

# Archaeology as fact and fiction

*Mats P. Malmer's archaeological writings 1948–2002*

MATS P. MALMER

EDITED AND COMMENTED BY STIG WELINDER



KUNGL. VITTERHETS HISTORIE OCH ANTIKVITETS AKADEMIEN  
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Mats P. Malmer (1921–2007) published many essential books and articles on prehistoric archaeology beginning in 1948. This book offers a selection of texts, most of which have not been translated into English before. Malmer's work deals among other things with Mesolithic artefacts, Neolithic culture groups, Bronze Age barrows and rock carvings, and decorated Iron Age artefacts such as gold bracteates. First and foremost he published on methodology: how to do archaeological research in an objective way using well-defined terms and concepts. He constructed a system of typological categories ascending from attributes of individual objects up to cultures in parallel with the New Archeology, and quantitative methods such as production diagrams displaying the chorology and chronology of artefact types. The main line in his work is the study of innovation, from his introduction to archaeology of the pleion concept in 1957 to his final book on the Neolithic in 2002.

Mats P. Malmer was head of the Stone Age and Bronze Age Department at the Swedish History Museum in Stockholm from 1959 to 1968. He held the professor's chair at Lund University 1968–1973 and at Stockholm University 1973–1988. He was married to Brita Malmer, professor of numismatics.

### *Keywords*

Archaeological method, archaeological theory, Battle Axe Culture, typology, chorology, innovation, material research, production diagram

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# Preface

WHEN MATS P. MALMER presented his doctoral dissertation *Jungneolithische Studien* in Lund in 1962, he initiated a period of intense debate about the archaeological research process. He emphasized that progress in archaeology, in the form of new horizons of understanding, is the result of both new finds and new ideas. In a series of acutely formulated articles he tackled fundamental problems, articles which led to fruitful discussions. But Mats P. Malmer was unusual in being a brilliant practitioner and a theoretical archaeologist combined in one and the same person. He did not like to write purely methodological or theoretical texts; instead he developed his new perspectives through empirical investigations.

In the 1990s Mats P. Malmer had plans to publish a collection of essays, revised versions of some of his most important articles. He never had the opportunity to assemble the collection, which was to have been entitled *Archaeology as Fact and Fiction*. We hope that Mats P. Malmer's perpetual discussions will continue.

*Anders Andrén   Evert Baudou   Kristian Kristiansen*  
*Lars Larsson   David Wilson*



## Editor's note

THE SELECTION OF articles, book chapters and manuscripts by Mats P. Malmer in this volume was made by his wife Brita Malmer, she too a professor, her subject being numismatics. Unfortunately, Brita Malmer died in 2013, before she had completed the editing of the collection. Her selection of literature is based on a draft on which Mats P. Malmer worked in the 1990s. He intended it to contain a comprehensive view of cultural changes in the Nordic region and a discussion problematizing methods and ideas in archaeology. Compared with Brita Malmer's selection, the present Section IV on fieldwork contains fewer titles. The couple's daughter Elin Malmer has kindly provided the photographs of Mats P. Malmer.

The texts are presented here unchanged, and accompanied as far as possible by the original illustrations. Any significant departures from this principle are stated in the comments on each section or in footnotes in the various chapters. Texts originally published in Swedish and German have been translated into English by Alan Crozier (Ch. 2–4, 6, 8–9, 12, 15–18). Texts originally published in English were translated into this language or revised by Alan Crozier (Ch. 10), Keith Bradfield (Ch. 1), Patrick Hort (Ch. 1), Eva & Simon Wilson (Ch. 11) and Laura Wrang (Ch. 5, 7). I have found no record of who translated Ch. 13–14. The introductory essay and the introduction to each section were written by myself and translated by Alan Crozier. Finally, Martin Rundkvist copy-edited the book.

We are profoundly grateful to all the publishers and journals that have kindly permitted the reissue of the texts. References to the original publications are assembled in Section VII of this book.

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# Introduction

## Mats P. Malmer's archaeological thinking in its contemporary context

*By Stig Welinder*

MATS P. MALMER aimed to express his archaeology in clear, unambiguous, logically coherent propositions. He used empirical data as a foundation for quantitative presentations and tables, diagrams, and maps. At the same time, he would cross the boundary into the realm of hypothesis. The type definitions which he formulated in verbally and logically stringent terms were hypotheses which were to be tested and reformulated. For Malmer, the accepted artefact types, defined through the occurrence or absence of essential typological elements, corresponded to prehistoric situations and events. This meant that the train of thought proceeded from organized data to interpretation. The span of Malmer's archaeology extends from uncompromising stringency in presentation, the description and analysis of empirical data, to boundless yet controlled imagination – “fact and fiction” as in the title of this book. The words are Malmer's own, from the working title of a book in which he intended to sum up his life's work, but which he never had the opportunity to finish.

By the word “fiction”, however, Malmer meant something objectionable. Hypotheses were not to be formulated on the basis of imagination, much less fantasy, ideology, or political views. They were to be grounded in testable facts: “A minor fact is worth more than a great fiction” (Ch. 7).

Malmer's strict demands on archaeological language were virtually ridiculed in the appeals and replies in a case concerning the qualifications for a professorship in the 1960s. His antagonist, Bertil Almgren, who was appointed to the chair in Uppsala, declared in almost scoffing terms that Malmer had written that potsherds with a particular decoration made up 50% of the total amount of potsherds. In the fine print one could read that the total number of potsherds was two. Malmer replied that it is actually true that one potsherd out of two potsherds is 50%: “A minor fact.” Swedish archaeologists stood on the sidelines looking on in wonder and amusement. Quantitative methods did not become common in Swedish archaeology until some years later, towards the end of the 1970s.

It is not evident in his works from the years around 1960, when Malmer laid

the foundation for his archaeology, that he had engaged in any profound study of philosophy, the history of ideas, or the theory of science. No such works, whether original texts or surveys, are discussed or referred to. At the University of Lund, however, he had evidently found an intellectual environment which was influenced by the analytical philosophy shaped by the Cambridge school with George Edward Moore, Bertrand Russell and Ludwig Wittgenstein, or rather by the Viennese positivism, also known as logical positivism, developed by Moritz Schlick, Rudolf Carnap, Carl Hempel, Karl Popper and others.

In informal conversations Malmer himself could mention that he read Torsten Hägerstrand and attended his seminars. Hägerstrand became professor of human geography at Lund University in 1957. His main interest at the time was studies of innovation. Later he devoted himself to questions of population, urbanization and that which he is perhaps best known for today, time geography.

Torsten Hägerstrand as a role model is obvious in the article from 1957 that stands out to posterity as the first genuinely Malmerian work. It appeared after eight years of conventional articles based on excavations and finds, dealing with Bronze Age barrows, unique Mesolithic artefacts, and so on (Section IV of this book). This work on the concept of the *pleion* (Ch. 8) can be described as Malmer's breakthrough. This was something that no one had done before in Nordic archaeology. Note that archaeology in Sweden around 1960 chiefly had a Nordic orientation. The circle of colleagues came from Denmark, Norway, and Finland. The university subject was called "Nordic and Comparative Archaeology". The comparative aspect meant that the chronology and cultural contacts of the Nordic countries were studied with reference to continental archaeology. Questions of this kind were the obvious starting point for Malmer's archaeology in the 1940s and 50s.

In the article about the *pleion*, the central question is where and how novelties were introduced to Skåne during the Neolithic. Malmer took the terms "innovation process" and "pleion" from Torsten Hägerstrand (1952, 1953). He also found the cartographic method – isarithm maps – in human geography. On the other hand, neither Hägerstrand nor any other human geographer was quoted in Malmer's interpretive text; he himself would have written "hypothesis-testing".

The way of doing archaeology in the article about the *pleion* was completely new to Nordic archaeology in the 1950s. This does not apply to the questions, nor to the ideas on which the answers were based. It was the stringent chorological methods that had never been seen before. "Chorology" was yet another term borrowed by Malmer from human geography. It is found in the title of Torsten Hägerstrand's 1953 doctoral dissertation. I myself got into archaeology a decade after the publication of Malmer's article. By then it did not appear re-

markable or controversial. This was after *Jungneolithische Studien* (Malmer 1962). A brief, sober summary can be found in *Swedish Archaeological Bibliography 1954–1959* (Cederschiöld 1965:15).

*Jungneolithische Studien* (Ch. 9 in the present book is a translated extract of an abridged version in Swedish from 1975) was required reading for students of archaeology in Lund in the 1960s. So it was for other archaeologists too, or at least it should have been. How many actually managed to read the 959 pages in German is a different matter. The dissertation is nevertheless written with a detailed table of contents, copious figures, tables, and summaries, all of which make it easier to follow the main line of reasoning with limited effort. There is a five-page summary in *Swedish Archaeological Bibliography 1960–1965* (Christiansson 1968:41–45), which begins with the words: “One of the most extensive archaeological works so far published [...]”

Malmer began working on his doctoral dissertation in the spirit of Nordic and Comparative Archaeology, with the aim of exploring the continental links of the Boat Axe Culture. After the publication of the dissertation it became known as the Swedish-Norwegian Battle Axe Culture. Malmer did not find that boat axes looked like boats. But he changed the view of the Boat Axe Culture and the way we do archaeology in more profound ways than that. *Jungneolithische Studien* includes a history of research which is an astute critical survey, a theoretical presentation of newly created terminology and methodology, and a thoroughly considered empirical example. Malmer found that he could not accomplish his study of the continental links of the Boat Axe Culture without describing his source material in a reproducible way. He had to be able to compare pots and battle axes with each other from all corners of Europe. Earlier archaeology had done this on the basis of unclearly defined types, inadequate descriptions, imprecise statistical data, and blurred photographs. Malmer started anew from the beginning. *Jungneolithische Studien* is a powerful construct of terminology and method, from typological elements and unambiguous type definitions to production diagrams displaying the chorology and chronology of the Battle Axe Culture (Ch. 9).

*Jungneolithische Studien* contains hypothesis-testing archaeology, following the lines of positivist science, in a way similar to the New Archeology that was elaborated in the 1960s at American and British universities. Its defining texts were Binford 1962 and Binford & Binford 1968. Malmer’s work also shows obvious similarities to parts of David L. Clarke’s mammoth 1968 book *Analytical Archaeology*. There were also remarkable differences, for example, in the fact that Malmer did not use systems theory or make any explicit references to social anthropology (Stig Sørensen 1999:784–786).

David L. Clarke wrote his 1970 dissertation on beaker cultures closely allied with the ones Malmer studied, in his case in the British Isles. Whereas Malmer believed that he was able to demonstrate that the Battle Axe Culture had arisen in the southern part of the Scandinavian peninsula, it was David L. Clarke's opinion, if I may exaggerate a little, that every new type of beaker in the British Isles was the result of a small new immigration from the Continent. Malmer, of course, asserted that the immigration hypothesis in his case had been falsified by stronger arguments than those seeking to verify it. These strong arguments indicated a verification of the hypothesis of local, autochthonous development. His view had such a swift and sweeping impact on Nordic archaeology that it provoked surprise and raised eyebrows when David L. Clarke presented his opinions during a lecture tour of Scandinavia.

Perhaps people might have pointed to the University of Lund as the birth-place of the New Archeology instead of Chicago and Cambridge if Malmer had not operated in a hopelessly small archaeological environment of no interest to English-speaking archaeologists. It was one where, if one wanted to be international, the language of publication was German. This choice of language for Malmer's dissertation was related to his desire to have full control over the contents. He did not believe that he could have this with a text in English. David L. Clarke included *Jungneolithische Studien* in his list of references in *Analytical Archaeology*, but all this amounts to is a summary of a quantitative description with a visually striking diagram of hollow-edged axes (1970:155–158). The almost identical structure of the analytical study of artefacts was something that David L. Clarke, consciously or unconsciously, said nothing about. It has been pointed out, chiefly by Leo S. Klejn (1977), that Malmer arrived independently at a number of the ideas put forward by the Anglo-Saxon New Archeology.

It should be pointed out, however, that Malmer did not link his archaeology to the ongoing debate about theory and method in English-language archaeology. A quick look at the reference list in *Jungneolithische Studien* shows his orientation. From the last two decades before 1960, the references show the distribution of the literature that was read in traditional Nordic and Comparative Archaeology:

Country	Number of titles
Sweden	88
Denmark	35
Germany (BRD & DDR)	31
Finland	10
Norway	7



The discussion of the relationship of the Battle Axe Culture to the continental Corded Ware Cultures and to the Beaker Cultures in Western Europe and the British Isles, however, required a study of literature from across a wide geographical area and in many languages, for which purpose Malmer had undertaken a grand tour of Europe so that he himself could register the relevant source material on the basis of his own principles:

Country	Number of titles
Britain	21
Czechoslovakia	8
Spain	4

... and up to three titles each from Austria, Belgium, France, Ireland, Italy, the Netherlands, Poland, Portugal, Switzerland and the USA.

This kind of archaeology did not interest the English-speaking world. The converse was true as well. Nordic archaeology did not follow the discussion that took place in the USA before 1960 along the path that led to the New Archeology of the 1960s (e.g. Griffin 1935; Rouse 1939; Taylor 1948; Ford 1949; Phillips & Willey 1953; Spaulding 1960). This early discussion was only introduced to Nordic archaeology by Berta Stjernquist after a study trip in the USA in the 1980s (1984). Later in the 1990s some of this could also be found in Malmer's writings (Ch. 4). Already in the book from 1963 (Ch. 1), however, Malmer referred to American archaeologists, although that was primarily to criticize them for lack of stringency.

There is thus a great deal to suggest that Malmer constructed his archaeology in parallel to and independently of the New Archeology. The theoretical foundation in logical positivism was in any case the same. Malmer became familiar with its demands on the practice of science through human geography, which was on its way to becoming the New Geography at the same time as the New Archeology was emerging. Yet Malmer was not working entirely alone. He himself pointed to the slightly older Carl-Axel Moberg as an interesting person. Moberg, however, left Lund in 1947, became a museum director ten years later and finally professor in Gothenburg. In the 1960s and 1970s he was one of the chief advocates of the New Archeology in Scandinavia, along with e.g. Knut Odner, Klavs Randsborg and Klas-Göran Selinge.

It may be discussed what Mats P. Malmer's wife since 1949, Brita Malmer, *née* Alenstam, meant for the shaping of Malmerian archaeology. Brita Malmer received her doctorate in 1966 on the strength of a dissertation about minting by Olof Skötkonung in Sigtuna in the AD 990s, when a kind of Swedish kingdom had only just been established (B. Malmer 1966). In its research strategy and in

countless details, her analysis of the small silver coins is the most consistent work accomplished in the wake of *Jungneolithische Studien*, using the same methods. Terminology, typological method, tables and diagrams are all strikingly similar. Even the layout of the dissertation is similar, as are the plates, the writing style, and the formulations. It goes without saying that her husband's works can be found in the bibliography, and he is frequently named in the text as the originator of the concepts and the methods used (B. Malmer 1966:248):

As regards methodology, *Nordiska mynt före år 1000* is an application to a new subject field of the methods worked out by Mats P. Malmer in *Jungneolithische Studien* and in *Metodproblem inom järnålderns konsthistoria*.

In the 1960s it was still a part of scholarly ethics to cite one's predecessors and never to claim to have originated something that someone else had thought and written before, even if one had thought the same thing independently of those who had gone before. In the latter case one simply had to swallow the bitter pill and cite. Otherwise one would have to endure public criticism during the defence of the dissertation. That is why Brita Malmer wrote what she did, but it is difficult to imagine anything but that much of what appears in *Jungneolithische Studien* had arisen in conversations around the Malmers' kitchen table.

One might expect Mats P. Malmer to have developed the empirical part of his archaeology – he himself sometimes called it *sakforskning* or “research on things” – towards electronic data processing. His unambiguous qualitative and quantitative registration of typological elements was the first step in any meaningful computer-based statistical analysis of large amounts of artefacts, and it was relatively simple statistical analyses of often large artefact groups or groups of antiquities that were relevant in the mid 1960s. In 1966 Malmer was one of a dozen Scandinavians who travelled to the first major international multidisciplinary symposium about mathematical and computer-based methods in the humanities. It took place in Rome and was dominated by the Americans and the French. Among the latter, Malmer was particularly impressed by Jean-Claude Gardin (1967).

On arriving home Malmer presented what he had learnt from the lectures and discussions to a Swedish-speaking audience (M.P. Malmer 1967a). He observed that computers had no other function than to save human labour, and that this would be made easier if archaeologists registered their source materials adequately from the beginning, by which he meant, implicitly, in roughly the way he himself had introduced to Nordic archaeology with *Jungneolithische Studien*. His own lecture presented his work with hollow-edged axes. A few years

later he published the lecture in a specialist journal devoted to electronic data processing in archaeology (M.P. Malmer 1969b). Malmer's vision of the future here is that mathematicians must develop suitable methods for archaeology, but that it is up to humanities scholars, to archaeologists, to learn how to apply them. This also happened a decade or so later with the advent of multivariate statistical methods. By then, however, Malmer was no longer onboard. He was evidently satisfied with the intellectual foundation which he had helped to establish. He was not at all involved in the huge computer revolution that came yet another decade later.

*Jungneolithische Studien* attracted attention in Nordic and continental archaeology, more than in English-language archaeology. There were numerous reviews. References can be found in *Swedish Archaeological Bibliography 1960–1965*. A review in English appeared in *Antiquity* (Thomas 1964). The reviewer, Stanley E. Thomas, was closely acquainted with Malmer after their time together in Lund, and offered a detailed discussion of Malmer's perspectives on "invasion" and "internal development". Otherwise the review is relatively short, summarizing the contents and praising the whole work: "This is an impressive and courageous study deserving the most serious consideration of all practitioners of archaeology" (p. 236).

A much more exhaustive review was published by Carl Fredrik Meinander, professor at Helsinki University, who served as opponent at the defence of the dissertation. The review fills seven pages in its printed Swedish-language version (Meinander 1965), to which Malmer replied in the same issue of the journal (M.P. Malmer 1965). Meinander emphasized the merits of the dissertation at the beginning and the end of his review (1965:75, 81):

The main emphasis in Malmer lies [...] in a meticulously careful analysis of the find material from graves and settlement sites, and in the case of battle axes also stray finds. For this he introduces statistical methods and viewpoints that are wholly new in part, which will undoubtedly be an interesting topic of debate for many decades to come.

...

The most valuable part of the dissertation are the extensive typological analyses, the exemplary stringency, and a mode of presentation that never leaves the reader in doubt about the author's views. It may seem mistaken to judge it by the theses set up by the author, but in the end there is no other possible way to assess the usefulness of new methods than to test the results obtained with these methods.

Malmer's "statistical methods and viewpoints" were not in fact discussed "for many decades to come." They have simply been accepted and copied. Meinander, however, was doubtful about some of the theses and results, and about

the design and application of one of the methods: production diagrams (Ch. 9). He thought they were based on too many assumptions, the validity of which were unknown, and on too small samples of pottery and battle axes. He perceived them as excellent illustrations of complicated reasoning, but they could not be called scientific in any other sense than, say, economic or meteorological forecasts.

Meinander devoted the major part of his review to the oldest finds of the Battle Axe Culture in Scandinavia and to the interpretation of them as an innovation. He delved deeply into Malmer's reasoning based on existing empirical evidence of the occurrence of the oldest axes in megalithic tombs, the derivation of work axes from other axes located in time and space, the significance of a key find of a problematic axe, the occurrence of the oldest pottery in different parts of Scandinavia, and so on. Meinander's own conclusion – based particularly on the origin of Swedish artefact types in the Corded Ware Cultures of the Finnish and Baltic areas – was that Malmer's (hypo-)thesis about a rise of the Battle Axe Culture in Scandinavia, especially in Skåne-Blekinge-Halland, was not compelling. He himself suggested (1965:81):

[...] that the immigrant contingent of the population was comparatively small and quickly assimilated. They merely had the role of a ferment. [...] It could presumably be correct as Malmer says, that the process of innovation of the Battle Axe Culture in Sweden and Norway denotes the spread of a new religion and a new social system, but with the addition that these novelties were introduced by a new people.

Malmer's reply begins with his personal history as a background to why *Jungneolithische Studien* was written the way it was. The state of research in the 1950s had given him no choice. He was forced to create new concepts and methods for archaeology to make it a worthwhile science. Production diagrams were thus a method through which one could not avoid clearly stating one's sources of error (Malmer 1965:89): "It seems extremely unfair that I should be attacked for stating, more clearly than my predecessors, what the sources of error are."

He also pointed out with some irony that the analyses in Meinander's *Die Bronzezeit in Finland* were based on less than a tenth of the number of battle axes in one of his own production diagrams. Malmer wrote that in his opinion, weather forecasts, production diagrams, and *Die Bronzezeit in Finland* were all science.

Malmer was a contentious figure. He did not let criticism pass without comment or reply. Many of his articles from the 1960s and 1970s are intended to repeat and clarify the ideas in *Jungneolithische Studien*. Despite his express opi-

nion that archaeology involves formulating, testing, and reformulating hypotheses, it is difficult to see that Malmer ever abandoned any of the hypotheses in *Jungneolithische Studien*. When Christopher Tilley, after a period as visiting researcher in Lund, wrote his analysis of the Battle Axe Culture (1982) and suggested a new chronology, Malmer condemned it as “unnecessary destruction”. Tilley incidentally also showed that twenty years after their original publication, Malmer’s catalogues and tables could be used for cluster analysis, principal component analysis and spatial analysis.

The gender studies that emerged at Swedish universities in the 1970s gained a strong foothold in archaeology in the 1980s. Malmer’s compilation of graves and grave finds in *Jungneolithische Studien* then became a frequently used source of data for studies – not least by students – of gender, that is, women’s and men’s relations and roles in prehistoric society. His dissertation was a rewarding basis for discussion, since Malmer had argued that people in the Battle Axe Culture buried their dead according to conventional norms, for instance with different rituals for women and men. He believed that he could also identify the gender of graves without skeletons according to how they were arranged and according to their content of gender-characteristic sets of artefacts. In his own terminology, these would be called find-association types. Helena Knutsson found in her 1992 seminar paper that only a few of the total number of graves perfectly followed the gender criteria established by Malmer. Nor did the few osteological gender identifications follow these criteria clearly. Most of the graves had to be left without gender identification. Knutsson’s paper is a good example of how *Jungneolithische Studien* serves as a foundation for constant new discussions. Malmer did not, to my knowledge, comment on the essay in print. Nor did he publish any opinion about gender archaeology in general. He presumably thought that its hypotheses were too far removed from what could be verified with empirical data.

*Jungneolithische Studien* was read and is still read at Swedish universities, although the proportion of doctoral dissertations that include the book in their bibliographies has fallen from 40–80% in the 1960s and 1970s to 10–20% since 1995 (tab. 1). If we include the abridged edition in Swedish from 1975 (Ch. 9), originally intended as an undergraduate textbook, and the study of methodological problems in Iron Age art history, *Metodproblem inom järnålderns konsthistoria* (1963), whose theoretical and methodological contents are the same (Ch. 1), the percentages are higher (tab. 1). Malmer is a scholar who is read and considered in Swedish archaeology, although less today than a few decades ago.

Tab. 1. Percentage of Swedish doctoral dissertations in archaeology which include works by Mats P. Malmer in their bibliographies.

	Jungneolithische Studien (1962)	Metodproblem inom järnålderns konsthistoria (1963)	Stridsvåpenskap i Sverige och Norge (1975)	Others (1948–2002)	No. of doctoral dissertations in Sweden
2010–13	12%	0	4%	24%	25
2005–09	10%	6%	3%	24%	87
2000–04	18%	13%	18%	38%	84
1995–99	18%	20%	10%	41%	49
1990–94	27%	27%	11%	38%	37
1985–89	36%	27%	15%	42%	33
1980–84	29%	29%	18%	36%	28
1976–79	43%	43%	14%	50%	14
1971–74	47%	40%		33%	15
1964–68	80%	20%		20%	5

So far I have placed Malmer in his contemporary intellectual setting in the human sciences. He was shaped as a researcher in a generally positivist post-war era at Lund University. More directly, the ideas and methods came from human geography at Torsten Hägerstrand’s seminar, which was on its way to becoming the New Geography, and which transformed the older human geography roughly in the same way as New Archeology reacted against and reshaped the earlier archaeology that is usually labelled “culture-historical archaeology”.

Malmer, however, was also part of the tradition of Nordic and Comparative Archaeology. His starting points for *Jungneolithische Studien* were the chronology and contacts of the Battle Axe Culture, which coincided with the main objective of Nordic archaeology ever since it became a modern science during the nineteenth century. He stood in the mainstream of the Nordic archaeological tradition and acknowledged this himself without reservation (Stig Sørensen 1999:784 f). Although Malmer would informally call Oscar Montelius “Sweden’s stupidest archaeologist”, he undeniably asked the same questions as Montelius had in the nineteenth century (Baudou 2012:235–250), and his archaeological method endeavoured to give Montelius’s Swedish typology an unambiguously reproducible form in terminology and logic. The typological method – ordering artefacts in

types and arranging the types in series – was for both Montelius and Malmer an essential part of the archaeological research process (M.P. Malmer 1995). Neither of them thought that the dating of find combinations – the ordering of closed finds in series – was a superior chronological method.

Malmer would also occasionally emphasize Montelius's aspirations to explore how people lived in prehistory, just as he asserted that it was his own goal as well (M.P. Malmer 1994:25 f). The young generation of New Archeologists sometimes expressed disdain for both Montelius and Malmer because their material studies were seen as an end in themselves. Malmer declared emphatically that there was no other way to knowledge about prehistoric people and societies. Malmer's own idol among the early archaeologists was Christian Jürgensen Thomsen, who saw and formulated, in the language of his time, the necessity of studying artefacts, and only them, in order to arrive at knowledge about prehistoric times. He was a sound archaeologist, with sound common sense (M.P. Malmer 1989; Ch. 4; Sørensen 1999:786–787).

The year after *Jungneolithische Studien*, 1963, saw the publication of *Metodproblem inom järnålderns konsthistoria* ("Methodological problems in Iron Age art history"). This contains an English summary of the kind of archaeology that Malmer had presented in his doctoral dissertation (Ch. 1). He had chosen German for the dissertation, since he felt able to say exactly what he wanted to say in that language. Around 1960, however, English was taking over the role of German as the leading foreign language in Scandinavian humanities, and therefore the new book was provided with a lengthy English summary, especially for American archaeologists who could not read German but worked in ways similar to that of Malmer (Baudou & Jansson 2015:74 f).

As empirical examples of his method, in this book he chose artistically decorated artefacts from the Iron Age: Roman Period fibulae, Vendel Period interlace decoration, rune stones, and above all the gold bracteates of the Migration Period. It was not out of deliberate malice, but mere circumstance, that it was to a large extent archaeologists from the Uppsala department who were the targets of Malmer's critique. Since the start of the twentieth century archaeologists in Uppsala, and to some extent in Stockholm, had done more work on the art history of the Iron Age than archaeologists in Lund. Malmer criticized Oscar Almgren's type definitions of brooches, Bernhard Salin's definitions of art styles, Bertil Almgren's curvature styles, Hans Christiansson's definitions of South Scandinavian art styles, and above all a century and a half of bracteate studies.

The chapter about gold bracteates is the longest one in the book. It starts anew with a clean slate by defining bracteate types with a flexible system for non-hierarchical types and subtypes for both the central image and the border,



based on mutually independent typological elements. This is followed by a chorological and chronological survey of the bracteates along the same lines as with pots and battle axes in *Jungneolithische Studien*. As in that book, the final results regarding the distribution of bracteates in time and space are presented as production diagrams for different regions from the south to the north. This made it possible to clarify a course of diffusion for ideas about what bracteates should look like.

The way that Nordic archaeology worked in the 1960s was that archaeologists were placed in specialized compartments from a rather young age. It was also accepted that archaeologists had priority of access to archaeological source material, not just the kind that they had excavated themselves, which could be kept at home under the bed or in locked cabinets at museums, but also to groups of source material which a person considered they were working with or planned to study. Archaeologists identified with their source material. That was what determined one's existence as an archaeologist. Through his early works and *Jungneolithische Studien*, Malmer had become classified as a Stone Age and Bronze Age archaeologist. He was also head of the Stone Age and Bronze Age department at the Swedish History Museum in Stockholm. His book about Iron Age art history thus dealt with the wrong period and with artefact classes about which other archaeologists felt that they were the obvious authorities, not Malmer. They were the ones who knew anything. Egil Bakka, associate professor at the University of Bergen, launched the counterattack. The forum for the debate was the *Norwegian Archaeological Review*, an English-language journal that had been started in the mid-1960s, with the ambition of cultivating the New Archeology and being a forum for debate.

Bakka's critique was extensive – thirty-one pages, plus his own typological system filling a further five pages – and thorough (Bakka 1968a, b). He was undeniably an eminent expert in the design and art history of the Migration Period. Bakka began by praising Malmer's archaeological thinking (Bakka 1968a:6): "This is a work of profound interest for anyone wishing to learn something about the theoretical basis of archaeology and to everyone taking scientific archaeological research seriously."

Reading the review, one finds that Bakka and Malmer had different perceptions of the demands that should be made on scientific archaeology. The latter was more uncompromisingly rigid; the former permitted and recommended a greater measure of personal experience and the artistic perception of the individual researcher.

Bakka had four main objections to Malmer's way of doing archaeology with the gold bracteates. Malmer's typological system had the consequence that two



bracteates could greatly resemble each other but still end up in two different types. This was something that Bakka could not permit, and he used some pictures as striking examples of what he meant by unfortunate classifications according to Malmer's system. Malmer, for his part, believed that the typology should be applied rigidly on the basis of the selected typological elements – a few unambiguously defined, essential elements – and not rely on subjective (his own word was “impressionistic”) perceptions of which bracteates ought to belong to the same type (M.P. Malmer 1968). Bakka also believed that in his definitions, Malmer had neglected to select and discuss typological elements that were essential in the iconographic interpretation of the motifs on the bracteates. In this context Malmer left the demand for unambiguous objectivity and instead discussed how he, unlike Bakka, perceived the bracteates as images of and associations with many gods and ideas borrowed from many places in the era's changing world. Here he found Bakka's reasoning too rigid and formalistic.

Bakka's critique of the production diagrams for the bracteates was based on source-critical principles. He found it misleading and unfortunate that a quantitative diagram with a high visual impact was employed to bring together such a diverse source material as the gold bracteates. These had been made, used, and deposited according to many different patterns, for example depending on the varying availability of gold. One of his main points was that though the bracteates were mostly hoard finds or stray finds, in Norway a large proportion were found in women's graves. These classes of find could not be comparable according to Bakka. He believed that the production diagrams could possibly serve as deposition diagrams, showing where bracteates had been deposited and then discovered. Once again, Malmer was clear in his reply, though he quoted himself, rather pettily, to demonstrate that in his book he had already anticipated and discussed Bakka's arguments. The production diagrams clearly showed the occurrence of bracteates. They provided an unambiguous foundation for considering the chronology of the objects and the intensity of their manufacture, which previous research had done on quite a shaky basis. He also challenged Bakka to prove his hypothesis about the significance of gold availability in a production diagram. If that were not possible, then Bakka's hypothesis presumably was not sufficiently elaborated.

Bakka went straight to the heart of Malmer's archaeology when he discussed whether types are constructed by the archaeologist doing the classification or identical to the types distinguished by people in prehistory, and thus rediscovered by the archaeologist. Leo Klejn has discussed the same problem, again on the basis of Malmer's writings (Klejn 2010; cf. Baudou & Jansson 2015:74). Malmer's fundamental thesis was that types are hypotheses based on unambigu-

ously defined typological elements. Archaeologists formulate and reformulate such type definitions until they believe they have found meaningful types to use in attempts at interpretation. The types may also need to be refined later as well. This Bakka could not accept at all (1968a:27): “[Malmer’s] dilemma lies on the theoretical plane and should be a warning to all archaeologists who have not thought out the theoretical problem”.

Bakka put the craftsmen at the centre of his reasoning. They did not know any verbal, logically correct definitions. Working with such was an intellectual game which did not lead to any understanding of the labour performed in the workshops or of the workshop traditions. Bakka acknowledged the importance of distinguishing specific typological elements, but an understanding of the craft required far more elements than the few employed by Malmer, and it also called for empathetic insight into the objects, the bracteates, as artistic products. Malmer’s way of doing archaeology was, with Bakka’s word, an illusion. His method was divorced from reality. In his reply, Malmer reiterated the necessity of working unambiguously, or any scientific discussion would be impossible. He described typological classification as an experiment. It was in comparisons of the outcome of multiple experiments that knowledge could ultimately be found.

It was not self-evident that Malmer’s archaeology would be recognized and applied throughout Nordic archaeology, even though it was practised at the archaeological seminar in Lund in the 1960s. It was not well received in Uppsala, where Bertil Almgren’s students concentrated on source criticism and curvature analysis. Malmer’s typological method receives remarkably little attention in Bo Gräslund’s doctoral dissertation about relative dating, defended in Uppsala in 1974. I found it entertaining to attend Arne Emil Christensen’s thesis defence in Oslo in 1985. The opponent, Carl Olof Cederlund, mounted a hard attack, in the spirit of Malmer, on Christensen’s definitions and typological classifications of pieces of wood from boats at Bryggen in Bergen (Christensen 1985). Christensen defended himself by waving both hands above his head and emphasizing that he knew about wood, woodworking, and boats. He had made his classifications as a boat builder and sailor would have done. That was the kind of archaeology that Malmer wanted to clean out with *Jungneolitische Studien* and *Metodproblem inom järnålderns konsthistoria*. His archaeology created antagonism between traditionally thinking archaeologists and innovatively thinking archaeologists, and between generations, but in fact there were few who understood him until after one or two decades.

A third empirical example of his own method for archaeological research was developed by Malmer in the 1970s. It concerned the Scandinavian rock carvings

from the Bronze Age (Ch. 4). This book provoked little discussion. Scandinavian rock art specialists had other interests than distribution studies with the aid of production diagrams during the first years of the introduction of post-processual archaeology. Gro Mandt, however, summed up her review (1983) with the words that charting geographical variation in the rock carvings – she drew the line at writing “chorology” – was “a fascinating and extremely valuable contribution to research on rock art far beyond the borders of Scandinavia”. One of her few critical observations was that Malmer, who had worked exclusively from the literature, had stumbled into some Norwegian pitfalls in the absence of personal experience of the landscape and the material. Whether or not production diagrams were a sensible method for compiling a lucid chronology of ships on rock carvings was a question on which she did not take a stance.

It is doubtful whether Malmer’s book on rock carvings was read very much outside Scandinavia. In one study it figures as an example of a way of working with rock carvings which the authors rejected (Bradley et al. 1994:374):

It is surprising, then, that studies of [rock art] so rarely take advantage of this connection with natural terrain. Instead, they have concentrated on the character of the designs, and a main emphasis has fallen on questions of style and chronology. In effect, the motifs have been separated from the rock and its place in the landscape and treated in exactly the same manner as portable artefacts (Anati 1976; Malmer 1981). It is our contention that this approach has obscured their full potential for research and, in particular, the contribution that they have to offer studies of the prehistoric landscape.

In the early 1990s when John Coles conducted field studies of rock carvings in the landscape of Sweden’s Lake Mälaren area, he read Malmer’s book about rock carvings as a matter of course. In one of Coles’s two books in this field, a comprehensive guide to the rock carvings of Uppland, Malmer is relegated to a list of further works for interested readers (Coles 1994:46). Coles’s other book on the subject has detailed descriptions and brief discussions, and here Malmer’s book is the starting point for the chapter “A Wider View” (Coles 2000:123–129). Coles cites Malmer’s opinion that previous research into rock carvings had engaged too much in mythology. He indirectly criticizes Malmer, however, on the same grounds as Richard Bradley and his co-authors had done (Coles 2000:127): “... that we need deeper analysis of rock carving sites in their local and particular settings, proximity to water and wetland, and structural or other evidence of activities on or in front of the rocks.”

Coles (2000:128) ends his brief discussion with the observation that “The rock carvings of the study area therefore have a place in the wider social landscape of the Bronze Age”. The two cited works in English are examples showing

that Malmer was read in the English-speaking world, and also that research on rock carvings was moving in new directions while Malmer was working with his chorological and chronological studies of innovation and production.

The 1960s and especially the 1970s was the era of positivist archaeology in Sweden. In retrospect – but not at the time – *Jungneolithische Studien* can be recognized as the opening salvo. At the time, people were turning towards American and British archaeology. Malmer often challenged archaeologists who did not comply with his guidelines for scientific archaeology with a positivist foundation. But he also engaged in polemics with archaeologists who, although they had the right ambitions, had not fully succeeded according to him. One example is his review (M.P. Malmer 1969a) of Carl Cullberg's doctoral dissertation on copper and bronze axes (summarized in Gräslund 1972) and a discussion of it in *Norwegian Archaeological Review*. The review includes a number of ironic observations about Carl Cullberg's "unnecessary" and "meaningless" reasoning. It is obvious between the lines that Malmer felt that Cullberg ought to have worked as Malmer did, instead of reinventing all manner of unsuccessful wheels.

*Norwegian Archaeological Review* highlighted Carl Cullberg's dissertation in its lively debate section in the 1970s. This usually meant that the dissertation was summed up by the author himself (although not in this case), was commented on by a few people (M.P. Malmer 1970; Voss 1970), and that the debate was rounded off by the author. Carl Cullberg chose not to reply; instead he printed a chapter which had not been included in the dissertation (Cullberg 1970). Malmer put considerable effort into trying to understand why the dissertation had produced such meagre results. None of the various classification experiments had, in his view, produced any meaningful tools for interesting culture-historical interpretations. One reason was that Cullberg presented his first attempts at classification as experiments, and then rejected them and presented new ones. Malmer saw no value in this. Cullberg ought to have performed just one study on the basis of concepts and methods of a modern and completely satisfactory kind, for example, Malmer's own (1970:49): "On the whole Cullberg has failed to profit by the experience gained during the last few years in the mathematical-statistical treatment of antiquities."

To this it may be added that Cullberg's typological system was designed for automatic data processing and suitable for work with polythetic types, which Malmer never dealt with. It can also be said that Cullberg's series of several classifications and their subsequent rejection actually illustrates Malmer's own research strategy: to work out hypothetical typological systems, test them, and if necessary reject them and reformulate them. For Malmer, however, it was more

important here to point out defects than to give credit for good work. Such one-sided negative reviews and comments are rare in Swedish archaeology.

In the years around 1980, archaeology in the English-speaking world, to which Nordic archaeology had belonged since the mid 1960s, changed character. The label “New Archeology” was replaced with the label “post-modern archaeology” and soon afterwards “post-processual archaeology”, in keeping with how New Archeology, towards the end of its era, had been called “processual archaeology”. One change was that the strict positivist demands for objectivity were replaced by a view of the researcher as a person making interpretations on the basis of individual circumstances, political in the broadest sense, and others. Archaeology simultaneously shifted its focus from ecosystems and social structures to the participation of material culture in human relations.

Malmer met the post-processual archaeology of the 1980s with his own ready-formulated way of working. Arne B. Johansen of the University of Bergen called it an “ideology” in a review of a publication from a major Swedish project in contract archaeology concerning antiquities and the cultural landscape, in connection with the expansion of hydroelectric power in the north of the country (Johansen 1979). Johansen blamed Malmer in part for what he dismissed as an unthinking, banal, data-accumulating archaeology. Johansen had declared that data, archaeological source material, must be collected through conscious selection according to a pre-formulated plan, a model of the studied society. Data should cast light on the phenomena to be studied in the society in question. He went so far as to claim that archaeological source material could not be classified without a model of the prehistoric artefacts constructed on the basis of what was conceived as a prehistoric artefact by one’s own society and by the individual archaeologist. A good research strategy would thus contain a model-directed selection of data to discuss on the basis of a model-directed perception of society. Anything else would be banal data accumulation, and Malmer’s archaeology was an example of this.

Malmer could never resist a gauntlet thrown down like this (1980:260):

Is archaeology on the way to becoming a science where ancient monuments mean very little, and ancient artefacts nothing at all? [...] But if the source material is uninteresting, what then is important? Everyone knows: *theory* is the word – the word that, quite simply, is not permitted to be absent in modern literature, and whose mere utterance is perceived by many as a guarantee that what is said is the height of science in its time.

Malmer recommended that “one must work as if total objectivity were possible” (1980:262). He wrote that this must in particular apply to archaeological

excavations. An excavation must proceed from many models – Malmer preferred the word hypotheses – which had to be tested and reformulated on the basis of Karl Popper’s falsification philosophy. The opposite could mean that data were neglected and that future ideas for interpretation would prove impossible to test. He referred to his own plan for the excavation of the Alvastra pile dwelling (Ch. 17). His example of the opposite was Heinrich Schliemann’s excavation of Hisarlik. Preconceived ideas of what Troy should look like according to the *Iliad* meant that Troy III–IX were demolished by the excavation and only Troy II was adequately documented. Future interpretations have had to be based on an excavation of a small section of occupation layers which Schliemann had left unexcavated.

There was a serious disagreement between Johansen and Malmer when the former asserted that prehistoric people could be understood only by using one’s own stance as a person. Unambiguous definitions and reproducible typological classifications of the archaeological source material were all fine and necessary, but they did not lead to a human understanding of prehistoric people. Malmer responded (1980:263): “If archaeology abandons the demand for unequivocality then it does not deserve to be called a science.” He called to mind the catastrophic archaeology of the 1930s and 1940s, chiefly in Central Europe. Malmer’s generation of archaeologists found it difficult to accept the archaeology of the 1980s, which argued that archaeology simply could not be apolitical. The same applied to gender archaeology. Yet Malmer could also appreciate an intellectual achievement and a challenge (1980:261): “I must confine myself to bringing up some points that I find crucial and interesting. And otherwise I urge those who have not yet read Arne B. Johansen to do so. You will not be disappointed.”

As we have seen, around 1980 a new orientation arose in archaeological thought, based in Cambridge and a circle of young researchers around Ian Hodder (1982). It was labelled as “post-processual archaeology”. It rejected the positivism of the New Archeology and the search for general laws to explain how people and societies behave and change. Societies were instead perceived as unique, each with its own situation. Their material culture, in its time, had been charged with symbolism and meaning, a code that archaeologists should try to understand on the basis of the find context, using, for example, structuralist theory. The individual archaeologist’s interpretation was held up as central to the research process. It too was said to be situationally determined and could only be reached on the other side of a number of hermeneutic thresholds.

This kind of archaeology did not interest Malmer; it was virtually alien to him. It was not compatible with his strict demand for objectivity and rational-

ity. Archaeology was supposed to test hypotheses on the basis of explicitly formulated, falsifiable arguments, not engage in discussion leading to understanding. Malmer nevertheless took part in the debate on the discipline's theory of science. His position involved clarifying and defending his theses from the 1960s. He stuck firmly to these through the 1980s and 1990s and until his pen fell silent in the 2000s. He was well read in the theoretical debate about archaeology after 1980 and had devoted a great deal of thought to it, as was evident from his stimulating lectures to research students at his department at Stockholm University in the 1980s (Hans Browall, pers. comm. 2015). Unlike the 1960s, in his printed contributions to the debate he quoted role models from the literature on epistemology and the theory of science. He mentioned Karl Popper and Hans Vaihinger (Ch. 2). He held up the latter as one of the most interesting philosophers of the twentieth century – and was astonished that others involved in the debate were not familiar with Vaihinger's work *Die Philosophie des Als Ob* from 1911. A researcher had to work as if objectivity were possible, although complete objectivity could not be conceived as existing. Other thinkers whom he quoted were Rom Harré and Roy Bhaskar (Ch. 4; Stig Sørensen 1999:780). Based on their thinking, he called himself a “theoretical realist”. He thus firmly rejected Thomas Kuhn's view of the history of science as a series of shifting paradigms. According to Malmer, archaeology, through time, was becoming a better science, not a different one, as well-formulated hypotheses replaced less good, falsified hypotheses. It was a matter of differences of degree, not of kind.

Perhaps, however, Malmer had the same difficulties as all other archaeologists of his time in bridging the gap between unambiguously defined and described types and their interpretation in terms of cultural history. He never took any interest in *middle-range theory* or *theories of material culture*, the attempts in the 1980s to clarify the interpretative process. Malmer would occasionally refer to analogies – as in the article about the *pleion* concept (Ch. 8), where he found use for a historical analogy with the grain and butter paid as tax in the Early Modern Period. Apart from this, he took no interest in the ethnoarchaeology of the 1980s. He found it far too concerned with woolly matters that fell outside the bounds that he had set up for archaeological thought and work.

When Swedish archaeology was wondering about ethnoarchaeology, the Swedish Archaeological Society held a discussion meeting. Opening remarks were given by Göran Burenhult, who understood his Irish megalithic graves on the basis of stone monuments in the South Pacific (Burenhult 1992), and by myself, who had produced an historical ethnoarchaeology of a village in Dalecarlia (Welinder 1992). On the subject of the megaliths, Malmer declared that a



stone block was just as heavy for people in the Stone Age as it was for people in the present. Malmer was a rationalist and occasionally a proponent of common sense. The idea that Stone Age people could find the roof slab on a passage grave light because the gods helped them to lift it was a non-idea to him. A heavy stone was heavy in the Stone Age too. Yet Malmer would sometimes formulate rather imaginative interpretations himself, as I shall allude to in my comments on Section III in this book.

Despite his sharp criticism of other archaeologists and his rejection of other ways of working as an archaeologist than those he had developed in the 1950s, Malmer was remarkably tolerant. As long as his students and doctoral candidates at the departments of archaeology, first in Lund and then in Stockholm, used unambiguous language and formulated coherently reasoned interpretations, they were allowed to ask whatever questions they wanted and give whatever answers they wanted – as long as they could justify their statements. Paraphrasing Frederick the Great, he said: “In my department everyone can go to heaven after his own fashion” (Ch. 18). Malmer was curious about all kinds of archaeology.

Through the decades then, Malmer was satisfied with the foundation for his archaeological work which he had elaborated in the 1960s in *Jungneolithische Studien*. In his last major work, published in 2002 but written during the preceding decade, he returned to the Neolithic (Ch. 10). This book was intended as part of a multi-volume, multi-author work about Swedish prehistory, for use as textbooks. All that was ever completed, however, was Malmer’s part about the Early and Middle Neolithic, 3900–2800 cal BC. The book has three main sections, one for each of three cultures: the Funnel Beaker Culture (TRB), the Pitted Ware Culture (GRK), and the Battle Axe Culture (STR) treated in *Jungneolithische Studien*.

For Malmer – as later for David L. Clarke (1968) – cultures were the top level in a typological hierarchy from typological elements, via types, to cultures. Already in 1967 he had explained in a pithy article how he viewed cultures as hypotheses defined from selected types, in the same way as types were hypotheses defined by selected typological elements. Other demarcations of cultures were unclear and hence difficult to discuss unambiguously, for example, to define cultures on the basis of characteristic find spots, a practice reflected in the fact that many of the cultures in archaeology are named after places, such as the Maglemose culture or the Jastorf culture.

Cultures were ideologies, built up around economy, religion, and art. Malmer had difficulties in arriving at a more profound view of what characterized the different ideologies – the cultures – and how they were altered and replaced (Rudebeck 2003). Immigration was no alternative for him, except possibly for



the introduction of the Funnel Beaker Culture along with the earliest agriculture. To Malmer, the Neolithic was distinctive in having an ethnically unchanged population. His way of defining cultures also led to an underemphasis of variation in favour of general features. His own excavation of the Alvastra pile dwelling (Ch. 17) could have problematized the defined cultures. It must be stated, however, that when Malmer's monumental book about the Neolithic was published in 2002, no major syntheses of the great 1990s projects in contract archaeology had yet been written. He did not read the individual excavation reports. Today the Neolithic is already a different period to scholarship than when Malmer tried to sum up what was known about the period. Nevertheless, through his encyclopaedic knowledge of the facts, the book is and will remain a work of lasting value. It will be the obvious inspirational starting point for much future archaeology.

Malmer constructed his archaeological system in the 1950s in a situation where he did not think he could work meaningfully with the concepts, language or methods then current in European archaeology. He started from scratch, inspired by the New Geography that had been developed at the Department of Human Geography at Lund University, where Malmer began his scholarly career. It was to become an impressive edifice; the foundation stones were verbally formulated, logically correct definitions, and the building stones were typological element, types, and cultures. For Mats P. Malmer all this constituted hypotheses to formulate, test, falsify, and reformulate in the study of the problems to which he devoted his archaeological life (Ch. 18).

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# I.

## From typology to actualism

After publishing his 1962 doctoral dissertation in German as *Jungneolithische Studien*, Mats P. Malmer wanted to make his archaeological thought known in English, which was becoming the increasingly dominant international language of scholarship. He did so in a lengthy introduction to his 1963 book *Metodproblemet inom järnålderns konsthistoria* (Ch. 1). In later articles (Ch. 2–7) he amplified, explained, expanded and defended his way of working in archaeology.

Malmer's archaeology was controversial when he presented it in the 1960s. It has never been accepted in its totality, even though much of it is taken for granted today in archaeological practice. The significance of Malmer's theoretical construct – which he preferred to call “method”, since he despised and sometimes spoke ironically about the frequent use of the word “theory” by the younger generation of archaeologists – for the history of archaeology, and still today its epistemological significance for research on artefacts, was summed up by Leo S. Klejn in Russian about the time when Malmer passed away (Klejn 2010). Klejn's book has an English summary. It has also been summarised and commented on by Evert Baudou and Ingmar Jansson in a 2015 article in English. They discuss five questions raised by Leo Klejn: Oscar Montelius's typology and real types versus Malmer's kind of classification and artificial types; Malmer's verbal definitions of types versus empiricism; Malmer's production diagrams; migration versus internal development as an explanation for the introduction of the Swedish-Norwegian Battle-Axe Culture; and Malmer's actualism, theoretical realism, and fictionalism. Malmer's views on these questions can be found in Ch. 1–7, 9, 18.

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## CHAPTER I

# The methodological grounds of material research

1963

“Complaints are often voiced today over the decay of the humanities. The problem is a grave one. But those who raise the most passionate complaints often forget that ‘humanism’ in the deeper sense is impossible without a thorough knowledge of the technological and scientific picture of the world, and this picture’s philosophical problematology. A humanism that avoids exact thinking is cultural barbarism.”

G.H. von Wright, *Logik, filosofi och språk*

### *The unity of material research*

A striking feature of Scandinavian archaeology today is the way in which research into the different stages of prehistory has taken different methodological paths. Mesolithic research has close links with geology, climatology, botany and zoology, whereas there is often no such interest in the natural environment of man in studies of the later stages of prehistory. The special mark of Neolithic research is that the material has been divided up by cultures, which are taken as corresponding to different tribes, some of which were contemporary, lived in conflict with one another and undertook migrations. Such a division by cultures is lacking in research into the metal ages, particularly the Bronze Age, where it is instead the division into chronological periods that is taken more or less for granted. And yet the beginnings and ends of these periods have never been defined, either by Montelius, or Sophus Müller or any later scholar. In the Pre-Roman Iron Age, on the other hand, it is precisely around the division into periods that discussion has centred, and numerous different systems have been proposed. In the Roman Iron Age, finally, and the Late Iron Age, the study of decorative details has predominated to an extent that has no counterpart in Stone Age and Bronze Age research.

### *Basic methodological problems in archaeology*

It is clear that the variation in methods employed from one stage of prehistory to another has made the results in some degree incommensurable, and has thus made it difficult to present Sweden's prehistory on a uniform, reliable basis. And yet the "one-sidedness" of research in the different stages of prehistory is only motivated to a very minor extent by the nature of the material studied. The direction of research seems rather to have been determined mainly by chance, or the nature of certain pioneer studies from the beginnings of modern archaeology. The varied approach to prehistory is thus largely due to the fact that archaeology is a young science, in which many essential problems have yet to be formulated clearly.

The essential cannot be separated from the irrelevant in archaeology on objective grounds as long as one is dealing with individual studies. To one scholar a vague hint on political conditions in prehistory may appear more important than a wealth of information on everyday life but the opposite view is objectively equally tenable. In the same way, it is a perfectly defensible view that the primary object of archaeology is to study connections in the field of the history of art. What *can* be objectively determined, however, is the central, common feature in all archaeological method, and if modern archaeology had paid more attention to this, then both the approach used and the results obtained in the different eras of prehistory would probably have been less incommensurable than they are.

### *Two groups of historical sources*

In historical science the sector concerned with epochs from which written sources are preserved has been earmarked as "history", while the period for which there are no such sources is commonly termed "prehistory". This terminology is unfortunate in that the "pre" tends to have a negative or pejorative sense, and the term "prehistory" to insinuate that archaeology is no "historical" science, or that it is an historical science of minor importance.

Another division common in historical methodology is that between two main groups of historical source, "remains" and "narrative sources". The former, it should be noted, includes such documents as have comprised part of the actual historical course of events, e.g. peace treaties and contracts of purchase, as well as the objects covered by the archaeological concept "artifacts", together with skeletons and other human remains, and language and customs (Bernheim 1926:124 ff.). But of course there are other large groups of material that



ought really to be included among “remains”, namely the traces of contemporary animals and plants and, by the same token, non-organic nature. But even without these additions, the material classified as “remains” has become far too variegated. It is clear, for instance, that “narrative sources” and the documentary “remains” are logically closer to each other than the latter and artifacts.

An unambiguous division of source material would seem possible if we first separate everything that belongs to the ancillary sciences of history: non-organic nature is studied by geology; plants and animals mainly by botany and zoology; human skeletal remains by human osteology; manners and customs by sociology and ethnology; and languages by different branches of linguistic research. This leaves us with two main groups of historical source, namely the written sources and the artifacts. These could correspond to two logically equally valid branches of history, which we can call “documentary historical research” and “material historical research”, or simply “documentary research” and “material research”. It is surely wrong to classify research concerning artifacts, i.e. material research, as an ancillary science to history proper, as has often been done. The object of the science of history is to clarify conditions throughout the existence of the human race, and one cannot just draw a line at the point where the written sources fade out. So far back as written sources are plentiful, documentary research will undoubtedly be the main instrument in laying bare the conditions of the human race, but before that point material research is an equally indispensable tool. The ancillary sciences are throughout of secondary importance.

### *The value of artifacts as evidence, compared with written sources*

That the importance of material research is fully on a par with documentary research is so self-evident that we inevitably ask ourselves how it has ever come to be regarded otherwise. One reason would seem to be an erroneous view of the nature of the two types of source. It is often imagined that the documentary sources give immediate information on the circumstances and conditions of bygone days, while the witness borne by artifacts must first be interpreted, and that this interpretation must be largely subjective. The value of written sources as evidence is thus taken to be much greater than that of artifacts. There is in fact a difference between the two as regards their objective value as evidence, only it happens to be quite clearly in favour of the artifacts. The narrative sources that make up a great part of the material of documentary research, contain not a single passage that can be assumed to be objective, and down to the least detail there is the possibility that such sources are consciously, or from human imperfection, biased. The first step must therefore be to scrutinize the sources.

The artifacts, on the other hand, are all “remains”, relics of the actual historical course of events, and contain as such an objective truth that is immediately available. In the later processing of archaeological material, of course, the sources here must be examined too, particularly as regards the evidence surrounding the finds. This criticism of sources in material research, however, has a much greater chance of reaching reliable results than is the case with documentary research, quite simply because all the written sources concerning finds stem from modern times.

The view that the nature of the sources obliges material research to be particularly subjective is thus completely false. On the contrary, the material permits an exactitude and objectivity otherwise unknown in the humanities or, as I would prefer to call them, the humanistic sciences. And yet the criticism that material research is subjective is unfortunately not entirely unjustified in practice: subjective elements have been introduced without due cause. This is beyond all doubt a result of the same lack of consideration to method as had led to research being concentrated on such different fields from period to period of prehistory.

