the Catholic priesthood. He is so far from being well disposed towards the new ideas that have spread throughout Europe since the French Revolution, that he is in fact the representative of a party that is struggling against the rapid and victorious spread of civilization. Even in the middle of the famine he calls for Repeal, in order to extract the last pennies from the unfortunate people! Repeal, Repeal, is the solution to everything; it will make Ireland so fertile that it will be 'able to feed the whole of Europe', it will restore trade and all at once make Ireland flow with milk and honey. The easily swayed, one might almost say fanciful Irish believed this for a long time, until O'Connell recently started to arrange remunerative government positions for his sons, family and friends. Many formerly loyal supporters of 'Dan and Old Ireland' then began to mistrust him, and founded an opposition party, called 'Young Ireland', whose leader, typically enough, is a Protestant. They are Repealers to their very core, but not, like O'Connell, 'loyal' repealers. Their ideas are still very wild and unformulated, but they seem to have the great advantage over O'Connell, that their intentions are honest.

O'Connell's great and immortal service is actually that he pushed through the Catholic Emancipation Act (1829), as a result of which the Irish have given him the title of 'Liberator'. England will now truly have to pay a dear reckoning for the huge injustice they in earlier times inflicted on the Catholic Irish. If the Protestants had previously treated the Catholics better, and tried to influence them through a beneficial strategy of education, there would not be, as there are now, nearly 7–8 million Catholics and only 2–2½ million Protestants in a

population of 10 million. Even in modern times, when the excellent national schools were established (1832) in which children of all denominations were to be educated together, the Protestant clergy immediately tried to take them over completely, and when they did not succeed, they opposed the establishment of the schools in every way they could. Their religious prejudices prevented them from seeing that enlightenment sooner or later would spell the end of the Catholic religion in Ireland. The Irishman has such a lively and alert nature that he needs only a little education in order to throw off the worthless monkish yoke. The national schools will undoubtedly create a better future for Ireland, but it is inevitable that it will be a long time before the powerful, one might well say fanatical, religious and political strife that has so far borne down and depressed Ireland, will cease.

I was sorry that I only received the letter from the Rural Economy Society after my arrival here in Dublin. I will with great pleasure gather as much information as it is in my power to collect. In a few days I intend to cross to England, where I will probably stay for a couple of months. After that it is my earnest desire, if possible, to travel home via Paris. With many greetings to Mr Collin KC, I remain, Councillor of State, your respectful and grateful—J. J. A. Worsaae.

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