### *Methodological unity of material research in different disciplines*

History is not alone in its division into one branch in which research is documentary and another where it is material. The same applies to a number of allied disciplines, such as numismatics, the history of art, and ethnology. There is general agreement that one and the same method, with criticism of sources as its foundation, is valid in all documentary historical research. In the same way all material research forms, from the methodological point of view, a single unit. The methodological differences between the disciplines mentioned above lie mainly in the quantitative proportions between written sources and artifacts, and in the chronological relationships between the two groups. Any science that has material at its disposal in the form of artifacts could thus be chosen for a study of the methodological grounds of material research. Prehistoric archaeology is to be preferred in practice, as it has the smallest proportion of documentary sources.

The following study is thus not primarily on the relation between written sources and artifacts in one and the same discipline, still less does it concern the possibilities of winning historical knowledge by archaeological, numismatic, aesthetic or ethnological research. The aim is to illustrate the logical grounds for historical research on the basis of a material consisting of artifacts, without assistance from contemporary written sources.

## Basic concepts in artifact research

### *Similarity and dissimilarity*

Artifact research is based on the primary laws of logic: “A is A” and “A is not not-A”. Its most important basic concepts are those of *similarity* and *dissimilarity*. A single, isolated artifact does not permit any conclusions at all. Artifact research demands two or more. In this there is an important difference as compared with documentary research, since a written source is of value only if it has something to say in itself. Artifact and documentary research resemble each other, on the other hand, in that the strength of conclusions drawn from them grows with the number of mutually independent sources, and that with a *very* great number of sources they provide more or less absolute certainty.

Sophus Müller was one of those who earliest and most clearly emphasised the importance of the concept of similarity in artifact research. “Where there is similarity, there must be relationship, a connection of some kind or other. This is an assumption as necessary to Man as that we live in an orderly and organised world” (Müller 1884:194). But what is “relationship” or “connection”? Naturally different forms of similarity. The basis for conclusions is thus the number of similar features: when only a few similar features are found it is unlikely that there will be others but the more there are the more likely it is that there are more still. To put it the other way round, it is improbable that many similar features should occur entirely at random. The central moment in artifact research is thus entirely governed by the probability calculus.

As an illustration of the law of probability, we can with Sophus Müller quote “our certainty that there is order and organization in the world” (Müller 1884: 185). More concretely, we can also note that original and independent thinkers are rare, and that most people, in their actions, usually repeat what they themselves, or someone else, has done previously.

### *Different kinds of similarity*

We can distinguish between three different kinds of objectively determinable similarity between artifacts, namely physical similarity, similarity of find combinations and chorological similarity. If one of these categories cannot be objectively determined, but the others are supported by a sufficient number of similarities, then the remaining similarities can be said to be probable. And if the number of objectively determinable similarities is sufficiently great, then there

is a probability of every other form of similarity, such as similarity in time, use, name and environment.

### *The criterion of contemporaneousness*

The decisive importance of chronology in all historical research warrants our distinguishing signs of contemporaneousness between artifacts with a special term: the *criterion of contemporaneousness* is comprised of many similarities between artifacts, and the strength of the criterion grows with the number of these.

### *The type*

If we were to compare every artifact with every other in order to determine similarity and dissimilarity then our work would take an unreasonably long time, and the results achieved would quickly become excessively cumbersome. If the procedure of comparison is to be practical, artifacts with essential points of similarity must be grouped into *types*.

We can distinguish several different sorts of type, depending on the nature of the similarity. Physical similarity permits the distinguishing of artifact types (object types). On the basis of similarity of find combinations we can distinguish types of find combination (e.g. razor, pair of tweezers and awl; wine dipper and strainer), and also grave types and habitation site types. Chorological similarity, finally, permits the distinguishing of local types.

### *Types and sub-types*

The procedure of comparison wins further in simplicity and clarity if we do not restrict ourselves to a horizontal system of types but create a hierarchy. The division immediately under a type can be called a *class*, the next division a *group* or a *variant*, and a still lower division a *sub-group* (Malmer 1962:4, 63, 400). The actual terms chosen, however, are of minor importance.

### *Typology*

The theory of types and their relation to each other should be termed *typology* (Malmer 1962:48, 881). Other terms, such as formology, morphology and type analysis (Gjessing 1946:144; Althin 1954a:XII; 1954b:46) have been suggested for this central aspect of material analysis, but they are hardly preferable to the term "typology", which is linguistically excellent, refers directly to the central

concept of “type” and has a long tradition (Hildebrand 1880:54; Montelius 1880:3). There would also seem to be practical reasons for preserving the term used by Hildebrand and Montelius, as although their presentations of typological method are admittedly diffuse in several respects there is no doubt that their methodical efforts concerned the very heart of material research.

### *Type definition*

The delimitation of types, typological groupings, is the most important phase in the process of material research. Unless it is quite clear which individual objects (or other units) belong to a certain, named, type, then the type is a concept without an exact, fixed meaning, and thus of little use as supporting evidence. There is only one acceptable way to delimit a type, and that is to formulate a logically correct *verbal definition* (Malmer 1962:6, 881). A large number of scholars have, strangely enough, refused to accept this basic and self-evident truth. In the archaeological literature, the correctly defined types seem to be in the minority and the verbally undefined or badly defined in the great majority. A scientific argument based on incorrectly defined types must inevitably suffer from logical shortcomings, sometimes to a catastrophic degree.

In principle, the faults to be found in the definitions in the literature are of two kinds: either the author restricts himself to a picture reference, and thus avoids the formulation of a verbal definition entirely, or else his verbal definition is incorrect. In the present work the type definitions to be found in the literature will be studied fairly thoroughly, but it can be in place even at this point to give a couple of concrete examples.

In Montelius' famous work of 1885 *Om tidsbestämning inom bronsåldern*, there are a large number of type definitions that are incorrect because they contain words and expressions with no exact meaning. In the following quotation, taken from the section on socketed celts, I have italicised the inexact expressions (Montelius 1885:53 f.):

Type A. *Large*. Above *usually* round, with several transversal, *narrow*, raised lines, traces of the narrow bands with which the shaft was bound fast to the palstave celts from which these socketed celts developed. The remaining part of the celt *usually resembles* the lower part of palstave celts of type F. Loop at upper edge. No punched ornamentation. Fig. 19.  
[...]

Type E. *Medium sized*. *Almost evenly broad*. The broad sides form a plane, which *usually* tapers upwards and is rounded. Loop at upper edge. Figs. 99 and 100.

Type F. *Small*. Like E, but *smaller, simpler*, comparatively *broad* and *usually* with a broad cutting edge. *With or without* loop at upper edge. Figs. 101 and 130.

Obviously, it is impossible to classify a newly found socketed celt on the basis of these definitions. Without picture references they would have been quite meaningless. It is sufficient to draw attention to the expressions “large”, “medium-sized” and “small”. These adjectives give no information whatsoever as to where Montelius drew the lines between the three types. If type definitions are to be based on size then we must use the metric system, and indicate the range of variation within the type, i.e. maximum and minimum measurements, and preferably also the median.

Another example we can take is a definition in the field of ceramics, from modern American archaeological literature, *Surface survey of the Virú Valley, Peru*, by Ford and Willey (1949:75):

Gloria Polished Plain (Plate 4 c–g). Gloria Polished Plain is a sand tempered, oxidized-fired, reddish ware that is *mainly* a polished version of its companion type, Castillo Plain. The polishing is *generally* on the exterior of the vessels and in contrast to the earlier polished type, Huacapongo, is *well done*, so that the tracks of the polishing tool are not apparent. While *most* of the forms are *similar* to those of Castillo Plain, the vessels are *somewhat smaller*. This group also includes some material which probably came from graves, and consists of bottles with stirrup-spouts and solid bridges. Such vessels have been found accompanying Gallinazo period burials.

Words and expressions with no exact meaning have been italicized also in this quotation. These are enough in themselves to make it very difficult for the reader to decide if a certain newly found sherd belongs to the type “Gloria Polished Plain” or not. But even elsewhere the wording is very general; very few truly distinctive marks can be adduced, and we have in fact only a general characterisation, not a logically correct definition. Modern American archaeology has studied ceramic problems much more intensively and purposefully than has European, and an attempt has been made to reach chronological results by calculating the find quantities of the different ceramic types. The principle for these attempts may be correct, and the schematic presentation is often highly advanced, but the results are still open to dispute as long as the type definitions, the basis of the whole thing, are not distinct and unambiguous (Ford & Willey 1949, figs 4–5; cf. Moberg 1961:13, fig. 3). There are examples of definitions that are much more exhaustive and with more objective details than these, but which draw no clear lines between the types (Phillips et al. 1951:69 ff.).

In the same way, Montelius’ faulty type definitions inevitably affect the value of the work based on them. Montelius defines, for instance, Period VI of the Bronze Age by indicating the types included, namely, among others, socketed celts “small, simple, of type F” (Montelius 1885:82). Period V is defined in part

by the existence of socketed celts of types E and F. A socketed celt is thus to be dated to Period V if it “resembles” type E but is smaller and simpler. If it is smaller and simpler still, however, it is to be dated to Period VI. As a further support for dating we have the information that a socketed celt of type F either has a loop on the upper edge, or else it does not. The field is thus open for everyone to think as he pleases, and any possibility of arguing from objective evidence has been prevented.

### *The logical grounds of type definition*

Faced with the countless unsuccessful and meaningless definitions he meets in the literature, the reader is bound to ask how it is that material research has managed to dispense with the proper determination of concepts that is the basis of all science. There can hardly be more than one explanation. It is obviously argued that we have, after all, *the objects themselves*, so what use is it translating them into words, arranging the words into logical categories and conclusions and finally returning to the objects themselves and ordering them in accordance with the results of this abstract course of thought. No, if we stick to the objects themselves, things will be much simpler. If we have the object in front of us, we can take it in, its form, its material and all its details, in a glance. Simply to describe it would take several pages, and the description would still not be exhaustive. It is true that we cannot send the original to every scholar to whom we wish to report the results of our researches, but fortunately we can send a *picture*, a photograph or a drawing. And with this the field is open for the picture, and its fatal role in material research.

It is, of course, perfectly true, as has been maintained not only by those employed in material research, that a picture tells more than a thousand words. But there is one thing that a picture can never give and that is a *type definition*. When Montelius writes “such bracelets as in Fig. 262” and “such brooches as in Fig. 143” (Montelius 1885:68, 157) he probably knows what he means. And his readers, looking at these figures, also know precisely what they mean. But if we compare the views of the different readers, we find that they are different as each of them will have understood the picture, with its countless details, in his own subjective manner. A picture is thus inadequate as a type definition.

The sciences dealing with material research, and prehistoric archaeology in particular, are without doubt in a special – and privileged – class in that they have at their disposal such quantities of physical and concrete material. But the guardians of such research should realise quite clearly that their science occupies no privileged status as regards logic, and that they must obey its laws like all the

other sciences. The artifacts are admittedly a priori existent, but the *types* are not a priori existent. They exist first in the instant there is formulated a logically correct verbal definition.

### *The concepts “definition” and “description”*

In those few verbal definitions that are to be found in material research, authors have often committed the error of collecting all possible properties of the artifacts, regardless of whether these can serve to draw a line against other types or not. When Montelius says of his Type F of the socketed celts that it is “with or without a loop on the upper edge” then this is of course an important piece of information, but the phenomenon is clearly not any *differentia specifica* delimiting Type F from other types.

A means of arriving at better type definitions is stricter formal discipline. We should thus distinguish between *definition* and *description* (Malmer 1962:6). The term “definition” should be reserved for the logical determination of concepts, the drawing of the boundary against other types. The basic meaning of the Latin *definition* is “the drawing of boundaries”. The description should include all other information on the type. Pictures belong to the logical category of the description. Definition and description thus complement each other, but *definition alone* indicates the type’s delimitations.

Another aspect of formal discipline is that it must be perfectly clear, in the written presentation, where the definition begins and ends, which words and sentences belong to it. It is also illogical to discuss the geographical spread and other properties of a type before it has even been defined. The definition must come first. The presentation of a type should thus not begin as in the following example (Öberg 1942:141):

*Group D IV.* This group, like the preceding one is *Norwegian*, although not to such a high degree. Of the Norwegian finds, most are from Rogaland. Some of these finds also show several examples from the same punch. What typifies this group, which is not so uniform as the preceding, is [...].

### *Definitions of the concept “type”*

The artifact type can clearly be defined as a number of objects with certain common properties, and the find-combination type can be defined as closed finds which include certain definite artifact types. But the main object of artifact research is not to set its material in order: it is to win historical knowledge. From



physical similarity, similarity of find combinations and chorological similarity we draw conclusions as to other forms of similarity such as in time, use, terminology and environment, or – briefly – the entire concrete historical situation. The artifact type (or the find-combination type) can therefore be defined also as a collection of objects corresponding to concrete historical situations, which are similar in essential respects (Malmer 1962:56).

### *The question of the a priori existence of types*

In American archaeological literature in particular discussion has at times been very lively as to whether the types, and particularly the artifact types, are to be regarded as existent a priori or not. One view is that the types are only a purposeful division of the material, created by the archaeologist. The other is that it is the job of the archaeologist to *discover* types, namely the types that the people of far-off days themselves distinguished (Ford & Willey 1949:38 ff.; Phillips et al. 1951:61 ff.; Ford 1954:42 ff.; Steward 1954:54 ff.; Shepard 1956:308; Moberg 1958:11; Malmer 1962, note 14, 586). The latter view can be said to be the more correct, in that the job of artifact research is, on the basis of the types, to draw conclusions as to concrete prehistorical situations. The possibility of reconstructing the concrete situation seems namely to be the greater the more “congruent” the type is with the situation. And the probability of such congruence seems to be greater the closer the type coincides with a type distinguished by the men of ancient times themselves, and designated by them with a definite name and always used for a certain purpose or in a certain situation.

There is no reason, however, to assume that a prehistoric society's material of artifacts was strictly divided into types, the definitions of which were known to all its members. In a modern society, after all, this is not the case. Certain *types* are very clearly defined, such as “coin” and “car”, and a few even more clearly, such as “Jaguar Mk. V” or “Penny with the head of George V”. But there are other types that are not at all clearly delimited, such as “book”, “coat” and “pleasure boat”. We get along splendidly with the concept of “book”, in spite of the fact that the type has not been clearly delimited from types such as “brochure” and “journal”. But this self-evident freedom to use vaguely defined concepts in our daily life naturally gives no corresponding right in the scientific context. If we were concerned, for instance, in investigating reading habits in different social classes or countries, an undefined use of the word “book” would be useless. In this sense we cannot be content to “discover” the types that prehistoric men distinguished: our type definitions must be much more exact. There is another objection, however, to the view of a priori existent types. The

languages of prehistorical peoples, and hence their concepts, are gone forever. Our attempts to reconstruct historical situations can be formulated in this way – that we are trying to discover, among other things, types. But what is vital is that the methodological procedure in this work of discovery is *identical* with the central moment of material research, the typological grouping of artifacts by similarity and dissimilarity. The necessary condition for a type's existence is a logically correct verbal definition. If anyone claims that by formulating the definition he has “discovered” the type, then we cannot confute him, but it seems more natural to say that he has delimited the type.

### *The typological series*

Our presentation so far has centred around the criterion of contemporaneity: similarity between the artifacts. If similarity is a criterion of contemporaneity, then dissimilarity is obviously a criterion of non-contemporaneity. The drawing of conclusions *ex silentio*, however, is always an uncertain venture, and a total lack of similarity is in practice therefore of no chronological interest. The chronological method of material research consists rather in a comparison between different degrees of similarity: close but not complete similarity is a criterion for a short distance in time, slight similarity a criterion for a larger time gap. Hildebrand's and Montelius' greatest contribution in the field of methodology was that they stressed, more clearly than any of their contemporaries, this possibility of exploiting chronologically the degree of similarity between types. There is thus all the more reason to retain the term they created for a number of types, set in order by degree of identity: a *typological series* (Hildebrand 1873:26 f.; Montelius 1884:18 f.; 1900:237 ff.).

### *Pure typology*

In their methodological discussion, Hildebrand and Montelius handled mainly the physical similarity of artifacts, and similarity of find combinations and chorological similarity, only to a lesser degree. It is clear that they considered physical similarity to be the most important aid in attaining a chronological order in the material of artifacts (Montelius 1900:265; cf. Hildebrand 1889:172):

In the vast majority of cases it is possible [...] by taking into account the purely typological circumstances alone, to decide which form is the oldest and which the youngest. In those

cases where this is not possible the answer is provided by the find combinations, which one should as a matter of general principle always try to investigate with the utmost accuracy.

In other statements by Montelius, however, the emphasis on the importance of physical comparisons is carried further, and it is suggested that such typological research should initiate the study of a given material of artifacts, and that a study of find combinations and chorology should wait until this has been carried out. The study, it is said, should be conducted so that one first arranges the material in order “in the manner that the internal criteria of the separate types seem to demand, and then investigate whether the conditions under which the separate types have been found confirm the correctness of the view as to the relative age of these types at which one has arrived” (Montelius 1884:2 f.). This order of work seems entirely arbitrary. As Sophus Müller pointed out with some sharpness there is no reason to restrict comparison to physical similarity alone (Müller 1884:167 ff.). He also writes that we must ignore “similarity of time and place”. This however is arguing in a circle, as similarity in time is never given, in material research (Müller 1884:170). On the other hand there is just as little reason for Müller’s statement that: “Typology cannot with certainty lead to a single determination of contemporaneousness and succession” (Müller 1884:172). When Müller contrasts “safe observations of find combinations” with “hazardous comparisons of form” he seems to be the victim of the same excessive faith in the concrete as has caused so many scholars to refrain from formulating verbal definitions (Müller 1884:182). There is, however, no logical difference between physical similarity, similarity of find combinations and chorological similarity, an argument that will be developed in more detail below. Consequently there is no difference in reliability between a time determination obtained from typological studies and one obtained by the study of find combinations. Neither way is it possible to attain complete certainty, but both can lead to a degree of probability bordering on certainty.

*Pure typology*, as we can call Hildebrand’s and Montelius’ study of physical similarity, disregarding the circumstances of the finds, has little practical importance, but as a theoretical experiment it is both justified and topical (cf. Malmér 1962:48).

### *Impressionism*

When ordering a number of types into a series after their degree of similarity, one must, in an objective science, naturally indicate exactly in what respects the types

are similar. In many cases this requirement has not been met. As an example we can quote from Åberg's presentation of a very long series (Åberg 1915:31 ff.):

In fig. 45 there is portrayed [...] an axe, lacking the nodular tying-off of the neck. This axe has analogies both with the types just described and with axes of type fig. 33, and also with the boat axes from Jylland. Seen from the side it is in perfect agreement with the boat axes. The outer sides, however, are not so uniformly curved as the latter, and the neck section is more compressed from the sides than it is as a rule in the boat axes. The axe fig. 45 represents a numerous and widely distributed group. From this there develop [...] the boat axes [...].

Axe fig. 51 is very close to the type portrayed in fig. 45. The blade and neck section are better developed in the characteristic style of the boat axes. In section, the axe is a rounded-off rectangle, and its neck section is slightly pressed in from the sides.

In the axe fig. 52 the original rectangularity has disappeared, and the form of the section is now oval. From above, the axe shows the characteristic profile of the boat axes.

Fig. 55, finally, shows a magnificent example of the fully developed Jylland boat axe type. The axe is perfected in both form and polishing. In section it is lens-shaped, with a greater arching of the lower than of the upper side.

It is no good italicising inexact expressions here, as was done with a couple of earlier quotations, as there is hardly one with an exact meaning. One can only surmise that Åberg, when setting up this typological series, had a number of pictures of axes, which he arranged in harmonic order according to his own personal taste. He then took up picture after picture and rapidly wrote down the impressions that first and most strongly came to his conscious mind:

From the side it is in perfect agreement with the boat axes [...] better developed in the characteristic style of the boat axes [...] a magnificent example [...] perfected in both form and polishing.

Material research is not the only science that has to struggle against subjectivity, but it can still be worthwhile distinguishing by a special term the form of subjectivity that is so characteristic, namely the tendency to abandon clearly defined concepts in favour of picture references, aesthetic value judgements, or confused accounts of the impression the objects have made on the scholar in question. This school of research can suitably be called *impressionism* (Malmer 1962:343, 354, 578 ff.). We have met it before in connection with the discussion on the theory of type definition, and we shall be meeting it frequently in the following.

## The typological elements

In the compilation of artifacts into a type, or in the ordering of types into a typological series, it is necessary not to get an impressionistic picture of some, general likeness between the objects or types, but to demonstrate objectively similarity or dissimilarity in as many details as possible. One or two similar features may be due to accident, but the more physical similarities there are, the greater probability there is of similarities between the artifacts or types in respect of time, use, name and environment.

We have distinguished above between three different forms of similarity, namely chorological similarity, similarity of find combinations and physical similarity. So far as the first of these is concerned, it clearly does not contain within itself any large number of similarities in detail. In the main there is only the aspect of distance: there is greater chorological similarity between artifacts found within a radius of ten miles than between those found within a radius of one thousand. Similarity of find combinations and its logical relation to physical similarity, is a very important problem that will be discussed later on. There remains physical similarity. We call the vast numbers of details in which physical similarity or dissimilarity can be demonstrated *typological elements* (Malmer 1962:50 ff.).

### *Different types of typological element*

The great mass of typological elements can be divided up in different ways. One possibility is to distinguish, under the name *decorative elements*, the great number that fall within the sphere of ornamentation, and sum up all the others under the name *technical elements* (B. Malmer 1961:262; M.P. Malmer 1962:54 f.). We can also split up the latter into four groups, making the total number of groups five: material elements, proportional elements, form elements, technical elements and decorative elements.

*Material elements* provide a great field of study, e.g. as regards types of stone in tools and weapons, the alloys of the metal ages, and the composition of the clay in ceramics (Becker 1952:66 ff.; Shepard 1956; Junghans et al. 1960).

The number of *elements of proportion* is practically infinite. We can distinguish between primary proportional elements, which are comprised of simple measurements such as the length and breadth of the object, and *secondary* proportional elements, or proportional elements proper, which are one primary element expressed in relation to another. In this way one can, for instance, define a concept "relative breadth" in terms of the breadth of the object as a per-

centage of its length (Malmer 1962:359 ff.). A *tertiary* proportional element is a secondary element expressed in relation to another primary or secondary element: as mentioned, the possibilities of defining new concepts are infinite. The figures provided by the proportional elements can be processed with a great variety of different mathematical methods (cf. Bohmers 1956; Heinzelin de Braucourt 1960; B. Malmer 1961:310 ff.; M.P. Malmer 1962:57 ff., 364 ff.). Proportional elements also include an object's volume and weight; it is obviously possible to set both in direct mathematical relation to other proportional elements. By *elements of form* are meant the surface typography of objects, apart from direct traces of manufacture, which come under the heading "technical elements". On a flint axe, for instance, the bulge of the sides and the curve of the edge are to be regarded as elements of form, while traces of the axe's manufacture that have not been rubbed away are technical elements. Elements of form, as this example illustrates, offer great difficulties for objective study in the case of flint articles and ceramics, but less so in the case of objects of metal or polished stone (cf. Malmer 1962:355 ff., 598 ff., 824 f.).

The *technical elements* are sometimes difficult of access for an objective study, as is the case for instance with the traces of manufacture of flint artifacts, but in many other cases they are easier to handle. Some examples can be mentioned: traces of ceramic manufacturing procedures (Shepard 1956); the degree of fineness of instruments used in decorating ceramics, such as comb stamps and thread, where the number of teeth or volutions per length unit can be measured (Malmer 1962:8 ff.); the drilling technique in shaft-hole axes of stone (Malmer 1962:607 ff.); and traces of casting and other metal techniques (Oldeberg 1942).

The main object of the present study is to investigate the possibilities of an objective study of the extremely numerous and varied *decorative elements*.

### *Objectively recordable and non-recordable typological elements*

It is one of material research's main tasks to study the artifacts' stock of typological elements, to determine the number and nature of these, and to investigate to what extent they are accessible for objective study. Unfortunately it is no exaggeration to say that material research has so far not carried out this task satisfactorily. The quotations given above are examples of how scholars, in the formulation of type definitions, have often selected the elements forming the definition entirely at random. Their formulations do not rest on any objective study of elements, and quite rule out the use of the definition in any objective argument. The expression of typological elements in known quantities (and not in the individual scholar's own subjective reactions) can suitably be called *objec-*

*tive recording.* Are all typological elements objectively recordable? The most suitable reply to this question is clearly that objective recordability should be a criterion of the existence of a typological element. Every objectively recordable physical detail or property of an artifact is a typological element.

Another question is: "Is it always possible in practice to carry out the objective recording of a typological element?" Here the answer must be "no". An example is the technique of hewing flint axes. A distinction has been attempted between "coarse" and "fine" technique, and in theory this is perfectly feasible. We can, for instance, define a "fine" technique of flint hewing by stipulating that the number of chips per surface unit shall not exceed a certain minimum, that the length of the chips shall not vary over a certain maximum, and that neither must the differences in level on the sides of the axe exceed a certain maximum. But the charting of one side of a single flint axe would be a very long and difficult job, so that in practice such objective recording is impossible (Malmer 1962:353 f.). In practice, then, the technique in which flint axes are hewn should be excluded from a scientific study. Many scholars would object to this conclusion. It may be difficult, they would reply, to put into effect a strictly objective recording method for the quality of hewing of an axe, but every scholar, with his own eyes, can decide, impressionistically, if an axe is hewn in a "coarse" or a "fine" technique. And to prove the possibility of arriving at reliable results by means of impressionistic judgement one could organise an opinion poll. A number of scholars could, independently, divide up a collection of flint axes into "coarse" and "fine". There is a very good chance that such a study would result in all scholars dividing the axes in roughly the same way. Only in the borderline cases, the "medium-coarse" or "medium-fine" axes, would the judgement of the impressionistically working researchers be entirely random. But it is also clear that impressionist judgements are inadequate as a basis for the objective adduction of evidence, as they would introduce an uncontrollable element of uncertainty. Impressionist judgements bear a certain resemblance to the approximate numbers of mathematics (even if they are far inferior to the latter as regards exactitude): the solution of a problem is often indicated most practically in the form of an approximation, but in the construction of the problem one aims at using exact values. It can be said of impressionistic judgements that they must never, under any circumstances, be used in type definitions. These must be formulated solely with the help of objectively recorded typological elements. If impressionist judgements have to be used for practical reasons then it must be at a later stage. There the danger of catastrophic errors is less, although it can never be eliminated in a presentation with impressionistic features.

## The criteria of continuity

Similarity between two types in a typological series always includes identity in respect of typological elements. In a ceramic series, similarity can consist, for instance, in both types having the typological element “comb ornament”.

Dissimilarity between two types in a typological series can be of either a contradictory or a contrary nature. An example of a contradictory dissimilarity is that one type shows the element “comb ornament” while the other does not. An example of a contrary dissimilarity is that one type shows the element “coarse comb ornament”, with a degree of fineness defined as varying between minimum  $5/3$  and maximum  $14/3$ , and the other shows the element “medium-fine comb ornament”, the degree of fineness of which varies between minimum  $15/3$  and maximum  $24/3$  (for the manner of designating the degree of fineness of a comb stamp, cf. Malmer 1962:8). Elements which, in comparison between the types, show similarity or contradictory dissimilarity can be called *constant elements*, while those that show contrary dissimilarity can be called *varying elements*. The two types of dissimilarity are the logical basis for there being two, but only two, criteria for the types in a typological series being placed in the right order so that they form a chronological sequence. These can suitably be called *criteria of continuity* (Malmer 1962:53).

### *The first criterion of continuity*

If the types in a typological series differ from each other in that constant typological elements successively fall away and are replaced by others, then we have *the first criterion of continuity*. If the types in the series are designated by figures and the typological elements with letters, the series could then have the following appearance:

- 1) A + B + C + D + E
- 2) B + C + D + E + F
- 3) C + D + E + F + G
- 4) D + E + F + G + H

### *The second criterion of continuity*

With the *second criterion of continuity*, one or more elements show a qualitative variation. In the following series B and E are varying and the others constant elements:



- 1)  $A + B_1 + C + D + E_1$
- 2)  $A + B_2 + C + D + E_2$
- 3)  $A + B_3 + C + D + E_3$
- 4)  $A + B_4 + C + D + E_4$
- 5)  $A + B_5 + C + D + E_5$

The continuity criterion here consists in all the varying elements varying in a *regular* manner. If we conceive the element as being represented by a graph, the following variations can be regarded as regular: 1) a steadily rising curve, 2) a steadily falling curve, 3) a curve that first rises and then falls, 4) a curve that first falls and then rises. Every other variation is to be regarded as irregular, and means that the continuity criterion is not satisfied. A change in pace, on the other hand, is compatible with regular variation, e.g. with the curve first rising slowly and then more rapidly, to resume, finally, the slower rate of rise. Obviously it is also compatible with regular variation for one element to be in a rising curve at the same time as another is in a falling. That regular variation is a criterion of continuity is only a special case of material research's basic assumption that, as Sophus Müller says "there is order and organization in the world", that people preferably repeat what they have done before, with little or no change, and that, to put it briefly, it is more probable that two physically similar artifacts are close to each other in time than two that are physically dissimilar. From an irregular variation no conclusions whatsoever can be drawn, but a regular variation must involve a connection of some kind, either in time or space, or probably both.

Clearly the first criterion of continuity is applicable more often than the second. The existence or non-existence of a certain typological element can, after all, always be shown objectively but it is by no means certain that a qualitative variation can be objectively recorded. We can observe, for instance, impressionistically, that a certain detail or ornament, a certain decorative element, gives in a few types an aesthetically good effect but in other types a less good one, but it can be enormously difficult to record this qualitative variation objectively. What qualitatively varying elements permit objective registration, and thus make applicable the second criterion of continuity, must be tested from case to case. One rule, however, can be set up, namely that these objectively recordable varying elements are all such as can be expressed in figures. The great majority of them are to be found in the groups of typological element that we called proportional elements and technical elements.

### *Independent typological elements*

The value of the continuity criteria as evidence depends on the number of typological elements. If in a type 1 only two constant elements can be demonstrated and only one of these is to be found in type 2, the second being replaced by a new, then the first criterion of continuity has not great value, in this case. If, on the other hand, in type 1 there can be demonstrated 200 constant elements, 175 of which can be shown in type 2 and 150 in type 3, with 25 new elements appearing in types 2 and 3 respectively, then, in this case, the first criterion of continuity is equivalent to logically conclusive evidence. If, in another series, the first criterion of continuity is not applicable and the second criterion is applicable only to a single varying element, then its value as evidence is slight. If, on the other hand, there are 10 varying elements, all varying in a regular manner, then the probability of continuity is great.

For this numerical calculation of the value of the continuity criteria as evidence to be valid, it is necessary, however, to have a satisfactory unit. We may think that "1 typological element" would be excellent, but this is not the case. A closer analysis shows that the typological elements of one and the same artifact are dependent on each other to varying degrees. Some entail the existence of others, some are dependent on each other to a lesser extent, and others are completely independent. The value of the continuity criteria depends on the number of such *independent typological elements*.

The independent elements and the problems surrounding them are of decisive importance for all artifact research, but in spite of this do not seem even to have been noticed in the earlier literature (Malmer 1962:54 f.). Arne Furumark however has observed that one and the same object can belong "to several different typological series", e.g. in respect of the "shape of the lug, that of the neck and that of the foot", but he has not studied to what extent these series are independent of each other (Furumark 1950:181 f.).

Some examples of the internal relationships of dependence of the typological elements can be given. The two elements "comb ornament" and "coarse comb stamp" obviously imply each other's existence. The decorative element "chevron" and the technical element "comb stamp" are strongly dependent on each other, as chevrons are executed most easily – and therefore most of them – with a comb stamp, but the two do not definitely entail each others' existence, as a chevron can also be executed with other instruments, such as thread, and a comb stamp can be used for other ornamentation than chevrons. Far more independent of each other are a proportional element such as the height of the vessel and a decorative element such as a chevron, although they are not in all

probability entirely independent as it is conceivable that there are both aesthetic and practical reasons for using chevrons preferably as decoration on the walls of vessels of a certain height. Against all these elements that are in some degree dependent on each other, there exist, however, a large number that are entirely independent. We can take, for instance, the proportion index of a vessel (defined as the greatest diameter, expressed as a percentage of the height) and the fineness of the comb stamp. Or the drilling technique used in the shaft-holes of stone axes, and the presence of plastic ornamental borders. Or the arsenic content of bronze artifacts, and spiral ornamentation. Or the damascene technique on iron swords and the ornamentation of the hilts.

How far the typological elements are dependent on each other must be considered from case to case. One general rule, however, can be deduced from the examples given here, namely that it is the manner of manufacture that decides dependence or otherwise: elements belonging to the same phase of work can be dependent, the others cannot. A potter who makes a vessel with a certain proportional relationship between diameter and height is not obliged, when he later comes to decorate it, to use just the comb stamp. If he does, it is probably because he has seen another potter combine these two elements, the proportional relationship and comb ornamentation. If then, we have two types of earthenware vessel, both showing the same proportion index and both with comb ornamentation, then the combination of these two independent elements is a criterion of contemporaneousness or continuity: we have the right to conclude that the persons who manufactured and used the two types of earthenware vessel lived in the same or almost the same historical situation. But let us take another example. The chevron is such a simple form of ornamentation that people discovered it independently in many different parts of the world. A potter who is to execute a chevron has several different instruments to hand but the comb stamp is the most practical. It can therefore happen that he chooses the comb stamp, in spite of the fact that the potter whose work he is imitating used a thread stamp, while a potter, on the other hand, who lived a century previously used a comb stamp. The two partly dependent elements chevron and comb stamp are thus not unconditional criteria of contemporaneousness or continuity.

Since it is the manner of manufacture that decides the dependence or otherwise of the elements, it is clear that the group of technical elements is particularly rich in independent elements. This is good reason for material research to employ itself with technical elements to a much greater degree than previously. So far as the decorative elements are concerned, although these have usually attracted the greatest interest, it is clear that an object's ornamentation is provided

as a single phase of work: all the details have a certain effect on one another. In spite of all the differences in cultural level and historical situation there exists a long line of general aesthetic laws. On the other hand there are many ornamental details so special that no aesthetic law can dictate them as a consequence of another, more trivial detail. The study of such peculiar details of ornament, which comprise independent typological elements, is one of the main objects of the present study.

### *Closed finds*

Of the three kinds of similarity, physical similarity, similarity of find combinations, and chorological similarity, we have so far concerned ourselves mainly with the first. According to the line of research that has dominated modern archaeology, the study of similarity of find combinations, i.e. the closed finds, is far more important. We have already, in another context, quoted Sophus Müller's contrast between "hazardous comparisons of form" and "safe observations of find combinations" (Müller 1884:182). This ill-considered statement has unfortunately been better remembered than many of that great scholar's most important contributions. It is enough to quote two examples from the long succession of statements in the same spirit. Forssander wrote (1933:33):

Used on archaeological material, the typological method primarily offers only a grouping of the material, but not any grading according to date ... Nearly secure chronological results can only be gained from a material ordered according to closed find combinations.

And according to a statement by Gjessing in 1946:

... one can never acquire anything like reliable evidence of a chronological nature from a purely typological investigation. Chronology should therefore today as in the time of C.J. Thomsen be built up on find combinations

The scholars who so emphatically emphasise the value as evidence of the closed finds are victims of a strange delusion that without doubt has its roots in a predilection for the concrete. It is argued, for instance, as follows: "This scholar, who is interested in typology, finds similarities between the sword hilt type A and fibula type B, and therefore suggests that the two types are more or less contemporary. It may be that he is right, but this is after all only a hypothesis, and soon someone will put forward another hypothesis. I, on the other hand, have myself studied the excellent grave in the parish of X, where I found a sword that

I call M and a fibula that I call N. Consequently, *I know* that swords of the M type and fibulas of the N type are contemporary.” The error lies of course in the very last phase of the argument. What the find combination researcher *knows* is in fact only that the sword and fibula he found in the parish of X are contemporary. This circumstance is in itself of no interest: it only becomes interesting when he can generalise and say that swords of the M type are contemporaneous with fibulas of the N type. But there are not two identical swords and two identical fibulas, and when the find combination researcher selects the sword that, together with the example from the parish of X, is to belong to the M type he uses *exactly the same method* as the scholar concerned with typology.

That there is logically no difference between typological dating and find combination dating emerges perhaps most clearly if we recollect that the method in both cases is based on the independent typological elements. If we designate the closed finds with Roman numerals, the types with Arabic numerals, and the mutually independent typological elements with letters, then a series of three closed finds that permits chronological conclusions can be written in the following way:

- I: 1) A + B + C + D; 2) G + H + J + K  
 II: 2) G + H + J + K; 3) M + N + O + P  
 III: 3) M + N + O + P; 4) R + S + T + U

With this can be compared a typological series, which can be written in the following way, if we still designate the types with Arabic numerals and the independent, constant typological elements with letters (but preferably *other* types and elements than in the preceding example):

- 1) A + B + C + D + E + F + G + H  
 2) E + F + G + H + J + K + L + M  
 3) J + K + L + M + N + O + P + Q

In the first example, each closed find in the chronological sequence has eight mutually independent, constant typological elements. In the second example, each type in the chronological sequence also has eight mutually independent, constant typological elements. This means that the two series quoted, the find combination series and the typological series, have exactly the same value as evidence. That the independent typological elements in the find combination series are divided over two objects is of no importance for their value as evidence. The type definitions “1”, “2”, “3” and “4” cannot after all contain any-

thing but a selection of each type's "stock" of independent typological elements, and thus have no value as evidence over and above the total stock of independent elements. The entire evidence rests, let us repeat, on the independent typological elements, and its strength can in no way be increased or decreased by giving the higher-ranking concepts a greater or lesser scope or by naming them groups, types or closed finds.

That this view is correct, and that the find combination researchers' maintenance of their method's exclusive excellence lacks logical foundation, is easily demonstrated by a concrete example. The three Swedish gold collars from Karleby, Möne and Torslunda are all stray finds, but no find combination researcher has dared suggest that they cannot therefore be dated in relation to each other. This in spite of the fact that when it comes to commenting the methods of material research it is boldly stated that a typological study can "never" give certain results. All scholars consider it proven that the three gold collars are closely contemporary, and the basis of their conviction is the great number of independent typological elements shown by these collars. Against this example we can set another. From the late Stone Age in Scandinavia we have a large number of flint hoards, each containing axes of different kinds, often in very large numbers. If the methodological thesis of the find combination school had any truth in it, this would provide a chronologically very important material, which it would be very easy to date. In actual fact the many depot finds of flint axes have for decades lain undated in our museums, one reason being the scarcity of independent typological elements. Another reason is that the few objectively recordable typological elements the axes do show have not been utilised, because of these scholars' lack of attention to methodology and above all their ignorance of the concept "independent typological element" (cf. Malmer 1962:342 ff.).

The great chronological value of the closed finds lies not in their permitting another or better method than other finds but in the fact that the combination of several objects means an increase in the number of independent typological elements. The closed finds also have two disadvantages, however. One is that a certain carelessness, in the investigation of the find in modern times can lead to confusion and jeopardise its value as evidence. The other is that objects of very different age can have been combined when the find was deposited. An individual object has rarely as many independent typological elements as a closed find, but on the other hand all its elements are unquestionably contemporary.

A further disadvantage of closed finds is their relative rarity. A scholar determined to rely solely on closed finds is therefore easily tempted to draw conclusions from a single find in which two types are found together. But even if a find

seems reliable, it can suffer from the two sorts of fault mentioned above. It is therefore necessary here as everywhere else to follow the laws of the probability calculus, and judge contemporaneousness between two types as probable only after they have been found together in several different closed finds.

### *Stratigraphic finds*

Stratigraphic finds are a special case of similarity of find combinations, and without doubt a very remarkable one. It is only natural that those scholars who put their trust in closed finds also are accustomed to cite stratigraphy as the primary support of chronology, along with find combinations. "With the aid of stratigraphy, the results can become unquestionable" says Forssander, for instance (1933:33).

In a stratigraphic find, two types or two find combinations are found under such circumstances as show that they cannot be contemporaneous. It is thus a question of a negation, a dissimilarity of find combinations. Theoretically one can also determine which of the finds is the elder, but seldom or never *how much* older it is.

A stratigraphic find which shows that object A is older than object B is in itself of no interest. Analogously with the argument that applies to closed finds, the situation becomes interesting first when one can generalise and say that objects of the A type are older than objects of the B type. But the selection of objects belonging to type A or to type B can only be performed typologically, and as a result the scholars who consider they are putting their faith solely in stratigraphic finds are in actual fact using the typological method that is the essence of material research.

Of the stratigraphic finds it can be said, as we said of the closed finds, that it is against the fundamental principles of the probability calculus to consider a single find as perfect evidence. A high degree of probability is achieved only when the same stratigraphy has been repeated in several different finds.

But even considering these limitations in the value of stratigraphic finds as evidence, we can easily understand the enthusiasm with which many are seized when faced with the idea of stratigraphic finds. For chronological research it must seem extremely attractive to have at one's disposal finds in which the artifacts are from the beginning arranged as it were in a system of shelves, with the oldest furthest down, and younger the higher up one comes. Unfortunately it must be stated that the idea of such finds, at least so far as Northern Europe is concerned, is a fair dream, with little counterpart in reality. Stratigraphic finds are rare, so rare that one seldom has a theoretical chance of being able to meet

the same stratigraphy repeated in several finds. When stratigraphic finds *are* made they are usually so complicated that their evidence is very much a matter of debate. Finally, in the few cases where there is a clear stratigraphy, the time interval between the strata in the finds is usually so slight that the gain for chronological research is insignificant or non-existent.

As an example of the slight practical importance of stratigraphy we can take the monograph by Forssander, from which the statement just quoted is taken, and in which he praises stratigraphy as the most outstanding of chronological aids. In this monograph, Forssander in reality uses a stratigraphic description in *one case only*. This is a famous burial mound in Peissen in Saxony-Thuringia with two inhumation graves, one upper and one lower. From this single find, Forssander draws far-reaching conclusions, thus offending against the rule of the probability calculus that says that the same stratigraphy must be repeated in several finds if it is to have any value as evidence. Even more remarkable is that Forssander considers the *upper* grave to be the older. The latest research has come to the more natural result that the *lower* grave is the older (Forssander 1933:138; Malmer 1962:865 ff.). Forssander's bizarre view that the upper grave was the older was derived from (non-binding) typological evidence, so that there is a long step between theory and practice in his work. The same can be said of all other scholars who claim not to use typological methods, but instead rely solely on closed finds and stratigraphy. Typology is the central pillar of material research, and must quite clearly be used by all.

### *Natural scientific aids*

Dating by natural scientific aids such as geochronology, pollen analysis, dendrochronology and radiocarbon analysis falls methodologically under "similarity of find combinations". The outstanding value of these methods is due to their increasing the number of independent typological elements. The content of radioactive coal, for instance, in a spear shaft found in a bog is a typological element that is quite independent of the breadth of the spear tip or its ornamentation.

We can say of this type of find association exactly what we said before of closed finds in the narrow sense and of stratigraphic finds, namely that it is in itself without interest that spearhead A was found together with a piece of wood of a certain, determined radioactivity. The circumstance becomes meaningful first when spearhead A is classified as a certain type, a procedure that falls within the central phase of material research, typology. It is typology that makes material research meaningful. Without it there would be no archaeology.



## Summary of the basic concepts of material research

Material research has to study the internal similarities of artifacts. The similarity can be of three kinds, namely physical similarity, similarity of find combinations, and chorological similarity. In the case of physical similarity, the first task of material research is to study the artifacts' typological elements. It is necessary to determine a) what typological elements are objectively recordable and b) which of these elements are independent of each other. The material of artifacts has to be divided into types with the help of logically correct verbal definitions, based exclusively on objectively recordable and recorded typological elements. The types are arranged in time sequence with the help of typological series, the correctness of which is confirmed by the two criteria of continuity, or with the aid of find combinations. In both cases the probability that the chronology discovered is correct depends on the number of independent typological elements.

Physical similarity entails a probability of every other form of similarity, i.e. similarity in respect of time, use, name and environment. A correctly defined type corresponds to a concrete historical situation.

### *Some basic archaeological problems*

The object of the present work is to study a number of problems of method in the history of Iron Age art, against the background of the presentation given above of the basic concepts of material research. As a further introduction to the main theme of the study it can be in place, however, to sketch in brief certain basic archaeological problems.

Those scholars who have claimed to be able to work without the aid of typology have often objected to the typological series on the grounds that one cannot know which type is oldest and which is youngest. Against this we can argue, first of all, that exactly the same criticism can be directed against a series of find combinations – quite naturally so, since the typological series and the find combination series have the same logical structure.

### *The direction of the typological series*

The main tool in finding out the chronological direction of a series is naturally to utilise points in the chronology that are already known. In a series of bronze objects we can theoretically conceive that iron appears from a certain point and persists to one end of the series. Since there is good authority that the Iron Age

follows the Bronze Age it is more than probable that the part of the series that contains iron is the younger. It is then a matter of indifference whether the iron appears in the form of independent objects, i.e. it is a question of a find combination series, or whether it appears in close association with the bronze on one and the same object, e.g. in the form of ornamental inlay, which would indicate a typological series. Let us take another example. At a certain point a series of bronze objects develops spiral ornamentation. The place of such ornamentation in Bronze Age chronology is well known, so that irrespective of whether it is a find combination series or a typological series, we know in which direction the series runs. Or let us take a third example. Radiocarbon analysis has shown that the content of radioactive carbon at one point in a series is lesser and at another point greater. We then know the direction of the series, as the former point must lie before the latter. And it is a matter of complete indifference whether the piece of wood from which the radiocarbon sample was taken was attached to a metal part rich in typological elements, which means a typological series, or whether the metal part and the wood part were found separately, which means a find combination series.

Such fixed chronological pegs, of which modern archaeology has a great many at its disposal, make it possible in most cases to judge the direction of series. Quite often it is also useful to study the genesis of a type, of which more below.

### *Division into periods*

The series – typological or find combination – of well defined types is in itself a good chronological division. But to attain a more handy chronology one needs a division into periods.

A good introduction to discussion of the theory of period division is provided by the following quotation (Almgren 1958:26):

Typological dating is based, among other things, on the idea of a gradual progression and a change that takes place with the steady implacability of a natural law, while division into periods requires a sufficient number of similar finds, corresponding to a stabilised manufacture – without changes – in the forms of a certain period. Logically, these methods would seem to cancel each other out.

The truth of this statement seems about the same as if we were to say: “It is logically unreasonable to say that the time is now 12 minutes past 10, as time never stands still and the hands of the clock are continuously going round.” The

opinion on the theory of period division given in the quotation has, however, strong traditions in Scandinavian archaeology and is undoubtedly connected to the persistent belief in the a priori existence of types.

When Montelius sets up his system for the division into periods of the Bronze Age his procedure is to indicate all the most outstanding types that belong to a certain period: a certain type of axe, a certain type of fibula, a certain type of belt ornament and so on (Montelius 1885:82 f.). But it is impossible that the production of all these types began simultaneously. Nor can it have ceased simultaneously. This means, briefly, that Montelius' periods are not clearly defined. But concepts that are not clearly defined have no place in the objective application of evidence. The reason Montelius fails is that he has tried in his system to incorporate two incompatible things, namely a time scale and a concept of what objects the people of the Bronze Age themselves considered as belonging together. With his belief that he had "discovered" objects and combination types that were existent a priori, Montelius thought he could avoid having to formulate a logically correct definition, and in this way committed the same mistake as countless archaeologists after him.

The only correct way of defining a relative chronological period is to state that it begins with the appearance in a certain area of a certain typological element, or a certain type (and that it ends with the beginning of the next period). If it can be so arranged that the period-defining elements or types belong to one and the same typological series, then the system of period division will have gained greatly in clarity (cf. Malmer 1962:89).

### *Relation between relative and absolute chronology*

Relative chronology has typological series and find combination series as its "building material", these being assembled in a logical sequence in which nothing, however, is known of the length of the individual "bricks". Absolute chronology, on the other hand, consists of a succession of time data, gained via written sources or radiocarbon analysis, and lacking internal connection. When it comes to combining absolute and relative chronology, we can fit in between a pair of absolute chronological points the pertinent relative chronological units – elements, types or periods. But it is uncertain whether such an interpolation is anywhere near correct.

There is, however, a method that imparts greater accuracy to the interpolation procedure, and that is to study the find quantities. If we assume that a certain population of a certain constant size has a constant requirement of a certain type, and that this type is deposited in the earth (e.g. in graves) in accordance

with a certain constant custom, and finally that the find conditions for this type in modern times are always similar, we can draw the conclusion that the find quantities in the groups into which the type in question can be divided are directly proportional to the periods of time during which the groups were produced. Such circumstances are presented most easily graphically, in the form of what we can call production diagrams (Malmer 1962:93 ff.).

### *Genesis of new types*

It has often been suggested, in criticism of the champions of typology, that the latter assume a gradual course of events, with the steady implacable progress of natural law, implying the possibility of setting up a typological series stretching from the beginning of palaeolithic time right up to our own day. This is obviously not the case. Both find combination series and typological series have a limited length, and relative chronology must be made up of numerous different series. A typological series consists, after all, simply of a number of types, the number of points of similarity (i.e. independent typological elements) between each link being sufficient for us to have logical grounds for postulating the probability of a connection. A break in a typological series occurs every time the number of points of similarity sinks below a certain minimum.

We can define the concept “genesis of a new type” by saying that it is a point in a typological series at which the points of similarity between the groups arranged in time sequence are few, possibly so few that we are not certain whether there is continuity or not. What, then, is the cause of such points, where development proceeds rapidly, where “new types occur”?

According to one remarkable theory a new type occurs by the degeneration of an older. “Through repeated copying of a given weapon design [...] craftsmen gradually lose interest” it is said (Forssander 1933:33, cf. 83): the details begin to be carelessly executed, misunderstood, and suddenly the new type is there. This is impossible. New types must in former days as now have been created not by bad craftsmen but by good, by men who were well aware of their artistic means of expression (Malmer 1962:133; cf. Ringbom 1938). Badly executed examples are certainly more common towards the end of a type’s period of production, but this is because the type is then produced mainly by plagiarists, while the good craftsmen have already gone over to producing a new type.

The continuity, in both typological and find combination series, depends on the average person’s tendency to repeat what they themselves or someone else has previously done. The breaks in the typological series, on the other hand, the genesis of new types, depends on the original minds, the creative artists.

### *Relation of the type to the concrete prehistoric situation*

The situation that led to the creation of a new type may have been experimented by a whole society, but the actual creation of the new type was without doubt regularly the work of an individual personality. Even in the case of extremely simple artifacts it is certainly no exaggeration to speak of a creative artist. All our experience tells us that the prehistoric, primitive society was extremely conservative, and the pressure of convention on the craftsman was thus extremely strong.

It has been stressed above that the object and meaning of the archaeological type is to reflect concrete prehistoric situations. The type fulfils this task in two respects. In general, the type mirrors the broad, essential features of the behaviour of a large group of persons during a certain period. But the types that suddenly appear reflect also by their very creation the essential contributions of important – although anonymous – individuals.

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## CHAPTER 2

# Archaeological positivism

1984

“Yes, there are gifted men,” said Nikolai Nikolayevich; “the fashion nowadays is all for groups and societies of every sort. Gregariousness is always the refuge for mediocrities, whether they swear by Solovyov or Kant or Marx”.

This quotation from Pasternak’s *Doctor Zhivago* may possibly be justified by the circumstance that the first of the thinkers named in it, Vladimir Sergeyevich Solovyov, in his doctoral dissertation from 1874, *The Crisis of Western Philosophy*, opposed positivism. But the quotation is relevant today in another, more important sense. Groups and societies may not be in fashion nowadays, but *words* are all the more so. There is a risk in not using the right word with the right frequency, as is evident from *Fornvännens*’ otherwise so insightful and balanced discussion of the theory and data of archaeology (Johansen 1979; 1982; 1983; Malmer 1980; Christophersen 1982; Herschend 1982; Welinder 1982; Furingsten 1983). One debater “finds it odd that Malmer is ironical about theoretically oriented works, since he [...] in several of his earlier works [...] urges the reader to be theoretically aware” (Furingsten 1983:119).

I have never been ironical about theoretically oriented works. I have expressed myself ironically about the fact that “theory” has become a trendy word, the mere mention of which seems to be perceived by many as a guarantee that what is said is the height of scholarship in its time. The word “theory” is pronounced or written as if it were a magical or incantatory formula.

A prominent archaeological colleague, in just one essay, uses the words “theory” or “theoretical” sixteen times in the first half-page; it has the effect of a Tibetan prayer wheel.

Many people seem to entertain the notion that concepts such as “theory”, “deduction”, “hypothesis” “model”, “falsification” were not common before the words became so. This notion illustrates the power of language over thought. The concepts have in fact existed in archaeology, and not just implicitly (to use a new and rather good word), but explicitly, although clad in a different linguistic style. Listen, for example, to Ch. J. Thomsen (1836:57):

Our collections are [...] still too new and our experiences too few for us, in most cases, to draw conclusions with any certainty. What we wish to put forward here must therefore be regarded solely as assumptions which, when these objects have received the attention of more observers, will surely be better elucidated and either be confirmed or corrected.

In modern archaeological language, the words translated here as “assumption”, “confirm” and “correct” would be “hypothesis”, “verify” and “falsify”. But the idea is no different from what it was 150 years ago.

Let us take a much more recent example of how quickly language can change. In a dissertation that is modern in every good sense of the word, we read: “The purpose of the dissertation is to give a practical example of a deductive research method. That is to say, that one develops the clearest possible model before the analysis begins” (Johansen 1978:9). I had precisely the same intention in a dissertation fifteen years previously. And in the model from which I proceeded, the first appearance in Sweden of the Swedish-Norwegian Battle Axe Culture was perceived as a result of an immigration. This model – or hypothesis – then had to be verified or falsified. When I wanted to start that work, it turned out that both material and tools were lacking. Data, concepts, and methods – everything was inadequate in my opinion. Inadequate to enable a true test of the hypothesis. My attempts to obtain data, concepts, and methods which were fit to work with filled most of the book. The result was an approach that did not differ in principle from today’s. Only the *words*, the terminology differed. Instead of the topical word “theory” I used (in accordance with the language of the time) “method”. Terms such as “deduction”, “verification”, and “model” were often replaced with words of my own manufacture; a particular form of model of chronology/settlement history, for example, was called “production diagram”.

The different-sounding words, of course, were no obstacle. It was possible to perform the work of attempting to verify or falsify the immigration hypothesis (Malmer 1962:677–878). The verification was a sluggish process. The falsification, in my view, went swimmingly. The immigration hypothesis proved to be unreasonable from the point of view of chorology, typology, and chronology. When the immigration hypothesis had thus been falsified, a new hypothesis then had to be formulated. In broad outline, the form it took was that the Funnel Beaker Culture and the Battle Axe Culture are two stages in the economic, social, and cultural development of one and the same south Scandinavian farming culture, and consequently cannot be contemporary in any single region.

This hypothesis of mine has now been subject to twenty years of attempts at



falsification in the form of radiometric datings. The falsification has not yet been successful: in radiocarbon years the Battle Axe Culture is always younger than the Funnel Beaker Culture in the same area.

What I have said hitherto ought to be enough to show that there are fashionable words in archaeology, words with high status, which are used to signal that one's research is of high quality. But there are also words with a kind of inverted status, which are used to brand a supposed or real opponent's research, in the quickest possible way, as hopelessly antiquated, logically unreasonable, and morally suspect. The word *positivism* now satisfies the exacting demands for infamy of this kind. A professional philosopher can speak, half jokingly, of "the dirty word positivism" (Halldén 1980:104). A social scientist points out that, when positivism is discussed, none of the debaters is willing to accept that he himself might be a positivist (Lindholm 1980:88). A Marxist archaeologist presupposes that "bourgeois positivism" is to be "demolished" (Christophersen 1982:144). The specifically archaeological term "find positivism", probably coined by Carl-Axel Moberg (1978:226), is one I have never seen used in anything but a derogatory sense. That there are nevertheless scholars who have a different view of positivism will become clear from what follows.

It is not easy to obtain a clear perception of what positivism actually stands for. Positivism, it has been pointed out, is a term that has been used and abused to such an extent that it has almost become unusable. In simpler styles of preaching, positivism has been allowed to represent whatever a person most detests in scholarship (Lindholm 1980:88). Before we attempt a somewhat broader orientation, however, there is good reason to emphasize an important feature in the positivist theory of science, namely, the thesis that a theory-neutral observation language exists (Gärdenfors 1980:77 ff.; Johansson & Liedman 1981:8, 63). This thesis, or rather the categorical denial of its validity, has played quite an important role in the theoretical discussion in archaeology. Axel Christophersen claims (1982:144) that "no primary data can be observed independently of, and therefore does not exist independently of the problem or problems that activated the source material in the first instance". And Arne B. Johansen says (1974:85), in even sharper terms, that "an existing body of material can never contain information over and above the ideas according to which it was collected".

Since I am responsible for the publication of a great deal of archaeological material and a large amount of data, when I consider declarations like those just cited, I must ask myself whether this material and these data lack information value for archaeologists who work according to other ideas than those which were relevant at the time of collection. Indeed, since my own world of ideas has

undoubtedly changed gradually, perhaps my publications of material are now of no interest even to myself?

I once undertook to measure the diameter of gold bracteates, or rather the diameter of the picture die. This may seem like a foolish enterprise, since almost everything about gold bracteates ought to be more interesting than the diameter. The purpose of the measurements, however, was to try to verify – or falsify – one of my main archaeological theses, namely that the farming culture in Scandinavia was polarized between the richer Denmark and the poorer central Sweden and southern Norway. The result of the measurements (Malmer 1963:206) was as follows, expressed in median values for different areas (Group C:II):

Jutland	23 mm
Danish islands	22
Skåne-Blekinge	20
Eastern Sweden	19
Gotland	18
Western Sweden	16
Southern Norway	15

From this steadily falling series I drew the conclusion that my thesis had been verified: Denmark was richer and the more northerly areas were poorer.

Since then, however, the manufacturing technique of the gold bracteates has been studied (Arrhenius 1975:102 ff., 106) and it has been found that the stamps were repeatedly copied by means of clay casts, which shrink slightly as they dry; hence the gradually decreasing diameter. A hypothesis like this, which now seems probable to me, never occurred to me in 1963.

The undeniable conclusion seems to be that the information value of data is *not* directly dependent on the idea or theory according to which they were collected. Data actually have intrinsic value, quite independently of the underlying theory. Data collected to test one hypothesis can often very well be used to test a completely different hypothesis.

Since this point is so important – in terms of theory just as much as for museums and research policy – there may be good reason to examine yet another case, of a somewhat different nature.

Arne B. Johansen discusses in one context (1982:218 f.) the term “artefact”:

What do we actually mean by this? – An object which has had its form modified by humans? But this then rules out all the objects that were of significance for people but were not modified in form – at least not so that we are able to detect it. My idea of the past is

that I can easily imagine a number of such objects with varying degrees of significance – and quality. This means that such objects should also be retrieved, for example, during the excavation of a settlement site, and analysed afterwards. [...] But how would Malmer be able to find them at Alvastra? I do not believe that his energetic desire to investigate and publish “in such a way that all now living and future researchers will be able, to the greatest possible extent, to obtain answers to the questions they ask about the material, no matter what line of interest they represent” would be of much help to him. I believe, for example, that people had large quantities of easily accessible throwing stones on or near their settlement sites. – Has Malmer looked at this, and has he collected them so that I can come to Stockholm and study them? [...] I believe that [...] the Alvastra people collected throwing stones varying greatly in size, weight, and shape. Does Malmer believe the same? – If he does not, then nor has he seen the stones that I need.

I must admit that the thought of stones for throwing did not occur to me. It is a first-rate and stimulating thought – but unfortunately, I did not think it. On the other hand, during the excavations by the Alvastra project *every* stone in the trench was three-dimensionally plotted and – whether worked or unworked – was kept. All the stones can be studied in the stores of the History Museum in Stockholm. The theory behind this method is as follows. Stones do not occur naturally in the Alvastra bog. This situation is as rare as it is favourable for Stone Age research. All the stones in the Alvastra pile dwelling are assuredly a trace of human action and human thought. Consequently, all stone should be kept, with adequate information about the find, so that it can be studied in more detail than what is possible or financially defensible to do in the field.

The first case discussed here – the diameter of the images on gold bracteates – concerned the collection of data in a corpus of museum material. The second case concerned the extremely critical element that is specific to archaeology, that of collecting data during a field investigation. In both cases it has been possible to register data that can be used to test – to verify or falsify – hypotheses that were unknown or not yet formulated at the time of collection. Data thus have an indisputable potential as knowledge regardless of the theory or model by which the collection took place.

Of course it is not possible to collect data in such a way that all researchers, now and in the future, will be able to test all their hypotheses, but that is a goal to strive for. A scholarly goal, because the stock of antiquities has a certain, steadily declining volume (especially, of course, when it comes to such a rare phenomenon as the Alvastra pile dwelling). And an economic goal, since financial resources are limited as well: excavating is cheaper if data can be used by a large number of researchers with different orientations. Cheaper than if – as my opponents claim – data have no information value over and above the more or less intelligent ideas that animated the excavator on the site, and con-

sequently if he collected only data that were interesting for his own research trend.

I thus maintain that, as long as we are in the archaeological field, there is a theory-neutral knowledge potential and in reality also, no doubt, a theory-neutral observation language. Whether this declaration brands me as a positivist is not something I can judge, especially in view of the imprecise character the word has acquired in recent years' discussion. But there may be good reason for a quick look at the history of philosophy, to see whether positivism really deserves the universal scorn that it is now fashionable to heap on it.

The founder of positivism, as we know, is Auguste Comte (1798–1857). According to him, the goal of science is to predict the future with the aid of the past and the present (Nyman 1948:453). "Positive" means the *real* as opposed to the imagined; science should thus devote itself to subjects that are accessible to our intellect, not to metaphysics. "Positive" means furthermore, what is *useful* as opposed to what is useless, what is *certain* as opposed to what is uncertain, and what is *precise* as opposed to what is *vague*. "Positive", finally, is the opposite of negative. Positivism has the goal of *organizing* instead of destroying (Lindholm 1980:89; Næss 1962:230 ff.). Archaeologists, moreover, have reason to observe especially that Comte criticized contemporary historical research for attaching greater importance to the particular than to the general and thus bringing about "the incoherent accumulation of facts which is unfairly called history". Positivists insisted that only the common, the regular, the ubiquitous can say anything about basic human conditions (Liedman 1978:16 f.). I believe that most archaeologists share that view.

Positivism is one of rather few philosophical orientations that have really provoked a broad engagement. In Stockholm in 1879, a "Positivist Society" was founded, modelled on a French equivalent. Its general aim was to teach people to "live for others"; more concrete goals were the elevation of the working class, the socialization of capital, universal suffrage, the separation of church and state, and the settlement of political disputes by arbitration (Nesselmark 1952:160 f.).

It is thus not easy to understand why Axel Christophersen (1982:144) and his sympathizers insist that "the positivist research ideal" must be "broken down". But perhaps it is not classical positivism that is the target, but neopositivism? This trend (also called logical positivism or logical empiricism) was founded in the 1920s, between the two world wars, by a group of philosophers and scientists in Vienna, of whom we may mention Moritz Schlick and Rudolf Carnap; one can also probably add Bertrand Russell and (the early) Ludwig Wittgenstein. An important characteristic of the neopositivists was the sharp distinction

between science and metaphysics, including the kind of metaphysics that was associated with fascism and national socialism. This brave stance had its inevitable consequences. The philosophers suffered greatly from Nazi persecution. Schlick was murdered by a student in 1936, others died in concentration camps, and Carnap managed to escape to the USA (von Wright 1957:21 f.).

Logical positivism is described in detail in easily accessible handbooks (besides those already cited, we may mention Andersson 1979). There is no reason, nor it is possible here, to consider, say, its research ideal in the natural sciences or its doctrine of explanation, abstraction, general validity, and impartiality, or the distinction it makes between facts and values, between emotion and reason, between science and personality, or between scientific and extrascientific knowledge. It is sufficient – once again – to point out the famous mistake of some positivist researchers who believed that it should be possible, with collected data as the sole basis, to arrive at a theory. This is now contested by everyone, for example, from the most authoritative quarter: “Scientific hypotheses and theories are not *derived* from observed facts, but *invented* in order to account for them” (Hempel 1979:23). One may wonder, however, whether the idea of self-explanatory data did not arise in the mind of some theorist of science – I find it very difficult to imagine that a researcher working with a concrete scientific problem would think along those lines. It will suffice – as regards archaeology – to cite as early an example as Sven Nilsson’s *The Primitive Inhabitants of the Scandinavian North, an Essay on Comparative Ethnography* (Swedish original 1838). As the title alone shows, Sven Nilsson acquires an explanatory hypothesis with the aid of ethnographical material from Indians and Eskimos.

It cannot be said that positivism, or neopositivism, *in all essentials*, deserves study, appreciation, and acceptance. For it is truly essential to realize that all hypotheses are *invented* by the researcher to explain observed data, not derived from data. This is precisely what is meant by explanation: to see a particular object of study in comparison with and in the light of something else. This is so self-evident that it is understood by all researchers – except possibly by theorists of science who have got lost in their own logic (cf. Folin 1983). But apart from this strange error, positivism – whether new or old – seems intellectually and morally attractive, and not at all deserving of the contempt expressed so emphatically, for example, by some younger archaeologists.

A truly serious consequence of the scorn for positivism is, as I have already pointed out (Malmer 1980:260, 263), that some more reckless thinkers have got the idea that all empirical research is now superfluous or harmful. They are bold enough to claim, for example, that “the ‘mission’ of empirical and positivist research has long since been completed in the humanities” (Lagerroth 1982).

Against this, fortunately, there is the acute objection that we should definitively not believe that “humanists need not bother to consult reality” (Liedman 1982), and that, although certain features of positivism can be criticized, “it is a large step from there to sweep out all empirical evidence as suspect dealings” (Frängsmyr 1982:2). Archaeology has been particularly hard hit by the term “find positivism”. The creator of the term no doubt wished to remind us, with the best of intentions, of the benefits of trying to formulate opinions about “the finds”, and testing the former against the latter. But misunderstandings have arisen. Many people have believed that it is now considered unscientific merely to “publish material”, and that at any rate it gives better qualifications to formulate hypotheses, however groundless, than to test them carefully against a sufficiently large sample of critically considered material. Hence the archaeology of the 1970s without antiquities and artefacts. Yet even in archaeology a clearer awareness now seems to be emerging, that our science is empirical (Furingsten 1983:126).

Many readers of the debate in recent years have no doubt seized on the strange state of affairs that there are researchers in archaeology who openly proclaim that they are positivists – and this seemingly without any sharp front line arising between these researchers and the ardent anti-positivists. Stig Welinder thus declares in the introduction to *A Stone Age Economy* that the work is based on “a fundamentally positivistic view on the archaeological research process” (Hulthén & Welinder 1981:7). Klas-Göran Selinge, in his highly readable survey of the New Archeology (1979:40), underlines the “fundamental *positivistic* outlook on the source material” among representatives of this trend. An explicit profession of positivism is found in one of the central works of neo-archaeology (Watson et al. 1971:113):

The logical position of archaeology with respect to the limitations of the archaeological record should be a strongly positivistic one: The information is there, it is the investigator's task to devise means to extract it.

Yet another specification of the meaning that neo-archaeologists ascribe to the term positivism is given in the following quotation (Hill & Evans 1972:252):

The positivist view is that phenomena do *not* have inherent or primary meanings to be discovered. Rather, any phenomenon, or set of phenomena, is assigned meaning by the human mind, and it may be assigned as many different meanings as the investigator chooses to give it.

All New Archeologists claim that, whether one thinks that one can distinguish natural groupings in the material or not, the archaeologist must subjectively determine how he wants to delimit, define, his types, and this appears in fact to be the most important content in what is called the *positivist* model. For my part, I have called precisely the same orientation *rationalist* (Malmer 1963a:20 f., 120 f.; 1967:376). The contrary stance, that it is the researcher's task to note a priori types in the material, has been called, by both New Archeologists and myself, the *empiricist* model (Malmer 1967:376 f.; Hill & Evans 1972:233). "For my part I wholeheartedly profess the rationalist tendency," I wrote then (Malmer 1963b:93), and needless to say I have not changed my opinion (Malmer 1980:263): "The world and the archaeological material have properties, but they do not have any *meaning*. The material does not acquire meaning until the researcher constructs a model." And I have always shared the New Archeologists' assurance of the great knowledge potential of the material: *the information is there*.

The reason why different archaeologists have such remarkably diverse perceptions of what the term "positivism" means is thus obvious. In classical and logical positivism, Hill and other New Archeologists have seized on the praiseworthy features, the optimistic faith in the knowledge potential of the material, and the rational and anti-metaphysical way of working: *this* is what they call "positivism". Arne B. Johansen and Axel Christophersen, on the other hand, have focused on the conspicuous weakness of positivism, the view that the hypotheses are derived from observed facts and that – to put it simply – the material thus explains itself: *this* is what they call "positivism". Of course it is unfortunate that a word as frequently used in the debate as "positivism" should have two completely different meanings. One way to resolve this difficulty would be if the research trend to which, if my survey of the field is correct, the New Archeologists, Arne B. Johansen, Axel Christophersen and I myself all belong, would use the same word that I used twenty years ago: *rationalism*. The research tendency that we all reject is currently known as *empiricism*.

The discussion of the term positivism of course invites a broadening of the scope. Is it correct at all to transfer concepts from other sciences to archaeology? Has the distinctive character of archaeology really been clearly defined? Without a thorough knowledge of one's science, and in the first instance the material it seeks to study, it is impossible to select from the conceptual apparatus of other sciences that which best serves one's own science.

The archaeological field is very long and very wide. It can be compared with historical research: in countries that developed early, the written sources go back 5,000 years in time. But the artefact material of archaeology, very cautious-



ly estimated, goes back one million years. This means that archaeology has 99.5% of the time span of working and creating humans to explore, while history and the other humanities account for 0.5%. But archaeology also has the same breadth as the sum of all other humanistic disciplines, because prehistoric art, religion, geography, economics, and so on, and prehistoric social life are also the responsibility of archaeology to study. (I emphasize, for safety's sake, that there is no implicit evaluation in what I have said. I have only expressed explicitly what both archaeologists and other humanities scholars tend to forget. Cf. Malmer 1971.)

Another important aspect of archaeology is the nature of the material and data. The sciences can be divided into two large groups: on the one hand the natural sciences (including technology, etc.), and on the other hand the humanities (including law, theology, etc.). But where then does archaeology belong?

The characteristic trait of the natural sciences is that their material is absolutely silent. Everyone can understand that if one wants to get something out of this material, one must formulate a hypothesis and test it against the data. Only in this way can one verbalize the material in the natural sciences, express it in signs, symbols, words and sentences.

But the humanistic material – N.B. with the exception of the artefacts of archaeology (and the fine arts, which I must ignore here for the sake of brevity) – consists from the beginning almost exclusively of *words*. The data are already speaking to us, crying out from all directions. Here too, of course, the only way to pursue science is to formulate a hypothesis, to ask a question. Then the confusing flow of words will fall silent, and data will answer our question.

But what characterizes the material of prehistoric archaeology? It is completely mute, just like the material of the natural sciences. Yet it is simultaneously just like the verbal material of the humanities, an expression of human ideas, emotions, and actions. The archaeological material is thus, in fact, of a *third kind*, alongside the natural scientific and the verbal humanistic material:

The material of the natural sciences is mute and non-human.

The material of archaeology is mute and human.

The material of the other humanities is verbal and human.

What does this mean for the way archaeology works? In many cases we can and must, of course, just like the natural sciences and the verbal humanities, formulate a hypothesis, and then collect data to be able to verify or falsify the hypothesis. In many cases, however, this is not sufficient, especially not in the case that is specific for archaeology, namely, data collection through excavation. Our



ability to formulate well-developed and fruitful hypotheses is far lesser than what would be necessary to do justice to a large and complex assemblage of material, which is moreover unknown when the hypothesis is to be formulated.

To formulate a narrow, specific hypothesis before one starts an excavation can sometimes be justified in terms of theory, finance and antiquarian considerations. In other cases, however, a hypothesis of this kind – if one really takes it seriously – can prevent the collection of relevant data.

A fruitful parallel to an archaeological excavation is a crime-scene investigation. It is not enough that the detective arrives at the scene of the crime with the sole hypothesis that a person X is guilty of the deed. The person who investigates the crime scene must have a range of hypotheses which is so rich that it would be almost impossible to express it in words. He must, quite simply, collect facts inductively. He must believe in the possibility of theoryless observation. Or, to put it better: the sole theory of the crime-scene investigator must be: “People have acted here”. And in many archaeological excavations that theory is the correct one, the only possible one. We can put ourselves in the place of the unformulated hypothesis.

Karl Popper tells an amusing story (Magee 1974:32) about “the man who dedicated his life to natural science, wrote down everything he could observe, and bequeathed his priceless collection of observations to the Royal Society to be used as inductive evidence”. And Popper continues:

Twenty-five years ago I tried to bring home the same point to a group of physics students in Vienna by beginning a lecture with the following instructions: “Take pencil and paper; carefully observe, and write down what you have observed!” They asked, of course, *what* I wanted them to observe. Clearly the instruction, “Observe!”, is absurd. [...] Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem.

Popper’s pedagogy is excellent, for he was talking to natural scientists. Field archaeologists (and crime-scene investigators) should note that the documentation of traces of human action in a particular natural setting is selective observation. In other contexts, incidentally, Popper has very modest demands for the degree of specification in the theory that must precede the observation, for example, when he points out that even a child that opens its eyes the first time begins with certain notions about the surrounding world (Holmberg 1983:14). Precisely. The child has nothing but its human faculty of perception, and that is something we retain the whole of our lives, a better scientific instrument than any verbally formulated hypothesis for distinguishing artefacts from nature.

In this essay I have chosen to declare my stance positively, rather than nega-

tively criticizing the texts of my co-debaters. In one case, however, I think I ought to discuss passages in text. Arne B. Johansen (1982:221) has seized on a statement by me, that I “want to work *as if* total objectivity were possible’. Malmer incidentally repeats this double subjunctive on the same page (p. 262). Does this mean that he nevertheless doubts his ability to discover the unexpected?” Axel Christophersen (1982:146) finds my sentence “cryptic”: “What does this mean? Either full objectivity *is* possible, and then one works on the basis of these possibilities, or else objectivity is *not possible*, and then one must accept the consequences of such a recognition.”

Despite Christophersen’s severe reprimand, I must persist. I expected my readers to understand the reference to Hans Vaihinger’s *The philosophy of “As If”* (1911), the major work of fictionalism. Vaihinger – who is usually designated as a positivist – is surely one of the twentieth century’s more interesting philosophers. I regard him as contemporary (he died in 1933), yet he is evidently unknown to my co-debaters.

Many sciences work with fictions, and in everyday life too, fictions play a large part, as anyone will understand after a little reflection. An example of a scientific fiction is the chemical concept of the atom. Many scientists have found it self-contradictory, but as we know, it has been found practical to reason *as if* there were atoms. Fiction differs from hypothesis in that it can be neither verified nor falsified; on the other hand it is warranted (“justified”) by the fact that it is valuable for research, or in many cases downright necessary.

In my article (Malmer 1980:262 f.) what I mean – of course – is that even if complete objectivity cannot be imagined to exist, we ought to work *as if* it did exist, just as society’s laws aim for justice, even though complete justice evidently does not exist.

I have clearly stated the alternative to objectivity in research to which I am primarily opposed: it is the politically coloured subjectivity which characterized Central European archaeology in the 1930s, and which constantly threatens to return, although now often with the opposite political colour. According to Christophersen (1982:142), the central question of archaeology is this: “What do we want knowledge about, and what knowledge do we want?” My answer is clear. We want knowledge about everything: more knowledge about what is crucial and less knowledge about what is banal. I do not believe there will be very much agreement about what is crucial, but if that should be the case it is good. The important thing is that we aim for objectivity, that is to say, that we do not suppress facts that conflict with our political ideology or our archaeological hypotheses.

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## CHAPTER 3

# Constants and variables in prehistoric society

1988

THE PURPOSE OF archaeology has been expressed in many ways, but most people ought to be able to accept the wording that our study concerns *cultural change*, which includes all forms of technological, economic, social, aesthetic, ideological, and other change. Bruce Trigger (1978:54) has pointed out that the archaeological discipline arose in a time characterized by technological and social change, specifically when change had become so rapid that it could be observed in the course of a single lifetime. Many innovations in archaeological theory therefore also correspond directly to significant changes in the society where the archaeologist is living. It should be emphasized, for the sake of clarity, that change is not synonymous with development, *evolution*, a term that implies improvement or at any rate suggests something unambiguous and long-term. Cultural change *can* be like that, but it can also be rapid, revolutionary, and with a meaning that be evaluated in different ways.

Since all archaeological literature, explicitly or implicitly, deals with cultural change, opinions about its meaning and causes display the greatest variation possible. Two closely related yet distinct statements may be compared. Tilley (1981:363) writes that “any explanation of change inevitably depends upon the investigator’s image of the interrelationship of man and nature”. Gunn & Adams (1981:87) consider that cultural change is due to at least three essential variables, namely, “environment”, “internal” and “external”. And they find that “during times of climatic stability, changes are attributable to internal forces such as powerful individuals in a proportion of Y per cent, or to external sources such as invasion by outsiders, Z per cent”. If one studies a period when the climate is constant, then, according to this statement, it is not necessary to have an opinion about the relationship between man and nature to be able to explain observed cultural changes. But the most important difference between these two passages is that only the latter notes that archaeology, like other sciences, has a use for the concepts of constant and variable.

It is in the nature of things that a prehistoric change in culture of any significance requires a grasp of the whole, a holistic outlook on the part of the archaeo-

logist. But it is just as obvious that this holistic view cannot be achieved without adequate documentation of a large number of cultural elements. And even that is not enough. We must also be sure about which elements *can* vary in the first place, and which – by definition – must be constant by logically imperative necessity (Malmer 1963:27). We must also be aware of which elements are dependent on each other and which are independent (Malmer 1963:28), and finally, we must try to determine in what manner and with what intensity varying and mutually dependent elements influence each other. The study of constants and variables is in fact wholly crucial for our potential to establish and interpret cultural change.

The terms constants and variables ultimately come from mathematics, and it can be illuminating to see what they mean there. One example of a mathematical-physical *constant* is specific gravity. The specific gravity of pure gold, for instance, is the same regardless of how large or small the gold object is, and regardless of whether it is shaped like a ring, like a gold bar, or like a solidus. The concept of a *variable* can be illustrated through an equation with two unknowns, for example  $Y=X^3$ . Both X and Y are variables: they can take on any value at all. But they are dependent on each other: Y varies according to the value that X adopts, and vice versa. This relationship between two variables, as we know, is called a function, and it is obviously a different kind of constancy, namely, a constant relationship between the variables.

In archaeological problems one can discern several different kinds of constants. One occurs in typological theory. If typological elements have been defined in such a way that there is contradictory inequality – for example between comb-stamp decoration and cord decoration – then these elements are constants. If, on the other hand, the definitions constitute contrary inequality – for example between coarse, medium-fine, and fine comb stamping, with a maximum 14, 15–24, and at least 25 tooth impressions respectively per 3 cm – then there is a variable (Malmer 1963:26 f.). Such distinctions are of crucial significance for the chronological discussion, but also for symbolic and ideological interpretation.

Another kind of archaeological constant consists of fundamental facts, such as that human physiology functions within certain set limits, that human longevity cannot exceed a certain maximum, and so on. Constants of this type are so fundamental that they never become operative: they shed no new light on problems and do not help to solve them.

A third kind of archaeological constant consists of those which are constant, or as good as constant, but only in a particular situation. In this slightly metaphorical sense the time factor is usually decisive. Things which seem wholly

constant in the short-term perspective that historical scholarship must apply can, in the longer temporal perspective of archaeology, prove to vary distinctly. This means, once again, that if one studies a certain short archaeological period, variables that vary extremely slowly can and must be perceived as constants. Slowly varying things of this kind are, in the first instance, geological, geographical, climatic, botanical, and zoological phenomena. If one studies the Finnish War of 1808–1809 and the Battle of Sävar and Ratan, the water level of the Baltic Sea is a constant, but if one wishes to examine harbours and navigable routes on the coast of Västerbotten during the Viking Age and the Middle Ages, then land uplift is an important variable.

The fertility of arable land, expressed in the size of crops, is of course not a constant, since it depends on the cultivation methods. But if one studies a time when farmers neither manured their fields nor drained them, then the size of the crop depends essentially on the natural fertility of the soil, and on whether it is self-draining or not. A verification of the thesis of the significance of good arable land is one of the results of a survey conducted more than 25 years ago of a dozen cultural elements of varying age, everything from the Early Neolithic to the Middle Ages in Skåne and Northern Europe in general (Malmer 1957, maps 1–14; 1962, Abb. 117–124; 1975, figs 70–77). Skåne is a suitable region for a test like this, since there is a great contrast there between poor and good arable land, and the latter is assembled in two large areas, the southern till area and the calcareous area (Malmer 1957, map 1; 1975, fig. 70). Although these two areas together make up only about a third of the area of Skåne, all the surveyed cultural elements show a heavy concentration there, for example, 82% of the megalithic tombs, 90% of the Battle Axe Culture (STR) flat-ground graves, 93% of the Bronze Age brooches and 73% of the rune stones. The main rule for the distribution of the monuments and artefact types of the farming culture can be formulated thus: the larger and more unproductive capital investment a type represents, the more likely it is to be concentrated on the best arable land (Malmer 1957:180). The constant significance of good arable land is verified to the point of total certainty, and the constant interest of the farming culture in unproductive investments – or, to put it positively, its interest in prestige and ideology – is verified with almost the same certainty.

The values measured for the dominance of the good arable lands are, as the examples show, quite constant. Any variations are due to cultivation, research lacunae, inadequate documentation, and similar sources of error, which can be counteracted by source criticism. The differences between the maps in figs 3:1 and 3:2 (Malmer 1962, Abb. 122, 124) cannot be explained with reference to sources of error. All the find details were collected by the author through per-



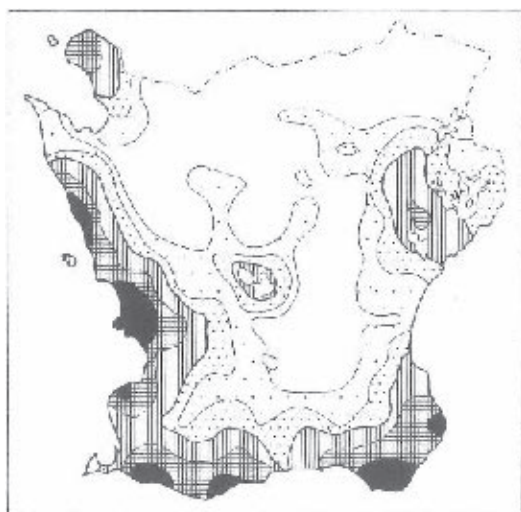
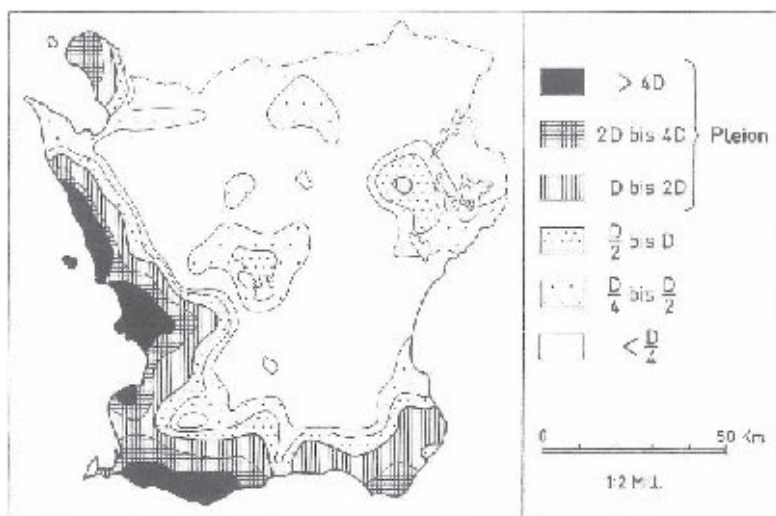


Fig. 3:1. The distribution of 6,515 flint daggers from the Late Neolithic/Bronze Age in Skåne. For a key to the symbols, see fig. 3:2.  $D$  = mean density of artefacts per surface area (from Malmer 1962).

Fig. 3:2. The distribution of 1,914 flint sickles from the Late Neolithic/Bronze Age in Skåne (from Malmer 1962).



sonal visits to museums and private collections. Of flint daggers (fig. 3:1) from the Late Neolithic/Bronze Age, a total of 8,736 items were noted, but 2,221 of these (or 25.5%) lacked a specification of the find spot. Of flint sickles (fig. 3:2) of the same period, 2,519 items were noted, of which 605 (or 24%) lacked a find spot. At the time when they were found, all the artefacts had a known find spot, and the percentages suggest that the loss of information for Neolithic artefact types among collectors and museums in the studied area during the collection period (i.e. from the start until 1956) is a constant whose size shows only insig-



nificant variation. The reason the sickles lack a find spot to a lesser extent than the daggers (24% versus 25.5%) is probably that they are rarer and therefore, from the collector's point of view, more interesting. Of the simple shaft-hole axes from the Late Neolithic/Bronze Age, 24.5% lack a known find spot, but of the rarer Swedish-Norwegian battle axes, only 17% (data from Malmer 1962:699 ff.).

The maps, fig. 3:1 showing the distribution of the daggers and fig. 3:2 showing that of the sickles, are similar in that the pleion areas (i.e. areas where the find density is greater than the average for Skåne as a whole) essentially coincide with the areas with good soil, the southern till area along the south and west coasts and the calcareous area in the north-east. The difference between the two maps is that the sickles have a much more distinct western distribution. The sickles (fig. 3:2) have a maximum find density (indicated by solid black) only in the west. The daggers (fig. 3:1) have smaller areas with maximum find density in the west, but also a couple of areas in south-east Skåne (Österlen), and are much better represented than sickles in the calcareous area to the north-east. Perhaps the first explanation that suggests itself for this difference is the hypothesis that cereal cultivation in the Neolithic was better developed in western than eastern Skåne (Oldeberg 1932:218 ff.). But this hypothesis is improbable, since Österlen has easily tilled, fertile, lime-rich, self-draining soil, surely the best in the Scandinavian Peninsula for primitive agriculture. The greatest concentration of sickles is instead found in the hundreds<sup>1</sup> of Rönneberg and Harjager on the west coast and Skytt and Vemmenhög in the western part of the south coast (cf. Malmer 1957, map 1). It is precisely the shores of these hundreds that have a copious supply of good flint, whereas the east coast totally lacks flint that can be used to make sickles and daggers. It thus seems as if the differences between the maps in figs 3:1 and 3:2 are due to the location of natural occurrences of flint. It may seem reasonable that this constant can affect the total frequency of flint artefacts, but how is it possible that the relationship between the number of daggers and the number of sickles varies with the distance from the flint deposits? The explanation lies in the find circumstances for the two types. The vast majority of daggers and sickles come from graves or hoard finds, but very few from settlement sites, and the quantity of daggers and sickles which have been lost elsewhere can be regarded as negligible. Of the well-documented finds, the daggers mainly occur in grave finds while the sickles mostly occur in hoards where, despite the small number, they are much more common than the daggers. Each buried individual is normally accompanied by one or two daggers, but the number of artefacts in hoards varies greatly (Rydbeck 1918:32 ff., figs. 79–94; Forssander 1936:132; Strömberg 1952). The natural conclusion is that the size of

1 For the hundreds of Skåne, see fig. 9:1 (SW)

votive hoards is heavily dependent on the availability of flint, which in turn is mainly dependent on the distance from the natural flint deposits. The variation in the proportion of daggers/sickles thus reflects – at least in Skåne – the variation in the cost of flint from one district to another (Malmer 1962:705).

The pressure-flaked daggers and sickles are obvious status objects, but a hypothesis that they could have been spread through the exchange of gifts would evidently be unreasonable. Both their number and the characteristic variation in the relative proportion of daggers/sickles falsify that hypothesis, and suggest instead that a market economy was already established during the Neolithic. The same result was obtained by a detailed study of STR flint axes found in graves; these were seen to decrease in size and number the further north one goes from the south Scandinavian flint deposits, and are gradually replaced by rock axes (Malmer 1962:380 ff., 430 ff.).

The study of daggers and sickles has left a rich amount of material to shed light on the concepts of constants and variables in archaeology. The location of natural flint deposits is a constant. Good arable land and the strength of the farming economy are two sides of the same constant. The ideological convention which prescribed that daggers should be deposited in graves during the studied period was constantly in effect, and the number of daggers per individual likewise seems to be constant, or varies only insignificantly. The ideological convention that sickles (and to a lesser extent daggers) should be deposited in hoards (hypothetically, as sacrifices to higher powers) is also constantly in effect. The ideological convention which prescribed how many objects each hoard should contain can be assumed on good grounds to have been constant within each settlement district, but this convention varied greatly from one district to another according to the distance from the natural flint deposits. This distance is thus a geographical variable, which together with the equally varying size of hoards reflects a market economy that was constant during the studied period.

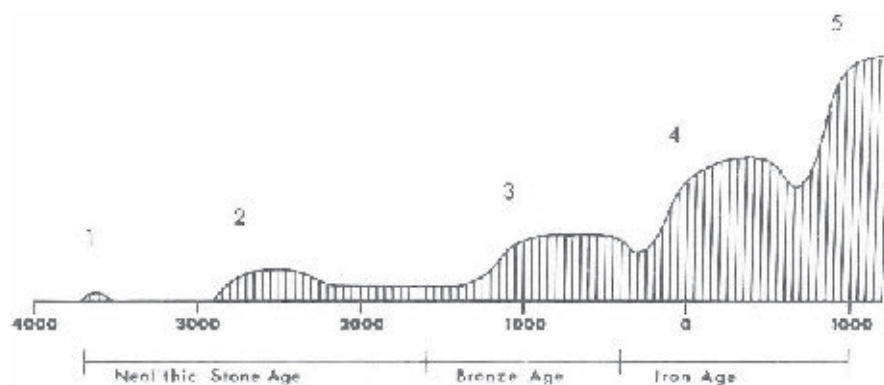
A situation wholly analogous to that exemplified above prevailed during the immediately preceding period, the STR: ideological conventions were modified by a constant market economy. That socio-economic changes occurred during the Neolithic is a well-corroborated hypothesis. The start of the STR appears to have entailed an individualistic revolution (Malmer 1962:809 ff., 815 ff.), and the start of the Late Neolithic may possibly have seen a collectivist reaction. Parallel to these upheavals, an economic evolution can be detected in the most intensively studied flint material, that of the STR, namely, a gradual increase in flint export to areas without their own flint supply (Malmer 1962:445 f.). Of all the variables discussed here, including mortuary practice, form, and decoration, flint export is the only one that, through its increase, involves a socio-eco-

nomic change. What was the cause of this change, this increase in flint export? It could possibly have been the opening of new flint mines – such can be found, for instance, in Oxie hundred (Olausson et al. 1980), located between the hundreds with beach flint, Rönneberg–Harjager in the north and Skytt–Vemmenhög in the south. A more likely cause was a desire among people in the areas with flint to increase their flint exports, and a desire in the flint-poor areas to increase their imports, along with a desire among both to improve communication, transport, and trade. The reason cannot – or at least cannot primarily – have been an increase in population, since the clearest reflection of the increased flint export is a gradual increase in the size of the flint axes (Malmer 1962, tab. 43). And even if the increase in flint export were synchronous with the opening of new flint mines, that too is a result of human ideas and human initiatives. To sum up, then, the observed socio-economic change, the increase in flint export, is not due to ecological factors, and not to population growth, but to ideological changes and human enterprise.

A variable of crucial significance in farming culture is the intensity of cultivation. That this is palaeobotanically measurable was demonstrated early on by Fries (1958). Berglund (1969:20 ff.) distinguished four expansions in cultivation, the first of them identical to the Neolithic landnam, the fourth in the Viking Age. Welinder (1974:98 ff.) defines five stages of expansion, separated by four stages of stagnation. The picture painted by both Berglund (1969, fig. 5) and Welinder (1974, fig. 30; 1975, fig. 9.1-1) is fairly similar over much of Scandinavia. Fig. 3:3 reproduces Welinder's diagram for the area he studied in Västmanland.

Climate change may seem to be a plausible cause of these changes in the intensity of cultivation, but the palaeobotanists at least would not identify them

*Fig. 3:3. Diagram of human influence upon the landscape (from Welinder 1974; 1975).*



as the main cause. Welinder (1974:234) concludes that the climate cannot be cited as a general explanation for fluctuations in the intensity of cultivation; some of them could possibly have been favoured or disfavoured by climate change that coincided in time. On the other hand, Welinder shows an association between cultivation intensity and agricultural technology in the form of implements, manuring, and the choice of land to till. Are these agricultural innovations the direct reason for the expansions of cultivation, or were they both forced by population growth (Boserup 1973; Welinder 1975:57 ff.)?

The intensity of cultivation is a central socio-economic variable, and it seems natural to compare it with the central variables of the basic archaeological material, namely, the artefacts and the monuments, and their chronological and chorological groupings, traditionally called cultures (whose usefulness or power over thought is not changed in the slightest if you call them "traditions" instead). The comparison proves to be surprisingly easy to make:

Expansion Stage 1 is by definition the start of the Neolithic and the TRB (Funnel Beaker Culture).

Expansion Stage 2 is the shift from TRB to STR.

Expansion Stage 3 is the shift from the Early to the Late Bronze Age.

Expansion Stage 4 is the shift from the Late Bronze Age to the Early Iron Age.

Expansion Stage 5 is the Viking Age and the shift to the Middle Ages.

This co-variation between important variables in prehistoric society appears to presuppose a causal connection. Can the variations in mortuary practice and artefacts have been *caused* by the expansions in cultivation?

Expansion Stage 2: The shift from TRB to STR has a general European background and cannot have been caused by an expansion in cultivation. Expansion Stage 3: The change from burial in barrows to cremation also has a general European background. Expansion Stage 4: The appearance of iron certainly cannot have been caused by an expansion in cultivation. Expansion Stage 5: The introduction of Christian ideology was a stage in the general European missionary activity, and so of course cannot have been caused by the expansion of cultivation. There is good reason to view the causal connection between the variables the other way around. What was imported to Scandinavia during Expansion Stage 1 was not primarily tillage and animal husbandry; what was introduced was an ideology comprising a social pattern, a religion and some forms of agrarian behaviour. Palaeobotanists detect clear efforts to domesticate plants and animals as early as the Mesolithic. What we call the start of the Neolithic meant that the people of the time became *conscious* of the Neolithic through the

import of a verbalized ideology: rules for cultivation, rules for artefacts, rules for cult, rules for burials – in short, rules for a social system. In a number of cases it can be clearly demonstrated that all these European rules or behaviours were modified to suit Scandinavian conditions.

The conclusion of this presentation is obvious: It is the ideological variable that is primary. A technological change is not significant until people have become *conscious* of it and their conceptual world has been affected by it. People are able to accept an ideology even if it lacks a close link to a technological change, as shown by Expansion Stages 3 and 5 studied here. For all innovations, whether ideological or otherwise, it is the case that, the more important they are perceived to be, the more unimpeded is the innovation process (Malmer 1962:796 ff.).

If the ideological variable is primary, the most important factor for the study of cultural change, how then can we improve the study of this variable, of the development of ideas? During certain phases in the history of archaeology – most recently, and perhaps most noticeably, in the 1970s – some archaeologists have tried to gain time and attention for an ideological study by devaluing the study of the archaeological finds, the artefacts and ancient remains. This attitude shows a confusion of the aims and the means of archaeology. It is only in the archaeological finds that ideology, technology, and socio-economy are reflected, just as it is only in the pollen diagrams that the intensity of cultivation is reflected.

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## CHAPTER 4

# From Thomsen to Binford. On archaeological theory and ideology before 1970

1993

IN THE FIFTH century BC, during our Pre-Roman Iron Age, the Athenian Thucydides wrote the history of the Peloponnesian War. He begins it with a brief sketch of the early history of Greece, and for this Homer is of course one of his sources. The catalogue of ships in the *Iliad* lists the vessels that accompanied each of the Greek kings to Troy. We read there, for instance, that no fewer than one hundred ships came from Mycenae – no other city contributed so many. But Mycenae is a small town, says Thucydides, and he wonders whether it is really possible that it could have provided the most ships. He is aware that there is often good reason to question what Homer says. Yet, Thucydides writes, the modest size of Mycenae is not sufficient reason to doubt the information about the size of the fleet (Book I:10). For we must consider our own times: if Sparta were destroyed, so that only the temples and the house foundations remained, future generations would find it hard to believe the stories about the great power of the Spartans. The city has no magnificent buildings and looks like a collection of villages, and so it would seem much less significant than its reputation. But if the same thing were to happen to Athens, one would guess from the remains that it had been at least twice as big as it actually is.

What is the theoretical foundation of this text by Thucydides? First and foremost, it is obviously source criticism – he does not automatically accept what Homer says. But it is also ethno-archaeology. Thucydides, who was an Athenian general (*strategos*) during the war, could imagine what the cities of Sparta and Athens might look like if they were captured and destroyed by the enemy – Sparta erased from the face of the earth, but Athens still splendid in its ruined state. And any tourist in Greece can see that Thucydides was right. Yet another theoretical facet can be easily detected in Thucydides' text. His hypothesis about Mycenae is evidently a general and predictive law.

I shall cite another scholar, from a millennium and a half later, but still reasonably old: Ibn Khaldun, the learned Tunisian historian, AD 1332–1406. In the famous introduction to his history of the world he writes: “It should be known that differences of condition among people are the result of the different ways

in which they make their living” (Ch. II:1). This is quite simply an expression of the materialistic perception of history which has been so significant for many interpretative hypotheses in archaeology.

With these two quotations, from Thucydides and Ibn Khaldun, it goes without saying that I cannot prove anything; at most I can possibly lead someone to believe that there is something reasonable or probable in what I say. And what I wish to assert by way of introduction is that the great mass – not all, of course, but the vast majority – of the results of archaeology hitherto are products of *common sense*. That is the reason why the two-thousand-year-old thoughts of intelligent men, who definitely were not archaeologists, are still of major interest to the archaeological discussion. In contrast, the atomic theory of Democritus is of no help whatever to today’s nuclear physicists.

I understand that my talk of common sense may sound provocative at a conference about theoretical archaeology. But what I am ultimately referring to is the tendency in the philosophy of science that is called *theoretical realism*, to which I shall return later.

For many centuries, the interest in archaeological things was just one sector of a broad interest in the curious and the beautiful. The excavations in Pompeii and Herculaneum in the first half of the eighteenth century were still nothing but treasure hunting. But occasionally one can find expressions of theoretical interest, just as isolated from each other as Thucydides and Ibn Khaldun. John Aubrey, for example, published plans of Stonehenge and Avebury (Trigger 1989:48), and Olof Rudbeck’s *Atlantica* contains wholly modern-looking sections through a couple of burial mounds at Gamla Uppsala (Klindt-Jensen 1975:31). For both of these gentlemen, the purpose was to illustrate and confirm the truth of old texts, whether about the Druids or about the Svear and Göter, but there is nothing fantastic about their drawings, unlike so many others; they display the realism of common sense. Aubrey’s drawing resembles plans of military field fortifications, and he was indeed living in the days of the English Civil War. Rudbeck’s sections are perhaps even more remarkable, and Klindt-Jensen notes that it was natural for a surgeon to display facts by means of sections. Among many other things, Olof Rudbeck was a surgeon – it was he who built the anatomical theatre that still stands in Uppsala. His interpretations of the sections, however, are not those of a surgeon but of a modern archaeologist: the lower layers are older, the upper ones younger. One would hardly expect anything else: stratigraphy, which has often been held up as the main chronological method of archaeology, was of course not invented by either geologists or archaeologists. It was, quite simply, common sense.

Christian Jürgensen Thomsen has often been described as common sense



personified, the competent young son of a merchant, who arranged coins and antiquities in the same solid and boring order in which he arranged the stock of the family business. And of course Thomsen was a man of common sense. But what he should be remembered for is that he was the world's first archaeologist with a clearly formed theory. Thomsen's motto – "First the things, then the texts" (*Først Sagene, derpaa Skrifterne*) – has been perceived as a confirmation of his fixation on material and his lack of theory. But the meaning of the motto is the exact opposite. What Thomsen wanted to say was that we must study artefacts and monuments carefully from all sides, and only then is it possible to express any opinion about them. And he exemplifies what he means by studying things carefully from all sides. One must observe how the antiquities are lying in relation to each other in the ground – this is often more important than the antiquities themselves. One must study the osteological material and one must perform chemical analyses of the content of pots. He recommends a method for interpreting the function of artefacts, namely, to study the "so interesting analogous pieces to be found in the collections of wild nations' weapons and tools, which explain very clearly how our earliest ancestors in the infancy of culture might have used these things" (Thomsen 1832–33:422). The whole of Thomsen's research, moreover, is permeated by an insight into the fundamental significance of the study of analogies for archaeology, especially in the study of artefacts, ancient monuments, and find combinations. It may be safely stated that most of the work done in archaeology in the subsequent century had its beginnings in Thomsen (Malmer 1989).

Emphasizing the importance of the careful study of material is not the only meaning of Thomsen's motto, "First the things, then the texts". In addition, he wanted to say that it is not possible to interpret the archaeological material with the aid of Old Norse sagas and other ancient texts, quite simply because the objects are so much older. This is so self-evident to us that we often forget that, for Thomsen's contemporaries, it was just as self-evident that the old texts were the *only* possible way to interpret the archaeological material. An authoritative statement by Jonas Hallenberg, Sweden's Custodian of Ancient Monuments, runs: "It is generally recognized, and must be recognized, that historical knowledge is the source of antiquarian knowledge, and not the other way around" (Hildebrand 1937–38:217). Thomsen's contemporaries and immediate successors did not understand the breadth of his material study, nor his critique of the saga literature, and therefore he has become known almost exclusively as the author of the three-age system. There is a great deal of truth in that, but when used as an epithet it does not increase the man's significance; it diminishes it (Malmer 1989).

Thomsen was obviously a child of the Enlightenment, born as he was in 1788, and with a genuine encyclopaedic interest. But when he was the first to distinguish a Stone Age and to treat it with respect, as a natural state, we hear an echo of Rousseau. The idea of the independent, carefree, healthy life of savages in exotic countries played an important part in Rousseau's critique of culture.

In 1854 Ferdinand Keller discovered the first Swiss pile dwellings and interpreted them as houses built on posts in the water (Keller 1856). He arrived at that interpretation through inspiration from an account of modern pile dwellings in New Guinea by the French admiral Dumont d'Urville (1830–35). As a whole, this was the heyday of colonialism: Livingstone's exploration of Africa mainly took place between 1849 and 1866. Darwin's *On the origin of species*, published in 1859, is also important in this context. Bruce Trigger (1989:113) has pointed out that one consequence of Darwin's theory of evolution was that he believed that less civilized people were also less developed intellectually and emotionally.

In Thomsen there are already hints of a division of the Stone Age into an early and a late period, but it was not until 1865 that the term Neolithic appeared in the literature, namely, in John Lubbock's *Prehistoric times*. This is a collection of standalone articles, one of which is about "The Danish kjökken-möddings or shell-mounds", one about "The lake-habitations of Switzerland", and several about "Modern savages". Lubbock thus had a remarkable ambition to encompass Europe and the whole world, naturally in the age of colonialism, and consequently he was also an evolutionist and a Darwinist. Through natural selection, ethnic groups came to differ, not only culturally but also in their biological ability to create and utilize culture. This explains why there are still savages in modern times, living at a Stone Age stage and inferior in every respect to the white man.

The archaeologist who most ostentatiously invoked Darwin is without doubt Oscar Montelius, especially in an article entitled (in translation) "Typology or the theory of evolution applied to human labour". The crucial statement here is famous: "What the species is for the natural scientist [...], the type is for the prehistoric archaeologist" (1899:237). This is, of course a false analogy. The species is given a priori, but the type is not, and the role they play in research is quite different. It is truly lamentable that what is perhaps the most explicitly formulated theoretical idea in the whole of nineteenth-century archaeology should be so wrong. The explanation for why this happened is almost certainly that Montelius' excellent typological work met with such superior intellectual criticism from Sophus Müller (1884). Montelius could very well have defended himself by improving the wording of the theory presented in the original ac-

count. Instead, however, he seized on the scholarly concept that enjoyed the greatest prestige at the time, evolution, and used it as a shield. This was the first time, but certainly not the last, that archaeologists have used scientific buzz words in this way. And of course it happens just as often in other disciplines.

The aspiration to have a general European view was natural in an age when it was possible to travel without a passport, crossing any border, except perhaps that of Russia. Ch. J. Thomsen travelled a great deal, as did his fine Swedish pupil, Bror Emil Hildebrand, who created the Swedish Museum of National Antiquities. All in all, this was the time when Europe's national museums were built, and the ambition for a pan-European outlook was obvious. Museums exchanged duplicate artefacts, with the result that most big museums in Europe have Danish flint objects and finds from Swiss pile dwellings in their comparative collections.

It is against this background that we can understand Hans Hildebrand's otherwise astounding work *De förhistoriska folken i Europa* ("The prehistoric peoples of Europe", 1873–80), a massive 700-page survey. Hildebrand divided the European Neolithic into eight provinces: Nordic, Central German, Franco-Italian, and so on. He counters an objection that the eight provinces might simply be due to the naturally dictated supply of different material for making tools. There must be a deeper reason for the provincial differences: "The tribes which inhabited the different parts of Europe ... each developed in a distinctive way within its area the culture to which there were tendencies and predispositions" (1873–80:427). "The Swiss Stone Age is only one of several coexistent cultures," he says, for example, and this is perhaps the first time the word "culture" was used in that way (1873–80:343). Hildebrand's interpretation, of course, is part of a larger context. He was writing in the decade after the national wars of unification in Germany, Italy, and Greece, and it was therefore natural for him to distinguish different cultures and interpret them as peoples and nations.

An interpretation of this kind fell on fertile ground of course, particularly in Germany, and the name that should be mentioned is obviously that of Gustav Kossinna, although most of his works were written in our century. The key word to his research is *Siedlungsarchäologie*, and by that he meant the method of establishing the settlement areas of ancient tribes by studying the distribution of artefacts and artefact groups in time and space. He preferred to work with *Die Herkunft der Germanen* ("The origin of the Germani", 1911). That archaeological cultures should be interpreted in ethnic terms was not something that Kossinna thought needed to be proved – it was an axiom. Behind his sweeping generalizations there was very little careful or detailed study.

His diametrical opposite in this respect was Sophus Müller, whose exception-

ally meticulous work enabled him to define a culture group that has seen more discussion than any other: Corded Ware (Müller 1898). As we know, his interpretation of the historical process in Denmark is that there was immigration from the south. What is noticed less often is how Müller emphasizes that the systematic and careful excavations in Denmark had no counterpart in Germany, which weakens the hypothesis. And he added (1898:281) that it is possible that “the existing data have been incorrectly evaluated in one respect or another, and that other circumstances will become known which could show the contrary, that the aforementioned characteristics are due to a domestic development, albeit taking place under foreign influence.”

Sophus Müller’s careful and sober interpretations represent the zenith of the trend that is usually called *culture-historical archaeology* (e.g. Trigger 1989:148), which is rather misleadingly translated into Swedish as *kulturbistorisk arkeologi*. It is called “culture-historical” because it mostly aims to interpret archaeological cultures in historical terms, but it differs a great deal from what is otherwise meant by *kulturbistoria* in Sweden. A characteristic of this culture-historical archaeology is that the only sector of methodology or theory that is considered in any detail is chronology. In Sophus Müller’s 127-page dissertation about the Single Grave Culture of Jutland, the revolutionary interpretation takes up just the last seven pages (Müller 1898:274–281). In a modern dissertation, as we know, the proportions are usually reversed. And this is in my view a step in the right direction, provided that the quality of the text describing the material is not diminished. In Sophus Müller’s text there is not a single superfluous line. Almost everything we know today about European prehistory, and much of what we will now in the future, has been achieved by the culture-historical archaeology that is now disparaged by so many. It mostly worked without any explicit theory, but with a considerable measure of common sense. With a method like this one can get quite far – although, of course, not as far as we *want* to.

Before I leave “culture-historical” archaeology, I cannot omit to mention a truly unique work, Ernst Wahle’s *Deutung frühgeschichtlicher Kulturprovinzen* from 1941. This is a serious and successful attempt to examine Kossinna’s ethnic identifications on his own conceptual level. Wahle’s conclusion is that, by and large, we cannot find historically known Germanic tribes in the archaeological material. And we should not misuse the material as a typological exercise, but instead try to ascertain how people lived. Wahle’s dissertation is clear and convincing. We should also bear in mind that it was printed in Germany during the war, in sharp opposition to the prevailing ideology.

The great turning point in the theoretical development of archaeology came with Gordon Childe. Despite this, he started in magnificent culture-historical style with *The dawn of European civilization* (1925) and *The Danube in prehistory* (1929). His diagrams with regions on the horizontal axis and time on the vertical axis are filled to the brim with cultures. A Kossinna with a broader outlook, greater attention to detail, and, we may suppose, without Kossinna's conviction that the Germanic peoples have always been the best.

But if we read Childe's *The Aryans* (1926) we find that he considers it natural that the Nordic peoples with their outstanding physique were the carriers of the superior Indo-European languages. He subsequently regretted that statement – and it is in fact a characteristic of Childe's that he often changed his mind. Or to put it another way: he was not dogmatic.

His most dramatic change of mind came at the start of the 1930s, when he abandoned the "culture-historical" archaeology in which cultures are usually interpreted as peoples and changes in culture are explained by migrations. Trigger (1989:244) says that Childe was influenced by the ecological trend in Scandinavian and Swiss archaeology. That was an inheritance of the kitchen midden commissions and excavations of pile dwellings, but it had also been actively developed in collaboration with geologists, botanists, and climatologists. Ecological archaeology led Childe to seek the cause of cultural change in economic factors. As late as in 1950, however, he wrote a book entitled *Prehistoric migrations in Europe*. There he says in his concluding discussions: "My Russian colleagues deprecate the incessant resort to migrations to explain cultural changes. Such should be interpreted preferably by technological advances and the consequent changes of social structure. I must say I am inclined to agree with them and I shall make a minimal use of migrationist hypotheses" (Childe 1950:10). When Childe changed the direction of archaeology, the most important motive force was almost certainly his adoption, in several respects, of Marxist theory. And this is of course yet another case of the development of archaeology following the currents of the time.

Grahame Clark, on the other hand, stayed with the ecological-economic, or if you wish functionalistic, interpretation of the archaeological evidence. Moreover, he emphasized the significance of ethnographic parallels for understanding the function of settlement sites or individual artefact types. He himself stressed his debt to the Scandinavian research tradition (1936:xi). A major difference between Gordon Childe and Grahame Clark lies in the way they present the archaeological material. Childe mostly employs sweeping surveys, presenting nothing exactly in either words or pictures. Clark, on the other hand,

displays his material with the utmost care, clarity, and detail. Many would no doubt claim that this has nothing to do with archaeological theory. For my part, I would counter by saying that the care with which the material is presented is a central issue in archaeological theory.

There was a not insignificant interest in this problem in the 1950s and 1960s. We may mention the Dutch archaeologist Bohmers (1956) and his French colleague Bordes (1950), who devised graphical methods for sharp and easily grasped presentation of settlement site finds so that they can be efficiently compared with each other. This kind of work was performed with the greatest perseverance and intensity in the USA, especially as part of the Midwestern taxonomic method.

Parallel to these efforts there were also people who believed that the benefits of statistical and graphical presentation are modest unless one first formulates sharp and clear definitions of the types with which one is dealing. One of the archaeologists who has most vigorously asserted that stance is the Frenchman Jean-Claude Gardin (1967).

The first steps towards the New Archeology of the 1960s are evident in Walter Taylor's *A study of archeology* from 1948. It has a polemical tone, which comes out particularly clearly in the critique of the main trend in American archaeology at the time, the Midwestern taxonomic method. The result was, of course, that Taylor was partly ignored, and that at the end of the 1960s he himself felt that he had to point out how much of the New Archeology was actually there in his twenty-year-old book.

The perception of Taylor is that he thought that the tendency in contemporary American archaeology towards classification and chronology was an outright obstacle to an interest in living conditions, changes in culture, and general laws of human behaviour. I do not know whether that is a correct interpretation, but if it is, one could remark that one good thing need not be an enemy of another. Archaeology is a large subject, but it loses some of its greatness if it does not retain all the possibilities for the production of knowledge that it has developed.

Taylor believed that the focus on classification and chronology led to routine behaviour in archaeological fieldwork and analysis, so that only "key artefacts" are retrieved, while botanical and zoological material, above all, is neglected. Taylor recommended instead what he called "the conjunctive approach". An individual key artefact should not be perceived as the normal unit of study, which should instead be the settlement site. And all the construction details and artefacts of the settlement site should be studied, and how they relate to each other. Taylor thus advocated the same method, in principle, for settlement site studies that had been used in Scandinavia and Switzerland a hundred years ear-

lier – and with the refined botanical and osteological methods available since the turn of the century. Of course, he was not aware of this.

Once the settlement site has been sufficiently investigated, according to Taylor, it should be compared with other settlement sites, and above all with the pattern of life in the entire region where it is located. In this way it should be possible to distinguish seasonal settlement sites and year-round sites, and to determine how the villages are related to power centres. The ultimate goal is to arrive at a knowledge of the prehistoric society similar to what social anthropologists can say about contemporary societies.

Childe and Clark placed great emphasis on the dependence of cultures on the natural environment, what is called ecological adaptation. Taylor, on the other hand, thought that cultural influence or change can be due to many factors and therefore must be studied individually. The reason for the change can even be chance contacts between societies. He had an idealistic – as opposed to a materialistic – understanding of culture, which meant that he defined it as a collection of concepts or ideas embraced by everyone in a society.

Walter Taylor's work shows that the birth of the New Archeology cannot be pinned down to the famous symposium in Denver in 1966 or to Binford's symposium publication two years later, *New perspectives in archaeology* (Binford & Binford 1968). On the contrary the new current was something that emerged in the course of fifteen years or more. Interest was especially concentrated on the problem of cultural change.

Leslie White, who was an ethnologist and one of Binford's teachers, believed that social systems are determined by technological systems, and consequently cultural development depends on technological development (1949). White is thus both a materialist and a technological determinist. Another ethnologist of the same generation, Julian Steward, is rather an ecological determinist who argues that cultural development is mostly due to the natural environment (1955).

Sahlins and Service (1960) distinguish between *general* and *specific evolution*. General evolution is almost an inescapable law of nature, whereas specific evolution depends on ecological adaptation. These two authors are responsible for the evolutionary series *band*, *tribe*, *chiefdom*, *state*, which has become highly popular despite its clumsy formalism.

In his own account of the birth of the New Archeology – *An archaeological perspective* (1972) – Lewis Binford emphasizes the difference with respect to the Midwestern taxonomic system which had tortured him during his studies, through Griffin (1952), Ford (1952), and others, and which he claims to be the generally practised traditional archaeology in America. Binford's own theoretical works, on the other hand, display a clear link to the development begun by



Walter Taylor. Binford states that the goal of archaeology is to arrive at an explanation of the entire scale of human behaviour – which was precisely what Taylor claimed. Binford asserts that traditional archaeology explains cultural differences with reference to geographical barriers and fixed traditions, and that it explains cultural change through the dissemination of ideas and through migration. But this is not true of, say, Gordon Childe or Grahame Clark. As a whole, the New Archeology has a tendency to caricature traditional archaeology rather than study how it actually works.

The central word in the description of Binford's New Archeology is, of course, *processual*. It seeks to use modern analytical methods not just to describe cultural evolution but also, with a strict hypothetical-deductive method, to explain its causes in detail. And the ultimate goal of archaeology is not a catalogue of detailed explanations, but general laws of human behaviour. In this way it is also *predictive*. Archaeology, according to Binford, is a *nomothetic* discipline, and it is a *science*, not one of the *humanities*.

According to Binford, an archaeologist must have training in ethnography-anthropology. This brings us to the modern ethno-archaeology, which admittedly only follows the guidelines drawn up more than 150 years ago by Ch. J. Thomsen. But Binford observes that archaeologists who work ethnographically know too little about relations between human behaviour and human ideas on the one hand and material culture on the other. His interest is therefore largely concentrated on a theory that concerns precisely these relations, what is known as *middle range theory*. His ethnographic and archaeological fieldwork has mainly had the aim of acquiring material for discussions of this problem. And it seems likely to me that most archaeologists agree that this is the very centre of gravity of archaeology, by far the most important sphere of work once the primary description of the material is under control.

The New Archeology in its American form has no interest in historical interpretation, at least not in comparison with the great goal of arriving at general laws of human behaviour. The European archaeologists who have adopted many of Binford's principles usually have a much more positive view of the possibility of reaching historical results, and of the value of these. The explanation, no doubt, is that European archaeologists feel a historical continuity back in time to our own ancestors in prehistory, which cannot be the case for their American colleagues in general. David Clarke, however, sees no contradiction between historical interpretation and general laws of behaviour and evolution – on the contrary, they support each other. Clarke's first major work, *Analytical archaeology* (1968), demonstrates a firm belief in the evidential value of artefacts. "The archaeologist's facts are artefacts – and their context," he says (1968:14),



and a reasonable explanation of this statement is that European artefacts display a much greater breadth of variation than American artefacts. In his later works he instead perceives, in the spirit of Walter Taylor and Binford, the settlement site as the basic unit of archaeological research.

The New Archeology has an explicit positivist outlook. An authoritative statement can be found in Watson et al. (1971:113): "The logical position of archaeology with respect to the limitations of the archaeological record should be a strongly positivistic one: the information is there, it is the investigator's task to devise means to extract it." Positivism, however, was seriously questioned already when the New Archeology declared in favour of it, and now, as we know, it has been virtually abandoned in the humanities. As for archaeology, the pendulum has swung in many places from positivist optimism to a relativism with Thomas Kuhn (1962) as its father, according to which one can never arrive at an objective or certain understanding of the archaeological evidence. One is therefore entitled to interpret and use it for any subjective purpose one chooses, for example, for political propaganda.

As I see it, this relativism is both misguided and destructive. We now obviously have a huge amount of objective knowledge about prehistory. We know that the Stone Age came before the Iron Age, and that there was often a Bronze Age in between. We know that there are passage graves in Scandinavia and pyramids in Mexico, and we have hundreds of thousands more facts of this kind, which no one has challenged or has a chance of disproving. Common sense tells us that we have large quantities of knowledge about prehistory, and hence also an understanding of it. Knowledge and understanding are not independent of each other; on the contrary, they are indissolubly united.

The explicit theoretical discussion that began with Gordon Childe and then continued, above all with Walter Taylor, the New Archeology, and the post-processual school, can in my opinion be regarded as the greatest step forward taken by archaeology since Christian Jürgensen Thomsen. These and other schools have scrutinized each other's doctrinal structure in meticulous detail. They have been much less successful in developing useful archaeological methods to assist the majority of the world's archaeologists who have stood outside the theoretical discussion.

Another characteristic feature of the archaeological debate of the last thirty years is that the so-called traditional archaeology has scarcely been considered other than to put brief and disparaging labels on it, and more or less scornfully reject it. But there is no paradigm shift in Thomas Kuhn's sense between traditional archaeology and New Archeology. It is a difference of degree, not of kind, as regards theoretical awareness. A necessary condition for a successful renewal

of archaeology is undoubtedly that we take the older literature seriously and really work with it. It is important to bring out the older authors' implicit, yet still accessible, theoretical foundations, and to examine them critically. If we express them in a way that is acceptable in terms of the philosophy of science, they will in many cases prove to be useful. New Archeology should not tear down the old, but make it operable in our times.

The 150-year history of archaeology, in my opinion, shows that its theoretical basis can be neither the extreme positivism of the New Archeology nor the relativism of post-processual archaeology. Better suited is the tendency called *theoretical realism*. This is represented, for instance, by the Englishmen Rom Harré (1970, 1986) and Roy Bhaskar (1975), to whom I refer here chiefly through Guy Gibbon (1989) and Christer Winberg (1990). Theoretical realism criticizes Kuhn's view that one theory is as good as another, that we can only see what we want to see, and that one cannot talk of scientific progress. If this were the case, say the realists, how can we explain that humanity has understand more and more about how nature works, and has learnt in increasing measure how to master it. This development would be impossible unless – just as the positivists say – there was a world around us with certain given properties about which we can acquire sure and increasing knowledge. Of course we have preconceptions – our observations are theory-laden, as it is often put nowadays. But the theoretical load does not take over to such an extent that realistic knowledge of the world and of the archaeological material is impossible. A theory-neutral language of observation functions over large fields.

But positivism also claims that we must stick firmly to what is observable: we must ascertain the regularities of the surface phenomena and formulate laws based on them. According to the realist philosophy of science, on the other hand, the observable surface is influenced by underlying, really existing forces and structures. These give patterns in what is observable and we can thereby draw conclusions about their existence and character.

This view of the potential of science appears particularly apposite for archaeology. Moreover, it seems like a good formulation of the aim of *all* research, in both the natural sciences and the humanities, that it is primarily an endeavour to establish causal connections between surface conditions and deep phenomena.

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## CHAPTER 5

# On Theoretical Realism in Archaeology

1993

What we are really digging for?

Well, certain jokers suggest that we are looking for Kalevipoeg's Party membership book...

Jaak Kross, *Väljakaevamised*, "Excavations" (1990)

"There is only one principle that can be defended under *all* circumstances and in all stages of human development. It is the principle *anything goes*." Paul Feyerabend's famous slogan (1975:28) is one of the philosophical tenets that produced a deep effect on the archaeological discussion of the 1980s. Probably no other humanistic discipline was so deeply affected by this kind of relativism.

From an archaeologist's point of view Feyerabend's most fundamental thesis is, that all observations and empirical data are to some extent subjective, or at least theory-laden. He does not stop at Kuhn's (1962) ideas about several competing paradigms, but even questions the very concept of scientific truth. Science is placed on a level with myths, religious systems and political ideologies.

Feyerabend's most extreme ideas are accepted by Shanks & Tilley, who deny that we can attain any objective truth about the past (Shanks & Tilley 1987:212 f.):

Choosing a past, constituting a past, is choosing a future. The meaning of the past is political and belongs to the present. [...] Archaeology, as cultural practice, is always a politics, a morality. [...] We do not argue for truths about the past but argue through the medium of the past to detach the power of truth from the present social order.

Watson (1991:280) justly summarizes their position thus: "Because archaeology is a deceit we should use it propagandistically". Trigger (1991:72) is less stern but very clear-cut: "All scientific activities have subjective elements, but studying the past is not the same as dreaming or writing a novel".

"Why don't we write historical novels instead?" is exactly the question asked by the historian Christer Winberg (1990) in a critique of both positivism and relativism. In Swedish historical research an animated debate on theory started in 1965 with an explicit appeal to use positivistic principles (Winberg refers to

Björklund 1965). It is well known that the American New Archeology recommended a consistent logical empiricist, or positivist, research programme (Binford 1968; Gibbon 1989). In Scandinavian archaeology there was a parallel development, beginning around 1960. An explicit terminology was shaped for the description of archaeological data and definition of types, and exact measurements, statistics and graphs were introduced (Welinder 1991). Evidently also the Scandinavian variant of New Archeology was influenced by positivism, but only in so far as source criticism, clearness and precision in the treatment of archaeological material was demanded. There was no attempt to introduce a formal deductive-nomological model of explanation into archaeology.

Positivism was out of date already when introduced into American New Archeology and Swedish historical research (Gibbon 1989:35; Winberg 1990:5). In Anglo-American philosophy it was sharply criticized since the 1950s, and in 1962 the new situation became evident to all the learned world by Kuhn's famous book. No wonder that there was soon much criticism, both external and internal, also against the positivist, "processual" New Archeology (Gibbon 1989:91). This started a debate, which resulted in many very valuable contributions to archaeological theory, and a few less valuable ones. In sum, archaeology certainly improved more than ever during the last 30 years.

Winberg is less happy about the development in the field of history. Attempts toward strict observation of positivist rules of inference led to superficial results (Winberg 1990:10). Still worse, when the philosophical criticism of positivism, especially in Kuhn's version, reached history, a relativism developed. Since all observation is loaded with our own theories, how can we know what *really* happened in history? Subjective hermeneutics tried empathy with the spiritual life of individual historical personalities. From the relationship between the historical event and its scientific representation, focus moved to the relationship between this representation and the reader. So why not write novels instead? (Winberg 1990:6).

According to positivists, the research strategy of natural sciences should be used also in archaeology and other humanities. This is eagerly denied by post-processualists and relativists, but it is astonishing how little energy has been used to define the distinctive character of archaeology. As a matter of fact the structure of archaeology is almost unique (Malmer 1984:266; 1990:69):

The materials of natural sciences are mute and non-human.  
The material of archaeology is mute and human.  
The materials of other humanities are verbal and human.

This means that in archaeology there is a much clearer distinction than in any other science between observable data and the reality about which we want to obtain knowledge. We are not interested in artefacts (unless we are antique dealers, or see them as works of art); we are interested in the social and private life of prehistoric man. But artefacts don't voluntarily tell us anything about prehistoric life; we have to use strict scientific method to make them talk (and this, of course, is the reason why scientific archaeology started very late, in the 1830s).

Also in the natural sciences there is no doubt a distinction between directly observable data and underlying forces and structures. A metal can be said to have "dispositional" properties, for example that it is hard, heavy, malleable, resistant to rust and melts at a certain temperature, but also "essential" properties, such as a certain atomic number and a certain atomic weight (Gibbon 1989:149). But whether these properties are "dispositional" or not obviously depends on one's technical competence: palaeolithic man could apprehend only that this lump of metal was hard and heavy. Only successively, as technology improved, could man discover that this metal is also malleable, rustless, meltable and has an atomic structure. Evidently, then, in the natural sciences there is no such clear dichotomy between observable data and "essence" as in archaeology.

The same holds true for other humanities than archaeology. History, for instance, tries to work out a verbal account of the essentials of a past situation, but its observable data is contained in other verbal accounts. More resemblant to archaeology is art history since its task is sometimes to discern and verbalize essentials of works of art, which are not accompanied by any verbal account, or even the artist's name. Still greater similarity exists between ethnography and archaeology, since reports about the exact function of ethnographic objects are sometimes missing. This analogy between ethnography and archaeology came to an end some 25 years ago, when ethnography transformed itself into social anthropology, with little or no interest in artefacts, thus deserting from one of the most interesting anthropological problems, viz. the reflection of ideas in the material world – and *vice versa*. Luckily archaeology has taken over this complex of problems in what is now called ethno-archaeology.

In sum: in all sciences there is a distinction between direct observable data and an underlying essential reality about which we want to obtain knowledge. But in prehistoric archaeology this distinction is uniquely clear, since we have to extract a verbal account from an absolutely mute material of artefacts (including, of course, ancient monuments and traces of human activities in nature).

How is it possible that archaeology in the last decade was so deeply affected

by relativism, that many archaeologists seem to be sceptical about the possibility to obtain safe knowledge about the prehistoric past? Richard Watson, who is a philosopher by profession, underlines that *philosophical* scepticism cannot be refuted (1991:280). But archaeologists are not philosophers, Watson maintains, and so they need not be concerned with metaphysical questions about reality. Extreme scepticism never has and never will undermine practice.

This is also Winberg's position. There must be a (present, historic, prehistoric) reality, about which we can obtain knowledge. The decisive proof is the fact that mankind to an ever increasing extent has learned to master nature, understood more and more how it functions, achieved scientific and technical progress (Winberg 1990:7).

Of course we can draw safe conclusions from mute artefacts and traces. If we observe footprints on the snow-covered ground, do we doubt that somebody walked here? If the footsteps lead to a house, do we call in question that the person arrived there? Of course not. The proof is not absolutely conclusive: somebody might have invented a cunning device to cheat us. But such things happen very rarely; it is overwhelmingly probable that our first inference is correct. Almost all inference in applied sciences is of this type: more or less probable, but not absolutely conclusive. Some relativists in archaeology admit that such inference is possible, but only concerning trivial problems, such as the function of tools and the like. It is often maintained that prehistoric man thought in ways that were so totally different from ourselves that we simply cannot understand his ideas and actions. This, however, remains to be proved. Anyone who is in doubt whether archaeology can achieve substantial results is recommended to compare our present knowledge with what was known 50 years ago. We do have reliable knowledge even of ideas and mentality, and we have good hope that we may achieve an ever more comprehensive understanding of what happened in the past (Trigger 1991:73).

Archaeology is based on all other sciences and on common sense, Watson concludes (1991:278), and it may be added that this was the case from the beginning of scientific archaeology (Malmer 1991:286). This seems reassuring, and archaeologists may be content with Watson's (1991:280) declaration that archaeologists need not be concerned with metaphysical questions about reality. Nevertheless it seems satisfactory, that since 20 years or more a philosophical school exists which allows archaeologists to speak of the prehistoric past as a *reality*, not as a construction (Johansson & Liedman 1981:101). Its initiators are Rom Harré (1970; 1986) and Roy Bhaskar (1975; 1979). Muurimäki (1982; 1986) gives a good introduction.

Harré and Bhaskar recommend a *theoretical realism*. Positivists maintain that



observation is the only source of sure and certain knowledge. What is not observable is in their terminology “theoretical”. But realists assert that theoretical terms have ontological status, i.e. that they have real existence (Gibbon 1989:48). On the other hand realists and positivists agree in that science must be objective and rational, and that scientific theories must build on empirical data (Gibbon 1989:143). Realists argue from observable data to hidden causes. We have to establish casual links between observable surface phenomena and underlying structures, and so description of even small observable details are not worthless – they may have important underlying causes (Gibbon 1989:167; Winberg 1990:12). The way of thinking recommended by the champions of theoretical realism is exactly the research strategy observed by all good archaeologists since the beginning of our science. Archaeology’s unique dichotomy between observable data and the reality about which we want to obtain knowledge has made this theoretical standpoint most natural. What has happened is simply – but indeed very important – that philosophy has provided us with an explicit approbation of our way of working. We don’t have to choose between positivism and relativism, which both have obvious defects.

Prehistoric artefacts are real, and the prehistoric past is real, which means that both levels are connected by logic. Consequently we have good chances to study prehistoric reality on the basis of the artefacts it produced. Or, as Bhaskar (1975: 113) puts it: “Whatever is capable of producing a physical effect is real and a proper object of scientific study.”

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## CHAPTER 6

# The distinctive character and value of mass finds

1994

THE TERM “MASS FIND” is normally employed to mean thousands, or tens of thousands, or even tons of objects which look exactly the same, and which moreover seem rather uninteresting when considered individually or as a type. If we wish to ascertain the distinctive character and value – or lack of value – of mass finds, however, it is best to begin with the truly *unusual* objects, the finds that are unique in the proper sense of the word. Or let us begin even further away and ask ourselves about the justification for the continued and constant collecting of material in archaeology. Few archaeologists today are likely to doubt that it is justified to collect material, in principle, but there is a pair of terms from the 1970s which really exude serious doubt. I am referring to the fashionable terms *material fixation* and *find positivism*. Many young archaeologists actually believed – and perhaps still believe – that collecting material had become an end in itself in archaeology, not leading to any increased knowledge about the past. A hidebound older generation of archaeologists was accused of labouring under the misapprehension that knowledge about the past flows unchecked from artefacts and ancient monuments as soon as they are described. The radical young generation thought that the time had come to stop digging for its own sake, or because the Ancient Monuments Act tells us to do it. The reason was that it seemed to them that most excavations only yielded confirmation of previous research findings. If any digging was to be done in the future, it should be in order to solve explicitly formulated problems. And among archaeologists in general, it was actually common to express oneself as follows: “The excavation business is getting bigger and bigger, but the growth of knowledge per decade is just getting smaller and smaller”.

Doubts about the utility of continuing to collect material did not mean, however, that archaeologists in general took a pessimistic view of the future of archaeology or its role in society. On the contrary, many thought it was possible to make great progress if only archaeology abandoned the alleged material fixation and instead created a new way of thinking, a new theoretical basis. In my view, there is no doubt that the last thirty years have been an important period

in the history of archaeology, probably one of the most important. Our theory has become explicit and archaeology has thereby attained maturity as a science. But does this mean that the generation of young archaeologists in the 1970s were right in their opinion that the time for excavating and collecting material was over? No, of course not. The slightest consideration will show that new material has been of crucial significance for the growth of our knowledge of prehistory, in recent years also. If we confine ourselves to Sweden we can mention, for example, the Mesolithic cemetery at Skateholm and the Stone Age settlement sites in Norrland with preserved house structures, the rock paintings in western Sweden, and the Neolithic cult site at Stävie, Bronze Age houses at Fosie and Apalle, the Eketorp ringfort and the trading site in Åhus, the Viking ships in Foteviken and King Olof's mint in Sigtuna, Helgeandsholmen and the warship Vasa. We have all this – and a hundred times more besides. This has given us crucially important new knowledge, and obviously this is not something we could have arrived at just by thinking; it is knowledge that required reconnaissance, survey, and excavation.

It is self-evident to most of us that the desire in the 1970s to get away from the material is enigmatic. The reason actually lies outside our own science, namely, in the development of ethnography into social anthropology. The science of the Third World's weapons and tools, houses and clothes, was transformed into a science of human relations, societies, and ideas. It was the possibility of a similar transformation that exerted an irresistible attraction on Swedish archaeologists in the 1970s. And of course, it is actually more important to know what Stone Age people thought about the authority of the tribal chieftain and the cohesion of the family than to determine how they polished their flint axes, however interesting that may be.

Knowledge about the transformation of ethnography into anthropology came from American archaeology. This is wholly natural, since North American archaeology has the same goal, namely, to study the culture and society of the Native Americans. It is therefore entirely correct when Willey and Phillips, in the most famous sentence in their famous book, say that "American archaeology is anthropology or it is nothing" (Willey & Phillips 1958:2). On the other hand, it was misleading in Sweden when the word American was omitted and the thesis was generalized as "Archaeology is anthropology or it is nothing". That wording is erroneous because Swedish prehistory is by no means included in the subject of social anthropology. Above all, the wording is dangerous because it insinuates that ancient artefacts and monuments are of subordinate significance in archaeology, that is to say, precisely the view cherished by many in the young generation of the 1970s.

That opinion is probably due to a deficient awareness of both earlier Scandinavian and modern Anglo-American archaeology. Nordic archaeologists, of course, have no living people to interview, as social anthropologists do, but it is profoundly unfair to claim that they have ever had a fixation on material. On the contrary, they have always been convinced that artefacts and ancient monuments are not just dead matter but also reflect prehistoric people's economy, ideas, and society. That approach can already be found in Ch. J. Thomsen, who also knew of a method that could be used to translate material culture into non-material, namely, ethno-archaeology (Thomsen 1836:67; M.P. Malmer 1989). Swedish artefacts increase greatly in knowledge value by being compared with the tools of present-day low-technology societies. This applies not just to technology but also to the sphere of ideas. Material symbols can be a way for people to show which group they belong to, says Ian Hodder in a well-known book (1982), but the symbols also have an effect on their own group and steer its behaviour in various contexts.

Ethno-archaeology is also a significant element in what Lewis Binford (1977) calls *middle range theory*, the theory of the relationship between human behaviour and human ideas on one hand and material culture on the other. Binford's method, as many people know, involves meticulously registering and presenting seemingly trivial details of archaeological excavations. In the essay "Smudge Pits and Hide Smoking" (1972:37), for example, we learn that the mean diameter of the smudge pits in question is 30.27 cm and the mean depth is 33.53 cm. They are filled with remains of carbonized corn cobs, bark, and branches along with cow dung and so on. Binford cites examples from the Sioux and Blackfoot Indians to explain these smudge pits and proves that they were used for smoking hides to make them stronger. Another example of Binford's ethno-archaeological method is his studies of the way Nunamiut Eskimos split bones to extract marrow, which explains the similar working of prehistoric osteological material (Binford 1981:148 ff.).

This way of working had already received its theoretical justification in Walter Taylor's epoch-making work *A study of archaeology* (1948). Taylor thought that the focus of early American archaeology on classification and chronology led to routine behaviour in fieldwork, so that archaeologists retrieved only what was called key artefacts, but not undecorated potsherds or other mass material, and above all they neglected botanical and zoological material. Taylor recommended instead what he called "the conjunctive approach". One should not study individual artefacts but the settlement site as a whole, with its structures and natural surroundings, artefacts and ecofacts, and how they were related to each other. This was no doubt new in America, but not in Europe, where Swiss

pile dwellings and Danish kitchen middens had been excavated in precisely this way in the mid-nineteenth century (Müller 1900). Sweden too can boast of settlement site excavations which would have met with Taylor's approval, if he had known of them, and in particular we have contributed geological and palaeobotanical methods which were not known or applied in the USA until much later.

For the continued discussion it is necessary to define the term "mass find" and distinguish it from a rare or unique find. But that definition will not be easy to formulate. It may seem natural to experiment with a numerical definition for a *differentia specifica*, but it soon proves just as difficult as defining a sand heap. One grain of sand does not make a heap, nor do two, or ten – it is simply impossible to define with that method.

Instead one can try to say that mass finds consist of objects that are exactly like each other. But that formulation will not do either, because there simply are no two objects, whether of stone, bone, pottery, metal, or any material, that are exactly the same.

In my opinion, there is just one way to define the term "mass find", namely, according to the attitude shown by researchers to such material. The definition will then read as follows: a type is reckoned as a mass find if no one in the present state of research finds it meaningful to divide it into subtypes.

The crucial element in this definition is, as everyone will understand, the qualifier "in the present state of research". That this is true can easily be illustrated from the history of research.

One of the very best works in Scandinavian archaeology is Georg Sarauw's "En stenalderens boplads i Maglemose ved Mullerup", the first work ever written about the Maglemose culture and the eponymous settlement site. There he depicts, among other things, a remarkable new type which he had found in only two examples (Sarauw 1903:208). It is a microlith. Sarauw's excavation at Maglemose is one of the best and most meticulous settlement excavations ever undertaken. He lists 3,667 objects and over 15,000 pieces of flint waste, and one wonders how it is possible that such a large settlement site had just two microliths, the most important key artefact of the Maglemose culture, of which there are over 1,200 at the Ageröd sites, to take one example (Althin 1954).

The explanation is easy to find in Sarauw's own lucid text. He had collected, to be precise, 15,469 pieces of flint waste, of which he says (1903:204):

A closer examination of this flint waste could surely lead to important conclusions about the shaping of the tools etc.; but such a study could easily become very wide-reaching and laborious, and therefore will have to be abandoned here.

Since microliths had still not been distinguished as a type at the turn of the century, they were simply lying among the flint waste, the mass finds that were not considered meaningful, in the state of research at the time, to classify in subtypes.

One might imagine that the flint waste from the Maglemose site was preserved in the stores of the National Museum so that later researchers could examine it. But this did not happen. During Sophus Müller's time as director of the museum a different practice was followed. In *Affaldsdynger* (Müller 1900:5 f.) he gives an account of the extremely careful excavation of Ertebølle and other kitchen middens. They dug in squares of 1 m<sup>2</sup> and in metric layers of 20 cm. The interesting artefacts – the antiquities proper, as Sophus Müller says – were numbered. Animal bones and charcoal were collected layer by layer and partly identified. And flint waste was counted layer by layer but was not kept.

These principles were followed for a long time. This is not how it is done nowadays, but in the post-war years Danish archaeologists still sorted flint in the field and the flint waste was simply buried in a suitable place. And when they sorted the flint in the field, they naturally picked out the types that were known and discussed in the literature. At the turn of the century, however, when the Maglemose site was excavated, microliths were not among the known types, although we now class them as the key artefacts of the Boreal period. Nor did they know about burins, which we now know were used for the important work with wood and bone.

On the whole it may be said that *everything* in the flint waste is of the utmost interest, even after the clearly worked types of tools and weapons have been sorted out. By identifying flakes and piecing them together, one can reconstruct in detail how axes and other artefacts were knapped (cf. Knutsson 1988:52 ff., figs 22–24). The limestone crust sometimes has scratched ornament (Althin 1950). Many pieces of flint waste show use retouch and other traces of use, which give important clues to activities on the settlement site. To put it briefly, flint waste, this typical mass find, which was sorted out and buried in Sophus Müller's time, is now among the most valuable finds.

Let us now move forward in time and listen to a congenial description of Neolithic mass finds:

Our historical museum [...] preserves a great many things, which are all in one way or another associated with our great memories. There are 2,000 stone axes, which are so exactly like each other that, if you put them in a sack and no matter how well you shake it, the Devil himself will not be able to pick out a single one that is different from another. [...] Of these interesting axes, which occupy the ground floor, only a few, unfortunately, are depicted in *The History of Sweden*, but the others are in progress, to be published in fascicles

in the course of 50 years. The first floor is occupied exclusively by safety pins from the Brass Age. They are also being published. The second floor is occupied by scrap iron, which has been published lock, stock, and barrel in ten stout volumes.

The author of this museum snapshot is well known: August Strindberg (1882) in *Det nya riket* ("The New Kingdom"). Oscar Montelius can hardly have been pleased – his atlas of Swedish antiquities, *Svenska fornsaker*, had recently appeared (1872–74). We all know the impact Strindberg's amusing description had. Whether it is journalists or writers on cultural matters or the general public who wish to express their displeasure with a boring museum, this is the obligatory cliché: row upon row of stone axes.

One might think that at least the museum people themselves ought to share Montelius' view of the collections. But no, even serious archaeologists have shown themselves remarkably willing to support Strindberg's opinion. In 1959, when I became head of the Stone Age and Bronze Age Department at the Swedish History Museum, I found that the stores were by and large in excellent condition, and very research-friendly. There was just one horrifying exception, namely, the stray-find store. A majority of the stone and flint objects had not yet been catalogued. Of course, I thought that this was because the work of cataloguing was taking longer than expected, but no, it turned out that it was intentional. These masses of flint axes were not considered to be of any scientific value. Instead they were used as gifts for people who had done services for the museum. Giving away catalogued objects would have caused legal and administrative difficulties. Although they were state property, stone axes which had not been catalogued could be given away.

What kind of objects did people at the History Museum in the 1950s still think it was best to part with discreetly? When Bror Emil Hildebrand became Custodian of Ancient Monuments in the 1830s, he found that the museum was lacking *everything*: premises, funding, staff, even antiquities. He did something about the latter deficit by starting to buy up private collections. The first collection purchased was to be transported at Christmas 1845 from Trelleborg to Stockholm on the schooner *Sankt Olof*, but the ship went under in a winter storm and all the people and cargo were lost. But Hildebrand tirelessly continued to buy private collections; between 1846 and 1849, for example, he made purchases from the curate Holmberg in Bohuslän, Doctor Ekman in Kalmar, Lieutenant Pettersson in Karlskrona, and Count Wrangel in Skåne. These and similar collections are well known to many archaeologists since they contain many precious objects such as bronze swords and gold rings. But does this mean that the collections of stone objects are scientifically worthless? No, of course



not. Many of them still have information about the find spot, often written on the object itself. A map of the stray finds of thin-butted flint axes gives a more accurate picture of the distribution of the Early Neolithic Funnel Beaker Culture than a map of dolmens and settlement sites. And more than that: flint was costly since most of the country lacks natural deposits. One can thus measure the economy of different areas through the number and size of the axes. The stray finds are of great statistical value as evidence by virtue of their sheer quantity. They also enable the study of manufacturing technique and use wear. It really is possible to distinguish one axe from another – in that respect Strindberg was totally wrong, and the truth is that no two axes are exactly identical.

Let us turn to a third type of mass find, namely, the Viking Age silver hoards. Up until the turn of the century – and, I personally believe, even later – it was common for the Royal Coin Cabinet to retain only a small part of each coin hoard, primarily the types that were not already represented in the collections. The rest were used for swaps with other coin cabinets in Europe and elsewhere in the world. In earlier times it even happened that coins were simply melted down.

This practice can be illustrated through a find from Öster Ryftes in the parish of Fole on Gotland, which was made in 1871, and the latest coin in which is dated to 1039 (B. Malmer 1982:76 ff.). As tab. 6:1 shows, only 430 of the 1,939 coins in the hoard were retained. And the preserved coins are not at all representative of the hoard: all the Byzantine coins and 37% of the German ones have been kept, but not one of the Swedish coins. These were coins of Olof Skötkonung, now topical again, and bore inscriptions that were difficult or impossible to read; only a careful study of the stamping could enable identification of the date and the place of minting, but that possibility is not available any more.

*Tab. 6:1. The silver hoard from Öster Ryftes, Fole Parish, Gotland.*

	Number of coins		
	Total	Preserved	Not preserved
Islamic	110	–	110
Byzantine	13	13	–
German	1,097	409	688
Bohemian	3	3	–
English	638	–	638
Irish	11	–	11
Scandinavian	54	4	50
Danish	1	1	–
Swedish	12	–	12
<b>Total</b>	<b>1,939</b>	<b>430</b>	<b>1,509</b>

We have also lost the possibility to study the bending and pecking suffered by the coins during their circulation when users tested the silver content. Such assay marks can provide information about the paths through Europe taken by the English coins, for example, before they were buried in the Gotland soil. But not one of the 638 English coins in the hoard is available for study any longer.

Now I can imagine that some of you would object that *nice* finds, such as microliths, flint axes, and silver coins, should obviously be kept, even if they are found in masses. These are not the kinds of finds that our seminar was supposed to be about, but truly *simple* finds such as nails and smashed porcelain from East India Company ships. My point, however, is that only a few decades have passed since mass finds were treated carelessly, whereas they are now regarded as valuable. Our task is to try to imagine what judgement researchers in the twenty-first century will pass on us if we now start to throw away nails and smashed porcelain.

Let us start with bones. Nils-Gustaf Gejvall tells in his memoirs (1990) of how it could still happen in the 1940s that excavating archaeologists complained that they had not found anything interesting, “just bones”. I hardly need to say anything about why human skeletons are interesting. But let us go back to Binford’s interest in animal bones that were split for the marrow and worked in other ways. This is not material that you study once and throw away. As long as our knowledge about tools and other material culture is not total, traces of working on animal bones will be of scholarly interest. The same applies to what the bones can tell us about animal breeds and domestication problems. There is obviously never any scientific justification for sorting out and discarding archaeological finds of animal bones.

Another kind of mass material is stone. The participants in the Eketorp excavation have told me that the limestone slabs which had fallen from the ring wall and the house walls were a worrisome problem since they concealed better finds. Participants had to carry the limestone out of the fort. But many of the stones were so big and heavy that the average archaeology student could not manage to push as much as one big stone in a wheelbarrow. They therefore broke up these slabs with a sledge hammer, making it much easier to get rid of this unpleasant mass material.

The crenellated creation that now bears the name Eketorps borg is thus built in large measure of newly quarried limestone. I would have found it emotionally more satisfying to use the original stones. Above all, it would have been of assistance in the reconstruction of the houses and the ring wall if it had been possible to use stone in the original Iron Age format.

In Medieval occupation layers, nails and iron items of unknown use are often

more common than welcome finds. Most of the iron ore that was mined in Sweden, or was extracted from lakes and bogs, was probably not used to make knives, spears, and chain mail, but nails, band-iron, and the like. Metallurgists can calculate the amount of extracted iron ore with the aid of the amount of slag, but presumably the amount of nails and other utility iron would be of assistance for that calculation. At all events, it is important for our knowledge of the Medieval economy to know which ironworks the metal came from. We can ascertain this through trace elements – and if that is not possible today, it surely will be tomorrow.

Let us, finally, consider the smashed porcelain from the East Indiaman *Götheborg*. It is of course a sorry sight for someone who loves Chinese porcelain. But is it absolutely certain that it is only intact bowls and plates that are of scholarly interest? The fact is that the wreck has an archaeological quality that has not been sufficiently emphasized. For it is a *closed find*, which is very rare when it comes to Chinese porcelain. Someone has questioned whether the *Götheborg* is a closed find since some of the porcelain brought up by earlier divers has now been lost. But the archaeological definition of a closed find dictates only that all the objects were deposited in the ground or the water on one and the same occasion, in contrast to an accumulated find, built up over a number of years. The fact that the porcelain find from the *Götheborg* is in some respects deficient is a characteristic it shares with most closed finds.

In the case of the *Götheborg*, most of the scientific value of a closed find is preserved. First and foremost, the fact that it is a closed find ought to ensure that the finds are coeval, more so than a dating on art-historical grounds. Moreover, it is precisely a find of this kind we need to be able to discern any personal styles in the execution of details. Fingerprints could perhaps enable us to draw conclusions about the age of the decoration painters, as has recently been done with Minoan pottery. And obviously a corpus of sherds like this also gives opportunities to take samples for chemical analysis of the paint, which would be unthinkable in the case of precious porcelain items which are intact. If someone were to claim that the porcelain sherds from the *Götheborg* lack scientific value, one could venture to place a large bet that future research will refute that claim.

There is no difficulty in generalizing the conclusions that I have drawn from all these examples of mass finds, from microliths to porcelain sherds. The rule that can be derived is that there are no mass finds that lack scientific value. In cases where mass finds have been considered worthless, and have been discarded in one sense or another, it has later again and again been proved that it was a serious mistake.

There is yet another general aspect of mass finds which must be emphasised.

If we compare history with archaeology, it is obvious that the two disciplines study both the individual and society. In historical research, however, the individual is of much greater significance. Archaeology is mostly a science about anonymous people, their material and non-material culture, and their society. That is why archaeological mass finds are of such great significance: they shed light on matters that concern the mass of people. If we stick to the Middle Ages, one could dare say that the mass of rusty iron nails is of greater scientific significance than the crown of King Erik the Holy, however precious it is.

Is the conclusion, then, that we should preserve the simple mass finds just as well as we preserve the crown in the reliquary in Uppsala Cathedral? I think the problem is easier if we view it in a slightly wider perspective. It obviously does not pay to deny that we suffer a loss of knowledge if and when we throw away seemingly simple mass finds. On the other hand, there is reason to compare this with the loss of knowledge we suffer in other archaeological situations.

Closely related is the example of the loss of knowledge we suffer every time an archaeological excavation is conducted. It does not help if the excavation is performed with the greatest of care by the foremost experts. There has never been an excavation where the total knowledge potential has been utilized. Each time an excavation permit is granted, it entails a loss of knowledge, just as definitive as when mass material is discarded. This is not changed by the fact that the excavation permit simultaneously entails the acquisition of other knowledge.

The removal of ancient remains and the dumping of mass finds is an *active* disposal of knowledge. But there is also *passive* disposal, passive destruction. Despite all the surveillance and all the caution, a large quantity of sites and artefacts – knowledge – is destroyed every year in Swedish gravel pits alone. Other evidence is destroyed by work in farming and construction. It is my hypothesis that the total quantity of knowledge that is passively allowed to disappear each year corresponds roughly to the contents of a small Swedish provincial museum.

Our Ancient Monuments Act is fine, and we must follow it, as we do all laws. Yet what we do as scientists is not to observe laws and ordinances, but to search for knowledge. We always have to weigh the resources against the knowledge we gain. With the resources we have, we must acquire as much knowledge as possible. Sometimes we are undeniably tempted to think that, for the same amount of money that it costs to store mass finds, we could buy more knowledge through new excavations. But when we perform that calculation we have to bear in mind that the mass finds we have in the museum stores are usually the result of costly excavations, and that experience teaches us that, sooner or later,

they always turn out to contain much more knowledge than we understood at first. Despite the cost of storing mass finds, it can prove to be a good saving to keep them, as one otherwise might have to undertake yet another expensive excavation to acquire the same knowledge. And my hypothesis is that, in most cases, it is more economical to keep the mass finds than to throw them away.

Altogether, the disposal or sale of mass finds seems like such an extreme measure that the discussion is almost unreasonable. The problem of mass finds is thus reduced to a question of suitable storage, and in this area there seem to be possibilities for new thinking. Keeping mass finds in heated stores, in buildings on expensive land, is expensive. But when it comes to material like flint waste, this kind of storage is not necessary from the point of view of preservation; it could just as well be buried. And simple iron objects, which are now often kept in museum stores without any conservation, for lack of resources, would be better preserved if they were buried in sand of appropriate chemical composition. This would of course make the material less accessible. It could not be dug up again too many times. But we know from experience that archaeological mass material, even if it is preserved in the most expensive and easily accessible way, is only really topical during documentation and publication and for some time after. It can take decades before the material becomes topical again, when research takes a new course.

The storage of mass material through burial may be viewed as an intellectual experiment with little prospect of being implemented in practice. There are after all many other possibilities for storage in places that cost less than the most expensive museum stores, and the real purpose of the intellectual experiment is to point out the possibility of innovative thinking about the storage problem. It is scarcely worthy of the Swedish museum system to resort to drastic solutions such as disposal or sale of mass material before this problem is analysed more carefully.

One thing, however, seems perfectly clear already in our present situation: the fate of mass finds should not be determined by the general public, financial directors, or journalists, not even if they happen to be named Strindberg. It is archaeological expertise that must assume the responsibility for assessing the scientific and economic potential of mass finds. For my part, I think it would be best if decisions about this were not taken by the individual museum, whether locally or centrally. The proper place in this and other cases, in my view, would be an annual national meeting of archaeologists of the same type as they have in Norway. Even with that kind of arrangement it would be impossible to avoid mistakes, but a series of minuted and published recommendations would be extremely instructive.

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## CHAPTER 7

# On objectivity and actualism in archaeology

1997

JACQUES LACAN SOMEWHERE said that a problem which each civilization must consider and try to solve, is how the dead can speak to the living and the living to the dead (cf. Gustafsson 1996:44). I think these words express the meaning and function of archaeology very well. Presumably Lacan did not have prehistoric times particularly in mind, but rather times with a written language. However, the quotation applies very well to archaeology, since it is unique among the humanities in that we have to give prehistoric times a language before a discourse is possible. Or, more exactly, we have to give prehistory a clear-cut language.

Clearness, objectivity and a critical attitude distinguished good historians of all periods. Thukydides writes thus (Book 1:22):

But as to the facts of the occurrences of the war, I have thought it my duty to give them, not as ascertained from any chance informant nor as seemed to me probable, but only after investigating with the greatest possible accuracy each detail, in the case both of the events in which I myself participated and of those regarding which I got my information from others. And the endeavour to ascertain these facts was a laborious task, because those who were eyewitnesses of the several events did not give the same reports about the same things, but reports varying according to their championship of one side or the other, or according to their recollection.

Leopold von Ranke, the founder of modern historical research, has a similar position in his field as Ch.J. Thomsen in archaeology. They were almost the same age (born 1795 and 1788, respectively), and both carried on traditions from the Enlightenment. In the preface of Ranke's first book are the famous words (Ranke 1824, transl. Tosh 1984:11):

History has assigned to it the task of judging the past, of instructing the present for the benefit of the ages to come. To such lofty functions this work does not aspire. Its aim is merely to show how things actually were, *wie es eigentlich gewesen*.

Ranke demanded of historians that they should use primary and contemporaneous sources, and that they should scrutinize them critically and objectively. Thomsen made exactly the same demands (Malmer 1989). But moreover, Ranke says, the prerequisites of every time must be understood, and its atmosphere and mentality has to be reconstructed. We must try to understand why people of the distant past acted as they did. This may be called a hermeneutic outlook.

Obviously it is good to try to understand the situation of people of the past. It gives our own problems more reasonable proportions. To be able to illustrate the long row of past centuries with clear pictures and tales will give most people a feeling of being secure and at home.

Archaeology is useful to the present society, above all owing to its long axis of time, which other humanities lack. Sometimes it is said that archaeology's field of research is only society in its entirety, whereas history often deals with individuals. But it is not quite like that. With the osteologist's help we can draw conclusions from graves, such as age and sex of the deceased, illnesses and possibly the cause of death. Grave-gifts can tell us about ideology, status and wealth. In fact we often know more about an anonymous prehistoric individual than about a Medieval person, about whom the written sources often tell nothing more than his name.

The research findings of archaeology are generally reliable, for our sources are numerous and moreover almost always both genuine and truthful. This is not always the case for instance in Medieval history, where an important event may be mentioned only in a single text, which furthermore may be tendentious or even faked.

Now, which are prehistoric archaeology's sources of knowledge? Obviously the artefact material. And by artefacts I mean, now and later in this paper, all that is manufactured or worked by man, prehistoric objects and monuments, ecofacts and all traces of man in his environment.

Hardly anybody has failed to notice that during the last few decades the study of artefacts has often been criticized. In many cases the criticism is not rational, but a disinclination for the study of artefact materials is rather shown. A jargon has been developed, in which one condescendingly speaks about a fixation for objects, as if it would be a mistake to examine and document the artefacts carefully. One speaks as if knowledge about prehistoric times could and should be searched for elsewhere than in the artefacts. But if this is considered, it is of course an illusion. All information about prehistoric times is exclusively in the form, substance and location of artefacts. Of course we want knowledge about a prehistoric world of ideas, about social systems, ideology and a lot else that is immaterial. But still the *only* source of information is nevertheless



material: the artefacts' form, substance and location. Sometimes our problem is such that it will not be meaningful to measure the object or study its form in detail. The best strategy is often to see the object in broad outline, to catch its aesthetic qualities. But such an aesthetic impression is *also* entirely dependent on the object's real, physical form.

My very first archaeological excavation made an ineffaceable impression on me. In November 1945 Professor Greta Arwidsson excavated an Iron Age house near the Jägersro race-course in Malmö, and I was her assistant. It rained, and it was cold and dark. The clay was stiff, and we found very little, just a few undecorated pottery sherds. In those days there was a famous jockey on the race-course, whose name was George Killick. Not only was he a skilful horseman but also a clairvoyant, a spiritualistic medium. He visited our excavation, and he quickly seized a pot sherd and put it to his forehead. Then he told us what he saw: how the house was furnished during the Iron Age, how fire blazed in the hearth, how men fought and women lamented. I shall not go into my own views about parapsychology, but the comparison between Killick's seance and our method was instructive.

Probably most archaeologists think that a careful record of the details of the artefact material is important. Nevertheless warnings are given against collecting a lot of data which will not result in increased knowledge about prehistoric times (Johansen 1979:129). I think that these warnings are unjustified and detrimental. No matter how carefully an artefact material has been studied, it is *always* possible to make an additional observation. Of course every observation is not equally informative, but *any* new observation will increase our knowledge about prehistoric times. However, obviously we need a theory to help us find those elements in the artefact material which will give the best possible knowledge.

The word *theory* has enjoyed changing popularity in the course of time. In the beginning of the 1980s I once found the words "theory" and "theoretical" 16 times in the first half page of a paper by a distinguished colleague. By that time "theory" obviously was a very fashionable word, used to embellish one's text. On the other hand I looked in vain for the word in my own doctoral thesis (Malmer 1962:V, 879). In the latest archaeological texts the word "theory" is no longer very popular; it has been superseded by other fashionable words. For my part I used "method" and "hypothesis" instead of "theory", because I was of the opinion that these words had a clear meaning, and clearness was something I really aimed at. "Theory", however, is not a sharply defined notion. Prawitz (1995:173) explains the meaning of the word thus: "A group of assumptions or statements which explains phenomena of some kind, and systematizes our

knowledge of them". So according to this definition theory is simply synonymous with sensible mental activity. Thus the difference between theoretical and practical archaeology is small. For example, it is quite correct to say that the planning of an excavation is a theoretical act (Apel et al. 1995:52). Sometimes it is put forward as something rather radical to have a theoretical base when selecting the material details which you intend to discuss, but that is of course self-evident.

In order to function as a good tool archaeological theory has to be structured, of course. Trigger (1989:20) has made a classification in three levels. On the lowest level are the data of the artefact material, as well as generalizations of them, usually in the form of defined types. Middle level theory includes generalizations of human behaviour/such as the economic, social and ideological functions of artefacts, but also for instance the organization of the family, the structure of the village and political circumstances. Binford's (1981) middle range theory aims at the relation between observable artefacts and archaeologically unobservable human behaviour. And this is, of course, a central point in archaeology.

In the case of high level theory it is necessary to make a more definite choice between systems such as ecological determinism, Marxism and idealism. Trigger points out that these high level theories cannot be tested effectively; rather, they are like religious dogmas. Nevertheless many archaeologists are mostly interested in these high level theories. And nowadays, unfortunately, many think that work on the lowest level is at best uninteresting and at worst meaningless.

Archaeology has two great groups of neighbouring sciences, namely the other humanistic disciplines and the natural sciences. The materials of the first group are verbal and human. The materials of the second group are mute and non-human. The material of prehistoric archaeology is different from that of all these disciplines in that it is both mute and human (Malmer 1984:266; 1993:146). Consequently archaeology is almost unique. Only somatic medicine may be said to have a similar position; and the comparison between archaeology and medicine is not so pointless as it may seem at first. The medicine of the old ages was, at best, common sense, and the same may be said about archaeology before Thomsen.

During the last few decades criticism has often been directed against a supposed ideal of natural scientific reasoning, which has been said to characterize archaeology especially during the first half of the 20th century. Possibly archaeological problems have sometimes been treated in such a manner that humanistic aspects are superseded by natural scientific ones, but in my experience, this has not occurred often. The opposite situation is much more frequent. Archaeo-

logical artefacts are 100% substance, even if they express psychological or ideological realities. Thus we cannot, with retained scientific reputation, analyse archaeological materials without mathematical, physical, chemical and other natural scientific methods.

Yet archaeology is not a natural but a humanistic science. The difference is obvious if one tries to use Thomas Kuhn's notion of *paradigm* in archaeology (Sterud 1973). Kuhn (1970:11) introduces this notion by quoting an example from the history of physical optics. According to Newton's *Opticks*, which was published in 1703, light consists of small particles. Einstein taught that light is transversal wave motion. Today physics textbooks tell that light is photons, i.e., quantum-mechanical entities. There is no possibility to combine these three explanations into a unitary theory; you have to explain the character of light in one of these three ways. This is the reason why the three theories replaced each other in a revolutionary manner, as the title of Kuhn's book suggests. Common to the three theories is, however, that they are abstract. They have nothing to do with a seeing man's experience of light, and so far they are inconceivable to human common sense.

Incommensurable explanations of this type, which replace each other in a revolutionary manner, cannot exist in archaeology or other behavioural research. Different schools of research certainly exist, but they don't exclude but rather complement each other, and they are, and must be, within the human sphere. Consequently the notion of paradigm does not function well in archaeology.

Of interest is, however, an anecdote about Thomas Kuhn, recently told by his student, Professor John Heilbron (1996). During a cocktail party at Harvard University Kuhn was suddenly asked what type of research he was doing. A silence fell over the room, and everybody listened with strained attention. Then Kuhn answered in real earnest: "I seek the truth".

Obviously archaeology should test the applicability of theories and models of explanation in neighbouring sciences, especially anthropology. Ethnoarchaeological research is a very important connecting link between the two disciplines. Ch.J. Thomsen introduced ethnoarchaeology as early as 1836, and two years later Sven Nilsson published a more detailed version of Thomsen's ideas. Anthropologists of today have largely abandoned ethnographic artefacts, since they are convinced that interviews with living people will give much richer and more clear-cut information about society and ideology. And of course they are quite right, provided that one speaks the language of the studied population, and speaks it quite well. It is really not enough to understand the main sense, one must also be able to detect the nuances in a conversation about delicate and

important subjects. But in fact it gradually turned out that even prominent anthropologists, such as Margaret Mead, needed help from interpreters. It also appeared that the persons interviewed sometimes told stories which did not really stick to facts, or even tried to make fun of the credulous westerner (Freeman 1989).

Of course it would have been much easier for anthropologists to test the veracity of statements about the functions of artefacts. But of late they have left exactly that undone. So ethnoarchaeology on the whole has been developed only by archaeologists. But in spite of the important work carried out by Binford (e.g. 1967), Hodder (1982) and many others, ethnoarchaeology has not yet acquired the central position that it deserves. Good results have been achieved, but most of the work remains to be done.

Archaeologists have a natural disposition to recognize themselves, so to speak, in prehistoric man. They find their own notions and ideas in the artefact material. In the recent literature there are many warnings against this. More seldom are there warnings against a nowadays rather common, opposite inclination: to describe prehistoric man as maximally different from people living today.

The founder of modern geology is James Hutton, who was an older contemporary of Ch.J. Thomsen. In his work *Theory of the Earth* (1795) he presented a theory which was later called *actualism*. This theory says that such geological processes which take place in present times happened in the same way in the past, during the historical development of the earth. Consequently the theory can be summarized thus: the present is the key to the past. However, the theory does not rule out that other processes occurred in the past, which have no counterpart in the present.

Obviously the term actualism is of use also in archaeology. One variant of *archaeological actualism* is ethnoarchaeological methods. Another variant is archaeology by experiment. But the notion of archaeological actualism is much wider. It comprises the totality of modern man's perception of the artefact material compared with the perception of the world around us, not least our everyday surroundings. Surface, weight, light, colour, water, stone and all other such elementary phenomena are probably experienced in the same way by man today as in prehistoric times.

If we want to find Stone Age habitation sites within a certain area we can start by listing those sites which are already known. Then we can make careful statistics of the position of these sites in relation to various elements of the terrain. After that we must revise the numbers with regard to those modern factors which caused the known sites to be discovered. Guided by these data, we can at last try to find new possible places for habitation sites.

But another method is simply to sit down on the hillside and feel whether we are sheltered from the wind and warmed by the sun. And if we feel comfortable on the hillside, it may be worthwhile to dig a test pit. Even if this simple form of actualism is insufficient as the only method to find new habitation sites, it may at least prove helpful.

In most artefact materials there is a polarization into two groups of data. The one group consists of phenomena which can be actualistically interpreted by means of modern ethnographic or Western material, or by means of our own personal experience. The other group of data cannot be interpreted in that way. After such a division we shall probably find that part of the actualistically interpreted first group actually seems to have a double explanatory potential, with a link also to the second group. In this way both groups of data will get new explanatory possibilities.

Fig. 7:1 shows a recently published bronze statuette from the Late Bronze Age, found on the mountain of Kullaberg in north-western Scania (Paulsson 1996). This is the fourteenth statuette known of this type (most of them listed by Malmer 1992:382). Ten of the statuettes have been found in Scania and Zealand, most of them on both



*Fig. 7:1. Bronze statuette from Kullaberg, Scania, Sweden. Length 134 mm. Weight 106 g. Photo: Christer Åkerberg.*

sides of the Sound. It has been possible to weigh ten of the statuettes, and six of them have a uniform weight of c. 107 grams. The other four obviously belong to the same weight system, for one weighs  $1\frac{1}{2}$ , one  $\frac{3}{4}$  and two  $\frac{5}{4}$  of the standard weight. The Kullaberg statuette weighs almost 106 grams, which means that the weight has once again been confirmed. Further confirmation is provided by the golden so-called oath-rings, which are calibrated according to the same weight system (Malmer 1992:380–383, figs 4–6). It is not unlikely that the basic weight unit of the system is 26.5 grams (Sperber 1996:50), which would mean that the weight of the majority of statuettes is equal to four units, whereas one statuette is equal to three units and two are equal to five weight units.

So it seems as well proven as anything in archaeology that the south Scandinavian Bronze Age was acquainted with a weight system, and used it at least for weighing gold and bronze objects. It is striking that the weight of bronze statuettes was as precisely calibrated as that of the precious gold rings. But a reasonable explanation is that the statuettes were weights and that the gold rings were among such things that were weighed. Weight systems in the Bronze Age of Greece and the Middle East support this hypothesis. For example, it appears that the weight of the Kullaberg statuette corresponds almost exactly to 24 Attic drachmas. And the same weight occurs in Egypt at the time of Akhenaten in the 14th century B.C. (Sperber 1992:617).

The statuette from Kullaberg and her parallels are so expressive that the explanation may seem quite obvious. Already at the turn of the century Arne (1909:178) wrote that they imitate the goddess Ishtar in Babylonia, who was called Astarte in Phoenicia and Aphrodite on Cyprus. In modern research it would be natural to discuss gender ideas as well. No doubt the statuettes express something about the position of woman in Bronze Age society.

Stenberger adopts Arne's ideas and adds some concrete details (1964:300):

In their clumsy and modest design they may constitute a cheap mass production, spread among ordinary people. It is close at hand to explain them as images of a goddess, idols, which were placed in the homes and served as a kind of household goddess.

On the whole this is pure fantasy, and undoubtedly Stenberger would not have explained the statuettes in this way, if he had known their weight. The first statuette incorporated in the collections of the Stockholm Museum of National Antiquities came from Sankt Olof in Scania. This happened in 1895, and you may ask why it took 100 years before the statuettes were weighed. Probably the expressive look of the statuettes was considered to speak for itself. To weigh them would have been a senseless pedantry. But of course the weighing should

have been a matter of routine. And if the statuettes had been weighed, one would no doubt have soon discovered that they were made in accordance with a strict weight system.

Weighing the statuettes is to use an actualistic method. Metal is expensive today, and it was even more expensive in the early metal age. The peoples of the European continent, who owned the mines, had no reason to send gold and bronze to Scandinavia in unlimited and unweighed quantities. An actualistic explanation of the weights of gold and bronze objects must be that Bronze Age people were scrupulous about these matters.

Of course the appearance of the goddess was by no means insignificant to Nordic Bronze Age people, nor were the tales which probably accompanied her from Phoenicia to Scandinavia. On the contrary, the characteristics of the goddess, and the protection she could give, were no doubt important to the Scandinavians. But the accurately calibrated weight could of course not enhance the fame and holiness of the exalted goddess. It would really be absurd if you had to take out a pair of scales to be sure that the statuette represented the right goddess. The weight of the statuette cannot be religiously motivated, but the case must be the opposite: the well-known effigy of the goddess must have legitimated the weight in roughly the same way as the royal hallmark right up to 1972 made the Swedish shopkeepers' weights valid. When weights in the Cypriote Bronze Age were shaped like a calf, the signification was the same (Malmer 1992:386). And the dance around the golden calf in Exodus is well known.

The weight of the goddesses is by no means an exception. On the contrary, it is a typical case. No reader of archaeological publications can fail to note that they are often bristling with details, which at first may seem unimportant but which later turn out to be very essential (Malmer 1994). Data no doubt have very different explanatory power, but there are none which are devoid of it.

From the beginning of archaeology the usefulness of chronological data has been regarded as self-evident, but lately it has been debated. Generally speaking, chronology matters less in the modern archaeological literature than in the earlier. Many modern museums no longer display their collections in strict chronological order, and if they do, the exhibition may be criticized as sterile and abstract (Shanks & Tilley 1987a:68). Surely it is correct to distinguish between a modern, linear perception of time, and an old, cyclical one. In the old peasant society birth, death and the four seasons of the year were noticed, but hardly the numerical sequence of the years (Frykman & Löfgren 1980:21–44). For my own part this cyclical perception of time is emotionally familiar, and I can actualistically imagine that it was common in prehistoric society. But this does not make chronological ordering less necessary. Even a person with a cycli-



cal perception of time needs a calendar, and a chronology is needed to make a good explanation.

The kind of actualism which I recommend is supported by general theory for the testing of hypotheses. As is well known, Popper (1935) maintains that a scientific thesis must be falsifiable. Many archaeologists, not least in recent times, based their hypotheses on the supposition that prehistoric people had quite different ideas from people living today. No doubt they often did, but hypotheses which are based on that supposition run the risk of being very turgid and imaginative. Above all we almost always lack facts to test them, so few such hypotheses will be falsifiable. We shall have a better logic if instead we assume that prehistoric people were quite like ourselves, well aware that in many respects they certainly were not. In that case we can actualistically contrast our own disposition and our own ideas with the artefacts of the studied period, site or region. With such a method we can, in a number of details, falsify the thesis that the ideas of prehistoric people were like our own. All details, in which the falsifying is successful, constitute the studied unit's specific traits. But those details, in which the falsifying is not successful, in all probability constitute universal traits.

Imre Lakatos (1970) to some extent modified Popper's theory about falsifying. He points out that a hypothesis will not be abandoned as soon as it is contradicted by facts and consequently falsified. For hypotheses are not judged isolated, but as parts of a great theoretical system. Such an approach fits archaeological actualism well, since its point is to contrast two great complexes of data, namely the prehistoric artefact material and the world of modern man, including our ethnographic knowledge and our experiments.

Together with social anthropology, archaeology partly developed not only a disregard for artefact material but also for objectivity. The following anecdote could have been fetched from any historical or behavioural discipline (Bergström 1987:7):

A few years ago I heard a candidate for a doctoral degree in a social science claim that the demand for objectivity could not possibly be met, and that he for his part intended to ignore it. Instead he meant to start by deciding what conclusions he wished to achieve in his research, his only problem being how to reach them. If such an attitude were to become common our view of science would probably be radically changed.

In archaeology several authors spoke about "so-called objectivity", without making the least attempt to explain why, or in what way, an objective archaeological research would not be possible. But also very clear and categorical state-



ments occur: "Theory is thoroughly subjective [...] No discourse on the past is neutral [...] A unitary and monolithic past is an illusion. What is required is a radical pluralism which recognizes that there are multiple pasts produced actively in accordance with ethnic, cultural and political views, orientations and beliefs" (Shanks & Tilley 1987b:212, 245). If this is to be literally understood, it is of course an untenable point of view. We investigate a single past, not more. If we observe a certain artefact such as a pottery vessel, it was obviously taken out of the kiln in a definite year, on a definite day and a definite minute. The potter who took it out of the kiln was not a strange compromise between an old man and a young girl. Last week the field was ploughed, or else it was *not* ploughed. Every detail in the artefact material has such an exact history, and of course Shanks and Tilley are well aware of that. But they are less interested in such details of the prehistoric past which can be absolutely and objectively established. Rather, they wish to tell a subjective story, which may prove effective in current politics.

What do we mean by objectivity? Is objectivity at all possible in archaeology? Yes, it is both possible and necessary. Objectivity means that we at least *try* to find the truth about what happened in prehistory. In science a minor fact is definitely worth more than a great fiction. Prehistory was not obscure; it consisted of mere distinct events, and these are what we search for in the first place. Secondly, objectivity means that we strive to base our investigation on a representative sample of the infinitely great number of data which the artefact material offers, and to treat these data in a logically faultless way. Thirdly, objectivity means that we do not suppress facts which are contrary to our political ideology or our archaeological hypotheses.

Archaeologists put different questions to the artefact material, because they have different interests and methods, and they judge the answers according to their personal valuations. But is it then really wrong to speak about multiple pasts? Yes, it is. The truth is that there is only one past, not many. Everything happened in one single way, nothing happened in many ways. But isn't this really to catch at words? If everybody is permitted to make a personal evaluation of the research results, could it not with a little poetic licence be allowed to speak about many pasts? No, such a formulation must be rejected because it is wrong, and above all dangerous. It will cause, and has already caused, archaeologists to present their picture of the past as if they were writing a novel rather than searching for the truth.

The issue of *so-called* objectivity, and of *choosing* a past, originates perhaps mainly in a despair over archaeology's possibilities to reach beyond the trivial and banal and produce a rich and lively picture of prehistoric times. Perhaps it

also springs from a reluctance to submit to the laborious work which archaeological artefact material always demanded, and will demand. Of course it is much easier to construct a picture of the past by hand, a picture which with this method can easily be aligned with one's own political ideas. That was how many historians wrote before Ranke, and also after that, before source criticism was generally accepted. On the basis of meagre sources with doubtful veracity, one wrote about one's own country's glorious history, in accordance with one's own political ideas.

If we really fear that archaeology, using a scientifically tenable method, will never produce anything but very simple facts about the prehistoric past, we must still say that it is better to strive for a perhaps never achieved, important and objective truth, than to abandon the demand for truth. But there is really no basis for a pessimistic view on the future of archaeology. The picture of the prehistoric past is continuously more and more concrete, rich and reliable. As a matter of fact archaeology made greater progress than most humanistic disciplines in the post-war period. This of course does not mean that archaeology was theoretically leading; the main reason is archaeology's constantly increasing quantity of artefact material. Many other humanistic and social sciences are now short of early material. For example, within Scandinavian languages the supply of Medieval texts has run short, and scholars have turned to the great material of modern texts. Fifty years ago most Swedish historians worked on problems in Medieval history, but now it is difficult to adopt new points of view even on very central events, such as the Kalmar Union between the Scandinavian countries.<sup>1</sup> For that reason the scholars' interest more and more has turned to modern history, where the material is overwhelmingly extensive. That Scandinavian archaeology made such great advances partly depends on a high interdisciplinary readiness to receive impulses from other subjects. In addition, and above all, archaeology is almost the only humanistic discipline which possesses a really extensive and moreover steadily growing material from the early and earliest ages. No historian believes that a document will suddenly be found which will solve the enigmas of the Kalmar Union. But for every point in prehistory there is a chance that new material will solve already formulated problems, or open quite new possibilities (Malmer 1994:9). Besides, archaeology's existing material is so extensive that by no means has it been examined from all relevant angles. Neighbouring humanistic disciplines seek new research objects, for instance precisely the archaeological artefact material. So it is absurd for archaeologists to despair of the relevance of their own material.

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1 The union formed in 1397 is one of the most discussed issues in Scandinavian Medieval history (SW).

To what extent is actualism consistent with the demand for objectivity? I mentioned three kinds of actualism: ethnoarchaeology, archaeological experiments, and the researcher's personal reaction to the artefact material. The third form of actualism, our personal reaction, is of course subjective in the real sense of the word. In the above the various forms of necessary objectivity have been discussed. What remains is to discuss the different kinds of subjectivism. With reference to Trigger (1989:22) we already stated that high level theories, such as ecological determinism or Marxism, cannot be strictly logically tested. They resemble the dogmas of a religious faith, which may be subjectively accepted or rejected. (But of course high level theories can be judged according to their effect on society.) On this level subjectivity is the only possible attitude. But a conscious suppressing of such facts in the artefact material, which are incompatible with one's own general theory, is of course not acceptable.

Also personal actualism exists in different forms. Sensory impressions such as cold and light, or the perception of materials which occurred both in the past and the present, such as water, flint and gold, are easily judged. We experience such phenomena subjectively, but no doubt people of the past experienced them in the same way; or the difference was at least so slight that it may be disregarded. And the list of these kinds of phenomena can be lengthened: food, scents, colours, sounds, weight, swiftness, strength, and hundreds of other experiences. This kind of actualistic subjectivity is obviously no obstacle to the understanding of prehistoric life. On the contrary, this actualism is of course the primary qualification to understand anything of prehistory.

It is more difficult to judge very specific actualistic explanations. As a single example, we can cite a hypothesis that Middle Neolithic megaliths of Västergötland were intended to "create, articulate, and objectify a ritual landscape" (Tilley 1991:76). The roofs of passage graves consist of the same igneous rock (usually diabase) as the flat-topped mountains with steep sides, which dominate the landscape. The upright walls of the megaliths consist of the same sedimentary limestone which forms the bedrock underlying the mountains. "The up-down, high-low contrasts of the landscape are reflected in the very choice of building stones used to construct the tomb". The cup-marks on top of the roofing stones "might represent constellations of stars in the heavens" (1991:74). Of course we cannot exclude the possibility that prehistoric people had very special motives for choosing the form of their monuments; in fact we must assume that they had. But the validity of an actualistic explanation may indeed be questioned if it refers to modern scientific achievements, such as star charts and geological stratification. The possibility of testing the hypothesis is, as always, to confront it with other facts. What proof is there that these very cup-marks were made at

the same time as the megaliths? At least the great majority of cup-marks are well dated to the Bronze Age, and consequently more than 1000 years younger than the megaliths. Many other Bronze Age petroglyphs are engraved on Swedish megaliths. Are there any other cup-marks that can be explained as star charts? Furthermore, is it really probable that Middle Neolithic people chose diabase blocks to cover limestone blocks in order to copy the geological stratification? No doubt people at an early stage knew that there is diabase in the mountains and limestone in the fertile plains, but not until the 19th century did anyone imagine that there, remarkably enough, is limestone *underneath* the diabase mountains. The reason why the roofs of the megaliths are diabase may well be that this kind of rock resists rain and snow very well. Limestone, on the other hand, is full of fissures in which rain-water runs down, and thus there is a great risk that the stone will be splintered by frost. For the walls of the megalith, however, limestone is very suitable since this kind of stone easily splits into flat blocks of a uniform thickness. And the wall blocks are protected against rain by the roof. This alternative actualistic hypothesis is based on the probable assumption that the qualities of rocks were as well known in prehistoric times as they are today.

The difference between the two discussed actualistic hypotheses is obviously the following. The first hypothesis presupposes that prehistoric man had the same, very special, scientific knowledge and conceptions as its author has: star charts and geological stratification. The second hypothesis presupposes only that prehistoric man possessed common sense in a very general branch of knowledge: the quality of rocks in the home district.

However, also rather special phenomena and conceptions can be actualistically explained, as shown by the goddess from Kullaberg and her sisters. What is the difference between, on one hand, explaining cup-marks and the selection of rocks in the cited way, and, on the other hand, explaining the statuettes as weights? The second hypothesis is not based on learned speculation but simply on a well-established fact, namely, that man in most and probably in all stages of culture had some measure for economic value, especially when it concerned an imported, rare and useful product. We can draw the conclusion that actualistic explanations in the first place must concern broadly human conditions. But in certain, and not so rare, cases also very special or individual problems may be solved by means of an actively actualistic attitude.<sup>2</sup>

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2 The chapter is an abbreviated version of a lecture given on the occasion of the author's 75th birthday. Figure 7:1 is published with permission of Jenny and Jonas Paulsson.

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## II.

### Innovation processes

THE INTRODUCTION AND diffusion of artefact types and cultures was Mats P. Malmer's chief interest in prehistoric archaeology right from his breakthrough article about the pleion concept (Ch. 8) up to a long chapter in his last major book about the Neolithic (Ch. 10). The method he developed to visualize an innovation process is the production diagram. This is a diagram showing how a type is introduced, culminates, declines, and is succeeded by another type introduced during the swansong of the preceding type. A series of diagrams along a geographical gradient shows how the types are introduced in one area after the other, with culmination phases of varying duration. The Middle Neolithic Battle Axe pottery was the first example of the method he elaborated (Ch. 9). The geometrical figures illustrating the life course of a type in the production diagrams resemble the battleship diagrams used in American archaeology to illustrate chronological seriation. From the 1970s onwards, this and similar seriation analyses were performed by computers (e.g. Doran & Hodson 1975, *Mathematics and computers in archaeology*, Edinburgh; for battleship diagrams, see p. 278).





## CHAPTER 8

# The concept of the pleion and its significance for the study of prehistoric innovation processes

1957

ONE OF THE most important landmarks for an assessment of cultural conditions during the Nordic Battle Axe Cultures is Äyräpää's observation that the Finnish Boat Axe Culture is associated with the warmest and most fertile parts of the country: the distribution of battle axes coincides with the clay areas west of the January isotherm for  $-8^{\circ}\text{C}$  (Äyräpää 1940:III, ills 19–20). This distribution is a strong indication that the Boat Axe Culture practised arable farming, as a culture subsisting exclusively on animal husbandry would hardly have been so closely linked to the clay areas, and in no circumstances would it have let itself be impeded by a slightly cooler climate. Already in 1922 Äyräpää made the claim that the Boat Axe Culture practised agriculture (Äyräpää 1922:162).

The Finnish and the Swedish-Norwegian Boat Axe Cultures are so closely related that it is easy to imagine that they had the same economic and social structure. Strangely, however, the question of their economic foundation has been judged in completely different ways. Whereas the Finnish Boat Axe Culture is assumed with great certainty to have introduced agriculture to Finland, where it was previously unknown, it is presumed that the Swedish-Norwegian Boat Axe Culture knew nothing of agriculture when it arrived in the Scandinavian Peninsula and only learnt it at a later stage from the Funnel Beaker Culture which had been tilling the soil for a long time. Most scholars probably agree with the view of the Swedish-Norwegian Boat Axe Culture as it is expressed in the following lines, taken from a recently published work (Stenberger 1956:17):

There is reason to assume that the Boat Axe people were primarily livestock herders. They knew the domesticated horse and they kept horned livestock. The circumstance that the graves of the Boat Axe people are scattered, singly or more rarely a pair or a few in one and the same place, suggests an ambulatory way of life.

The Swedish-Norwegian Boat Axe Culture is usually contrasted with the Middle Neolithic Funnel Beaker Culture, which is believed to have been indigenous, peaceful, mostly agricultural, sedentary, and tied to the best areas for tillage.

The Boat Axe Culture, on the other hand, is described as an immigrant culture, martial, and – at least in its earliest stage – practising animal husbandry, nomadic, and avoiding the agricultural districts dominated by the Funnel Beaker Culture (e.g. Oldeberg 1952:212; an opposite view is held by Åberg 1949; Moberg 1951:43 f.). There can be no doubt that this description of the Boat Axe Culture is based mainly on typological differences between the Funnel Beaker and the Boat Axe Cultures. The differences are so great, it is thought, that the Boat Axe Culture cannot possibly have developed out of the Funnel Beaker Culture, nor is it likely to have arisen through diffusion; it must be a result of immigration. It seems less probable that one farming culture should have occupied the territory of another farming culture. The process is much more plausible if one envisages that the immigrating culture primarily occupied areas that had previously been wholly or partly uninhabited. But these areas were less suited to agriculture; therefore, it is probable that the carriers of the Boat Axe Culture were nomads.

It can be seriously questioned whether archaeology has a reliable method to assess the significance of such typological differences as those between the Funnel Beaker and the Boat Axe Cultures. An analysis of the stock of domesticated animals in the two cultures presents no methodological difficulties; but here we lack the material instead. At all events, there is no horse or horned livestock in the 35 Boat Axe graves which have contained identifiable animal bones (Møhl-Hansen unpubl.). The only reliable method for testing the reliability of the traditional picture of the Boat Axe Culture as an immigrant pastoral culture would thus be the chorological one. Fortunately, we have a very large amount of material from the Boat Axe Culture for which we know the find spots. Some chorological aspects of this material will be considered in the following.

The January isotherm for  $-8^{\circ}\text{C}$ , which has proved so significant for assessing the Finnish Boat Axe Culture, crosses the Gulf of Bothnia roughly on a line from Brahestad to Piteå. It then turns south and follows the coast of the Gulf as far as Hälsingland, where it turns west and passes, via Siljan, into Norway in the region of Kongsvinger. At the latitude of Bergen it turns north again and follows the coast of Norway all the way up to the Arctic Ocean (e.g. Ångström 1946, Pl. 1). The isotherm thus divides the Scandinavian Peninsula into two parts of roughly the same area. In the southern half we find all the roughly 240 graves of the Swedish-Norwegian Boat Axe Culture, the northernmost of them (in the Oslo district) at a distance of some 50 km from the isotherm. In the southern half we likewise find all the pottery of the Boat Axe Culture, the northernmost examples (in Hälsingland) immediately south of the  $-8^{\circ}\text{C}$  isotherm. Of the roughly 2,600 boat axes currently known from the culture, only

20 or so, or approximately 1%, have been found north of the isotherm. It is of particular interest that the isotherm in Västerbotten runs much further inland than it does in more southerly areas; this means that all the big hoard finds of hollow-edged flint axes occur south of the critical isotherm (for a map, see Becker 1952:34, fig. 1). The significance of the  $-8^{\circ}\text{C}$  isotherm for today's agriculture is evident from the fact that the area of wheat and rye as a percentage of the entire cultivated area is vanishingly small north of the isotherm. The area where haymaking occupies more than 70% of the cultivated land, on the other hand, coincides fairly exactly with the area north of the isotherm. In the same area the proportion of cattle, sheep, and goats per areal unit is incomparably larger than in the southern part of the Scandinavian Peninsula (Jonasson et al. 1952:51 ff., 115 ff.).

The Boat Axe Culture is thus undeniably associated with the part of the Scandinavian peninsula where the conditions for agriculture nowadays, and probably also in the Neolithic, were best; in contrast, it avoids the large area that is suitable for animal husbandry and no doubt was so in prehistoric times. The same applies to the Funnel Beaker Culture. But the similarities are not confined to this. If we compare the distribution of boat axes (Forssander 1933:25, ill. 13) with the battle axes (Åberg 1935:338) and thin-butted flint axes (Oldeberg 1952, Ill. 314) of the Funnel Beaker Culture, we find a striking similarity, as Åberg in particular has asserted. All three types are represented most frequently in Skåne and in the plains of Västergötland, Östergötland, and the Mälaren valley. An objection that has been heard to this is that the concentration of the boat axes in these flat agricultural districts is illusory: most boat axes, it is argued, have been uncovered by agricultural work; this is why they appear to be concentrated in today's plains (Althin 1953:934). Another objection that has gained general support was formulated by Forssander: the oldest boat axes do not belong to the flat settlement districts at all, but to the sparsely populated areas between them; but when the Boat Axe Culture, in a later stage, occupied the rich and densely populated plains districts, the production of boat axes multiplied (Forssander 1934:123, 141). These divergent opinions about the distribution of the Funnel Beaker and Boat Axe Cultures in Sweden show that one cannot arrive at sure results without a more detailed examination, which in this case means confining the study to a small area. The area selected here is Skåne. This province is well suited to an intensified chorological study, since both the cultures concerned are more richly represented there than anywhere else in the Scandinavian Peninsula.

The primary aim of the study is to elucidate three questions of crucial significance, namely: 1) whether it is justified to conclude from the distribution of

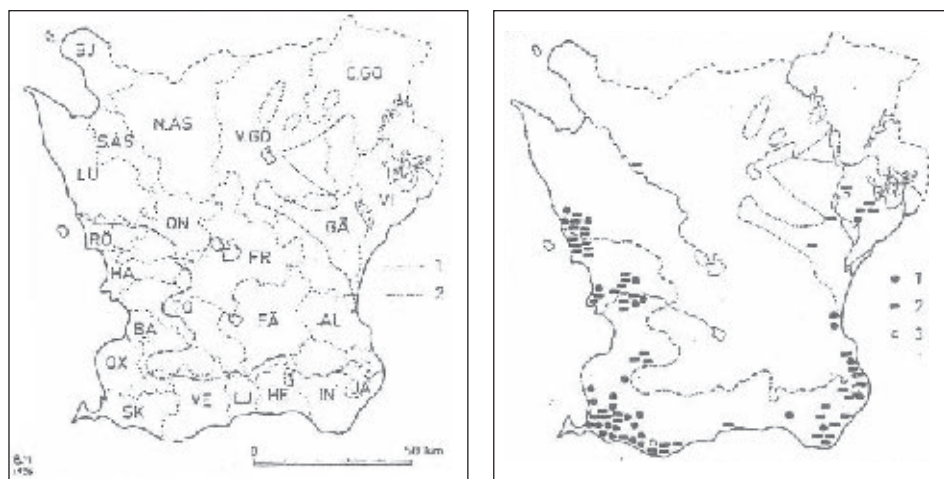
the Funnel Beaker Culture that it practised agriculture; 2) to what extent the distribution of the Boat Axe Culture differs from that of the Funnel Beaker Culture; 3) to what extent the earliest stage of the Boat Axe Culture has a different distribution from the later stages.

Proof that the Funnel Beaker Culture tilled the soil has been sought by comparing the distribution of thin-butted flint axes with that of arable land today (Oldeberg 1952, ill. 314 compared to Anrick 1921). The maps are undeniably similar, but it is easy to demonstrate obvious differences in details. As regards Skåne, for example, it will be noticed that the area of arable land in the north-western part of the province is very large, while there are strikingly few thin-butted axes. A comparison with the present-day extent of arable land does not take into account the differences between modern and primitive agriculture, besides leaving scope for the source of error inherent in the fact that the chances of finding artefacts grows with the intensity of modern agriculture.

For primitive agriculture it must have been crucial for the land to be easily worked, that is, with neither stones nor heavy clay, and that it was fertile, meaning, above all, with a sufficient lime content (Arrhenius 1955:81). The arable parts of Skåne have been treated in two fundamental studies by Ekström (1936; 1950). The majority of the arable soils in Skåne are characterized in large measure by different kinds of glacial till. Of fundamental significance, according to Ekström, is the difference between the Baltic tills, which occur in a belt about 20 km wide along the southern and western coasts of the province, and the till in the north-east, which covers the much larger remainder of the province. The north-east type mostly consists of primary bedrock till and slate-primary bedrock till. These are both gravelly, sandy, rich in stones and boulders, and the primary bedrock till is moreover virtually free of clay. Of the Baltic tills, those in the south-east and the south-west are the best arable soils. The south-east till, with Österlen as the main area, is described as not very stony, sufficiently clayey, and easy to work. The south-west till is the fertile soil of the Söderslätt plain and the Lund plain, clay till that is virtually free of stones. The north-west till is distinctive for its higher stone content and lower clay content.

Arrhenius' map of the pH value of arable soils shows that the Baltic tills are mostly alkaline or neutral, in other words, sufficiently calcareous, whereas the north-eastern tills are mostly acidic, which means that they require added lime (Arrhenius 1950). An exception is the primary bedrock till in the Kristianstad area, which has a large admixture of chalk from the bedrock and is thus sufficiently calcareous and highly alkaline.

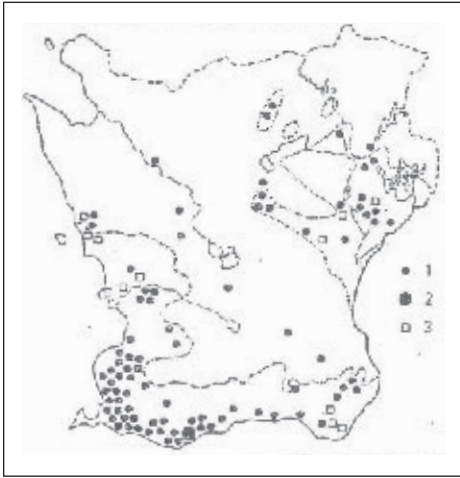
With the guidance of the scientific descriptions of the different arable areas of Skåne, it seems fully possible to judge which areas were most attractive for



Map 8:1, left. The boundaries of the southern till area and the calcareous areas according to Ekström 1936 and 1950. 1 = Boundary of the southern till area; 2 = Boundary of the calcareous area. AL = Albo; BA = Bara; BJ = Bjäre; FR = Frosta; FÄ = Färs; GÄ = Gärds; HA = Harjager; HE = Herrestad; IN = Ingelstad; JÄ = Järrestad; LJ = Ljunits; LU = Luggude; N.ÅS = Norra Åsbo; ON = Önnestad; OX = Oxie; R = Rönneberga; SK = Skytts; S.ÅS = Södra Åsbo; TO = Torna; VE = Vemmenhög; V.GÖ = Västra Göinge; VI = Villand; ÖG = Östra Göinge.

Map 8:2, right. Megalithic tombs (data from Rydbeck 1938 with additions). 1 = Dolmen; 2 = Passage grave; 3 = Dolmen or passage grave.

primitive agriculture. It must have been the southern till soils and the calcareous area in the north-east. Map 8:1 shows the boundaries of these areas; the map also marks the boundaries of the administrative districts known as *härader* or hundreds. The boundary of the southern till area follows Ekström (1950). The calcareous area, on the other hand, is given the slightly broader scope it has in Ekström (1936). The reason for this is that the map in Arrhenius (1950) shows that such seemingly peripheral parishes as Norra Mellby, Vinslöv, and Kviinge have neutral or even alkaline soils with pH values partly exceeding 7.5. Skåne has thus been divided into three areas, which we can call the southern till area, the calcareous area, and the northern till area. Skåne has a total area of about 11,280 square kilometres. Of this, the southern till area occupies some 2,505 km<sup>2</sup> and the calcareous area 1,415 km<sup>2</sup>; these two areas thus comprise just under 35% or about one third of the area of Skåne. Of the total 5,660 km<sup>2</sup> of arable land in Skåne, about 2,635 km<sup>2</sup> is in the southern till area and the calcareous area (SOS 1946). It should thus be noted that the southern till and the calcareous areas are



*Map 8:3. Graves of the Boat Axe Culture. 1 = Flat-ground grave; 2 = The Bedinge cemetery; 3 = Megalithic tomb with battle axe or pottery belonging to the Boat Axe Culture.*



*Map 8:4. Inhumation graves under flat ground from the Roman Iron Age (data from Stjernquist 1955). 1 = Flat-ground grave; 2 = Cemetery with five or more graves.*

by no means identical with the present-day area of arable lands in Skåne; the northern till area covers over 53% of Skåne's arable land today. In the southern till and the calcareous areas, arable land makes up approximately 67% of the total area; the hundreds of Luggude and Södra Åsbo in the northern till area are more cultivated today, with an arable area constituting around 75% of the total area.

We shall now test the association of these boundaries with the distribution of some prehistoric cultures, or more precisely, some important cultural elements.

Map 8:2 shows the distribution of the dolmens and passage graves of the Funnel Beaker Culture. Of the total 87 megalithic tombs, 71 are in the southern till and the calcareous areas, only 16 in the northern till area.

Map 8:3 shows the distribution of the flat-ground graves of the Boat Axe Culture (including the Bedinge cemetery with 10 graves), along with the megalithic tombs containing pottery or battle axes belonging to the Boat Axe Culture. Of the total 106 graves, 95 are in the southern till and the calcareous areas, and 11 in the northern till area.

Map 8:4 shows the distribution of inhumation graves under flat ground from the Roman Iron Age (Stjernquist 1955:166 ff.). Only two graves are in the northern till area; all the others are in the southern till and the calcareous areas.

Map 8:5 shows the distribution of runestones from the Viking Age. Four rune-





*Map 8:5. Runestones from the Viking Age (data from Jacobsen & Moltke 1942).*



*Map 8:6. Hexagonal grid used to produce isarithm maps 8:7–10.*

stones belong to the northern till area, all the other 51 to the southern till area.

From a methodological point of view, there is a similarity between the megalithic tombs and the runestones in that the chances of finding these monuments ought not to be affected by the intensity of present-day agriculture. There is an even greater similarity between the inhumation graves of the Boat Axe Culture and those of the Roman Iron Age: both types have no marking above ground and both were dug to roughly the same depth under the ground surface. The chances of finding these graves must be greater in cultivated than uncultivated land. None the less, the distribution of inhumation graves shows a striking agreement with that of the megalithic tombs and the runestones: all four types are heavily concentrated in the southern till area and the calcareous area. The distribution of the Boat Axe graves does not seem to provide any support whatever for the hypothesis that the carriers of this culture were more nomadic than people in the Funnel Beaker Culture or the Roman Iron Age. It also appears to disprove the thesis that the concentration of the Boat Axe Culture in the plains areas (or more precisely: some of the present-day plains areas) is only illusory. The northern till area has a larger expanse of arable land than the southern till area and the calcareous area together and thus should have more flat-ground graves than these, if it really were the case that the number of graves now known were in direct proportion to the intensity of modern agriculture. In reality the northern till area has 10 flat-ground graves belonging to the Boat Axe Culture, which means a density of 0.33 graves per 100 km<sup>2</sup> of arable land, while the

southern till area and the calcareous area have 84 flat-ground graves, that is, a density of 3.2 graves per 100 km<sup>2</sup> of arable land. The southern till area and the calcareous area thus have a density of graves that is ten times greater than in the northern till area; when calculated by the total area, of course, the difference in density is even greater.

The striking agreement between the cultures of the Neolithic and the Iron Age, as shown in maps 8:2–5, may justify a sample test of the Bronze Age as well. Tab. 8:1 shows the distribution of the different soil areas and the flanged axes of the Early Bronze Age, fibulae from Periods II–V, and settlement sites.

*Tab. 8:1. The distribution of Bronze Age artefacts in the different soil areas (data from Oldeberg 1933; Forssander 1936; Strömberg 1954).*

	Southern till area and calcareous area	Northern till area
Flanged axes	89	9
Fibulae, Period II	18	4
Fibulae, Period III	37	1
Fibulae, Period IV	6	1
Fibulae, Period V	15	2
Settlement sites	14	1

The find conditions for the culture elements in the table are quite different from those presented in maps 8:2–5, since we are dealing here with barrows, hoards, stray finds, and settlement site finds. Despite this, it is perfectly clear that the connection of the Bronze Age culture to the southern till and the calcareous areas is as great as in the case of the Late Neolithic and Iron Age cultures.

The distribution of graves from the Funnel Beaker and Boat Axe Cultures seems to be a clear indication that both cultures had the same economic foundation and that agriculture was of equal importance to both. The number of graves, about 100 for each culture, allows a fairly high statistical certainty. But this certainty increases considerably if we turn to examine the stray finds from the two cultures: the number of thin-butted flint axes of known provenance found in Skåne is over 4,300, and the number of boat axes of known provenance is about 370. As a control group we can use Late Neolithic artefacts, of which daggers of known provenance amount to over 6,500.

It would entail major technical difficulties to show as many as 4,000 or 6,000 find spots on one map. The type of map most frequently – indeed, almost exclusively – used in archaeological literature is a dot map; the site where each object was found is marked by a dot. To be at all visible in print, the dots must be of a certain minimum size. In areas with a high density of finds, the dots are



clustered very close together; it simply is not possible to put them in the right place without a map on an unreasonably large scale. Another, even greater, disadvantage of the conventional dot map is that it cannot show anything but the location of the find spot. Yet in many cases the circumstances of the find are crucial. If a dot map shows a very high density of finds in a certain part of the country, it need not mean anything other than that the area is heavily cultivated nowadays. These technical and methodological difficulties can be overcome if we use density isarithms instead of dots, as is common in modern human geography. The isarithm technique is not new: it was invented exactly a hundred years ago this year by the Danish naval lieutenant Niels Ravn for a population map of the Danish kingdom (Ravn 1857:XVII).

Density isarithms, as the term suggests, are lines that link places with the same density, in our case the same find density, just as isotherms link places with the same temperature. The isarithms are boundary lines between areas with a certain average density (Hannerberg 1937:228). It is easiest to produce an isarithm map with the aid of a grid covering the map. The cells can take the form of equilateral triangles, squares, or hexagons; for several reasons, hexagons are preferable (Hägerstrand 1953:28). The hexagonal grid has an advantage over squares, namely, that difficulties never arise with interpolation because of contradictions at crossing diagonals. Another advantage is that a hexagon is closer than a square to a circular form, which means that the orientation of the grid is of less significance for the density within individual cells. Hägerstrand recommends the quadratic grid in preference to the hexagonal because it can be linked to the sheet division of the economic map and thus does not need to be placed arbitrarily. In the present essay the requirement of objectivity when laying out the grid is satisfied in that the grid is the same as that used by Hägerstrand to study the density of motor vehicles in Skåne (Hägerstrand 1952:6, fig. 1). Map 8:6 shows the hexagonal grid used for the preparation of the following isarithm maps 8:7–10. The size of the grid is adapted so that each hexagon corresponds to an area of 100 km<sup>2</sup>. With the aid of this hexagonal grid one can envisage producing the isarithm map as follows in theory (and in some cases also in practice). The places where an artefact type has been found are marked with dots in the usual way. If, for example, there are 5 dots within a cell, then the density in this cell is 5 per 100 km<sup>2</sup>. This density value can conceivably be placed in the centre of the hexagon. One can also, find a “centre of gravity” closer to the actual location of the find spots in the cell (Hägerstrand 1952:7, fig. 2): if one draws a straight line east–west through the cell in such a way that as many dots fall north as south of the line, and following the same, principle, draw a line north–south, the two lines will intersect at the “centre of gravity”. To produce

the maps in this essay, the centres of gravity have been placed at the centre of the cells for practical reasons.

When all the cells have been given their density values we can draw an isarithm for a find density of, say, 5 artefacts per 100 km<sup>2</sup>. This will pass through the centre of all cells where the density is 5. If two adjacent cells have a density of 4 and 6 respectively, the isarithm for 5 will run on the boundary between the cells; and if two cells have a density of 1 and 7 respectively, the isarithm for 5 will pass a point located at one third of the distance from the centre of the latter cell in the direction of the centre of the former. Since each cell is surrounded by six adjacent cells, it is possible to determine the location of the isarithm at six points around the centre of each cell.

To produce the isarithm maps shown here (with the exception of Map 8:8), the location of the individual artefacts could not, of course, be marked with dots, partly because of the large number, partly because the find spot of many artefacts is not recorded more specifically than by the name of the parish. The number of artefacts within each cell has instead been determined with the aid of a map that includes both the parish boundaries and the hexagonal grid; most parishes in Skåne have a much smaller area than 100 km<sup>2</sup>, and one cell can hold more than ten parishes. It was considered very important to eliminate the source of error inherent in an increased find frequency caused by intensive modern agriculture. For that reason the area of arable land in each cell was ascertained (SOS 1946). Then the find density within each cell was calculated, both per 100 km<sup>2</sup> total area and per 100 km<sup>2</sup> arable land; finally, the arithmetic mean of these two values was calculated. (It might be thought that the area of *land*, not the *total* area, ought to have been chosen as the reduction base. But the lakes, rivers, etc. of Skåne constitute about 3% of the total area, while the boat axes, for instance, found in these lakes and rivers make up 7% of the total. The density of boat axes is thus *greater* in the waters of Skåne than on dry land; the same probably applies to many other types of archaeological artefact, and there is thus no reason to subtract the area of water.) The value thus obtained, which we can call “mean density”, is what is assigned to the centre of each cell, serving as the basis for drawing the isarithms. This approach is grounded on the following reasoning. Of all categories of find circumstances (such as “finds from excavations”, “finds from gravel extraction”, etc.) the category of “finds from agricultural work” has probably yielded most Neolithic finds. If *all* the finds had been obtained by agricultural work, it would have been correct to express find density as the number of artefacts per unit of arable area. But some finds are placed in other categories, and the chance of discovering such finds is probably roughly proportional to the total area. The “real” find density, which cor-

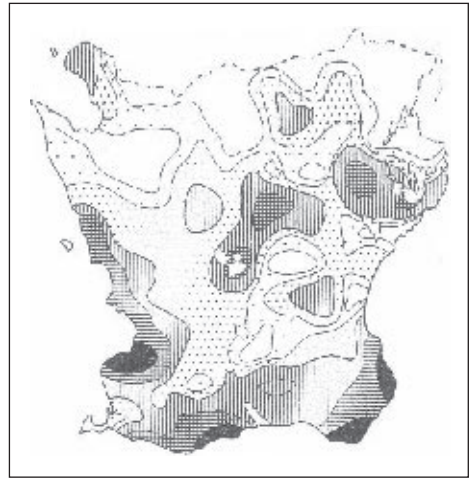
responds to the distribution of all now known and now unknown ancient artefacts, should thus lie somewhere between the density per total area and the density per arable area. Careful study of the find circumstances must surely be one of the most important tasks of research in settlement history. The isarithm map seems to be the best conceivable instrument for presenting the results of such studies, because, when ascertaining the find density within each cell there is no need at all to choose between total area and arable area as the reduction base. One can choose population density, excavation density, or the occurrence of gravel pits, or any circumstance at all that seems significant and is objectively measurable. The maps published by Therkel Mathiassen are neither dot maps nor isarithm maps (Mathiassen 1948). They are problematic.

For each period there are two maps. For the time of the thin-butted axes, for example, the different parishes are hatched in different densities according to the intensity of settlement. Other maps are divided into squares of one square kilometre, hatched in a similar way. Maps like these can thus be described as isarithm maps which have remained at the grid stage without isarithms being drawn. It is surprising that, in the very homeland of the isarithm technique, the possibility of using isarithms is not even discussed. The same hatching scale is used for all the maps, which makes comparisons more difficult because some maps are very light while others are very dark. What makes it even harder to interpret the maps, however, is the fact that they seek to show find density during a certain period, not the density of one particular type. The different types of artefacts and monuments are assigned numerical values according to an arbitrary system: each object is given the value 1, but in the case of amber beads or potsherds it is divided by 10 (except in some cases when it is divided by 100), a settlement site or barrow is sometimes given the value 5, sometimes 10, sometimes 50, and there are diffuse statements such as "several artefacts" which equals 2 and "a considerable number" which equals 5, and so on.

On the map one can draw as many or as few isarithms as desired. On Map 8:7, showing the thin-butted axes, the highest value for "mean density" is 395 and the lowest is 4. One could envisage selecting a constant interval between the isarithms and draw the isarithms for, say, 50, 100, 150, etc. But a constant isarithm interval has several disadvantages; above all, it makes it harder to compare a type that is richly represented with one that is rare. In any case, the numerical value of the density is of little interest. What is of extraordinary interest, on the other hand, is to find out which parts of the province have a greater mean density than Skåne as a whole; if this is done for each type, it is easy to compare the maps. *One* isarithm is thus given: the isarithm for mean density in Skåne, a value that we designate with the letter *M*. Otherwise a further four isarithms have been drawn



Map 8:7. Thin-butted flint axes (data from Oldeberg 1952). Key to symbols: see map 10.



Map 8:8. Boat-shaped battle axes.

on all the maps, determined so that the values form a geometric series with the ratio 2, that is, from the lowest to the highest:  $M:4$ ,  $M:2$ ,  $M$ ,  $2M$ ,  $4M$ . The fields between the isarithms are hatched according to the scale in the legend to map 8:10. Each shade of hatching thus denotes a find density that is on average twice as great as the nearest lighter hatching, and solid black areas have a find density that is *at least* 16 times as great as in the all-white areas.

The isarithm for  $M$  is marked with a thick line. It demarcates the areas (marked on the maps with solid black, squares, and lines) with a find density exceeding the mean density for the whole province. It seems desirable to have a word to designate these areas with an excess, greater than the average, and we may choose the word *pleion*. The term *pleion*, originating in climatology, has been introduced by Hägerstrand into human geography, where it denotes areas with a positive anomaly (Hägerstrand 1952:10; 1953:35).

Map 8:7 shows the distribution of thin-butted flint axes. Details of the finds have been taken, without changes or additions, from Oldeberg's survey (1952:247 ff.). The map is based on 4,318 axes of known provenance.  $M$  (= mean density in Skåne) is 57.25. (The value of  $M$  is calculated from the 4,318 axes of known provenance, not from all the axes found in Skåne; 5,528 according to Oldeberg 1952. The method is the same as the one used in the individual cells: the arithmetic mean is calculated on the basis of the density per 100 km<sup>2</sup> total area and 100 km<sup>2</sup> arable area.) It should perhaps be underlined that the white areas are by no means devoid of finds: the maximum density in the white areas is 14.31, which is more than in a rich province like Västergötland, where the mean density of

thin-butted flint axes is 11.70. (This value is calculated on the basis of *all* the axes found in Västergötland, whereas the *M* value for Skåne is based solely on axes of known provenance.)

Map 8:8 shows the distribution of boat axes of known provenance.  $M = 4.88$ .

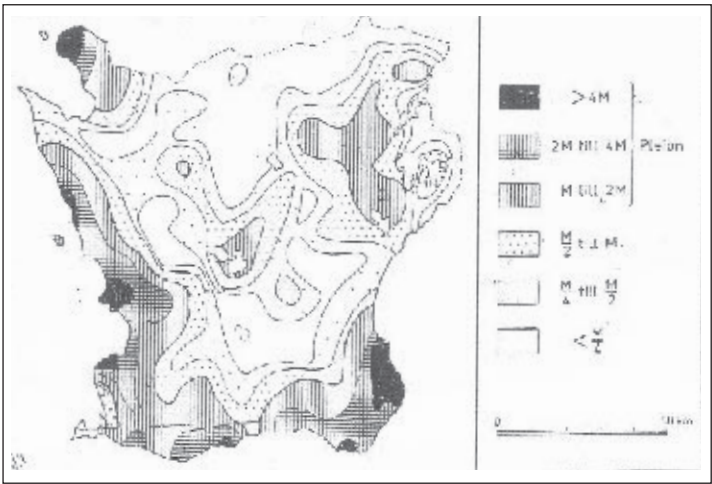
Map 8:9 shows the distribution of Late Neolithic flint daggers. The map is based on 6,515 daggers of known provenance.  $M = 86.39$ . (The find data for flint daggers and shaft-hole axes, along with a few other types, have been collected by the author in 25 museums, mostly in Skåne, and a number of private collections. A total of 8,736 flint daggers were noted, but there were no exact details of find circumstances for 2,221 of these.)

Map 8:10 shows the distribution of 1,352 Late Neolithic shaft-hole axes of known provenance.  $M = 17.93$ .

The maps show a striking agreement in the distribution of the four types. All four have by far their largest pleion area along the south and west coasts, thus corresponding to the southern till area on maps 8:1–6. Another pleion is found in the calcareous area. Otherwise the thin-butted flint axes have two smaller pleion islands, the boat axes five, the flint daggers once again two pleion islands,



Map 8:9. Late Neolithic flint daggers.



Map 8:10. Late Neolithic shaft-hole axes.

and the Late Neolithic shaft-hole axes four. There are numerous similarities in details, for examples the notable pleion character of the Bjäre peninsula (marked BJ on map 8:1) and the Ringsjön district (beside FR on map 8:1). Notably weak parts of the southern till area are Ljunits hundred and the coastal region south of Malmö (between OX and SK on map 8:1). The most important similarity in detail is seen in the areas with maximum find density (over 4M), all of which are along the coast.

Maps 8:7 and 8:8, showing the thin-butted flint axes and boat axes, display striking similarities in the main features, but also one difference: boat axes have more and larger pleion islands in the northern till area. But this discrepancy is scarcely a difference in the association between these cultures and the good arable soil; it is a difference between an artefact type of flint and one of stone. The Late Neolithic flint daggers and shaft-hole axes, which belong to the same time and culture, display the same difference. The similarities between maps 8:7 and 8:9 and between maps 8:8 and 8:10 are obvious. Flint was evidently not equally available during the Neolithic, not even in an area like Skåne with a relatively plentiful supply of flint. Sure proof of this can be obtained by studying the relative proportions of stone and flint objects in the graves of the Boat Axe Culture. The natural deposits of good flint are confined to the areas of Baltic tills, chiefly the south-western till area (Ekström 1950:58). The Boat Axe graves in the south-western hundreds Oxie, Skytt, and Vemmenhög contain 15 boat axes and 55 flint axes, thus almost four times as many flint axes. The Boat Axe graves in the whole of the rest of Skåne contain 28 boat axes and 62 flint axes, only slightly more than twice as many flint axes.

Maps 8:2 and 8:3, showing the graves of the Funnel Beaker and Boat Axe Cultures, and maps 8:7 and 8:8 showing the axe types, thus seem to demonstrate unanimously that both cultures are closely associated with the best arable soils in Skåne. The results of the study are summarized in tab. 8:2, showing the percentages of grave types and axe types in the soil areas.

*Tab. 8:2. The distribution of Funnel Beaker Culture and Boat Axe Culture artefacts in the different soil areas (data as in maps 8:2, 3, 7, 8).*

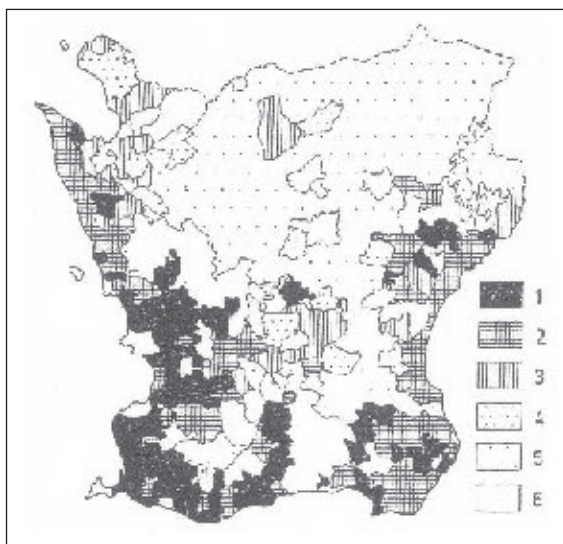
	Southern till and calcareous areas	Northern till area
Megalithic tombs	82	18
Thin-butted flint axes	70	30
Boat Axe graves	90	10
Boat axes	72	28



The table appears to show 1) that the grave types are more closely associated with the good soils than the axe types, and 2) that the Boat Axe Culture is more closely associated with the good soils than the Funnel Beaker Culture. It is hard to think of any potential objection to the first thesis. A possible objection to the second thesis would be that the types of the Funnel Beaker Culture, to the extent that they belong to the northern till area, are nevertheless concentrated closer to the boundaries of the southern till and calcareous areas than the types of the Boat Axe Culture. Here it can be said, once again, that the natural flint deposits are concentrated in the southern till area; and regarding the graves it may be pointed out that the megalithic tomb and the Boat Axe grave furthest from the boundaries of the southern till area and calcareous area happen to be in the parish of Klippan, Norra Åsbo hundred (map 8:I). It is not possible to take the discussion any further in this context; at all events, the differences are so small that it seems justified to suggest that the Funnel Beaker Culture and the Boat Axe Culture in Skåne have essentially the same distribution.

We have now seen comparisons of types belonging to the Funnel Beaker Culture, the Boat Axe Culture, the Late Neolithic, the Bronze Age, the Roman Iron Age, and the Viking Age; all the types have proved to show a heavy concentration in the fertile, easily worked soils of the southern till area and the calcareous area. Many more examples of artefact types and cultural products of all kinds could be cited with almost the same distribution. A late and at the same time unusually illustrative example is seen in map 8:II, redrawn from Dahl's map of the proportions of grain and butter in the peasants' taxes and other dues

*Map 8:II. Proportions of grain and butter according to the cadastre of 1663 (redrawn from Dahl 1942). 1 = Grain only; 2 = More than twice as much grain; 3 = More grain but not over twice as much; 4 = More butter but not over twice as much; 5 = More than twice as much butter; 6 = No data.*



according to the cadastre of 1663 (Dahl 1942, Pl. 13; see also Pl. 12 for the records of Medieval manorial dues). The map shows a sharp contrast between the dominance of grain dues in the southern till area and the calcareous area and the dominance of butter dues in the northern till area; the dues reflect the relative importance of grain cultivation and dairy farming in the different areas. There is every reason to assume that this concrete picture of the economic base of agriculture is largely valid for prehistoric times as well: during the Neolithic, cereal production must also have been more important in the southern till area and calcareous area than in the northern till area. The Funnel Beaker Culture must have worked more with tillage in the southern till and calcareous areas, and more with livestock in the northern till area, and the same goes for the Boat Axe Culture; but no difference can be demonstrated between the Funnel Beaker Culture and the Boat Axe Culture.

When discussing the economy of the Battle Axe Culture, too much notice has been paid to the fact that impressions of grains have not been found in the pottery of the Swedish-Norwegian Boat Axe Culture and that they are rare in the Battle Axe Culture of Jutland (Glob 1944:249; Oldeberg 1952:205). The distribution of the Finnish Boat Axe Culture, as demonstrated by Äyräpää, and the distribution of the Boat Axe Culture in Skåne, prove more firmly than any grain impressions that the Battle Axe Cultures practised agriculture. The lack of grain impressions in the pottery of these cultures is undoubtedly due to the high technical quality and thin walls of the vessels. It is undoubtedly no coincidence that the oldest grain impressions in the Jutland culture are found in pots belonging to Glob's F and G groups, that is, the big amphorae and the relatively thick-walled multi-footed bowls (Glob 1944:249). A new examination of the pottery from the Swedish Boat Axe Culture has however been undertaken and found that grain impressions are not lacking at all; some of these impressions have already been published (Hjelmqvist 1955:30). For Skåne five certain impressions of hulled barley or naked barley have been identified; one of them is on a pot of Forssander's oldest cord-decorated Style I:a (Forssander 1933:Pl. V). Several other grain impressions have not been possible to identify as to species. All pots with grain impressions have been found in the southern till area and the calcareous area.

Since the Funnel Beaker Culture is found in the southern till and the calcareous areas and also in the northern till area, and since most megalithic tombs are in the former areas, the culture could be divided into a megalithic group practising agriculture and a non-megalithic group practising animal husbandry. Such a division has also been made, at least for the Early Neolithic (Becker 1954:128 ff.). The division is undoubtedly significant, and it is further under-



lined by typological differences in the pottery. But there is surely no need to assume any profound oppositions, for example of an ethnic kind, between the megalithic and the non-megalithic groups. As regards the economy too, it cannot be more than a difference of degree: people in the southern till area and the calcareous area kept livestock too, and people in the northern till area no doubt grew cereals. What, then, was the nature of the difference? The sharp boundary for the distribution of megalithic tombs demands an explanation. Why are so many prehistoric types concentrated within the boundaries of the southern till area and the calcareous area, and why are the boundaries of the pleion area of the artefact-rich types largely the same?

All the prehistoric types that have been mapped have one feature in common: they have nothing or very little to do with the type of production that was vital for the farming cultures. They are instead capital investments. Vital production comes under the headings of tillage, animal husbandry, fishing, and hunting. Even if there was trade in Neolithic flint axes, as is likely, and in bronze objects, which seems certain, the production of and trade in such goods must have been of minor significance compared to the agricultural pursuits. At all events, the distribution maps of stone and metal objects do not reflect to any great extent the economy of the producers, but almost entirely that of the consumers. An occasional trading hoard of, say, bronze objects, cannot upset that picture to any serious degree: the vast majority of monuments and artefacts represent capital investments undertaken by farmers. Consequently, all our maps of monuments and artefacts reflect the degree of profitability of agricultural activities in different parts of Skåne. All monuments and artefacts may be assumed to represent roughly the same value over the whole province; only the flint objects must be presumed to have commanded higher prices in the north-eastern and interior parts of Skåne.

We can thus detach the concept of pleion from the purely mathematical definition it has on the isarithm maps and instead speak of the pleion of farming cultures in Skåne: the areas where agricultural activities show a higher than average profitability. This pleion must have had roughly the same extent, that is, coinciding broadly with the southern till area and the calcareous area, right from the start of the Neolithic until well into modern times. It ought to be possible to define pleion areas with an economic background different from the good arable soils, for example, during the Mesolithic, and pleion areas with a background that is not economic in nature. But no pleion area can have anything like the great significance for the study of Skåne's prehistory as the pleion of the farming cultures, and the same is almost certainly true of much of Scandinavia and Europe.

The main rule for the distribution of monuments and artefacts from the farming cultures is that, the larger and more non-productive the capital investment represented by a type, the more certain it is to be concentrated in the pleion area. Poor areas could not afford to make non-productive investments, especially not large ones. The megalithic tombs (like any expensive mortuary ritual) were very large and non-productive capital investments and are thus concentrated in the pleion. Bronze axes, flint axes, and greenstone axes are productive investments and not particularly expensive; bronze, however, was most expensive, flint less so, and greenstone cheapest. This is reflected in the distribution of the types in that bronze is most heavily concentrated in the pleion, flint less so, and greenstone least of all.

An illustration of the tendency of the pleion area to make non-productive investments, and simultaneously a piece of late evidence for the significance of the farming pleion in Skåne, can be obtained by studying the distribution of plough types in Skåne around 1800. The enormously heavy and impractical carriage plough was still being used then, pulled by fourteen oxen, and the distribution of this peculiar type of plough coincides largely with the pleion (Jirlow 1954:17, fig. 10). Eighteenth-century agricultural reformers tried in vain to get the peasants of Skåne to abandon the carriage plough. Linnaeus however understood why they clung so stubbornly to it (Linné 1751:174):

When a peasant comes driving with 6 or 7 pairs before the plough, he stands with arms akimbo and thinks himself a bigger creature than a lord driving with the same number of horses, and for the same reasons with which one can advise a lord to use only two horses for his carriage, one can also get a peasant to do so. When the peasant drives so many pairs, he is shouting out loud, and his neighbours hold him in respect; but when he drives with one pair, he does not make much noise in the field.

Of course, this is not the whole truth about the tendency of the pleion area to make non-productive investments. The prosperity of the pleion area gave incomparably better conditions for craft work than in poorer areas. Most objects of high artistic quality, whether pots, stone objects, or metal artefacts, were made within the pleion. The interest in decorative art led people to seek new forms, and they were much more receptive to outside influences than people in poor areas. The procedure by which a new type arises and spreads is referred to by human geographers as an innovation process (cf. Hägerstrand 1953). The term seems deserving of acceptance in archaeology too; it helps to avoid hastily committing oneself to expressions such as immigration, import, and cultural influence. The crucial point in any innovation process is the relationship of the

new type to contemporary or older pleion formations. Most types in Skåne have never been found in closed finds together with ploughs, querns, or grain impressions, but they can still be assigned to a farming population since their distribution is concentrated in the agricultural pleion. This pleion often adopted new types from other countries, but rarely from poorer parts of Skåne, which instead received influences from the pleion. A normal innovation process is thus characterized by the way the older forms of a type are concentrated more strongly in the pleion than the younger types.

When Forssander studied how the Boat Axe Culture arose in Skåne, he found that the type of boat axe that he considered oldest, the Hurva type, was concentrated in the central and south-eastern parts of the province, basically the northern till area. The younger main type of boat axe, the Vellinge type, was instead concentrated in the south-western coastal areas, basically the southern till area. Forssander's interpretation of this was that the Boat Axe Culture first occupied the less fertile parts of the province, but at a later stage drove the Funnel Beaker Culture out of the fertile coastal areas (Forssander 1933:108 ff.). The map to illustrate this process is surprising in that the Vellinge type is lacking in the central parts of Skåne; the Hurva and Vellinge types are largely distributed on either side of a boundary line (Forssander 1933:197, Ill. 50). It seems wholly improbable that the carriers of the Boat Axe Culture, in the later phase of the history of the culture, should have abandoned their first settlement sites in Skåne. Forssander's map in fact shows something quite different from what he assumed, namely, that the Hurva and Vellinge types are mostly coeval; only two coeval types can exclude each other in this way on a distribution map. The oldest Swedish-Norwegian type of boat axe is certainly not the Hurva type but, as Äyräpää was first to demonstrate, the continental type without a shaft-socket or a knob on the butt (Äyräpää 1922:161). A slightly younger form of this, with a socket, has been found, as has the Sösdala type on which the narrow sides are rather broad, together with the oldest cord-decorated pottery of Forssander's Style I. In Skåne, on the other hand, the Hurva type has only been found together with the comb-stamp-decorated Style II.

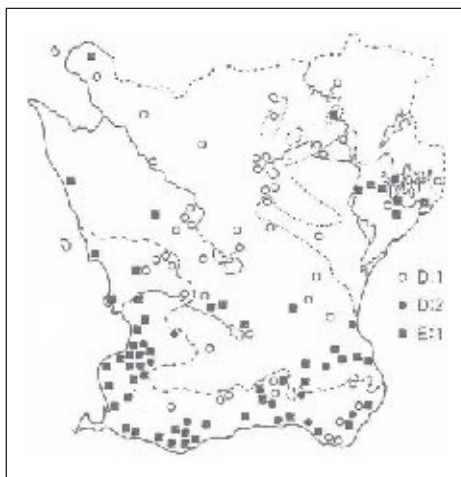
Maps 8:12–14 show the distribution of all the boat axes known to me and identifiable as to type and provenance.

Map 8:12 presents the continental type (A), its later development with a socket (B), and the older Sösdala type (C:1), with broad sides and shoulders in front of the shaft hole. The three A and B axes, and the majority of the C:1 axes, fall within the pleion.

Map 8:13 shows the Hurva type (D:1 with shoulders and D:2 without) and the Vellinge type with concave narrow sides (E:1). Types D:1 and E:1 exclude



Map 8:12. Boat-shaped battle axes: earliest types.



Map 8:13. Boat-shaped battle axes: Hurva type (D) and Vellinge type (E).

each other in most places, even though the number of finds has increased since Forssander's time.

Map 8:14 shows the youngest types: the younger Sösdala type with narrow sides running evenly from the knob on the butt to the edge (C:2) and the Vellinge type without concave narrow sides (E:2).

Of the oldest axes shown on map 8:12, 86% fall within the pleion and 14% in the northern till area. Of the younger axes shown on maps 8:13 and 8:14, 70% fall within the pleion and 30% in the northern till area. The older axes are thus

more closely associated with the pleion than the younger ones. The situation is similar with the pottery. The oldest cord-decorated pottery, Style I, has been found in Skåne at 11 places (flat-ground graves, settlement sites, and megalithic tombs), 10 of which are within the pleion and 1 in the northern till area. Style II has been found at 21 places, 16 of them within the



Map 8:14. Boat-shaped battle axes: latest types.

pleion and 5 in the northern till area. This has thus increased its share from 9% of Style I to 24% of Style II. A fundamental feature in the innovation process of the Boat Axe Culture is thereby clear: it first appeared in the pleion and spread from there to the northern till area. The Boat Axe Culture in Skåne is thus a typical representative of the long series of successive farming cultures.

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# Production diagrams

1975

## 1. The distribution of pottery groups

Tab. 9:1 and fig. 9:0 give a general picture of the distribution of all the pottery.<sup>1</sup> It can be seen that the majority of vessels – nearly two-thirds of the total – come from settlement sites. One third of the pots come from the flat-ground graves of the Battle Axe Culture. And a small number of vessels – 20 – come from the megalithic tombs of the Funnel Beaker Culture.

It is important to note the strong southern tendency of the pottery. Skåne and Blekinge, although small in area (together approximately 14,000 km<sup>2</sup>), have almost two-thirds of all the pots, whereas three larger regions each of roughly the same area, Western and Eastern Götaland and the Mälaren area (each 35,000–39,000 km<sup>2</sup>), each have about one tenth of all the pots. If we look at the number of find spots for pottery, we find that the position of Skåne-Blekinge is not as strong; the two provinces account for half of the total for Scandinavia. If one undertakes a critical scrutiny of this difference between the number of find spots and the number of vessels in Skåne-Blekinge, there are two main explanations to choose from: either these provinces were richer in pottery during this period of prehistory, or else they have seen greater archaeological attention and better excavation techniques in modern times. There might possibly be something in both explanations.

Tab. 9:2 shows the distribution of each group and subgroup in the geographical areas. A table like this can be hard to survey, requiring detailed study. A quicker overview is provided by the maps in figs 9:18–21. By comparing the maps it is easy to see some characteristic features. In all the maps the main concentration is, naturally, in Skåne-Blekinge – except in fig. 9:19, which shows the distribution of group F. It is obviously a characteristic feature of this group that it mostly belongs to Western and Eastern Götaland and the Mälaren area. A

<sup>1</sup> Contrary to the strongly held opinion of Mats P. Malmer, the types of Battle Axe pots are here presented only as illustrations (figs 9:1–17); for the verbal definitions, see Malmer 1962:8–37 (SW)



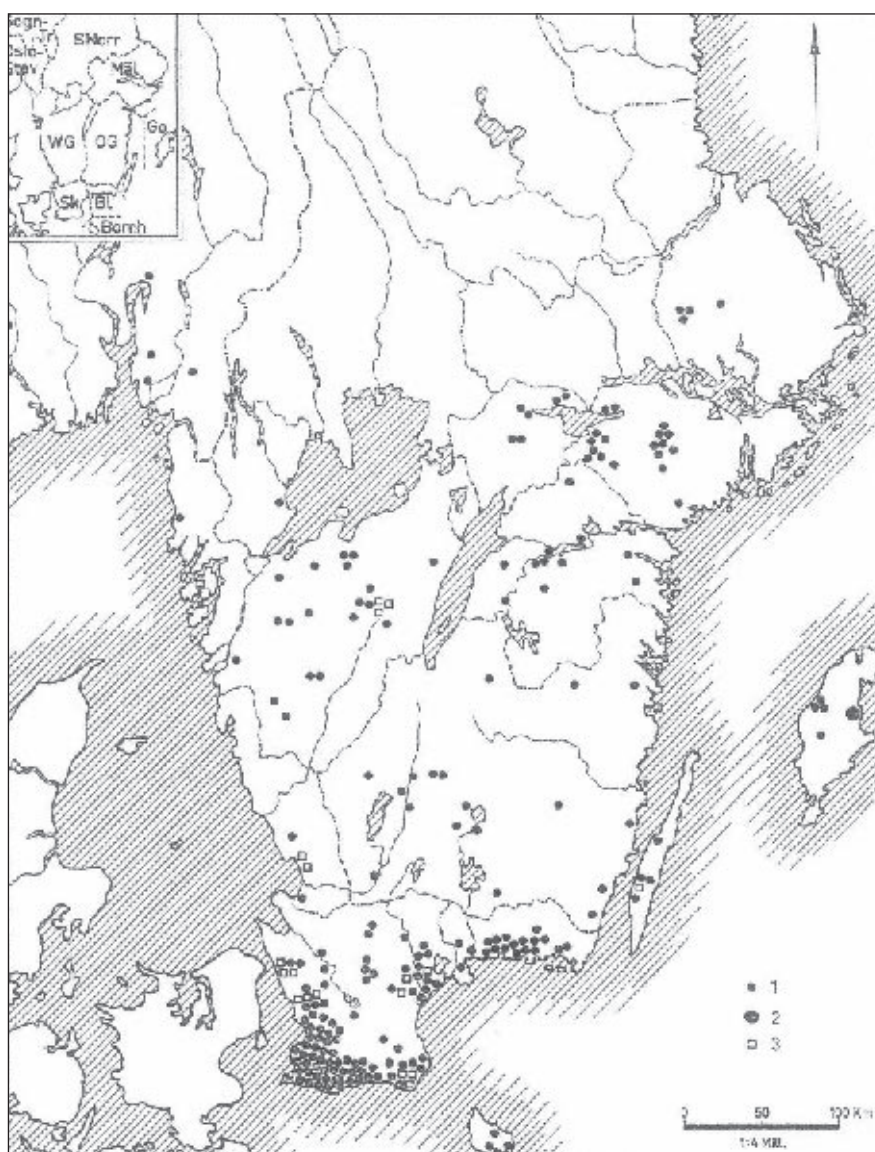


Fig. 9:0. Distribution of Battle Axe pottery in Sweden and Norway.



*Tab. 9:1. Distribution of pottery by type of find spot and geographical area (from Malmer 1962).*

	Flat-ground graves		Megalithic tombs		Settlement sites		Total	
	Find spots	Pots	Find spots	Pots	Find spots	Pots	Find spots	Pots
Skåne	28	52	9	13	20	60	57	125
Blekinge	7	12	–	–	10	55	17	67
Halland	–	–	1	2	4	9	5	11
West Småland	5	6	–	–	2	3	7	9
Västergötland	7	12	2	3	1	1	10	16
Bohuslän	–	–	1	2	2	3	3	5
Dalsland	–	–	–	–	–	–	–	–
West Götaland	12	18	4	7	9	16	25	41
East Småland	2	2	–	–	6	14	8	16
Öland	–	–	–	–	–	–	–	–
Östergötland	5	7	–	–	2	4	7	11
East Götaland	7	9	–	–	8	18	15	27
Gotland	–	–	–	–	1	1	1	1
Södermanland	6	8	–	–	3	9	9	17
Närke	3	4	–	–	1	3	4	7
Västmanland	–	–	–	–	2	4	2	4
Uppland	4	5	–	–	3	4	7	9
Mälaren area	13	17	–	–	9	20	22	37
South Norrland	–	–	–	–	1	3	1	3
Oslo-Stavanger area	2	2	–	–	1	1	3	3
Sogn-Trøndelag area	1	1	–	–	–	–	1	1
Bornholm	1	1	–	–	3	6	4	7
<b>Total</b>	<b>71</b>	<b>112</b>	<b>13</b>	<b>20</b>	<b>62</b>	<b>180</b>	<b>146</b>	<b>312</b>

comparison between groups A–E (fig. 9:18) on the one hand and groups G and H (fig. 9:20) on the other shows that G–H have a particularly heavy concentration in Skåne-Blekinge, whereas A–E are fairly well represented in the north. Groups J–O (fig. 9:21) are perhaps harder to assess, but the distribution pattern seems to be intermediate between A–E and G–H.

We have to find an explanation for these differences in the distribution (or else we have to show that the differences are due to chance).

Tab. 9:2. Distribution of pottery by group, geographical area, and type of find spot (from Malmer 1962; partly corr. by SW).

	Number of find spots												
Group	Skåne	Blekinge	West Götaland	East Götaland	Gotland	Mälaren area	South Norrland	Oslo-Stavanger area	Bornholm	Total	Flat-ground graves	Megalithic tombs	Settlement sites
A:1	3	2	1	2	–	4	–	–	–	12	6	–	6
A:2	–	–	2	–	–	1	–	1	–	4	2	1	1
ad A	4	1	3	1	–	–	–	–	–	9	–	3	6
A	7	3	6	3	–	5	–	1	–	25	8	4	13
B:1	2	2	–	–	–	2	–	–	–	6	3	–	3
B:2	1	–	–	–	–	–	–	1	–	2	1	–	1
ad B	2	–	–	1	–	1	–	–	–	4	–	–	4
B	5	2	–	1	–	3	–	1	–	12	4	–	8
C	3	2	1	–	–	–	–	–	–	6	–	–	6
ad C	–	1	–	–	–	–	1	–	–	2	–	–	2
C	3	3	1	–	–	–	1	–	–	8	–	–	8
D:1	1	–	–	–	–	1	–	–	–	2	1	–	1
D:2	2	–	–	1	–	1	–	–	–	4	1	–	3
ad D	–	–	–	–	–	–	–	–	–	–	–	–	–
D	3	–	–	1	–	2	–	–	–	6	2	–	4
E:1	–	3	2	2	–	2	–	–	–	9	1	1	7
E:2	3	3	4	–	–	–	–	–	–	10	2	–	8
ad E	–	–	–	–	–	–	–	–	–	–	–	–	–
E	3	6	6	2	–	2	–	–	–	19	3	1	15
F:1	–	–	4	2	–	1	–	–	–	7	6	–	1
F:2	1	–	–	–	–	–	–	–	–	1	1	–	–
F:3	–	–	–	1	–	1	–	–	–	2	2	–	–
ad F	1	–	4	1	–	2	–	–	–	8	4	1	3
F	2	–	4	4	–	4	–	–	–	18	13	1	4
G:1	1	1	–	–	–	–	–	–	–	2	2	–	–
G:2	3	–	–	1	–	–	–	–	–	4	4	–	–
G:3	3	2	3	1	–	–	–	–	–	9	9	–	–
G:4	–	–	–	1	–	–	–	–	–	1	1	–	–
ad G	6	6	3	1	–	1	–	–	–	17	2	–	15
G	13	9	6	4	–	1	–	–	–	33	18	–	15
H:1	2	1	–	–	–	–	–	–	–	3	3	–	–
H:2	3	2	2	–	–	–	–	–	–	7	6	1	–
H:3	2	–	–	–	–	–	–	–	–	2	2	–	–
ad H	4	2	–	3	–	–	–	–	1	10	3	1	6
H	11	5	2	3	–	–	–	–	1	22	14	2	6
ad GH	9	4	–	1	–	2	–	–	–	16	–	2	14
J:1	1	–	–	–	–	–	–	–	–	1	–	1	–
J:2	3	1	–	–	–	2	–	–	1	7	5	–	2
J:3	6	–	–	–	–	1	–	–	2	9	7	–	2

	Number of find spots												
Group	Skåne	Blekinge	West Götaland	East Götaland	Gotland	Mälaren area	South Norrland	Oslo-Stavanger area	Bornholm	Total	Flat-ground graves	Megalithic tombs	Settlement sites
ad J	11	5	3	1	–	7	1	1	2	31	9	5	17
J	21	6	3	1	–	10	1	1	5	48	21	6	21
ad K	3	1	–	1	1	1	–	–	–	7	1	1	5
K	4	1	1	1	1	1	–	–	–	9	2	2	5
L:1	–	–	–	–	–	1	–	–	–	1	1	–	–
L:2	2	–	–	–	–	1	–	–	–	3	3	–	–
ad L	–	–	–	–	–	–	–	–	–	–	–	–	–
L	2	–	–	–	–	2	–	–	–	4	4	–	–
M	1	–	1	1	–	–	–	–	–	3	–	–	3
ad M	–	–	–	–	–	–	–	–	–	–	–	–	–
M	1	–	1	1	–	–	–	–	–	3	–	–	3
N	3	–	1	–	–	–	–	–	–	4	2	1	1
ad N	1	–	–	–	–	–	–	–	–	1	–	–	1
N	4	–	1	–	–	–	–	–	–	5	2	1	2
O	2	–	1	–	–	2	–	–	–	5	5	–	–

## 2. The find combinations of the pottery groups

The diagram in fig. 9:22 shows which find combinations occur in the graves. The diagram shows all the groups and subgroups in finds from flat-ground graves where more than one group or subgroup is represented. Each dot marks a flat-ground grave. Groups C, E, K, and M have not yet been found in closed find combinations with other groups. The same actually applies to group F with angle-band decoration (but the combination F:1 plus ad F does occur).

Some main features are obvious from the diagram. The cord-decorated groups A and B form a closed class together with D; they are combined with each other, but not with other groups. In the same way, the angle-band or angle-line groups G, H, and J form a class unto themselves. Groups L, N, and O are not combined with each other, but are combined with the G, H, and J class.

## 3. The chronology of the pottery

### 3.1. Some main lines

The largest pottery groups (tab. 2) are as follows: group A (25 find spots), B (12), E (18), F (16), G (32), H (18), and J (46), in addition to which there is group ad

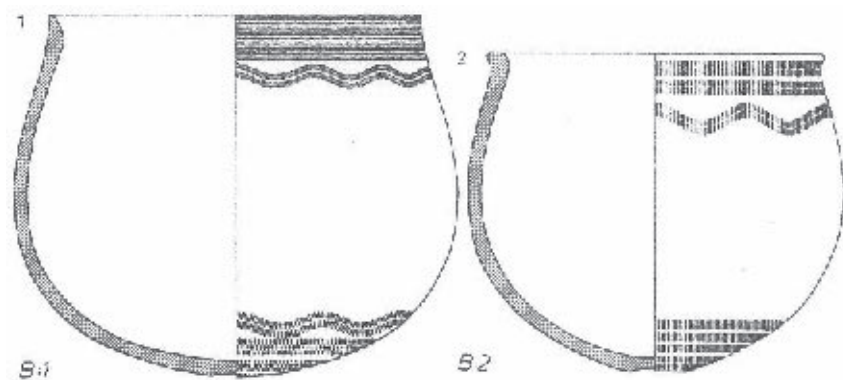
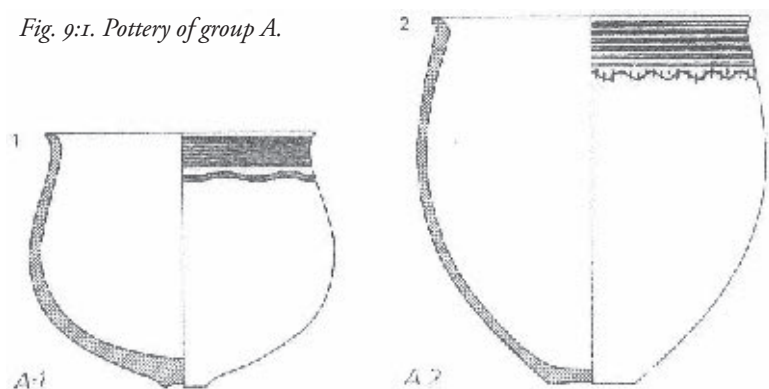
GH (16 find spots). Of these, group E occupies a special position in that it consists partly (group E:1) of coarse utility pottery, which is also confirmed by the fact that the E pottery is found four times more frequently at settlement sites than in graves. Group F likewise has a special position in that it is sparsely represented in Skåne-Blekinge. It is unlikely that the chronological position of the pottery groups is exactly the same over the whole large distribution area, for example, the same in Skåne as it is a thousand kilometres to the north. A good starting point for a chronological study should be to examine conditions in the richest area, Skåne-Blekinge, and then make comparisons with more northerly areas. The conclusion to be drawn from all this is that it is easier to grasp the main lines of development by first concentrating on groups A, B, G, H, and J.

One can study these groups from a number of different angles and always arrive at the same result, namely, that they combine two contrasting classes, A–B and G–J. Group F sometimes goes with the A–B class, sometimes with the G–J class. A–B is mostly decorated with cord, while G–J is mainly decorated with comb stamp and cord stamp, and group F can have both stamp and cord. A–B (and F) are partially decorated, while G–J are decorated all over. A–B lacks angle bands and angle lines, but this is the predominant decoration on G–J (and F). A–B never has decoration on the base, whereas G–J usually does (and F sometimes). It may also be noted that the vessel walls become thinner towards the rim more often in A–B (e.g. figs 9:1, 2) than in G–J (figs 9:9–12); group F more often has a thinner band at the rim if the undecorated belt in the middle of the vessel's side is wide (fig. 9:8:1) than if it is narrow (fig. 9:8:2). The diagram in fig. 9:23 shows that all the A–B pots are slimmer than all the G–H pots, while the F pots occupy an intermediate position (and group J in this case has a character all of its own, with wide variation from very slim to broad vessels).

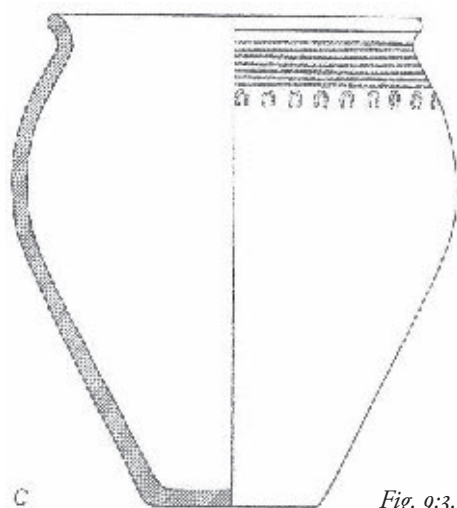
Like the beaker cultures from the last part of the Neolithic in the rest of Europe, the Swedish-Norwegian Battle Axe Culture has technically advanced pottery. The clay is finely to medium-coarsely tempered, the vessel walls are thin and their outsides smoothed, the firing is smooth on the surface while a fracture usually reveals a grey or black core – all of which are features found in related cultures across Europe. The colour of the fabric on the surface is mostly greyish-yellow or a light yellowish-brown, but there is sometimes a reddish brown or a warm brownish-red colour. Red pots occur only in groups F–L, never in A–D.

Deliberate red colouring occurs quite often in bell beakers on the Continent, but hardly ever in the beakers of the Corded Ware Cultures. Several other differences presented here between the A–B class and the G–J class can also be found through a comparison between Continental corded beakers and bell beakers. The corded beakers, of course, have cord decoration, and they are also

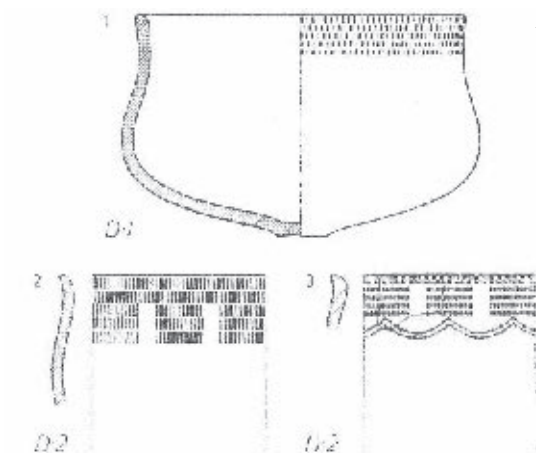
*Fig. 9:1. Pottery of group A.*



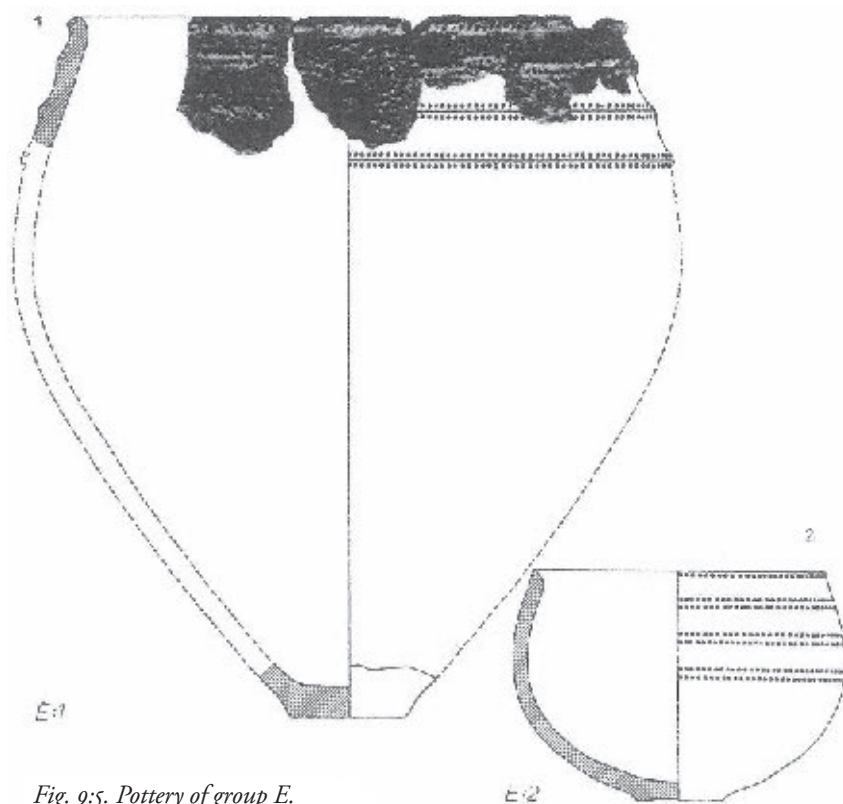
*Fig. 9:2. Pottery of group B.*



*Fig. 9:3. Pottery of group C.*



*Fig. 9:4. Pottery of group D.*



*Fig. 9:5. Pottery of group E.*

partially decorated; bell beakers are normally comb-stamped, sometimes with angle bands, and decorated all over (although in a different sense from the G–J pottery of the Battle Axe Culture, since the “classical” decoration consists of horizontal zones, alternately with slanting lines and without decoration; but in a later stage they are more like the G–J pottery). Bell beakers not infrequently have base decoration, but corded beakers do not, at least not in their older phase. Corded beakers are slimmer, bell beakers stouter.

All in all, then, the main features of the Battle Axe Culture pottery suffice to provide material for a hypothesis that the A–B class is associated with the Corded Ware Cultures on the Continent, and the G–J class is associated with the Bell Beaker Culture. A hypothesis that follows naturally from this is: since the start of the Corded Ware Cultures can be shown, partly through radiocarbon dates, to be earlier than the start of the Bell Beaker Culture, it is also reasonable to imagine that the A–B class began before the G–J class. The circumstances in Jutland’s Battle Axe Culture (the Single Grave Culture) are particularly important. There it is the cord-decorated pottery, Glob’s groups A and B (Glob 1944:65 f., figs 29, 33), that is oldest, and some of the younger groups show distinct Bell Beaker influence.

The chronological difference between the A–B and G–J classes of the Swedish-Norwegian Battle Axe Culture is confirmed by the diagram showing find combinations (fig. 9:22): the two classes never occur together in closed finds, even though they are distributed over roughly the same area, as the maps show (figs 9:9–18, 20, 21). The diagram in fig. 9:22 also gives some support to the hypothesis that the A–B class is oldest, because A–B is combined with just one other group, D, whereas G–J is combined with three, L, N, and O, besides showing greater variation as regards vessel shapes (fig. 9:23) and decoration. Broader variation, generally speaking, is more common in the later phases of a culture than at the beginning.

### *3.2. The typological relationship between A–B, F, and G–J*

Support for the hypothesis that the A–B class, with its predomination of cord decoration, is older than the G–J class can be obtained by studying the frequency of the different types of vessel base (cf. note 1). The percentages are calculated in tab. 9:3. The A–B class has exclusively diminutive bases, but in the G–J class it is only group G that has a small proportion (10%) of pots with a diminutive base. The diagram of closed finds (fig. 9:22) shows that in five graves G pottery is combined with H pottery, and in four graves H pottery is combined with J pottery, but G and J never occur together. The chronological se-

Tab. 9:3. Frequency of the vessel-base types in groups A–B and F–J (from Malmer 1962).

Group	Diminutive base plate %	Round base plate %	Round base with marking %	Round base without marking %	Total %
A	100	–	–	–	100
B	100	–	–	–	100
F	73	9	18	–	100
G	10	40	45	5	100
H	–	28.5	57	14.5	100
J	–	6	47	47	100

quence must therefore be G–H–J or J–H–G, but since G is the only group where the diminutive base typical of the A–B class occurs (albeit sparsely), the chronological order must be G–H–J.

When seeking to establish whether the chronological sequence in the A–B class was A–B or B–A, we also find indications. Generally speaking, one can say that it is probable that group A is older since it shows close similarities to cord-decorated pottery in Denmark (Glob 1944:65, fig. 29) and Finland (Edgren 1970:77, fig. 26) and on the Continent, while group B seems to be an exclusively Swedish–Norwegian speciality. Yet one can also find indications pointing in the same direction in the indigenous material. Group A (both A:1 and A:2) can be divided into three varieties of decoration. *Variant 1*: A wavy line in cord technique under the cord belt at the rim, no decoration around the base plate (fig. 9:1:1). *Variant 2*: No wavy line under the cord belt at the rim, no decoration around the base plate. *Variant 3*: Decoration around the base plate (Forssander 1933, Pl. 3). One can now investigate whether there is any difference between A-group pots discovered together with B pottery in closed grave finds and those found in graves with only A pottery. To eliminate the risk that such a comparison may show a chorological rather than a chronological difference, one should confine the study to a single area, and it is natural to choose the richest one. Skåne–Blekinge has 7 pots of the A group which are so well preserved that all the details of the variant decorations can be checked. Of variant 1, one pot has been found together with B pottery, against three pots not found in combination with B pottery. All the pots of variant 2 and 3, by contrast, were found in graves which also contained B pottery (Malmer 1962:67, tab. 9). In archaeology one must try to draw conclusions even from low figures, and in this case the conclusion is obviously that the A pottery changed to some extent during the time it was produced, that variants 2 and 3 are largely contemporary with B pottery, whereas variant 1 is mostly older than all B pottery. One can conduct a cor-



Fig. 9:6. Base decoration  
of types 1:a and 1:b.

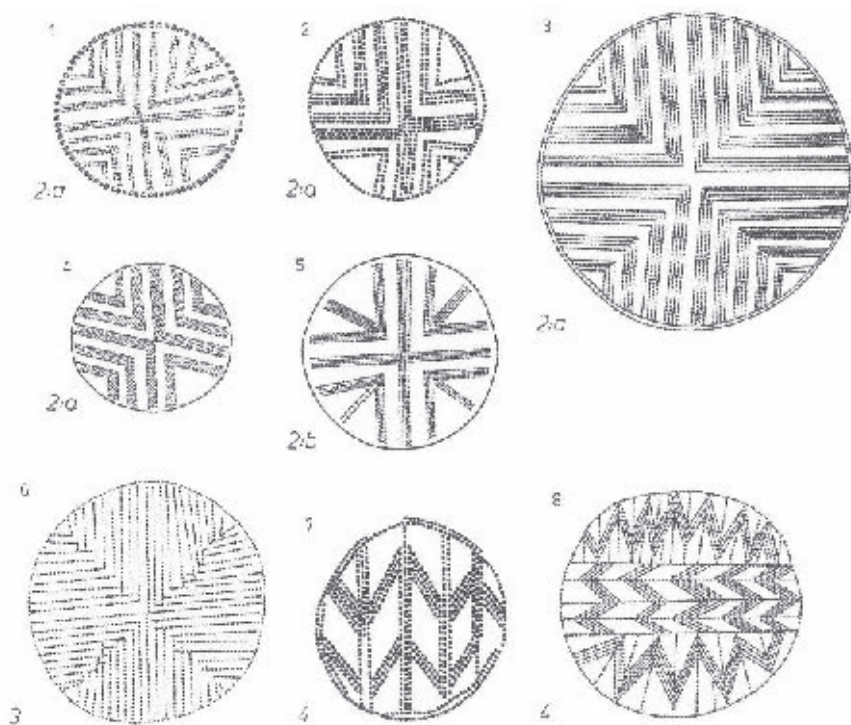
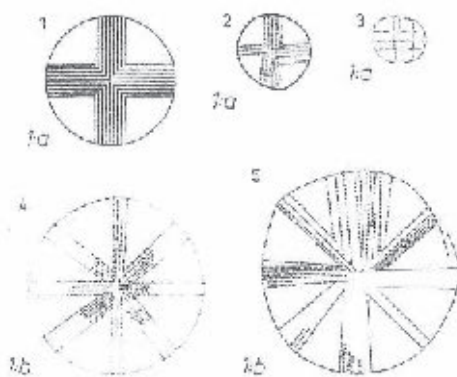
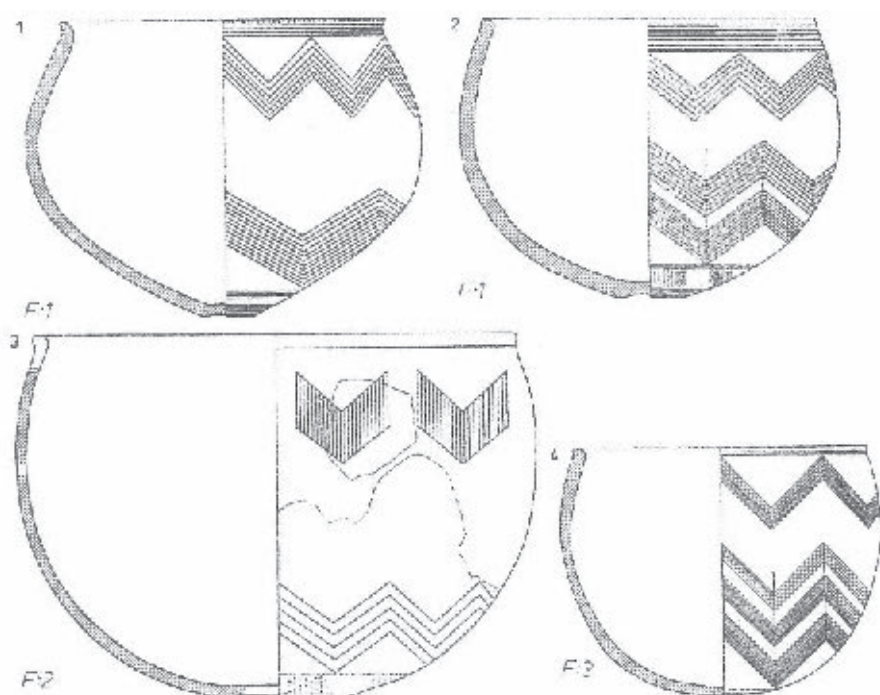
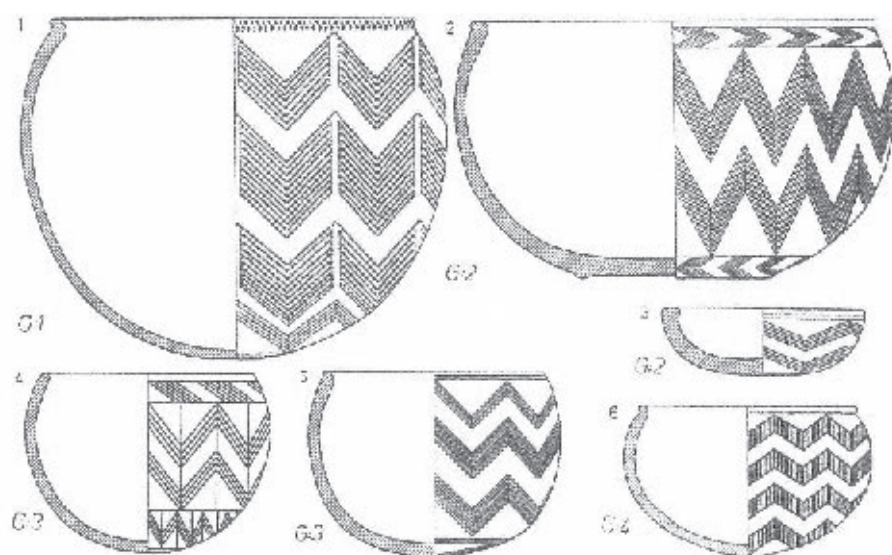


Fig. 9:7. Base decoration of types 2:a, 2:b, 3, and 4.



*Fig. 9:8. Pottery of group F.*



*Fig. 9:9. Pottery of group G.*

responding study of the even smaller stock of B pottery, and then one will find that group B:1, which resembles A pottery in having a cord belt at the rim, is always combined in graves with A pottery. But the only grave find of a pot from group B:2, which lacks cord decoration, is *not* combined with A pottery.

All the indications cited hitherto are thus concordant in suggesting that the chronological sequence must be A-B-G-H-J.

Continued analysis can further reinforce the evidence. If one studies tab. 9:3 closely one will find that all the types of base increase gradually in frequency and then decrease again; the series A-B-G-H-J is thus relevant according to the second criterion of continuity.<sup>2</sup> The diminutive base plate has its maximum in groups A and B, the large base plate in group C; the round base with ornamental base marking has its maximum in group H and the round base without any marking in group J.

The fineness of the decoration instrument is a typological element which is wholly independent of the form of the vessel base. The median value for the fineness of the comb stamp<sup>3</sup> is as high as 22/3 in groups G:1 and G:2, while it is 18/3 in groups G:3 and G:4; in group H it is likewise 18/3 and in group J as low as 14/3. It is highly unlikely that this series of figures could be due to chance. The way the comb stamp becomes increasingly coarse with time is an indication that the posited series is correct – especially in combination with the wholly independent factors of find combinations and base types.

### 3.3. *The role of F pottery*

The intermediate position of F pottery that has been demonstrated in several respects, between the A–B and the G–J classes would in itself justify a hypothesis that group F is also an intermediate chronological stage between A–B and G–J, so that the entire sequence would be A-B-F-G-H-J. This was also the clearly declared view in earlier research (Forssander 1933:74 ff.).

What speaks against that hypothesis – that the F pottery is a stage in a continuous development – is, above all, its unusual distribution (fig. 9:19). Only 13% of the find spots for F pottery are in Skåne-Blekinge, which has between 33% and 80% of each of the other pottery groups (Malmer 1962:78). The figure of 13% seems particularly remarkable if one places it between Skåne-Blekinge's share of the B pottery, 58%, and of the G pottery, 65%. In most cases, groups or types with differing distribution cannot be combined into a single series of

2 For a discussion of the criteria of continuity, see Ch. 1 (SW)

3 The fineness is defined in Malmer (1962:XXXI) as a number, e.g. 20/3, "which states how many teeth of the stamp or twists of the cord occur per 3 cm" (SW)

chronological relevance (an exception can be made only for frequently exported artefact forms, but Stone Age pottery does not count as that).

There is another unsatisfactory aspect to the hypothesis presented in earlier research. It was imagined that the garlands on the B pottery (fig. 9:2:1) were “developed” into angle bands on the F pottery (fig. 9:8:1) and that the undecorated belt on the middle of the vessel’s side was gradually reduced (fig. 9:8:4) so that G pottery finally arose (fig. 9:9:1). But new forms of high artistic quality rarely or never arise in such a mechanical way. New forms are instead created by important artists working freely on the basis of their experience. The difference between prehistory and the present lies mainly in the degree of difficulty in having an innovation disseminated and accepted: many facts suggest that prehistoric society was very conservative.

The hypothesis about the origin of the F pottery that is most naturally suggested by its distribution and decoration is as follows: F pottery is actually later than G pottery, which is an innovation created in Skåne-Blekinge on the basis of influence from the Bell Beaker Culture. The northward spread of this innovation in the Scandinavian Peninsula encountered resistance in that people in Götaland and the Mälaren area, although they accepted the decorative details of the comb stamp and the angle band, retained the old decoration scheme with an undecorated belt on the middle of the vessel side. With traditions from the old A–B pottery and with influences from the new G pottery from Skåne-Blekinge, they thus created the F pottery. Only at a later stage did the G–J pottery in the form typical of Skåne-Blekinge gain acceptance in the northern areas as well.

### *3.4. Quality differences between A–B and G–J*

The best A–B pots in terms of artistry and technique are found in Skåne, which is not surprising in view of the great pottery traditions in that province since the time of the Funnel Beaker Culture. What is more remarkable is that the difference in quality between pots from Skåne, Götaland, the Mälaren area, and Norway is actually relatively insignificant. The sherds of A pottery from Grave 145, Karleby Parish, Västergötland (Forssander 1933:101, ill. 46), or settlement site 50, Lilla Malma Parish, Södermanland (Florin 1959:39, fig. 21) belonged to pots that were scarcely inferior to the one from Grave 64, Norra Mellby Parish, Skåne (fig. 9:1:1). And the B pot from Grave 235, Råde Parish, Østfold (fig. 9:2:2), is very similar to a sherd from settlement site 12, Munkarp Parish, Skåne (Forssander 1933:110, ill. 51:1), and its quality is hardly lower than that of the pot from Grave 19, Fjälkestad Parish, Skåne (fig. 9:3:1). The high and consistent

Fig. 9:10. Pottery of group H.

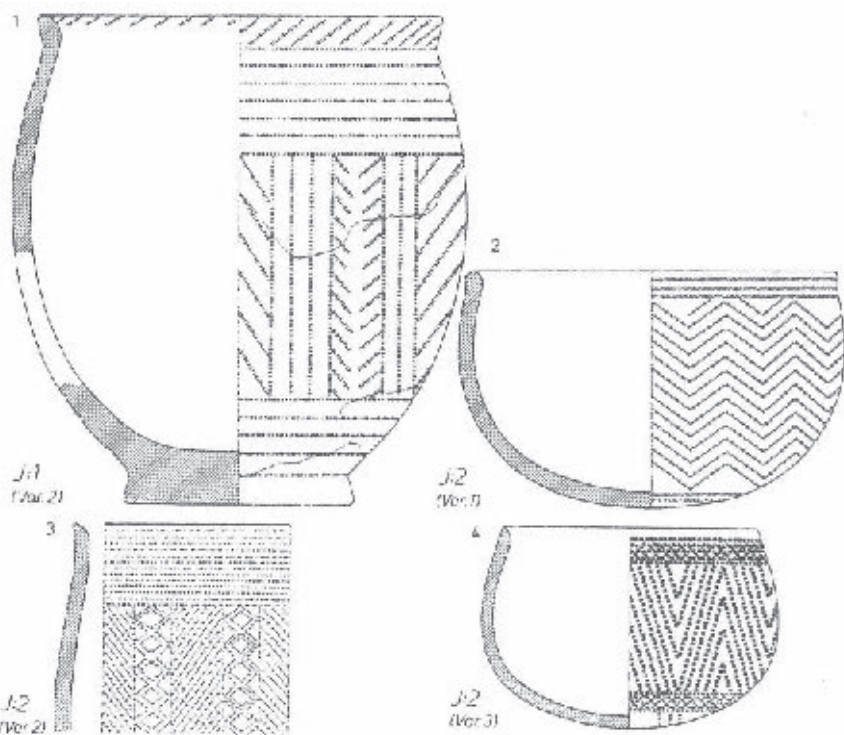
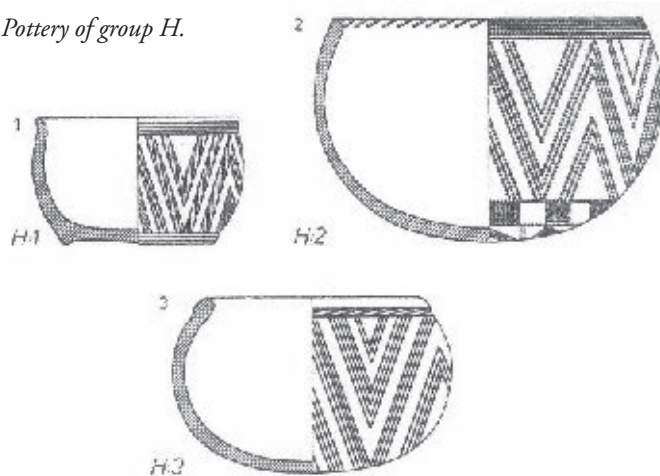


Fig. 9:11. Pottery of group J.



Fig. 9:12. Pottery of group J.

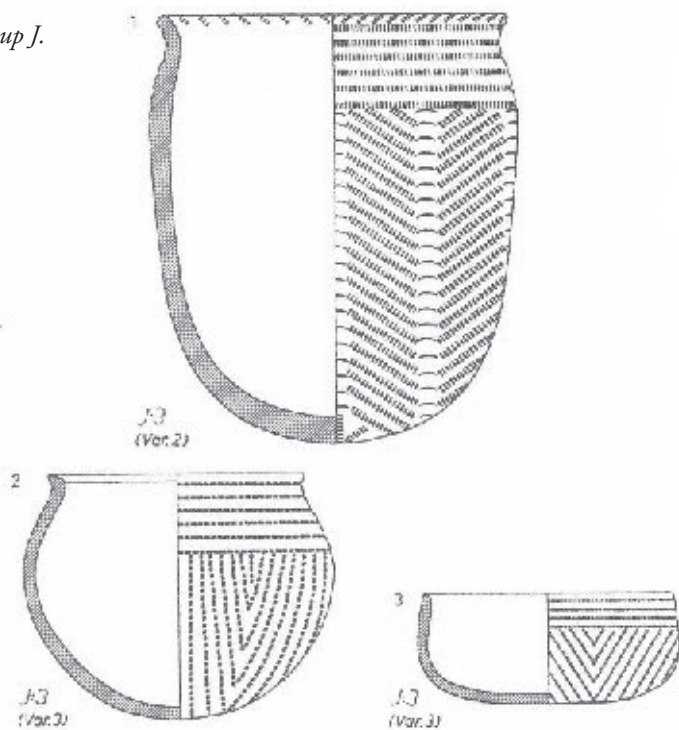


Fig. 9:13.  
Pottery of group K.

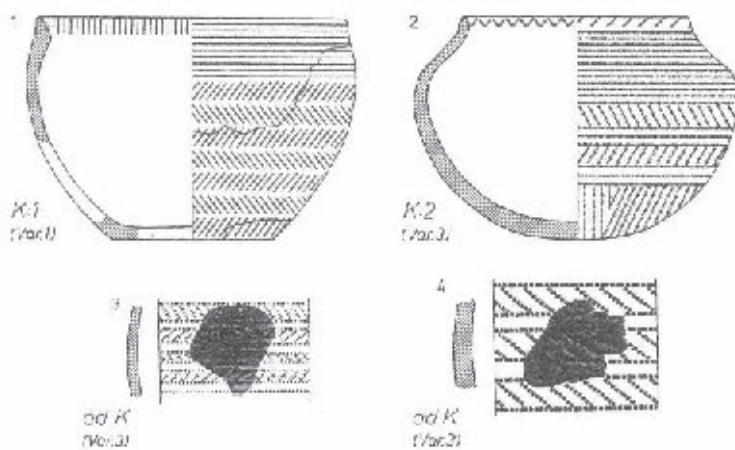


Fig. 9:14.  
Pottery of group L.

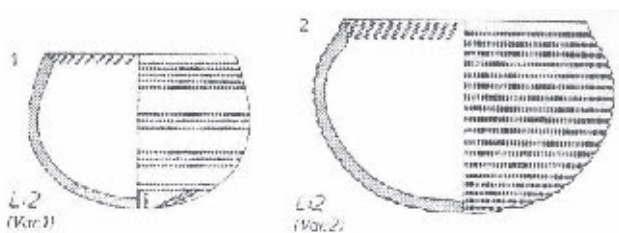




Fig. 9:15. Pottery of group M.



Fig. 9:16. Pottery of group N.

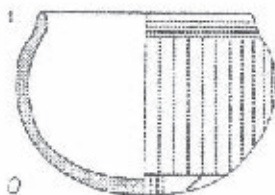


Fig. 9:17. Pottery of group O.

quality of all A–B pottery, wherever it is found in the large distribution area, is important.

The picture for the G–J pottery is different. In quantitative terms, Skåne-Blekinge dominates there much more than in the A–B class (tab. 9:2; figs 9:19, 21). Yet in terms of quality too, there is a difference: at least the G and H pots are both technically and artistically better in Skåne-Blekinge than in the more northerly areas. This subjective observation can be proved objectively through a study of the fineness of the comb stamp. If we combine all the pottery with the designations G, H, and ad GH, and if we divide the fineness of the comb stamp into four classes, 11–15/3, 16–20/3, 21–25/3, and 26–30/3, we find that only the two lower classes are represented in the Mälaren area. In Götaland the three lowest classes are represented, and in Skåne-Blekinge all four classes, including the highest (Malmer 1962:73, tab. 11). A fine comb stamp is an early feature but it is also a southern feature.

The simplest interpretation of the differences between the A–B class and the G–J class as regards both quantitative and qualitative variation is as follows: A–B pottery was spread quickly over the Scandinavian peninsula through an *unimpeded* process of innovation. For some reason, people willingly accepted this novelty (no doubt for weightier reasons than merely new and aesthetically pleasing vessel forms and decoration). The G–J pottery, on the other hand, was spread northwards from its place of origin in Skåne-Blekinge through a process

of innovation that was more *impeded*: only after some hesitation was this novelty accepted, and then only partly.

### 3.5. *The periodization of the Battle Axe Culture*

Among the facts considered hitherto, no clear evidence can be found that the oldest pottery of the Battle Axe Culture, the A–B class, first arose in Skåne-Blekinge and then spread from there. There are, on the other hand, convincing reasons for believing that Skåne-Blekinge was the centre from which the G–J class spread, and generally speaking most novelties in the later part of the Neolithic and the Bronze Age arose first in Skåne-Blekinge and spread north from there.

The most efficient way to discuss the chronology of the Battle Axe Culture is to divide it into periods. Many periodizations in archaeology suffer from the error that the periods' content of artefacts and other cultural phenomena is described in detail, but the boundaries of the periods remain blurred. This vagueness is often justified by the claim that one period blended smoothly into the next. This argumentation is conceptually confused. In cultural history, continuity is the norm; sharp breaks are rare. But a division into periods fills the same function as counting by years. A boundary between two periods should be as clearly defined as, say, the boundary between the nineteenth and the twentieth centuries. In both cases, cultural continuity usually flows across the boundary without interruption, but the flow is less strong across a period boundary, since this has at least some concrete justification, whereas the boundary between two years is completely abstract.

Based on the pottery sequence A–B–G–H–J, the periods of the Battle Axe Culture can be defined as follows:

*Period 1* begins – over the whole distribution area of the Battle Axe Culture – with the first occurrence of group A in Skåne-Blekinge.

*Period 2* begins with the first occurrence of group B in Skåne-Blekinge.

*Period 3* begins with the first occurrence of group G in Skåne-Blekinge.

*Period 4* begins with the first occurrence of group H in Skåne-Blekinge.

*Period 5* begins with the first occurrence of group J in Skåne-Blekinge.

*Period 6* begins with the first occurrence of group C in Skåne-Blekinge.

Each of periods 1–5 ends with the start of the following period. Period 6 ends when group C ceases to be made.

*Group C*, which defines period 6, has not been studied in detail above. There



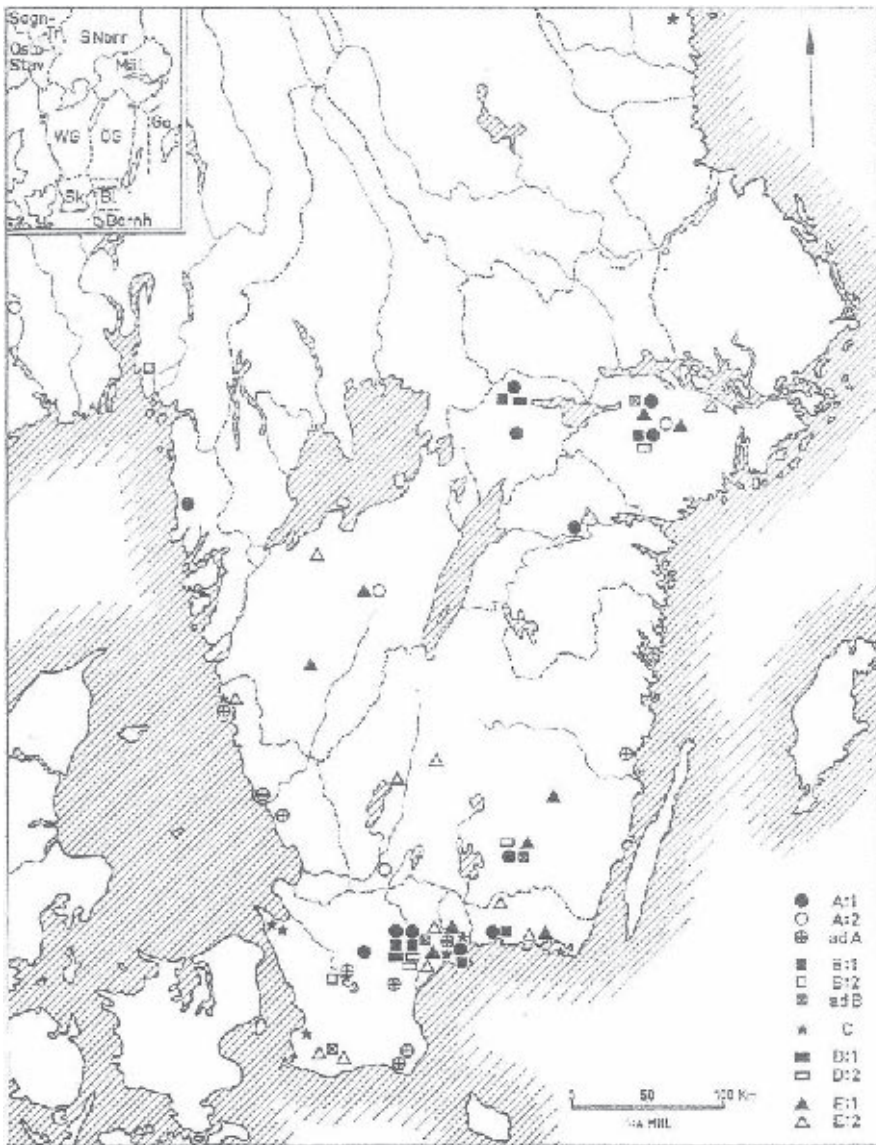


Fig. 9:18. Distribution of pottery groups A-E (Bornh = Bornholm; Bl = Blekinge; Go = Gotland; Mäl = the Mälaren area; Oslo-Stav = the Oslo-Stavanger area; Sk = Skåne; S Norr = South Norrland; Sogn-Tr = the Sogn-Trøndelag area; WG = West Götaland; ÖG = East Götaland).

are a number of indications that this cord-decorated pottery belongs to the Late Neolithic. The strongest evidence is the pot in fig. 9:3, which was found at settlement site II, Malmö, Skåne, in a waste pit together with pottery of unmistakable gallery-grave character.

### 3.6. *Quantitative studies*

With the conditions given in the preceding sections, it is obvious that the content of the periods cannot be the same in the northern areas as in Skåne-Blekinge. It must have taken some time before the production of a particular group which, according to the hypothesis, began in Skåne-Blekinge, managed to spread to Götaland or the Mälaren area and the Oslo-Stavanger area. But since the period that is valid for the whole Scandinavian chronology began when the corresponding period-defining pottery group started to be produced in Skåne, the production of the immediately preceding period-defining group must have continued longer in the northerly areas than in Skåne.

This lag in the northerly areas can scarcely be demonstrated through studies of find combinations, and it is even less possible that the *length* of the delay can be calculated by such means. If all the groups on their way north had an innovation process that was impeded to the same extent, if they were all delayed equally much, then the closed finds in the northern areas will evidently have the same appearance as in the south, even though they are from a later date. And even if the speed of the diffusion of the different groups varied (which was undoubtedly the case), there are far too few closed finds to allow us to calculate the amount of the delay in the different cases. The only chance of performing such calculations is in using a quantitative method.

Simple estimates of expanses of time based on find quantities often occur in the archaeological literature. We read, for example, that a certain frequently represented find group must have existed for a longer time than another group represented by fewer finds. Such assumptions or claims can be dangerous. The wealth of finds during a particular period may admittedly mean that the period in question was long. But a number of other interpretations are also possible. The intensity of production, for example, may have been particularly high. Or perhaps the prehistoric people followed some custom which has made it especially easy to find these types in modern times, for example, in easily found and excavated graves. Or modern excavations may, for some reason, have been particularly aimed at the site type in question. To be able to study one of these interacting or counteracting variables, the others must be constant – or at least it must be possible to *assume*

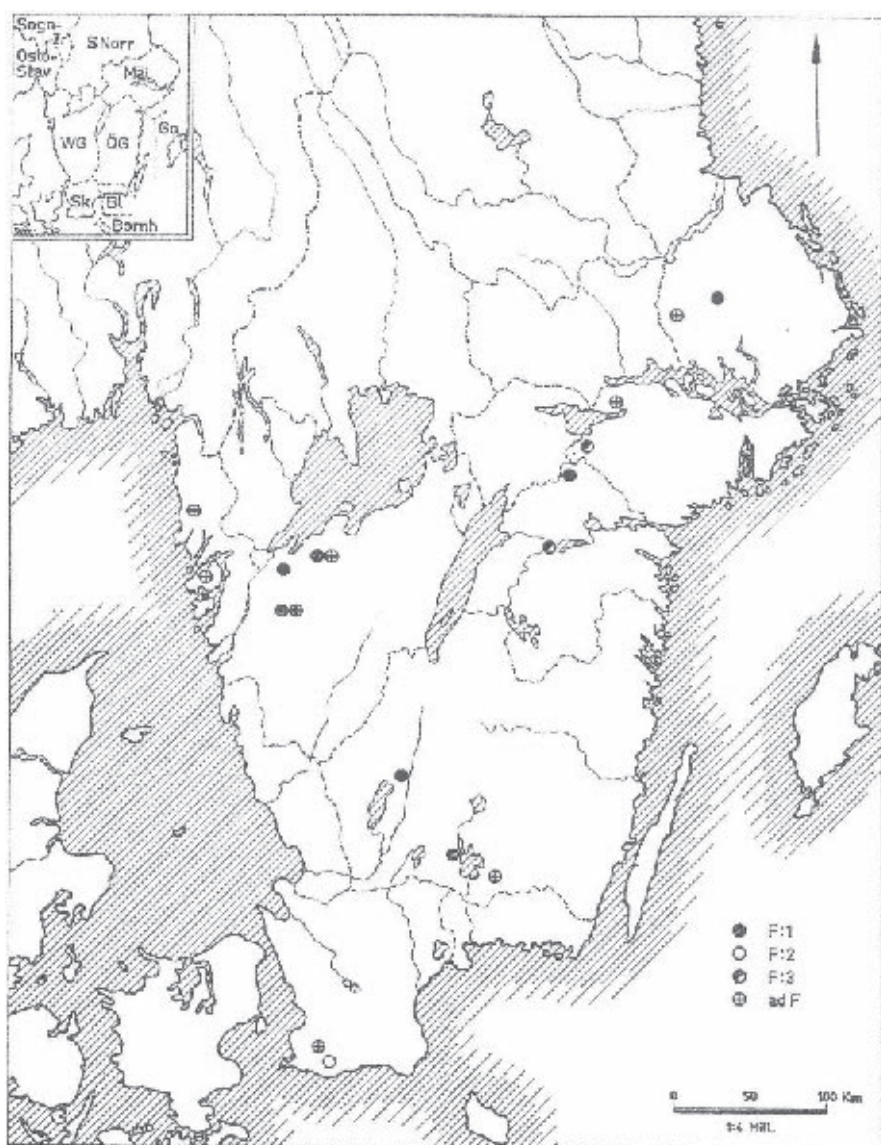


Fig. 9:19. Distribution of pottery group F (for area names, see fig. 18).

that they were constant. The best conditions for constancy exist, of course, if the entire investigation is confined to one and the same culture.

The Battle Axe Culture, despite the contrast between cord-stamped and comb-stamped decoration, displays several features of firm traditionalism. The form of the pots is essentially unchanged, so it may be assumed that the pots were used for the same purposes throughout the duration of the culture – in sharp contrast to the situation in the Funnel Beaker Culture, where pedestalled bowls and funnel beakers, for instance, must have had different uses. Only the big pots in groups C and E of the Battle Axe Culture may be assumed to have been coarse household ware. In accordance with these considerations, all the groups of the Battle Axe Culture have the same find circumstances – the distribution among flat-ground graves, megalithic tombs, and settlement sites is roughly constant, if we ignore groups C, E, and M, which are mostly found at settlement sites (tab. 2).

In the present case, which concerns investigating a presumed lag using quantitative methods, the comparison must be confined to Skåne-Blekinge, Götaland, and the Mälaren area, which have the largest quantities of pottery. The three areas are also well suited to a study of an assumed south-to-north course of innovation. The comparison between the three areas should be confined to the period-defining groups A, B, G, H, and J, plus group F which is significant in the north; the other groups cannot be dated with the same certainty. The find spots of these groups, C, D, E, K, L, M, N, and O, expressed as a percentage of the total number of find spots with pottery, amount to 25% in Skåne-Blekinge, 29% in Götaland, and 26% in the Mälaren area. With a distribution over the country that is as even as this, the exclusion of these groups does not constitute any significant source of error.

The find circumstances must be assumed to be different in the different geographical areas. Both agriculture and building activities (which have led to most finds of flat-ground graves from the Battle Axe Culture), and archaeological monitoring have varied in intensity from one area to another. Yet these and other conceivable differences are of no significance when it comes to comparing the three areas with each other, since all sources of error ought to be the same for all the pottery groups within one and the same area.



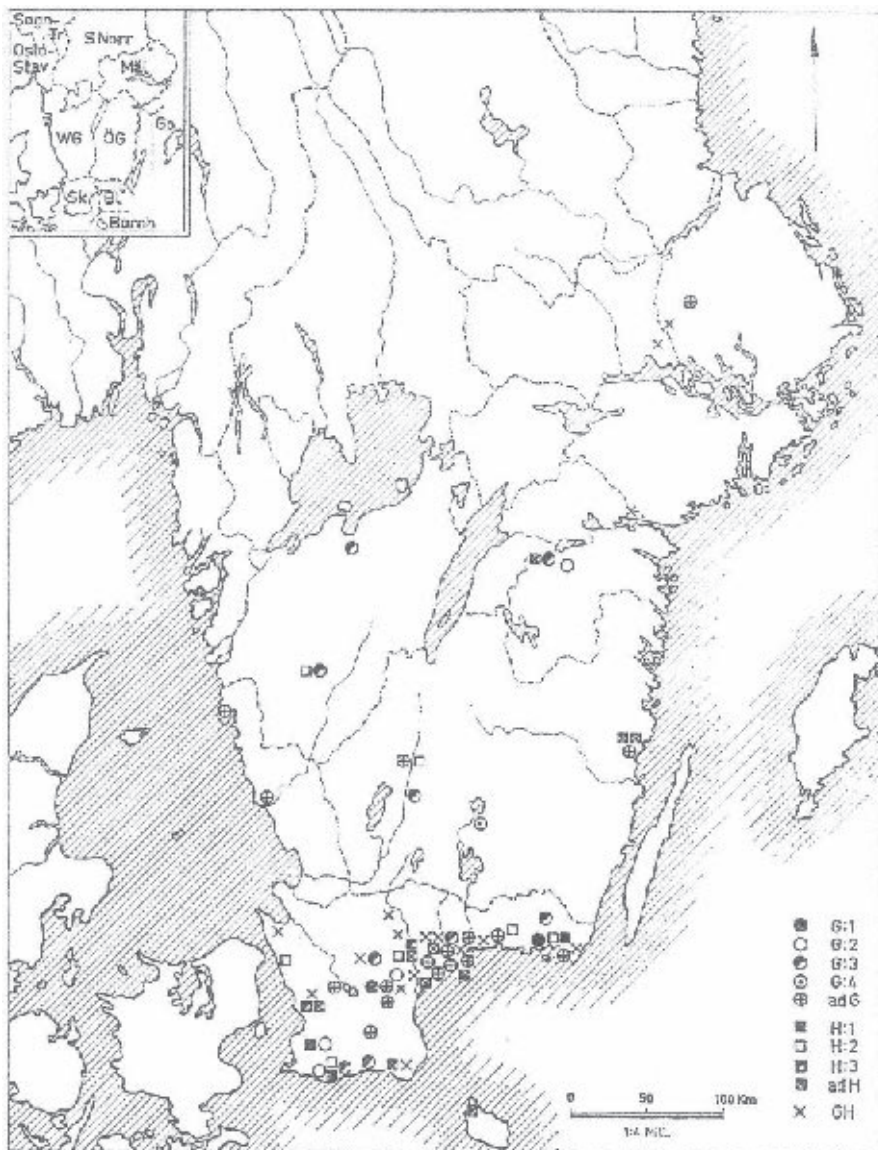


Fig. 9:20. Distribution of pottery groups G and H (for area names, see fig. 18).

Tab. 9:4. Frequency of groups A, B, F, G, H, and J in each of the three geographical areas (from Malmer 1962).

Group	Skåne-Blekinge %	Götaland %	Mälaren area %
A	11	22.5	20
B	8	2.5	12
F	2	25	16
G	27	25	6
H	23	15	6
J	29	10	40
Total	100	100	100

Tab. 9:4 shows the percentages of each of the five pottery groups in each area. The percentages in a table like this are almost impossible to interpret unless they are displayed graphically. This is done in fig. 9:24. As the diagram is drawn here (diagrams 1, 3, and 4), one can mentally include several more factors than just the graphic representation of the percentages in tab. 9:4. Since the groups (with the exception of group F) are arranged in chronological sequence according to the existing indications, the horizontal axis of the diagram represents *time*, from the start of period 1 to the end of period 5. The differently hatched areas of the groups are directly proportional to the *find quantities*. But if one assumes that the custom of depositing pottery in graves was constant during the five periods (and likewise the consumption of pottery on settlement sites), and if one also assumes that the find circumstances (within one and the same area) were the same for all the groups, then the hatched areas of the groups in the diagram must also be directly proportional to the prehistoric *pottery production*. The vertical axis of the diagram therefore represents the *intensity of production*, that is, the number of vessels manufactured per year. As the diagram in fig. 9:24 is drawn, this means that the intensity of production is assumed, for the sake of simplicity, to have been constant during the five periods. The *period boundaries* have been drawn in accordance with the definitions.

In modern theory of science, a bundle of facts and hypotheses like this – often presented in graphic form as in fig. 9:24, so that one can easily make a visual assessment of how the hypotheses agree with the facts and with each other – is called a *model*. In *Jungneolithische Studien* graphic presentations like fig. 9:24 are called “production diagrams”, meaning a “diagram of production, based on find quantities, and with the purpose of finding fixed chronological points”, cf. Malmer 1962. (The reason for not using the term *model* was that the word had

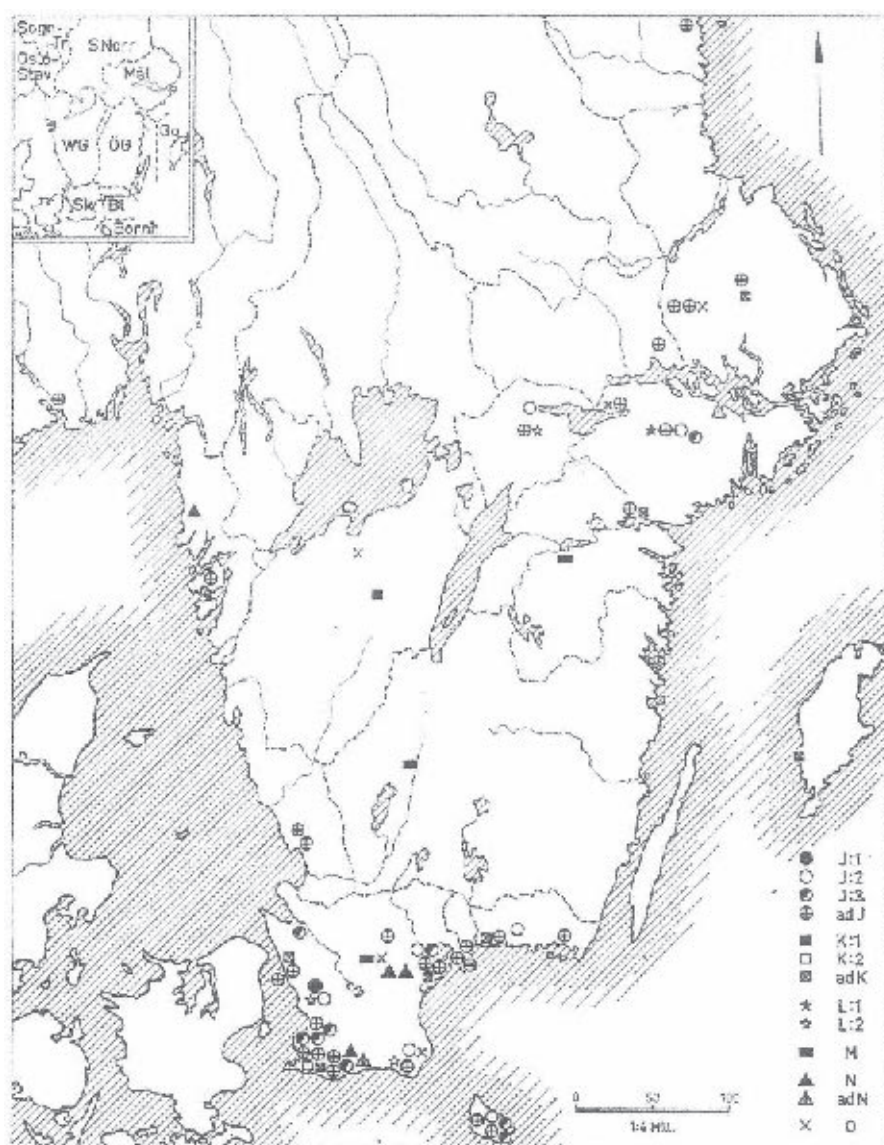
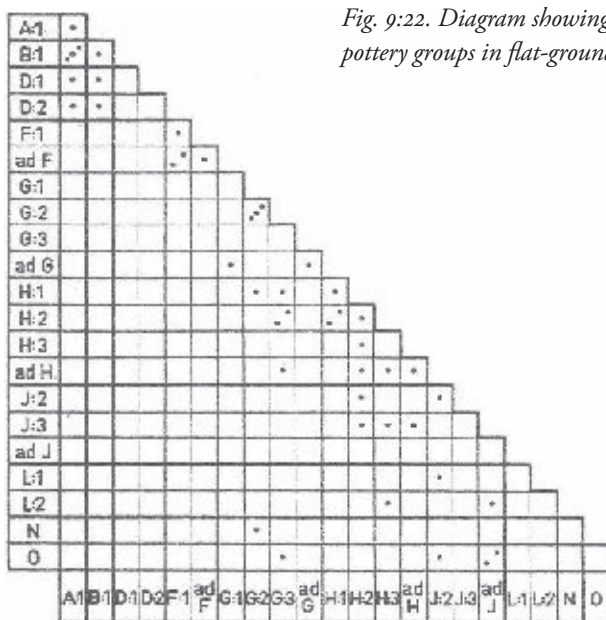


Fig. 9:21. Distribution of pottery groups J-O (for area names, see fig. 18).

Fig. 9:22. Diagram showing the find combinations of the pottery groups in flat-ground graves.



not come into use in that sense when the book was published in 1962, much less when that section was written in 1957. *Model* as a term and a methodological aid came into use in subjects like sociology and geography in the 1950s and in archaeology at the end of the 1960s; cf. Clarke 1972:1 ff.)

One characteristic of a scientific model, as of a material model of a building or a machine, is that one can try changing one detail to test whether it affects the whole. Diagrams 1, 3, and 4 in fig. 9:24 are based on one condition (not previously stated), namely, that the production of J pottery ceased in the Mälaren area and Götaland at the same time as in Skåne. This is unlikely. If the northern areas show a lag as regards the beginning of the pottery groups, there must also have been a lag in the closing phase of the pure Battle Axe Culture (and the start of the Late Neolithic). As the diagram in fig. 9:24 is drawn, groups B, F, G, H, and J would all have begun earlier in the Mälaren area than in Götaland. This is not plausible if we wish to stick to the hypothesis of a south-to-north course of innovation. There is thus double reason to assume that groups A, B, F, G, H, and J in the Mälaren area represent a somewhat longer time than in Skåne-Blekinge. And since the A–B pottery is so uniform over its entire distribution area, and consequently was spread through a very rapid innovation process, it is probable that the longer time for the sequence A–J in the Mälaren area entailed continued production of J pottery in this area at a time when Skåne-Blekinge had already switched to the Late Neolithic Culture. Based on these considera-



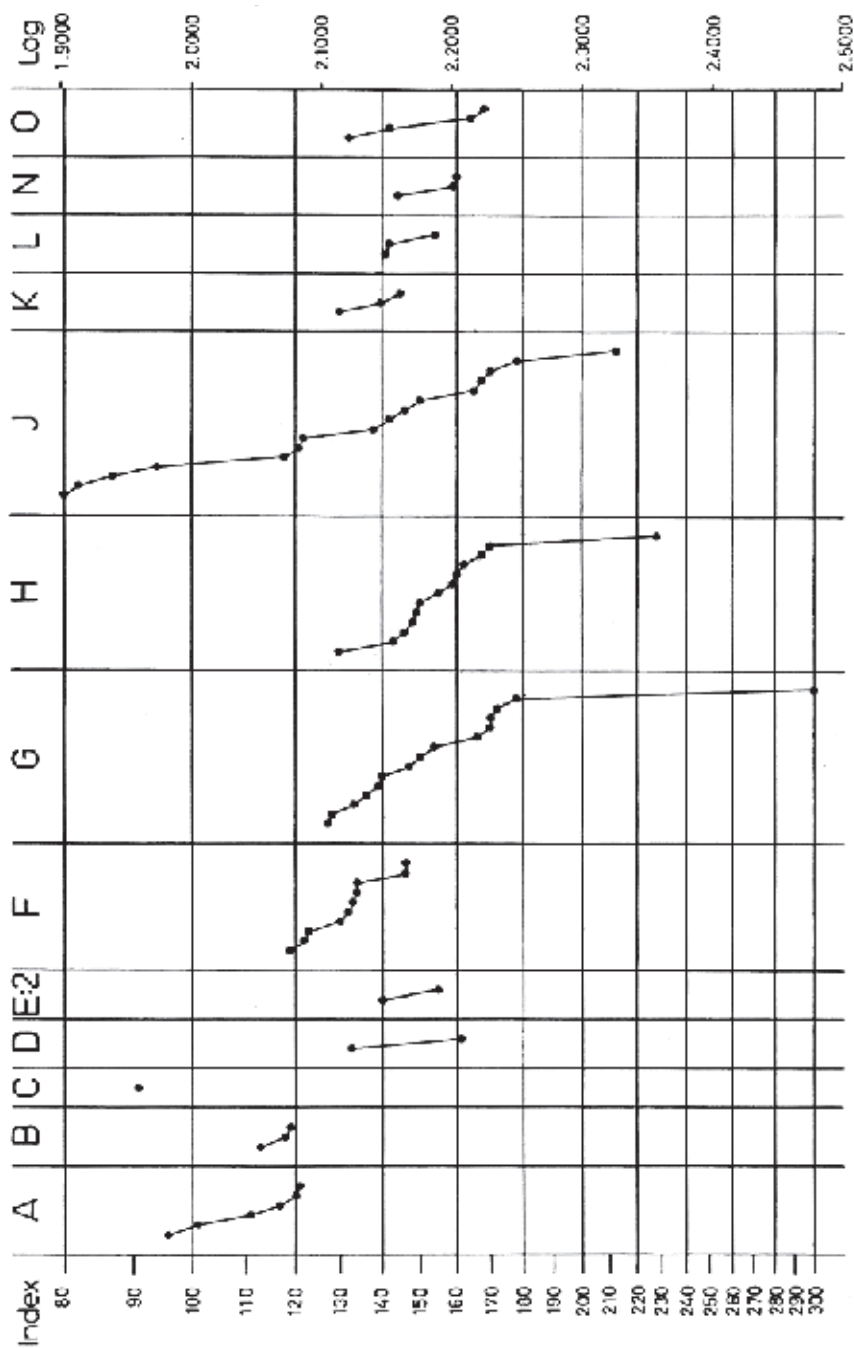


Fig. 9:23. Vessel indices for all measurable vessels in the Swedish-Norwegian Battle Axe Culture. Logarithmic scale.

tions, the diagram for the Mälaren area has been redrawn on a larger scale (but, needless to say, retaining the same internal proportions) in the form shown by diagram 2 (fig. 9:24). The location selected for the start of J pottery in the Mälaren area is just after the start of J pottery in Skåne-Blekinge and simultaneous with the start of H pottery in Götaland.

It is possible that J pottery continued to be produced in Götaland too after the start of the Late Neolithic Culture in Skåne-Blekinge, but we have no data to show how long any such lag may have lasted. In the form shown by diagrams 2–4 (fig. 9:24), the model shows group J starting later in Götaland than in the Mälaren area. In reality, of course, it is improbable that group J, in its diffusion up to the Mälaren area, should have passed Götaland without trace. It is likely instead that only part of the area (evidently Småland; cf. the maps in figs 9:20, 21) stuck conservatively to the G–H pottery, while the path of innovation for J pottery via Halland, Bohuslän (and presumably Västergötland) was able to reach the Mälaren area. To let the model give a visual impression of this division of Götaland would be too complicated, but the final form of the model presented below (fig. 9:25) gives a picture that has fewer contradictions.

It is now possible to work further with the model, which is still unsatisfactory in at least two respects. First, group F cannot be placed before group G in the model, since there are clear indications that the F pottery is in fact a blend in the northern areas of A–B and G pottery. Second, and more far-reaching, the boundaries between the groups in fig. 9:24 have been made vertical, which would mean that a group in a certain area immediately ceased to be produced as soon as the next group was introduced. In reality, however, the normal course must have been that each type or group of any originality or independence was created by a single person on one specific occasion, and if the type was accepted by other people, then it was subsequently made and used in ever wider circles and on an increasing scale until a maximum was reached. The maximum probably often coincided with the introduction of the next type, the production of which gradually increased as the production of the previous type decreased. It should thus be possible to represent the normal picture of the production of a type as a lenticular figure: introduction, increase in production intensity to a maximum, followed by a decline and the ultimate cessation of production (Clarke 1968:202 ff.; Moberg 1969:146 f.).

The model should thus be redrawn with oblique dividing lines between the groups, indicating that they were produced simultaneously for a certain time. There should now be just one possible way to put forward a fact-based hypothesis as to how long two groups were produced simultaneously, namely, to study flat-ground graves containing two or more pots. One can formulate an

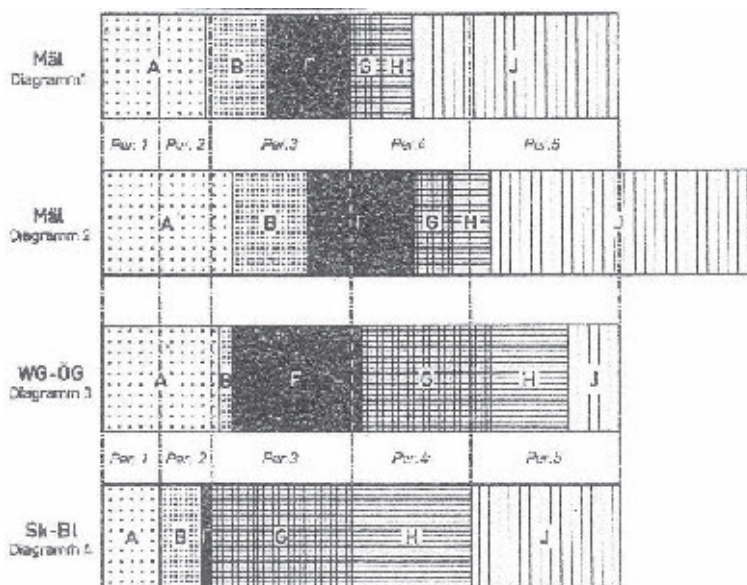


Fig. 9:24. Model of pottery production in Skåne-Blekinge, Götaland, and the Mälaren area in periods 1-5, assuming constant intensity of production.

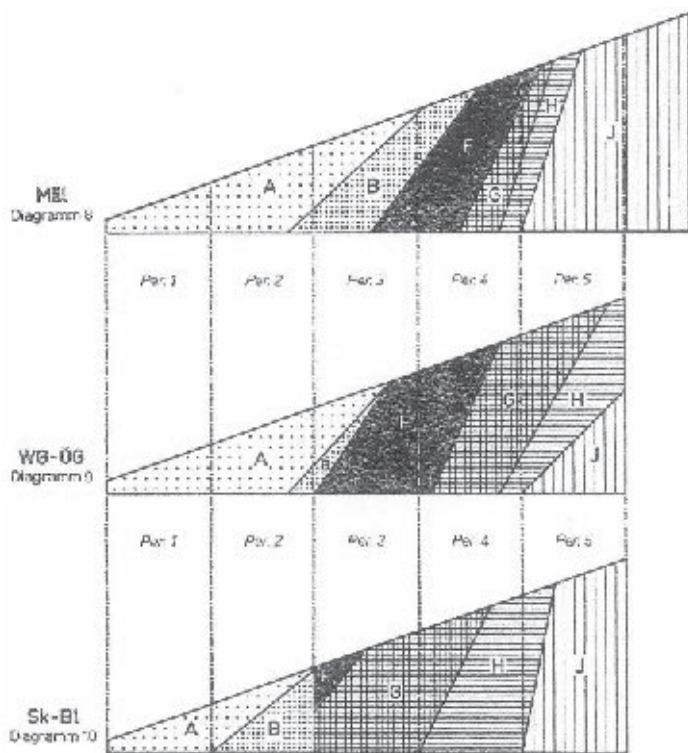


Fig. 9:25. Model of pottery production in Skåne-Blekinge, Götaland, and the Mälaren area in periods 1-5, assuming gradually increasing intensity of production and constant period length.

auxiliary hypothesis, that the people of the Battle Axe Culture wished that the vessels deposited in graves should be representative of the pottery that was used in day-to-day life. If two period-defining pottery groups were produced simultaneously, then, the grave was furnished with at least one vessel from each group, but if only one group was produced, then two or more vessels from that group were placed in the grave; all this applies on condition that the custom and the status of the deceased called for more than one pot.

One could thus calculate how contemporary two pottery groups are by expressing the number of graves containing pots from both groups as a percentage of all the multiple-pot graves of either group. But now it is actually possible that multiple-pot graves increased in frequency during times when two period-defining groups were produced, precisely because people wanted to include a representative selection of the pottery that was in use. With that possibility in mind, the percentage in question – two-group graves as a percentage of all multi-pot graves – is clearly a *maximum* measure of the contemporaneity of the two groups. A *minimum* measure of this contemporaneity can be obtained by expressing the two-group graves as a percentage of all graves where only one of the groups is represented. It is scarcely possible to arrive at any other basis on which to choose a value between the maximum and the minimum measures. The scope for choice can be as narrow as between 20% and 22% (the contemporaneity of group H with group J), and at most between 38% and 100% (the contemporaneity of group A and group B); the reason the maximum measure here can be so great may possibly be that the idea of having multiple pots in graves did not arise until there were two groups being produced at the same time, A and B. With the aid of the calculated percentages one can redraw the model in such a way that the perpendicular projection of the oblique line between two groups against the horizontal time axis indicates the time of their contemporary production (Malmer 1962:106, 116, tab. 14, ill. 32).

It is clearly noticeable in fig. 9:24, and even more so in its redrawn state taking account of the estimated contemporaneity of the groups, that the periods, from 1 to 5, become gradually longer and longer. It is not easy to find any rational explanation for this phenomenon. On the contrary, it would seem reasonable to assume that the periods were of roughly the same length, so that the decorative styles changed at fairly constant intervals, perhaps as often as once a generation, or approximately every thirty years (Almgren 1955:72 ff.), which would give a total duration of almost two hundred years for the six periods of the Battle Axe Culture. At the same time, the condition set for the model in fig. 9:24, that the intensity of production was constant throughout the Battle Axe Culture, is less probable. At least when it comes to an expansive culture like the

Battle Axe Culture, it is reasonable to imagine that the use of its pottery spread gradually to more and more areas and broader strata of society.

In fig. 9:25 the model has been revised in accordance with this assumption, that the length of the periods was constant but production was constantly increasing. For the final decline in the production of Battle Axe pottery that is probable, generally speaking, there is room in the period that is not included in the diagram, period 6, the age of C pottery, when the Battle Axe Culture met with competition from the growing Late Neolithic Culture. The hatched areas for the different groups are naturally of exactly the same size in fig. 9:25 as in fig. 9:24, since they are in direct proportion to the find quantities. Group F, which has never been found in closed finds together with other pottery groups, has been placed in the diagram in accordance with indications that it is younger than the first occurrence of G pottery in Skåne and is a mixed form of A–B and G.

To draw diagram 10 (fig. 9:25) for Skåne-Blekinge, the previously calculated values for the contemporaneity of pottery groups have largely been followed; the exception is small discrepancies at the G/H and H/J boundaries. Group H, for example, is shown as being 25% contemporary with group J, against the calculated maximum value of 22% (cf. Malmer 1962:119). The reason why the calculated values could not be followed may be that the intervals between the first appearance of the period-defining groups in reality were not as exactly identical as assumed in the model.

The majority of flat-ground graves with pottery from two period-defining groups are found in Skåne-Blekinge, and the figures calculated for the contemporary production of the groups are thus less relevant for Götaland and the Mälaren area. Diagrams 8 and 9 are therefore intended also to show the other kind of contemporaneity mentioned previously, namely, that an older pottery group was produced in one part of the area at the same time as another part of the area had already switched to producing a younger group. For example, the section for period 5 in Götaland is intended to show how groups G and H were still produced in Småland while Halland and Bohuslän (and probably Västergötland) had already adopted group J (cf. the maps in figs 9:21, 22). The numerical values for the contemporaneity *at an individual location* cannot be calculated for Götaland and the Mälaren area until there has been a significant increase in the number of grave finds.

The model in fig. 9:25 – constructed with the aid of available facts and the most plausible hypotheses – serves as a basis for the chronology of the pottery and thus of the entire Battle Axe Culture. It should be underlined once again, however, that fig. 9:25 is a *model*, which can and should be redrawn if and when new finds so require.

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# Innovations – their nature and explanation

2002

## 1. The concepts of objectivity and actualism

I have tried as objectively as possible to describe the three great innovations in the Southern Swedish Neolithic: those of the Funnel Beaker Culture (TRB), the Pitted Ware Culture (GRK), and the Battle Axe Culture (STR).<sup>1</sup>

“Objectivity” has many meanings and has thus been evaluated differently. For me, objectivity in archaeology is primarily an endeavour to arrive at the *truth* about what happened in prehistoric times. Finding the truth naturally means exerting oneself to retrieve and present all the relevant evidence, but above all trying not to let one’s personal opinions and political preferences affect the results of one’s research. It is of course impossible to bring out the whole truth and nothing but the truth about prehistoric times; nor is it possible in a court trial, yet that is precisely what witnesses in many countries swear to do. And regardless of the extent to which we succeed in our endeavour to arrive at an objective truth about our prehistory, this ambition prevents people from giving free rein to their imagination, which is evidently also the intention of taking an oath in court.

In recent times, the possibility of reaching an objectively true picture of the past has often been denied, for example, when it is said that a unitary and monolithic past is an illusion. Instead some scholars stress the value of a radical pluralism, which recognizes that there are multiple pasts produced by the scholar himself in accordance with his ethnic, cultural, social, and political views, orientations, and beliefs. “Choosing a past, constituting a past, is choosing a future. The meaning of the past is political and belongs to the present” (Shanks & Tilley 1987a:245; 1987b:212). Declarations like these contain a truth verging on truism, namely, that all interpretations of prehistoric and historical evidence can be criticized in a virtually infinite number of ways. There is, to be sure, a great amount of historical source material, the meaning of which many histori-

1 The radiocarbon dates of the periods referred to in the chapter are: EN = 3900–3300 cal BC; MNA = 3300–2800 cal BC; MNB = 2800–2300 cal BC (SW)



ans agree about, regardless of their various political views, orientations, and beliefs. Yet there is an even greater amount of historical material about which there are differing opinions, and the same is true of the majority of humanistic subjects. This kind of relativity does not apply to mathematics, and only in small measure to the natural sciences, which can therefore with good reason be called exact sciences.

These assessments of the character of different sciences are generally held. There has been much less reflection, even among archaeologists themselves, about the distinctive character of the archaeological evidence. This distinctiveness is so great that prehistoric archaeology is in fact totally alone between two large blocs of scholarship, the humanities and the natural sciences. The material used by the natural sciences is dumb and non-human. The material used by prehistoric archaeologists is dumb and human. The material used by other humanistic sciences is human and for the most part verbal (Malmer 1984a:266; 1993:146). The material of prehistoric archaeology is just as dumb as that used by the natural sciences, but at the same time, just like the verbal material of the humanities, it is an expression of human ideas and emotions.

Since a trend within archaeology in recent decades has doubted or contested the possibility of arriving at an objectively true picture of prehistory, one should as a consequence of this also contest the potential of the natural sciences to arrive at an objectively true and useful picture of the physical world. Most people avoid going so far, but instead there has often been criticism of the natural-science research ideal which has been considered to have influenced archaeology, especially in the first half of the twentieth century. It is possible that archaeological problems have occasionally been treated in this way, so that humanistic aspects have been displaced by a scientific outlook, but in my experience this has not happened often. The opposite behaviour has been more common, especially in the last few decades (fortunately, however, parallel to the development of a laboratory archaeology oriented to the natural sciences). Archaeological artefacts are entirely made of matter, even if they express psychological or ideological realities. For this reason, if we want to retain any scholarly respect, we cannot analyse them without objective methods: mathematical, physical, chemical, and medical.

Objectively and exactly determined scientific properties in prehistoric artefacts and monuments, however, are intrinsically without interest from a historical point of view. They only become interesting when they are put in relation to modern people's perception and experience of them, and the same is true of their human-bestowed form. Our perception and experience today of prehistoric artefact types can be assumed to correspond closely to those of people who

were contemporary with the artefacts and monuments. Man's physical and mental equipment has almost certainly been the same in Scandinavia during the six thousand years that have passed since the start of the Neolithic. And people's essentially similar perceptions and behaviour can, in my opinion, be presumed to apply all over the world and far back in time. This seemingly simple central element in the archaeological method may be called actualism (Trigger 1989:92, 362; Malmer 1997:11). In geological research, from where the word has been borrowed, the term actualism concerns a theory that the geological processes taking place at present went on in the same way in the past, during the historical evolution of the earth (Norin 1947). The parallel with archaeology is obvious: man's physiological, psychological, aesthetic, and moral equipment was essentially the same in prehistoric as in modern times. The theory may thus be summed up in the words that the present is the key to the past. However, this does not rule out the possibility, either in geology or in archaeology, that other processes may also have occurred in the past than those now active.

One form of archaeological actualism is the methods of ethnoarchaeology. Through studies of present-day ethnographic evidence we can draw conclusions about the practical, social, and ideological functions of the artefacts in prehistoric times. Another form of archaeological actualism is experimental archaeology. Yet the concept of archaeological actualism has an even broader reference. It comprises the archaeologist's total perception of the prehistoric artefacts in comparison with the world around us today, and in particular our everyday environment. Elementary phenomena such as surface, weight, light, heat, colour, water, and stone are no doubt perceived by modern people in the same way as by people in prehistory.

If we want to find Stone Age settlement sites within a particular area, we can start by registering those which are already known and compiling statistics about how they are located in relation to different landscape formations, and then on that basis we can do reconnaissance on the ground. Another method, however, is simply to sit down on a slope and feel whether there is shelter from the wind and whether the sun provides warmth. If it feels pleasant, it may be worth digging a test pit. Even if this simple form of archaeological actualism is not sufficient alone as a survey method, it can be of good assistance.

In most bodies of artefact material we find a polarization into two groups of data. One group of data consists of the kind that can be interpreted actualistically with the aid of modern ethnographic or Western material, or with the guidance of our own personal experiences. The second group of data cannot be interpreted in this way. After a division like this, one will in all probability find that some of the actualistically interpreted first group of data in fact has a doub-

le interpretation potential in that it also has associations with the second group. In this way, new possibilities of interpretation arise for both groups of data (Malmer 1997:11).

The actualism that I advocate is supported by the general theory for testing hypotheses. A scientific thesis must, as we know, be falsifiable (Popper 1935). In the most recent archaeology hypotheses are not infrequently based on the notion that prehistoric people had completely different conceptions from those of people living today. This is probably true in many cases, but hypotheses based on this assumption risk becoming sweeping and over-imaginative, and above all there are usually no data to test them against. If we instead proceed from the assumption that prehistoric culture was identical to our own (which in many cases it naturally was not), then the state of the evidence is much better. We can then contrast our own concepts and practical experiences with the entire corpus of prehistoric artefacts and monuments and on a number of details falsify the thesis that the prehistoric culture we are studying resembled our own. The details for which falsification succeeds are the specific features of the prehistoric period we are studying. But the details for which the falsification fails are the general human traits that unite the prehistoric culture with our own.

It may seem as if this method could not result in anything but very simple facts about the past. Small but sure results, however, are preferable to airy and imaginative but unprovable or at least unproven hypotheses. These may be of use in choosing a new line of research, but in archaeology they have often been confusingly mixed with proven facts. Careful, detailed work is the method that has yielded the majority of the tenable results that have been achieved in archaeology since its foundation as a scientific discipline by Thomsen (1836). The corpus of archaeological artefacts and monuments can be made to yield an extremely large amount of perfectly true historical details, which can be used even more intensively than has hitherto been the case (Malmer 1963:250–266). The task requires a great deal of work, but otherwise it mainly calls for common sense (Watson 1991).

Lakatos (1970) has to some extent modified Popper's thesis about falsifiability. According to him, it is not the case that a hypothesis is abandoned as soon as there are facts to contradict it – in other words, that it is falsified. Hypotheses are not judged in isolation, but as parts of a larger theoretical system, which Lakatos calls a research programme. This view agrees well with archaeological actualism, the idea of which is to contrast in detail two large complexes of data: the prehistoric artefacts and the world of modern man, including our experiments and our ethnographical knowledge (Malmer 1997).

## 2. Interpreting the innovations

In this work I claim, as before (Malmer 1962), that the three great innovations in the Scandinavian Peninsula during the Neolithic cannot be explained by means of a simple reference to waves of immigration. In my opinion, most facts indicate that migrations were of subordinate significance in the innovation of the TRB, even less that of the STR, and probably not at all in the case of the GRK. This view conflicts with what is often claimed in both earlier and more recent literature, where the appearance of not only the TRB and STR but also the GRK is interpreted as military or at least large-scale invasions (cf. Malmer 1962:677 ff.). Speaking against such hypotheses are not just facts of detail but also general viewpoints. Above all, it does not seem reasonable that farming people in neighbouring countries to the south should have found it meaningful to invade Scandinavia when it had been inhabited for 5,000 years, at least not the areas north of Skåne, with a physical geography which would have been alien to them. In their homelands they had a more favourable climate and better soils, and above all they undoubtedly had considerable reserves of still unused cultivable land. Another unreasonable hypothesis is that the hunting/fishing people of the GRK would have left their homelands east of the Baltic Sea to invade Southern Sweden, which was then inhabited by a TRB with agriculture or hunting/fishing as the main economic activities, and with the emphasis on one or the other, depending on the climate and soils of each district.

The innovations cover very large areas. All three cultures are represented from the southernmost province of the Scandinavian peninsula, Skåne, to the far north. Through finds of thin-butted flint axes the TRB is reported as far north as Ångermanland, and with known settlement sites at least in Uppland. The GRK's most northerly known settlement sites are in Ångermanland and the STR's most northerly known graves are in Trøndelag in Norway. The provinces of Trøndelag and Ångermanland are both at about latitude 64° north. The extent of the three cultures in a north-south direction is about 900 km, which roughly corresponds to the distance from London to the Shetlands or from London to Marseille. A further 300 km or so to the north, in the province of Västerbotten, are the northernmost hoard finds of flint axes belonging to the STR. The great distances make it even less likely that the innovations would have involved a military or large-scale invasion.

The fact that the spread of TRB and STR innovations from the south to the north is unusually clear from a European point of view is largely due to the geography of Scandinavia. The North Sea separates the Scandinavian peninsula from the British Isles, and the Baltic Sea separates it from Finland and the Baltic

countries. Innovations thus run most easily in a south-north direction or, more exactly, both from the south to the north-west, to Southern Norway, and from the south to the north-east, to Uppland and the coast north of it. East-west connections across the Gulf of Bothnia are certainly not negligible, but they only have a minor effect on our three Neolithic cultures, since these almost all belong to the land south of these east-west links.

The foreign connections of both the Swedish TRB and the STR are thus mainly with Denmark and to some extent the countries south of the Baltic. That the innovations run northwards, and not in the opposite direction, as has sometimes been claimed, is obvious. The Swedish TRB is only the northernmost offshoot of a huge Central and North European culture group (Midgley 1992), and the STR is likewise an offshoot of an even larger group of beaker cultures, spread over the whole of Central, Western, and Southern Europe, with the Balkan peninsula as the sole exception (Malmer 1962:822–878).

Naturally, the transition from the hunting and fishing EBK<sup>2</sup> to the farming TRB was a significant economic change, among other things because the vegetable products of agriculture, especially grain, withstood storage much better than the animal products of hunting and fishing. Likewise, animal husbandry must have given a much more secure living than hunting. Against this, however, grain cultivation, at least in the northerly provinces, appears to have been relatively insignificant, and in Mesolithic cultures it had its counterpart in the collection of wild food, such as hazelnuts. Domesticated animals probably gave a certain security in the TRB economy, but the supply of game animals gave a similar security in the EBK. Pigs were undoubtedly an important asset for both the EBK and TRB, and the difference between wild and domesticated pigs was slight or non-existent. The great difference between Mesolithic and Neolithic culture, between the EBK and the TRB, however, was in the potential of Neolithic culture to *increase* the production of foodstuffs by tilling new soil and enlarging the stock of animals. The EBK had nothing like the same potential to increase its hunting, since the Mesolithic economy must have required a balance between the stock of game and its exploitation through hunting. Something similar applies to fishing, although the stock of fish varies more through time, independent of fishing, than the stock of terrestrial game animals.

The transition from a predominantly hunting and fishing economy in the EBK to a mainly agricultural economy in the TRB probably meant a slightly greater security, but above all it meant that food production increased in volume. This in turn meant, in all probability, that the population rose, but also that there was more storage of food, at least during years with favourable weather, good grazing,

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2 EBK = the Late Mesolithic Ertebølle Culture (SW)

and a good harvest of cultivated crops. This, again, enabled people to hold regular feasts to mark harvesting and slaughtering. A final consequence that can be hypothetically envisaged is a social division into people who controlled the stored food and those who had to accept what other people decided.

Conceivable reasons for the transition from the EBK hunting and fishing economy to the TRB agricultural economy are perhaps above all an improvement in climate, making it more favourable for primitive farming. In fact, however, the Neolithic expansion advancing from the south paused for more than a thousand years when it had reached the far north of Central Europe before taking the step over to Scandinavia (Welinder 1998:60), although the temperature and humidity at that time seems to have been virtually optimal. At the time of the TRB innovation in Sweden, the climate was admittedly favourable as well, but not as much as a thousand years previously (Welinder 1998:24). Perhaps land uplift and the reduced salt content in the sea were significant, as a disturbance in the EBK economy and hence its social balance. Changes in sea level have greater consequences for the area of available land in Denmark than in the Scandinavian peninsula (with the exception of the province of Uppland, where the consequences are even more far-reaching), and it was no doubt also in Denmark that the transition from the EBK to the TRB was initiated. And the change was quick. The EBK constituted a mental and social community, and such a community has a tendency either to reject a significant innovation or to accept it quickly (Malmer 1991). If the innovation is accepted, this happens without exception in the central part of the community, and in the case of the EBK this would have been in Denmark.

The introduction of agriculture in Denmark and the rest of Southern Scandinavia thus undoubtedly had economic causes, but not exclusively or perhaps not even mainly. The acceptance of TRB forms of expression, with sacrifices, cultic houses, collective graves, and sophisticated artefact design cannot have happened for economic reasons, of course. On the other hand, the work organization and the social system once again had largely economic grounds. The conclusion must be that the introduction of the TRB in Scandinavia chiefly meant a new religious/social system, in which agriculture was only one important part. True, sacrifices, graves, and artefacts did not attain their richest forms until almost a thousand years later, but the clear and vigorous development of such elements shows that the ideas were there right from the beginning.

If the TRB innovation had only meant the spread of an agricultural economy, then the probable result would have been that only fertile areas, specially suited to primitive agriculture, accepted the new culture, whereas other areas would have stuck to their hunting and fishing economy for periods of varying

length. But in a uniform culture like the EBK, with good internal communications, it is hard to imagine a division of the people in one village or district into farmers and hunters/fishermen. The fact that the TRB was spread by an unimpeded course of innovation (Hägerstrand 1953; Malmer 1962:796; 1991), with all geographical areas up to Uppland and Southern Norway accepting the new culture, must instead have been justified by religious/social considerations. One aspect of the TRB was evidently cult, ceremonies, feasts, and aesthetically advanced handicrafts. Another aspect was just as evidently much stricter social organization than that of the EBK, which enabled great productivity in agriculture and the construction of temples and megalithic graves.

The rapid TRB innovation, the Neolithization of the southern third of the Scandinavian peninsula, can in a sense be said to be above all a mental phenomenon. Traces of slash-and-burn and ring-barking are found already in EBK times, and grain cultivation was still on a modest scale in the EN. It is true that no cultivation and no domesticated animals other than the dog are known from the EBK settlement site of Skateholm, but hunting and fishing were still of significance at TRB settlements, especially in the northerly provinces. The large cemeteries at Skateholm show that EBK settlement was stationary and of long duration, just as the long barrows and flat-earth graves show in the case of the TRB. The TRB had vigorous foreign connections, but so too did the EBK, as demonstrated most clearly by the pottery and the import of *Schuhleistenkeile*. On the other hand, the TRB differs from the EBK in three respects above all: first, through the mortuary practice, which was sophisticated right from the beginning and became increasingly complex at least until the start of the MNA; second, through the offerings made to higher powers; third, through the pottery with its complex form and decoration, which surely has a deeper meaning than pure aesthetics. It is these three expressions of the culture's mentality that above all characterize the TRB and make it into a remarkable era in the whole of Sweden's prehistoric cultural development. Sacrifices, mortuary practice, and aesthetics together make up a mental whole which can best be expressed in religious terms (Malmer 1962:811).

In my view, the innovations of the TRB, GRK, and STR should all be interpreted in economic and ideological terms, or rather in economic/ideological terms as an indivisible unit. The climate was perhaps the triggering factor, although Sweden's cultural status still cannot be certainly correlated with the fluctuations between warmer and cooler or between drier and wetter climate, which are admittedly securely attested in the EN and MNA (Welinder 1998:24). The TRB innovation, in my judgement, was the most powerful, the most radical ever undergone by Sweden apart from the first human immigration. Agricul-



ture with both tillage and animal husbandry was introduced, along with the associated technology, above all the grinding of flint axes, which enabled joinery and carpentry work, including the making of ploughs. Parallel to the practical sides of the innovation, but in interaction with it, an ideology developed, a religion with sacrifices to higher powers and a complicated ceremonial mortuary practice. Yet another integrated parallel phenomenon was a fixed social system, the existence of which must be presupposed behind the megalithic graves and large-scale sacrifices. The pottery, which is unique in Scandinavian prehistory through its artistry, is associated with all these aspects of the TRB, perhaps least with the practical and most with the ideological and social aspects. The TRB in Sweden evidently has strong European roots but also a clearly Danish form, and also certain specifically Swedish features. Some degree of migration at the start of the TRB is a reasonable assumption, since EBK settlement, despite the proven (and self-evident) interest in collection and to some extent the cultivation of useful plants, is nevertheless concentrated around the coasts and rivers of Skåne, less so on the cultivable soils.

It is obvious that the spread of innovations in the Northern European Stone Age could not have happened in any other way than by personal contact. A reasonable hypothetical estimate is that the dissemination across land could take place at a rate of at least 5 km per year. At this speed, the innovation of an agricultural economy through the TRB from Skåne to the Mälaren area would not have taken more than about 120 years, a time so short that it cannot be securely determined by the number, density, and accuracy of the existing radiocarbon dates. The same applies to the STR innovation. In addition, there is the securely attested communication over seas and lakes, which must have been much faster. For example, the little island of Trøna, on the Arctic Circle, which was inhabited during the Neolithic, is about 40 km west of the Norwegian mainland, and Gotland is more than 80 km east of the Swedish mainland, so that the journey to these and other islands required spacious and seaworthy boats. Only oak dugouts are preserved from the Swedish Neolithic, and conceivable larger vessels could have been rafts or leather boats with frames and keel of wood (Gjessing 1941:87), although neither type of boat has been found as yet. In all probability the boats were propelled not only by oars or paddles; the wind could have been used in some way, although no textile sails are known. The trip to Gotland, for example, must have taken a few days, or with unfavourable winds at most a few weeks. Generally speaking, for any voyage of greater duration than a day trip, a boat or raft was probably the normal means of transport in a Scandinavia which was mostly forested during the Neolithic and which is mountainous in many places. GRK settlement sites tend to be coastal, and all

the TRB and STR settlement districts are at least easily accessible by water. The same is also true of the great concentration of megalithic graves in the Falköping district in Västergötland, which is more than 100 km from the coast of Skagerrack, but which was in fact unusually easy to reach along the many quiet-flowing rivers through the plains of Västergötland. The most striking example of the significance of water routes is the finds in Västerbotten of hoards of flint axes, and water was surely the natural, and often the only possible route for all long and heavy transports.

The GRK innovation, which follows immediately after the halt of the TRB innovation in the Mälaren area, meant a regression in most respects. The northern limit of the Neolithic cultures was pushed northwards, however: the TRB is represented through now known sites as far north as Uppland, but the GRK at least 300 km further north, in Ångermanland. The appearance of the GRK, however, meant above all the gradual recession of the TRB all the way down to Skåne. The first result of this process is that the custom of building megalithic graves never reached the Mälaren area. The TRB's EN pottery ceased to be manufactured, without being followed by the MNA pottery from the southern areas, with its diversity of forms. This was replaced by a wholly dominant form of vessel, the simple GRK pot, which was made in varying sizes, from large storage vessels to miniature pots. The vessel shape shows some similarities to the combed ware east of the Baltic, and also to pottery elsewhere in Europe, but the most natural explanation is that it is a development of the very similar TRB storage vessel. Besides this all-purpose pot, the only ceramic product is the clay disc, an obvious inheritance from the TRB, which is also confirmed by the fact that it is mostly found in the provinces closest to Denmark, Skåne and Bohuslän. The GRK work axes of flint and stone and symbolic weapons are essentially identical to those of the TRB. The extant stock of jewellery is in many respects identical to (although richer than) that of the TRB, for which the probable explanation is that personal grave goods in the TRB were very sparse, whereas in the GRK they were lavish, especially in Gotlandic graves.

GRK settlement sites are mostly on the coasts, not on fertile cultivable soil. The GRK thus seems to have meant a heavy economic regression, in that the agrarian activities of the TRB gave way in many places to a hunting and fishing economy. One weakness of this conclusion is the fact that the TRB and GRK have such similar flint and stone tools that often the only difference in the material is in the pottery. Inland settlement sites without pottery have therefore perhaps often been assigned to the TRB, whereas it is in fact possible that they belonged to the same population as the seasonally occupied coastal GRK sites with pottery. The GRK pottery may possibly have had a specific practical func-

tion, associated with coastal hunting and fishing. The almost total absence of house structures at coastal sites with GRK pottery also seems to indicate that they were not permanently occupied. The Alvastra pile dwelling,<sup>3</sup> with the richest and best-preserved corpus of organic material of any GRK site, gives a complementary and in significant respects divergent picture of the settlement pattern of this culture. As at other settlement sites, both the TRB and the GRK are represented at the pile dwelling, and it is highly likely that it was the same population that changed its social system in the course of a relatively short period. The pile dwelling is a seasonal site, but evidently not intended to serve as a hunting station, or for any economic purpose, but as a place of assembly, feasting, and cult in hunting and harvest seasons. It is possible that the clearly TRB-influenced pottery belongs to the very first year, before the devastating fire in year 2 of the pile dwelling's own chronology, and the very careful division into rooms must have been done in this very first year. There is thus a possibility that the fire in year 2 marks the transition of the pile dwelling from the ideological system of the TRB to that of the GRK. The change in the plan of the pile dwelling may have been made on this occasion, from a strict division into 16 "rooms", each with a centrally placed hearth, to irregularly placed hearths with little consideration for the original room division. Against this possible explanation as a sudden change there is the evidence that everything suggests continuity. The pile dwelling thus has larger quantities of grain – barley and wheat – than any other Swedish Neolithic site, and the equally profuse osteological finds are dominated by domesticated animals, chiefly cattle. The economy is still, as in the TRB, essentially characterized by animal husbandry and grain cultivation, although the nearby megalithic grave, not much older, was abandoned, and with it the typical TRB pottery used in it. We do not know to what extent the many species of medicinal plants at Alvastra also occurred at other GRK sites, since preservation conditions are generally poorer.

Because of its short period of use and its good preservation conditions, the Alvastra pile dwelling allows us unusual opportunity to assess the relationship between the TRB and its successors. In Alvastra the GRK retains the TRB's agrarian economy and also its undoubtedly symbolically charged double-edged battle axes, but its equally symbolically charged pottery was replaced by GRK ware, and the megalithic graves were abandoned. In my opinion, the changes can be explained most simply as a reflection of a change in religion and a new social system. The ultimate causes of or reasons for all the changes, however, can scarcely be sought in the geologically and climatically favoured agricultural land of the Alvastra area.

3 See Section IV of this book.

The change from TRB to GRK, from an economy that was in principle agrarian to one mainly based on hunting and fishing, started in the Mälaren area some time around the EN/MNA transition, as shown by radiocarbon dates and by the fact that the TRB's MNA pottery underwent no development there. The change from TRB to GRK pottery spread gradually to the south and west, finally reaching Skåne, north-east Denmark and Southern Norway. The change from an agrarian economy to hunting and fishing, on the other hand, did not take place with the same regular course. Like the TRB innovation during the EN, the economic regression during the MNA was heavily dependent on the natural conditions in each area. Grain cultivation does not seem to occur at all at certain EN sites in Uppland, such as Anneberg (Segeberg 1999:111), while the Alvastra pile dwelling in the very fertile central plains of Östergötland in the middle of the MNA has full-scale agriculture with two kinds of cereal and all the domesticated animals of the Neolithic, admittedly in combination with equally full-scale hunting and a significant amount of fishing. At settlement sites along the Baltic coast from Uppland to Småland there are few or no traces of agriculture, but Siretorp in Blekinge has domesticated animals and cereals in both TRB and GRK layers. In Skåne, Denmark, and Norway there are bones of domesticated animals at many sites, although bones of wild animals predominate. As a rule, sites with GRK pottery are on the coast and have an obvious hunting/fishing economy, unlike sites with TRB pottery, which are always on cultivable soil and virtually always have traces of an agrarian economy, which at least in Skåne was sometimes wholly dominant.

The economic regression from the TRB to the GRK thus seems as a rule and in principle to have been rationally justified: where agriculture was profitable it was retained, and where it was not, people to a greater or lesser extent switched to hunting and fishing. The large proportion of pig bones at GRK sites is striking (Welinder 1998:102). It seems irrefutable that the coastal areas were more densely settled during the GRK than the TRB. It seems less obvious that the agricultural areas should have been correspondingly less densely settled or even in places abandoned. TRB and GRK sites are found through their respective ceramics. If people did not perform the ceremonies in which TRB pottery not improbably had its central function, and if they did not do the practical work in which the GRK pottery was presumably used, then we have great difficulties in assigning a site to either culture. If the site was in addition poor in flint, as is the case in all the Baltic provinces north of Skåne, then its insignificant remains will probably not be discovered at all.

With these reservations, it is nevertheless most probable in the current state of research that the change from TRB to GRK did in fact involve a significant

decrease in agricultural activity and perhaps, although less certainly, also an increase in hunting and fishing. A conceivable reason for this change is the climate. The mean annual temperature varied during the MNA, but the water level in lakes fell (Welinder 1998, diagrams p. 24 f.), indicating a dry climate which could not have been favourable for primitive agriculture. One can thus interpret the shift from the TRB to the GRK wholly in terms of natural determinism: the changed climate forced people to change the basis for their economy. It may be questioned, however, whether the change in climate was so significant, and so negative for an agricultural economy that it would have compelled such great changes in the way of life. A parallel, although in the reverse order, is the first spread of the TRB during the EN. It showed a speed and intensity that can scarcely be explained by purely economic reasons, such as that the climate just then should have become so much more favourable for agriculture. The motive force must instead have been largely on the mental and ideological plane, both at the introduction of agriculture at the start of the EN and at the partial regression to hunting and fishing in the course of the MNA. When agriculture and high-class art and architecture – as seen in pottery and megalithic graves – occur simultaneously in a society, the art may possibly be regarded as a kind of by-product of its economic base, agriculture. In my opinion, however, it is more natural to see both economy and art as expressions of one and the same ideology or religion. The regression from the TRB to the GRK began in the Mälaren area roughly at the EN/MNA transition, in other words, at the time when megalithic graves and advanced high-class pottery began to be created in the TRB of Southern and Western Sweden, following Danish models, but also with independent Swedish features. Instead of adopting the new southern influences, people in the Mälaren area and other peripheral areas reacted to the TRB behaviour that had been accepted during the EN: grain cultivation, which was never very important in the north, stopped in most places, and there was a significant reduction in animal husbandry, albeit not as heavy as that in tillage. Hunting and fishing, on the other hand, probably increased, especially fishing. The only ceramic forms retained were the storage and cooking pot (apart from the clay disc which occurs only in the south-west), but the stock of tools of flint and stone was retained, and people still continued to follow the TRB's general Scandinavian artefact forms, although flint objects became less frequent in many places. The transition from the TRB to the GRK largely meant a simplification of cult and ceremony and a concentration on practical concerns. In one social and no doubt also a cultic respect, however, the GRK maintained or even strengthened the tradition from the TRB, namely, as regards the Sarup-type sites. The GRK's assembly places for feasting, worship,

and sacrifice are scattered over the whole area of the culture from Gästrikland and the Norwegian province of Vestfold to Gotland and Skåne, and the Alvastra pile dwelling is the best-preserved and most illuminating example. From Alvastra and the other assembly places about which too little is known, one might possibly dare to draw the conclusion that people in the GRK assembled for local harvest and hunting feasts, whereas the TRB's Sarup-type sites, with their huge dimensions, more likely served for the social cohesion of larger areas. The relation between the TRB and the GRK, and the switch from one culture to the other, could hypothetically be summed up as follows. The rules for grain cultivation, cult, feasting, and tributes to the common good, or to priests and chieftains, brought by the TRB from more fertile regions, such as Denmark and Skåne, may have felt like a burden in more northerly regions, first in the Mälaren area. In addition, a climate change to the detriment of agriculture seems to have occurred. Subsequently a new orthodox TRB ideology, coming from the south, prescribed further behavioural rules, such as megalithic graves and temples as well as advanced artistic pottery. Behind these new rules, there is moreover reason to assume a reinforcement of the power of society or of the elite and hence a greater compulsion imposed on the individual. One particular detail in the complex of behavioural rules coming from the south, for instance, grain cultivation, may have seemed rather meaningless to people in the Mälaren area, and even more so the new requirement to build megalithic graves. Yet rejecting an important detail in an orthodox religious/social system entails a great risk that the entire system collapses, and that in my view is what happened at the transition from the TRB to the GRK. This reform or revolution began in the Mälaren area and spread from there to the south and west, finally reaching as far as south-west Skåne. It seems clear that the TRB survived for varying lengths of time parallel to the GRK in individual districts of provinces, especially in western Götaland and Skåne, with the result that the TRB is naturally found in the best agricultural land and the GRK normally on the coasts. The detailed chronology is difficult to ascertain, however.

Scandinavia's first farming culture thus split into two parts: a shrinking part in the south-west, the TRB during the MNA, where agriculture was more important than hunting and fishing and where spectacular religious cult played a significant role; and a growing north-eastern part, the GRK, where hunting and fishing were much more important than agriculture. Assembly places for feasting, worship, and sacrifice occur in both cultures. They seem to be particularly common in the GRK, perhaps as a counterpart to or a replacement for the feasts and ceremonies at the megalithic graves. Parallel to the GRK's rejection of TRB orthodoxy, there was also a simplification *within* this culture: although people contin-

ued to use megalithic graves, they did not construct any new ones, and the pottery was simplified, becoming increasingly inferior in both form and decoration.

In contrast to what has sometimes been claimed (Becker 1950), there is nothing to suggest any ethnic opposition between the TRB and the GRK. The fire in year 2 of the Alvastra pile dwelling is the only case in the current state of our research where it is conceivable that there was violent conflict, but if it occurred, it was of a social and not an ethnic character. The nearby megalithic grave makes it reasonable to envisage an elite of priests/chieftains in the TRB, who ruled the society before the disaster, and the finds after the fire clearly bear the stamp of a more egalitarian GRK. There is thus a hypothetical possibility, although it certainly cannot be proved, that the fire was part of a violent revolt against the TRB social system. For such a hypothesis to be probable, we would above all need further concrete signs of violent conflicts between the TRB and the GRK. With what we know at present, however, most of the evidence indicates peaceful, continuous development: for example, the similar artefacts in the two cultures (apart from the pottery), and as regards Alvastra also the clear continuity in the economy. In significant respects the TRB has the same form all over Southern Sweden, apart from the fact that it did not last as long in the north as in the south. The GRK has the same emphasis on hunting and fishing throughout its large distribution area, but as regards pottery it is more divided into local groups than both its predecessor the TRB and its successor the STR. Trade in flint also seems to have been less developed in the GRK. Communications between the different parts of the large distribution area were evidently good in the GRK, better in the TRB, and best of all in the STR.

In the present state of our knowledge, the time when the STR appeared can be hinted at through a description of the course of innovation in the southernmost part of the Scandinavian peninsula, in Skåne.<sup>4</sup> The oldest pottery, groups A, B, D, and E:I, are best represented in the north-east of the province, in the calcareous area. The earliest battle axe groups, A, B, and C:I, on the other hand, are represented in the whole province, but best in the calcareous area in the north-east and in the far south-west, the western part of the southern moraine area (Malmer 1957, karta 12 = Ch. 8, map 8:12). One of the rare battle axes of Group A, for example, was found in the passage grave at Gillhöög on the coast of the Öresund in the southern moraine area (Malmer 1962, tab. 31). The find-spots of the earliest pottery in Skåne are too few in number to allow any statistically certain conclusions to be drawn, but they can at least serve as a basis for a hypothesis. Let us assume that the earliest STR pottery actually had a similar distribution to that of the now known finds – mainly in the calcareous area in the

4 An extensive version of the following paragraphs is found in Ch. 9



north-east – while the earliest battle axes are known to have been strongly represented in the western part of the southern moraine area (albeit not as frequently as the later STR display axe, E:1). The obvious conclusion is that the STR had strong support in the north-east early on, while its battle axes were at least tolerated in the south-west of the province. This part of Skåne was then characterized by an agrarian culture with the TRB's late Valby pottery and GRK pottery which are so like each other that no one has made the attempt to distinguish them by logically clear definitions. In this area which is wholly agrarian but indifferent as regards artefacts, the STR gained a foothold with its battle axes in its period 1, but its pottery was not accepted until period 3. The reason for the delay in the STR innovation in south-west Skåne is the agrarian economy which had undoubtedly persisted there continuously since the EN. At least remains of the TRB religion probably also survived there from the time of the megalithic graves.

The STR was spread quickly from Skåne to the north, both to the Mälaren area/Southern Norrland and to Southern Norway/Trøndelag, as is clear from available radiocarbon dates and even more from the small type variation shown by the artefacts and monuments from the south to the north. In particular, the A-type pottery, the oldest STR group, has almost exactly the same design in the north and the south. As a result of the STR innovation an agrarian economy once more gained a foothold in the parts of Southern Sweden and Norway which had mostly pursued a hunting and fishing economy in the GRK. Although there are not many impressions of grains in the pottery, they are distributed over the entire chronological duration and geographical extent of the culture, and the scarcity of impressions can probably be mainly explained by the fabric of the pottery, which means that the grains more easily end up on the surface of the vessel, where they can be removed by the potter. The grain impressions in the STR have mostly been identified as barley, whereas wheat predominates in the TRB (Malmer 1962:804; Welinder 1998:100, 486). The situation is similar in the Danish Battle Axe Culture, and the difference in the choice of species is probably due to cultural rather than climatic differences.

Flint is much more frequent in the STR and TRB than in the GRK, and more frequent in the western than the eastern part of Southern Sweden. Comparisons between the STR and TRB are difficult because of different find circumstances, but there is nothing to suggest any differences to speak of in the frequency of flint between the two cultures. Skåne with its natural deposits of flint is of course always richer in flint than the northern provinces, but the flint deposits in Västerbotten, at a distance of 1,200 km from Skåne, show the power and intensity of the STR's internal communications.

The sequence EBK–TRB–GRK–STR follows an internal logic. The EBK is a

local Scandinavian hunting/fishing culture, although towards the end of its time it was influenced by the Continent in the form of pottery, *Schuhleistenkeile*, and tentative attempts at agriculture. The TRB is in origin a Continental/Danish agrarian culture which swept up the Scandinavian peninsula in a vigorous wave of innovation, reaching the Mälaren area and Southern Norway. The GRK is a local Scandinavian hunting/fishing culture, although preserving an agrarian element from the TRB. The STR is in origin a continental/Danish agrarian culture which swept up the Scandinavian peninsula as a powerful wave comparable to the TRB innovation process. The TRB and STR are visible and comprehensible to us primarily through graves and other ceremonial monuments and artefacts, while the EBK and GRK are primarily visible through settlement sites. (Gotland is an exception: the TRB and STR are weakly represented, but the GRK is heavily coloured by the ceremonial features of these cultures.) The EBK artefacts, and to some extent those of the GRK, are almost wholly borrowed from the TRB and other agrarian cultures. The TRB and STR artefacts follow the patterns of related continental (and to some extent British) cultures, although with prominent national features, particularly in the most important ceremonial objects: battle axes and pottery. In the course of 1,500 years there were thus exchanges between local, indigenous hunting/fishing cultures and general European farming cultures. The triggering factor in these exchanges may have been changes in climate, but a more profound reason was no doubt the major currents of ideas, which in the case of the STR were pan-European.

The basis for the TRB's magnificent monuments and the STR's outstanding uniformity over large areas, or in other words its strong conventions, is indubitably a fixed religious/social system. The much smaller uniformity in artefacts and mortuary practice in the EBK and GRK indicates a less complex religious system with looser social ties. However, we find few or no traces of hostilities between cultures or local groups.

Archaeological finds are of varying distinctness, and we most easily find objects of a ceremonial character which our science has long observed and classified. Humbler finds of everyday character easily escape attention. A hunting/fishing culture and an agrarian culture probably existed in parallel, and in close proximity to each other, throughout the Neolithic (and much later). The GRK cannot be viewed solely as a result of regression from the TRB; it is also a continuation of hunting/fishing settlements which must have survived as remains of the EBK and other Mesolithic cultures (Malmer 1969:100). In the same way, agrarian activities, especially animal husbandry, survived to some extent throughout the duration of the GRK. With the STR came the restoration of agrarian activities and a fixed religious/social system.

### 3. The concept of the archaeological culture

Swedish artefacts and ancient monuments have been classified in this work in the categories of EBK, TRB, GRK, and STR, which in established archaeological terminology are called cultures. In recent years scholars, especially in Scandinavian archaeological literature, have avoided the word 'culture', replacing it with something else, usually 'tradition'. In the choice between these two, 'culture' is preferable, in my opinion, partly because of its long use in archaeology, and partly because 'tradition' is often used for special, short-lived behaviours. 'Culture' comes from the Latin *cultura*, 'cultivation', thus meaning a defined way of working or behaving that is repeated for a long time.

It is important, of course, that all four cultures were in fact very long-lived phenomena, each embracing a whole or half millennium, which may be compared, for example, with the whole Scandinavian Bronze Age, which also lasted a thousand years. In the present work, all four cultures are perceived as carried by an ethnically unchanged population, with the sole exception that the TRB may perhaps have meant a certain influx of population; if so, they would mainly have come from Denmark west of the Öresund. Naturally, all the cultures were spread by personal contact, but the precondition for the acceptance of the innovations at all, and their rapid acceptance to boot, was undoubtedly that they were *not* associated with violence.

The four cultures evidently differ in terms of economy. But the innovations, the changes in livelihood and culture, do not seem to have been mainly *caused* by economic factors. At the transition from the EBK to the TRB, the introduction of a fully agrarian culture was clearly not caused by an improvement in climate occurring just then; this in fact occurred a thousand years previously, without leaving anything but insignificant traces in Scandinavia. At the TRB/GRK transition, followed by a switch to an economy more dominated by hunting and fishing, the cause was hardly a climate more unfavourable to agriculture. At the transition to the STR from a Sweden that was divided between a TRB receding towards the south and an advancing GRK, it is not possible to discern any significant climatic causes either. The crops cultivated in the STR were slightly different from those in the TRB, which is only natural in view of the fact that the two innovations are separated by more than a thousand years.

The successive changes from EBK to TRB to GRK to STR were thus caused neither by ethnic conflicts nor by climate change. The differences between the cultures are instead on the ideological plane. Culture is ideology.

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### III.

## Quantifying the Bronze Age

MATS P. MALMER discussed innovation and diffusion during the Bronze Age as well. He chose to do so with reference to images: rock carvings and bronze carvings. He would probably have called the former *vresiga* (“cross-grained, contrary”). He used to say that about objects which were hard to handle, the kind with few registerable, independent typological elements. He stressed the merits of the bronze carvings. Being part of bronze objects, they were excellent datable images (Malmer 1970). Yet he wrote his major Bronze Age work about rock carvings. Using stringent typological classifications and production diagrams, he outlined the rise and chorology of the rock-carving tradition (Ch. 11). In articles he additionally emphasized that rock carvings undoubtedly contained narratives and mythology. Yet he also underlined that the types, chronology, and chorology had to be examined first; only then could the discussion of interpretation be meaningful (Malmer 1989a; 1989b).

Malmer’s interest, verging on passion, in what was measurable resulted in two articles about weight systems in the Early Bronze Age. He believed that massive bronze axes and statuettes could be demonstrated to have been cast using specific amounts of bronze, making up even fractions and multiples of a weight unit (Ch. 12–13). He may have modelled this on similar studies weighing and discussing Migration Period gold and Viking Age silver. The idea is daring for the Bronze Age, and would undoubtedly indicate an interesting innovation with a distant origin. In his last work about the Bronze Age he discussed the diffusion of artefacts and ideas over long distances, from Greece to Scandinavia (Ch. 14).

A very sharp critique of Malmer’s interpretations based on weighing Bronze Age objects was put forward in Swedish by Anders Gustafsson (1995). He described Malmer’s calculations as numerology and compared them to dubious archaeoastronomy. It was unusual for Malmer to be subjected to such severe criticism in such harsh words within Swedish archaeology. He did not respond to the criticism in print, because his views had already received ample support in the statistical work of Erik Sperber (1993; 1996), cf. Ch. 14.

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# A chorological study of North-European rock art

1981

## I. Terminology and classification

First of all it is necessary to define some of the more important typological elements found in ship designs:

- ❖ The *line of the gunwale* is the upper horizontal limit of the ship. If the ship is represented by a *single* horizontal line, this line is regarded as the gunwale.
- ❖ The *line of the keel* is the lower horizontal limit of the ship. If the ship is represented by a *single* line, it is regarded as having no keel.
- ❖ The *end lines* are vertical, oblique or curved lines at the stem and stern, which join the lines of the gunwale and keel to complete the hull.
- ❖ The *double prow* of type A ships (defined below) is formed either by both gunwale and keel lines extending beyond the end lines (fig. II:1, AI right) or by the end line curving inward and creating a concave prow (fig. II:3, left). In type B ships the line of the gunwale is forked (fig. II:1, BI). In ships of types C–E, the end lines form concavities centred on vertical (fig. II:1, CI) or horizontal (fig. II:1, DII left) tangents. The expression ‘double prow’ obviously does not conform to accepted nautical terminology, but the constructions portrayed in rock-engravings of ships do not conform to constructions found in any known ships.
- ❖ A *single prow* indicates *either* the continuation of the line of the gunwale or the keel (but not both) beyond the end line (fig. II:1, AIII), *or*, in the case of ships with gunwale only, that this line is not forked (fig. II:1, BIII). Ships are *also* said to have a single prow when both gunwale and keel lines finish at the end line, and the end line continues above the level of the gunwale (fig. II:1, EIII).
- ❖ A *hammered-out hull* is a hull where the surface between the gunwale and the keel has been removed – or in other words, where the hammered-out surface of the hull is at least twice the width of the lines which make up the prow (fig. II:1, C).

























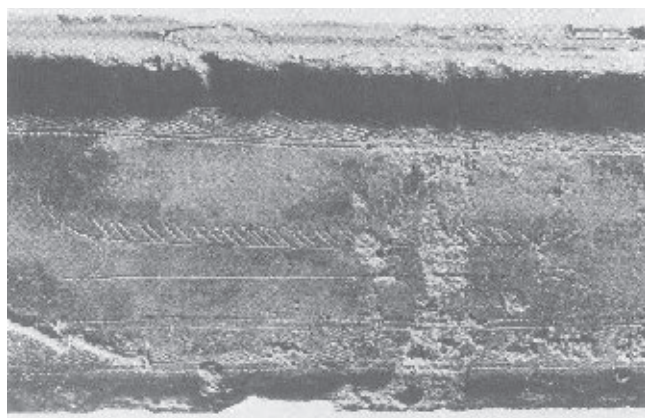
	I	II	III
A			
B			
C			
D			
E			

Fig. 11:1. The type-defining elements of ship designs. Series A-E (horizontal lines, hull) and I-III (prows). All the illustrations in this chapter are taken from existing rock-engravings, but they are not drawn to a uniform scale. Sites: Alar: Åmøy, Rogaland. AIIa2: Tøse, Bohuslän. AIIIc1: Himmelstalund, Östergötland. BIa1: Lökeberget, Bohuslän. BIIc1: Backa in Brastad, Bohuslän. BIIIa1: Helgerød, Østfold. CIc1: Leonardsberg, Östergötland. CIIa1: Backa in Brastad, Bohuslän. CIIIb1: Himmelstalund, Östergötland. DIc1: Åmøy, Rogaland. DIIc1: Åmøy. DIIIc1: Åmøy. EIa1: Berg, Östergötland. EIIa1: Åmøy, Rogaland. EIIIb1: Åmøy.

- ❖ A *contoured hull* is a hull where the line of the gunwale, with its extension into the prow, the keel line (but not necessarily the keel's extension beyond the end lines) and the end lines themselves, form a closed outline (fig. 11:1, D).
- ❖ The *crew* is represented by two or more parallel lines meeting the gunwale from above at right or acute angles (fig. 11:2, a); in the latter case the lines representing the crew incline towards the stem of the vessel (fig. 11:3).
- ❖ The *head* is the rounded terminal which sometimes occurs at the top of individual lines which represent the crew (fig. 11:3).

	1	2	3
a			
b			
c			

*Fig. 11:2. The type-defining elements of ship designs. Series a–c (crew) and 1–3 (ribs, hull decoration). Sites: A1a1: Åmøy, Rogaland. A1a2: Tose, Bohuslän. A1a3: Ekensberg, Östergötland. C1b1: Himmelstalund, Östergötland. A1b2: Skjeberg, Østfold. A1b3: Himmelstalund, Östergötland. A11c1: Himmelstalund. A1c2: Madsebakke, Bornholm. A1c3: Himmelstalund, Östergötland.*



*Fig. 11:3. The ship design on the Rørby sword (photo by Bertil Centerwall).*

- ❖ The *ribs* are those lines which connect the gunwale to the keel, provided that they are not end lines, and that they join the gunwale and keel lines at angles between 60° and 90° (fig. 11:2, 2).

The ship designs show a considerable number of variations, but the main elements, which are subject to variation, are few:

- ❖ Horizontal lines (number and sheer);

- ❖ The prows (single or double, height, concavity, and ornament);
- ❖ The crew; and
- ❖ The ribs.

These elements are well suited to a declension classification system using abstract symbols. In the terminology listed below, the four groups of typological elements are assigned upper case letters, Roman numerals, lower case letters and Arabic numerals respectively.

- A Ships with a gunwale and a keel (double-line ships).
- B Ships with a gunwale but no keel (single-line ships).
- C Ships with a hammered-out hull.
- D Contoured ships.
- E Ships with a gunwale and a keel and at least one intermediate horizontal line on the hull.
- I Ships with two double prows.
- II Ships with one double and one single prow.
- III Ships with two single prows.
- a Ships with crew, which sometimes have heads, but (apart from ornamented prows) no other feature above the gunwale (e.g. more detailed human figures or trumpet-like designs).
- b Ships with designs (in addition to crew) above the gunwale.
- c Ships with neither crew nor other designs above the gunwale.
- 1 Ships with no ribs; the hull may have a hammered-out surface or one or more horizontal lines, but no other lines or designs in the area circumscribed by the gunwale, keel and end lines.
- 2 Ships where the hull is decorated exclusively with ribs.
- 3 Ships where the hull is decorated with designs other than ribs and/or more horizontal lines.

The above list serves as an index defining the symbols A, B, C, D, E, I, II, III, a, b, c, 1, 2 and 3. There is no differentiation in status between these symbols; all are of equal importance, both *a priori* and in the opinion of the author.

The symbols may also be regarded as representing types. Thus, for example, type A includes all ships with two horizontal lines (i.e. gunwale and keel) and type III includes all ships with single prows: these types overlap. If, therefore, mutually exclusive types are to be defined, capable of embracing all ship designs found in rock art, the symbols must be combined to form a typological system of four symbols, one from each of the groups of elements A–E, I–III, a–c and 1–3.

Theoretically this system has a capacity of 135 mutually exclusive types. But, because the characteristics represented by 2 and 3 are incompatible with B and C, the real capacity is  $3 \times 27 + 2 \times 9$ , which totals 99 types; a sufficient number to produce an outline survey of the North European ship designs. The flexibility of the system also makes it easier to memorize the characteristics of the ninety-nine types – which would hardly have been possible with a rigid classification system.

Figs 11:1 and 11:2 demonstrate the construction of the system (the illustrations are taken from existing rock-engravings). Practical difficulties prevent a diagrammatic representation of all the ninety-nine types; fig. 11:1, therefore, only illustrates the complete range of variations of the horizontal lines (types A–E) and the prows (types I–III) while the examples of variations involving the crew (types a–c) and ribs (types 1–3) are incidental. In fig. 11:2 the emphasis is reversed and the complete range of variations of the two latter groups (a–c and 1–3) are illustrated, while examples of the former (A–E and I–III) occur only incidentally.

To complete the diagram it may be imagined that in fig. 11:1 each of the squares in columns A, D and E is subdivided into nine squares (corresponding to fig. 11:2) and that each of the squares in columns B and C are subdivided into three squares (corresponding to fig. 11:2, column 1).

## 2. The logical sequence of the classification system and its hypothetically chronological application

Within the groups the typological elements have been arranged as far as possible according to their degree of similarity:

- ❖ I, two double prows. II, one double and one single prow. III, two single prows.
- ❖ a, lines representing crew, but no other designs above the gunwale. b, other designs, possibly together with crew, above the gunwale. c, no designs above the gunwale.
- ❖ 1, no ribs or other designs on the hull. 2, ribs, but no other designs on the hull. 3, other designs, possibly together with ribs, on the hull.

The determining factor in deciding which element should be placed first in each short series was the appearance of what are probably the earliest of the ship designs which can be dated with any reasonable accuracy, those found on the Rørby sword (fig. 11:3) and the Wismar trumpet (fig. 11:4); the representations

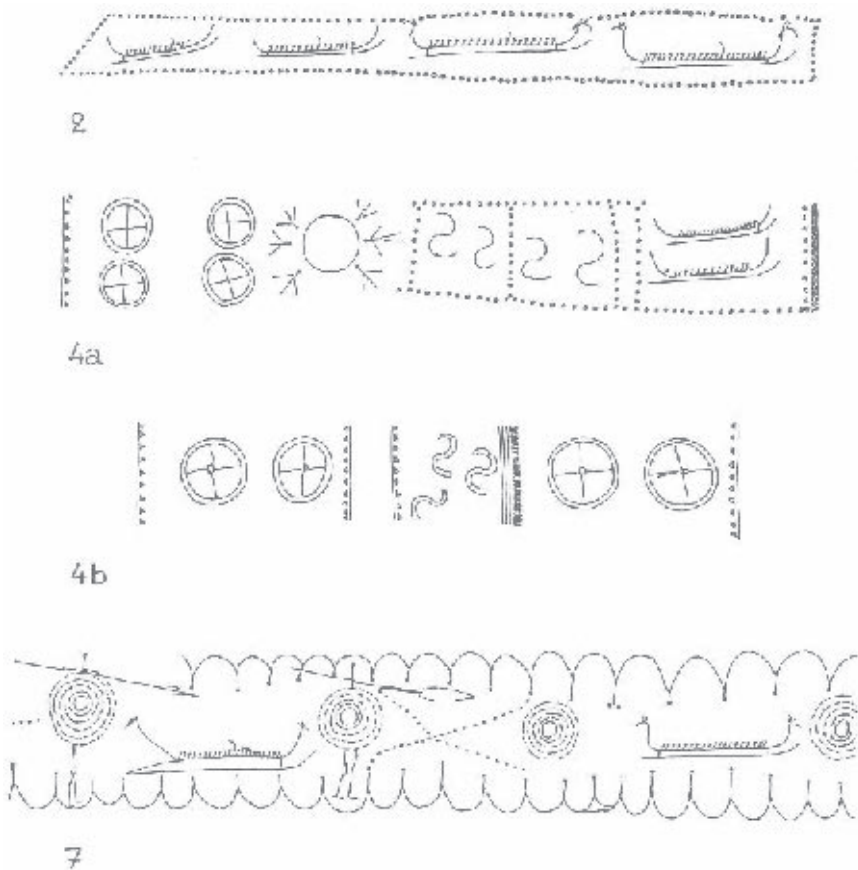


Fig. 11:4. Designs on the Wismar trumpet. Decoration zones number 2, 4 and 7, counting from the bell (drawings from the original by Brita Malmer).

of ships on the second stone slab in the Kivik grave were presumably also of this type (fig. 11:5). The arrangements of the gunwale and keel in these designs have therefore been assigned the symbol A, and the complete designation for these designs is A1a1. The elements B-E can all be seen as modifications of A, but they cannot be arranged in sequence.

Fig. 11:1 demonstrates clearly that the designs in the horizontal column A and the vertical column I show a greater similarity to A1 than the eight designs in the squares below and to the right. It is therefore possible to hypothesise a date for the origin of these design type thus:

- ❖ A1 is the earliest.
- ❖ B1, C1, D1, E1, AII and AIII are later.
- ❖ BII, CII, DII, EII, BIII, CIII, DIII and EIII are the latest.



Fig. 11:5. The eight decorated stone slabs from the Kivik grave (wash drawings by H. Faith-Ell, scanned from the originals, image layout by Andreas Toreld in *Fornvännen* 2015:1).

Similarly fig. 11:2 gives rise to the following hypothesis:

- ❖ a1 is the earliest.
- ❖ b1, c1, a2 and a3 are later.
- ❖ b2, c2, b3 and c3 are the latest.

A comparison of this kind cannot however provide any information concerning the length of time the older types may have *survived*.



### 3. Other classification systems

Ekholm's (1916) classification system was based partly on a chorological study in Uppland. Single-line ships (type B in the present study) are referred to the same period as double-line ships (type A).

Schnittger (1922) argues that the single-line ships are older than the double-line ships, because the former are more 'primitive'.

Gjessing (1935:127, 132) also places the single-line ships first in the chronological sequence. On the site Bardal, Beitstaden Parish, Nord-Trøndelag, Gjessing observed that where single-line and double-line ships cross, the latter are nearly always more deeply cut; he therefore inferred that they were of later date. But a deep line cutting through a shallower line does not in itself indicate a stratigraphical relationship. If, however, a shallow line could be seen to have made an impression in a deeper cut at the place where the two lines cross, it would imply a definite chronological relationship and the shallower line would obviously be of later date.

Althin (1945:47, 156–169) refutes the suggestion that the Scandinavian single-line ships derive from West-European chamber-tomb engravings. His thesis, that the original form of the Scandinavian ship designs must have been a double-line ship, is methodologically important.

The classification system published by Eva and Per Fett (1941, Pl. 82) is characterized by a notable clarity and capacity. It differs, however, from a declension classification system in that the numerals 1–3 mean one thing when combined with the letters A–G, and another in combination with the letters J–K.

Marstrand (1963:76–77, Pl. 64) strongly stressed the importance of stylistic considerations. Almgren, too (1964:158; 1970), declared his intention to date rock art by its style. Stylistic studies differ from other branches of typological research in that the scholar initially forms an intuitive interpretation of the stylistic intentions of the prehistoric artist. However, a scientific stylistic investigation demands an analysis of the basis for such intuitive impressions, and a recognition and definition of the relevant typological elements. The more rich and complex the art, the more useful are these intuitive impressions (in the study of styles such as Baroque and Rococo, for example); but in the case of simpler art, accurate analytical methods must take their place. Rock art, although rich by comparison with other prehistoric North European art, is of course in this context poor.

Gro Mandt's classification system (1972:14–16, 55–60) was published only after the manuscript of the present study was completed. Her classifications are fundamentally very similar to the system presented here, but it is a hierarchical

system, not a declension. Only two main types are recognized, the single-line ship (type I) and the double-line ship (type II). The common type C of the present study was not distinguished as a separate type, for the simple reason that it is rare in western Norway. Crew and ribs were not expressed by symbols and were not subject to statistical processing.

In a survey of Danish rock-carvings, published after the completion of the present study, Rostholm (1972:19) recognizes four of our hull types arranged in the sequence B, A, E and C. Chronologically the double-line ships are placed first; and there is an excellent quantitative and chorological survey.

#### 4. The chorology and chronology of the corpus

This corpus consists of 3877 classified ship designs: 72 from Denmark, 2381 from Sweden, 1289 from Norway and 135 from Finland-Karelia (fig. 11:6). No ship designs occur in North Germany. It is impossible to tell how far these figures reflect the actual proportion of ship designs that survive. It seems likely, however, that Denmark is represented comparatively comprehensively, due to the recent, excellently illustrated publication of all known rock-engravings in this area. The number of ship designs in Denmark is therefore likely to become proportionately even smaller in the course of time.

Tab. 11:1a, b shows the distribution of the ship designs by type and area. The vast majority of ship designs occur in a zone of central Scandinavia, from Östergötland in the east (with 645 ship designs) to Bohuslän (1155), Østfold (316) and Rogaland (499) in the west: a total of 2615 ship designs representing at least two thirds of the entire corpus.

The classification system used here has a capacity of  $3 \times 27 + 2 \times 9$  or 99 types. Of these types 78 are actually represented in the corpus. All nine B-types and all nine C-types are represented, and only two A-types are absent (AIIa3 and AII-1a3). Nineteen D-types are represented, but eight are absent, and of the E-types only sixteen are represented, while eleven are absent. The many D- and E-type ship designs which are not represented must to some extent be due to the fact that D- and E-type ships are generally rare.

Tab. 11:2 illustrates the frequency with which the fourteen type defining elements (used in the classification system to define the types) occur both within each of the geographical areas and in Northern Europe as a whole. The proportion of each element in the different groups of elements (A–E, I–III, a–c and 1–3) is expressed in percentages in tab. 11:3. Element 1 (ships without ribs) has been distributed between ships where ribs are logically possible (A, D and E) and ships which cannot have ribs (B and C). All elements vary greatly in fre-



Tab. 11:1b. Ship designs by type and area (B–E).

Area	A	Bla1	b1	c1	Bla1	b1	c1	Blla1	b1	c1	Cla1	b1	c1	Clla1	b1	c1	Clla1	b1	c1	D	E	Total
2. Denmark	36	8	2	2	-	-	3	11	1	4	1	1	-	1	-	-	-	-	-	2	1	72
— 2a. West Denmark	3	7	2	-	-	-	-	10	1	-	1	-	1	-	-	-	-	-	-	-	1	26
— 2b. Bornholm	33	1	-	2	-	-	3	1	-	4	-	-	-	-	-	-	-	-	-	2	-	46
3. Scania	59	5	2	5	1	-	1	4	-	1	23	-	8	-	-	-	-	-	-	-	3	112
4. SE Sweden	39	-	3	-	5	6	-	7	2	-	-	-	1	-	-	-	-	-	-	1	2	66
5. Östergötland	358	12	-	-	9	-	3	20	-	-	102	5	74	24	1	14	2	1	6	5	9	645
6. Mälaren district	96	3	-	-	2	-	-	-	-	-	23	-	1	4	-	-	2	-	-	5	-	136
9. Ångermanland	187	-	-	-	4	5	1	16	4	-	-	-	-	-	-	-	1	-	2	-	-	220
11. W Sweden	13	6	1	1	3	-	-	16	-	-	1	-	2	-	-	-	1	-	-	1	2	47
12. Bohuslän	406	145	32	21	90	25	22	86	13	13	147	66	50	13	3	5	2	-	3	4	9	1155
13. Østfold	122	19	5	5	28	4	9	70	3	23	18	-	1	5	-	1	-	-	-	1	2	316
14. S Norway	74	2	-	1	7	-	3	20	-	4	-	-	1	-	-	-	-	-	-	-	3	115
15. Rogaland	229	23	-	5	35	4	21	112	4	15	1	-	-	1	-	-	-	-	22	27	499	
16. Middle Norway	32	-	-	-	3	-	1	2	-	1	-	-	6	-	-	-	-	-	-	-	5	50
17. Trøndelag	117	12	-	16	5	-	7	28	-	30	11	6	14	1	-	-	1	-	1	36	3	288
18. N Norway	10	-	-	-	2	-	-	2	2	1	-	-	-	-	-	-	-	-	-	4	-	21
19. Finland-Karelia	2	-	-	-	4	-	1	16	4	1	17	16	13	14	32	5	2	5	3	-	-	135
— 19ac. S Finland – Omega d.	-	-	-	-	4	-	1	16	4	1	-	-	-	1	-	-	-	-	-	-	-	27
— 19d. White Sea d.	2	-	-	-	-	-	-	-	-	-	17	16	13	13	32	5	2	5	3	-	-	108
Total	1780	235	45	56	198	44	72	410	33	93	344	94	171	62	36	25	11	6	15	81	66	3877

Tab. II:2. Type-defining elements of the ship designs.

Area	Total	Horizontals					Prows			Crew			Ribs			
		A	B	C	D	E	I	II	III	a	b	c	ADe1	BC1	2	3
2. Denmark	72	36	31	2	2	1	52	3	17	26	6	40	9	33	29	1
— 2a. West Denmark	26	3	20	2	-	1	14	-	12	22	3	1	3	22	-	1
— 2b. Bornholm	46	33	11	-	2	-	38	3	5	4	3	39	6	11	29	-
3. Scania	112	59	19	31	-	3	101	6	5	78	6	28	62	48	1	1
4. SE Sweden	66	39	23	1	1	2	44	11	11	47	17	2	33	21	12	-
5. Östergötland	645	358	44	229	5	9	505	95	45	382	35	228	240	272	102	31
6. Mälaren district	136	96	5	30	5	-	122	11	3	107	2	27	80	35	12	9
9. Angermanland	220	187	30	3	-	-	2	14	204	66	18	136	128	33	57	2
11. W Sweden	47	13	27	4	1	2	23	7	17	29	9	9	11	31	3	2
12. Bohuslän	1155	406	447	289	4	9	837	189	129	688	235	232	214	736	193	12
13. Østfold	316	122	166	25	1	2	164	55	97	237	30	49	79	191	44	2
14. S Norway	115	74	37	1	-	3	71	14	30	73	10	32	62	38	14	1
15. Rogaland	499	229	219	2	22	27	230	109	160	242	32	125	218	221	56	4
16. Middle Norway	50	32	7	6	-	5	29	14	7	11	2	37	5	13	32	-
17. Trøndelag	288	117	98	34	36	3	167	21	100	95	29	164	95	132	46	15
18. N Norway	21	10	7	-	4	-	8	2	11	8	7	6	12	7	2	-
19. Finland-Karelia	135	2	26	107	-	-	46	57	32	53	57	25	-	133	1	1
— 19ac. S Finland – Omega d.	27	-	26	1	-	-	-	6	21	21	4	2	-	27	-	-
— 19d. White Sea d.	108	2	-	106	-	-	46	51	11	32	53	23	-	106	1	1
Total	3877	1780	1186	764	81	66	2401	608	868	2142	495	1140	1248	1944	604	81

Tab. 11.3. Type-defining elements of the ship designs. Percentages.

Area	Horizontals						Prows			Crew			Ribs		
	A	B	C	D	E		I	II	III	a	b	c	ADE1	BC1	3
2. Denmark	50	43.5	2.5	2.5	1.5	100	72	4	24	100	36	8.5	55.5	100	1.5
— 2a. West Denmark	11.5	77	7.5	-	4	100	54	-	46	100	84.5	11.5	4	100	4
— 2b. Bornholm	71.5	24	-	4.5	-	100	82.5	6.5	11	100	8.5	6.5	85	100	-
3. Scania	52.5	17	27.5	-	3	100	90	5.5	4.5	100	69.5	5.5	25	100	1
4. SE Sweden	59	35	1.5	1.5	3	100	67	16.5	16.5	100	71	26	3	100	-
5. Östergötland	55.5	6.5	35.5	1	1.5	100	78.5	14.5	7	100	59	5.5	35.5	100	5
6. Mälaren district	70.5	4	22	3.5	-	100	90	8	2	100	78.5	1.5	20	100	6.5
9. Ångermanland	85	13.5	1.5	-	-	100	1	6	93	100	30	8	62	100	1
11. W Sweden	27.5	57.5	8.5	2	4.5	100	49	15	36	100	62	19	19	100	4
12. Bohuslän	35	38.5	25	0.5	1	100	72.5	16.5	11	100	59.5	20.5	20	100	1
13. Østfold	38.5	52.5	8	0.5	0.5	100	52	17.5	30.5	100	75	9.5	15.5	100	0.5
14. S Norway	64.5	32	1	-	2.5	100	62	12	26	100	63.5	8.5	28	100	1
15. Rogaland	46	44	0.5	4	5.5	100	46	22	32	100	68.5	6.5	25	100	1
16. Middle Norway	64	14	12	-	10	100	58	28	14	100	22	4	74	100	-
17. Trøndelag	40.5	34	12	12.5	1	100	58	7.5	34.5	100	33	10	57	100	5
18. N Norway	47.5	33.5	-	19	-	100	38	9.5	52.5	100	38	33.5	28.5	100	-
19. Finland-Karelia	1.5	19.5	79	-	-	100	34	42.5	23.5	100	39.5	42	18.5	100	0.5
— 19ac. S Finland – Omega d.	-	96.5	3.5	-	-	100	-	22.5	77.5	100	77.5	15	7.5	100	-
— 19d. White Sea d.	2	-	98	-	-	100	42.5	47.5	10	100	29.5	49	21.5	100	1
Total	46	30.5	20	2	1.5	100	62	15.5	22.5	100	58	12.5	29.5	50	2

quency from one area to another. The elements are listed in tab. 11:4 with their relative frequencies throughout Northern Europe.

*Tab. 11:4. The relative frequencies of certain typological elements in North European rock-art ships.*

I	62%	C	20%
a	58%	II	15.5%
BC <sub>1</sub>	50%	2	15.5%
A	46%	b	12.5%
ADE <sub>1</sub>	32.5%	3	2%
B	30.5%	D	2%
c	29.5%	E	1.5%
III	22.5%		

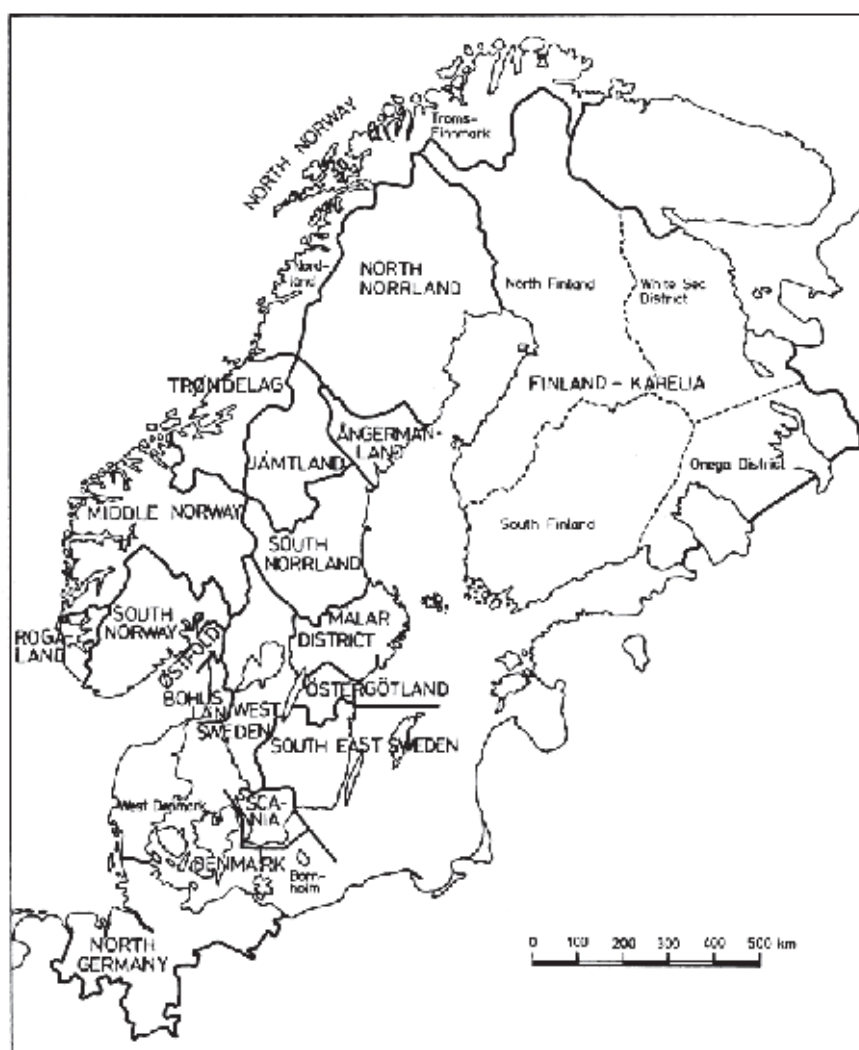
It can be seen that the elements which occur most frequently are indeed the four elements which comprise the hypothetical prototype (AIaI) of the classification system i.e. the Rørby–Wismar–Kivik type, although the elements occur in a different sequence, i.e. IaAI (I, ships with two double prows; a, ships with crew; A, double-line ships and I, ships without ribs).

The least frequent elements are E (ships with three or more horizontal lines), D (contoured ships), 3 (ships with ribs and other designs on the hull), b (ships with crew and other designs above the gunwale), 2 (ships with ribs) and II (ships with one single and one double prow). These features occur most frequently in those type combinations which on logical grounds are latest.

Thus it would seem that those elements which occur most frequently are the earliest, and those which occur least frequently are the latest. If this is indeed true, it may be explained by the greater diversification of the ship motif towards the end of the period in which it was used. This would cause each element then in fashion to occur less frequently.

Features of medium frequency are elements B (single-line ships), c (ships without crew), III (ships with two single prows) and C (ships with a hammered-out hull). By comparison with the least frequent elements, the features of medium frequency can be said to be simpler modifications of the elements of the hypothetical prototype. It seems possible at least that the elements of medium frequency originate somewhere near the middle of the period in which the ship motif was





*Fig. 11:6. The geographical areas of North European rock art.*

used. But the hypothetical chronological system outlined here clearly needs additional factual supporting evidence before it may be considered proven.

Tab. 11:3 shows that the frequency of the elements varies greatly from one area to another. For example, element A (double-line ships) has a frequency of only 11.5% in West Denmark, whereas in Rogaland it is as high as 46%, in Östergötland 55.5% and in Ångermanland 85%. On the other hand, element I (ships with two double prows) has a frequency of 90% in Scania, but only 1% in Ångermanland. Element B (single-line ships) has a frequency of 77% in West

Denmark and 52.5% in Østfold, as against only 6.5% in Östergötland. Element C (ships with a hammered-out hull) has a frequency of 35.5% in Östergötland and only 7.5% in West Denmark and 8% in Østfold.

The distribution seems confused and may convey the impression of an arbitrary pattern of frequencies which changes from one area to another at random. Is it, then, possible to trace the pattern of the innovation process of the ship motif?

In prehistoric Northern Europe new impulses normally spread northwards from the south, this tendency being particularly marked from the beginning of the Neolithic period. For this reason we shall first examine the possibility that the ship motifs in rock art spread in this way. The distribution pattern of what are probably the earliest fairly accurately dated ship representations (all of type Alal, i.e. Rørby, West Denmark; Wismar, North Germany and Kivik, Scania), would tend to support the theory of a spread of innovations from south to north.

Type A ship designs (double-line ships) have a frequency of only 11.5% in West Denmark (the natural innovation centre of the Scandinavian Bronze Age culture); this might be seen as an argument against the theory of an innovatory pattern moving north. A distribution pattern is often interpreted thus: the area where a type is best represented is the area where it originated – and in many cases this can be proved to be correct. But sometimes it is patently wrong, as for example in the case of the Corded Ware of the Battle Axe Cultures of the Middle Neolithic (Malmer 1962:760, tab. 91). In a creative centre (as Denmark has frequently been in the past) a type will not be produced for very long before new impulses (for example from the continent of Europe) cause the creation of new types. But peripheral regions (central and northern Scandinavia were often such regions during the Stone Age and Bronze Age) often lag behind to a considerable extent, so that types may be produced in such areas long after they had gone out of fashion in the central regions. In other words: the spread of the earliest of a series of culturally related types would follow an uninhibited innovation pattern, while the pattern of spread of subsequent types would be more inhibited. The result is that the earliest type is proportionately best represented in peripheral areas.

A preliminary examination seems to suggest that elements A–E, which determine the main design of the hull, vary regionally both from north to south and from east to west. Chorological relationships of this complexity can often best be illustrated in diagrammatic form. In figs 11:7–10 each area is represented by a horizontal bar, the length of which corresponds to 100%; the values are taken from tab. 11:3.

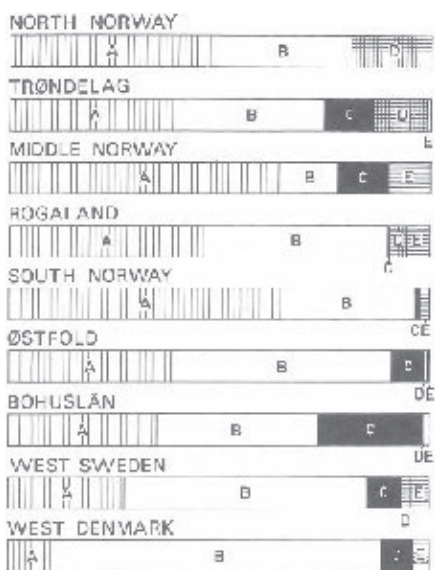


Fig. 11.7. The frequency of ship design types A–E in nine western areas.

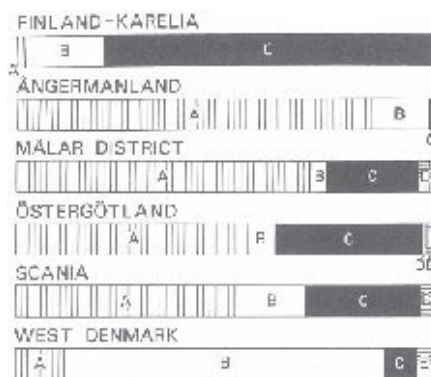


Fig. 11.8. The frequency of ship design types A–E in six eastern areas.



Fig. 11.9. The frequency of ship design types A–E in four eastern areas.

Fig. 11.7 shows nine western areas from West Denmark in the south to North Norway. By contrast fig. 11.8 shows six eastern areas, again beginning at the bottom with West Denmark – on the assumption that this is the innovation centre – and continuing with more northerly areas to finish with Ångermanland and Finland-Karelia at the top.

The two diagrams correspond in that the proportion of type A ships (double-line ships) by-and-large gradually increases from south to north. At the same time the diagrams differ in that ships of type B (single-line ships) are particularly strongly represented in the west, while ships of type C (ships with hammered-out hulls) are correspondingly strongly represented in the east. A third main tendency to emerge from the diagrams is the concentration of unusual ship types – i.e. type D (contoured ships) and type E (ships with three or more horizontal lines) – in Norway, especially in Rogaland and areas to the north.

An apparently confused image has become more explicit with the aid of the diagrams. The most obvious interpretation, as demonstrated by the earliest type

(type A), is that we are dealing with an innovation pattern, moving northwards from an area in the south; this diffusion process caused contrasting developments in the areas to the west (type B) and to the east (type C) at a slightly later stage. During the final stage, localised forms (types D and E) seem to have developed, especially in the north-west.

The increased proportion of ship designs of type A in areas to the north does not in itself justify the conclusion that the direction of the innovation trend was from the south towards the north; it could just as well have gone in the opposite direction. Some further indications, however, favour an innovation pattern in a northerly direction:

- 1) If the direction of the diffusion process was from north to south, we have to postulate a very large and scattered innovation centre, including North Norway and Trøndelag as well as the Mälär District (the role of Ångermanland being uncertain). West Denmark, on the other hand, represents an innovation centre of suitable size.
- 2) As the type A ship designs occur in West Denmark no later than the transition between Montelius' Periods I and II of the Bronze Age, an innovation pattern from north to south would imply that the designs originated very early in North Norway and the Mälär District.
- 3) Probably the earliest ship designs which can be dated with any certainty are found in the south: i.e. Rørby, Wismar and Kivik.
- 4) The innovation patterns of prehistoric farming cultures – and not least those of the Bronze Age – usually begin in the south, in West Denmark.
- 5) If the innovations started in North Norway and spread strongly southwards into Denmark, why then did the process not continue further south into North Germany? A movement from south to north better explains the southern boundary of the ship motif.

There are two exceptions to the general tendency for type A ships to be better represented in the northernmost of two adjacent areas; fig. 11:7 shows that Rogaland has a smaller proportion of type A ships than South Norway and that Trøndelag has a smaller proportion of these designs than Middle Norway. These irregularities may be most readily explained if we postulate that Rogaland and Trøndelag acted as secondary innovation centres which received impulses from West Denmark before any other areas; from these centres there was a subsequent diffusion into the larger and poorer areas of South Norway, Middle Norway and North Norway. The same argument also applies to Østfold.

Type C seems to have a more local distribution than both types A and B, and is confined mainly to Scania, Östergötland and the Mälär District. Fig. 11:10,



*Fig. 11:10. The frequency of ship design types A–E in three south-eastern areas.*

which shows the situation in South-East Sweden and Bornholm, confirms this interpretation; here types A and B are again predominant.

The reason why the area of South-East Sweden is seen in conjunction with Bornholm is the fact that the rock-engravings of the former area are concentrated along its south-eastern periphery – thus Blekinge has 34 ship designs and Gotland 23. Only one ship design occurs on Öland and 8 in Småland, through which presumably lay the natural route connecting Scania and Östergötland.

The quantities in which the sub-types are found can presumably also be used for chronological studies. In West Denmark, for instance, there are 11.5% type A ships of only one type (A1a1); in Rogaland there are 46% type A ships and 17 types (8 of type A1, 5 of type A2 and 4 of type A3); and in Ångermanland there are 85% type A ships and 13 types (3 of type A1, 2 of type A2 and 8 of type A3). The figures indicate the improbability of type A ships being produced during the same length of time in the three areas. This would suggest rather that type A ships originated more or less simultaneously in West Denmark and Rogaland, but the motif was discontinued in the former area earlier than in the latter. Type A ships were presumably introduced into Ångermanland later than in Rogaland but probably continued to be used there for a longer period. This may be deduced from the fact that most type A ships found in this area are of a form far removed from the postulated prototype A1a1.

The recognition of an innovation pattern from south to north, and of a contrast between western and eastern areas of Scandinavia, has so far been based on a study of elements A–E only: that is those concerned with horizontal lines. The conclusions would carry more weight if they were based on comparisons between all the seventy-eight types involved. The graphic representation of such large numbers of type frequencies within a number of geographical areas is technically complicated; however, the kind of cumulative diagram which has been extensively used in recent years in the study of Stone Age settlement sites, may perhaps prove useful.

Fig. 11:11 illustrates how this kind of diagram is constructed: 43 A-, B- and C-ship types represented in the corpus are set off along the horizontal axis with an additional space for types D and E respectively. The 35 D- and E-ship types have not been marked separately, as their combined percentages are so low that the

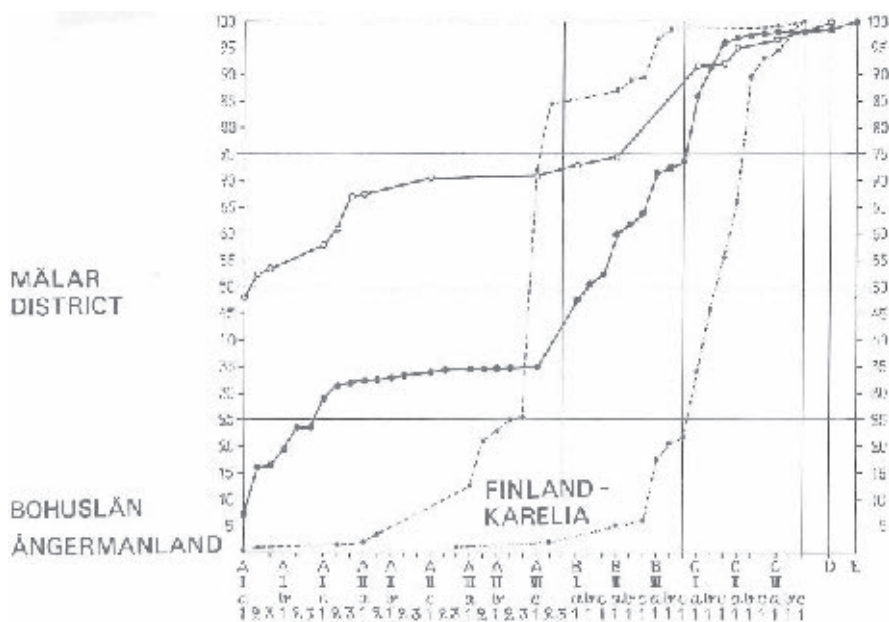


Fig. II:11. Cumulative diagram of the ship design types.

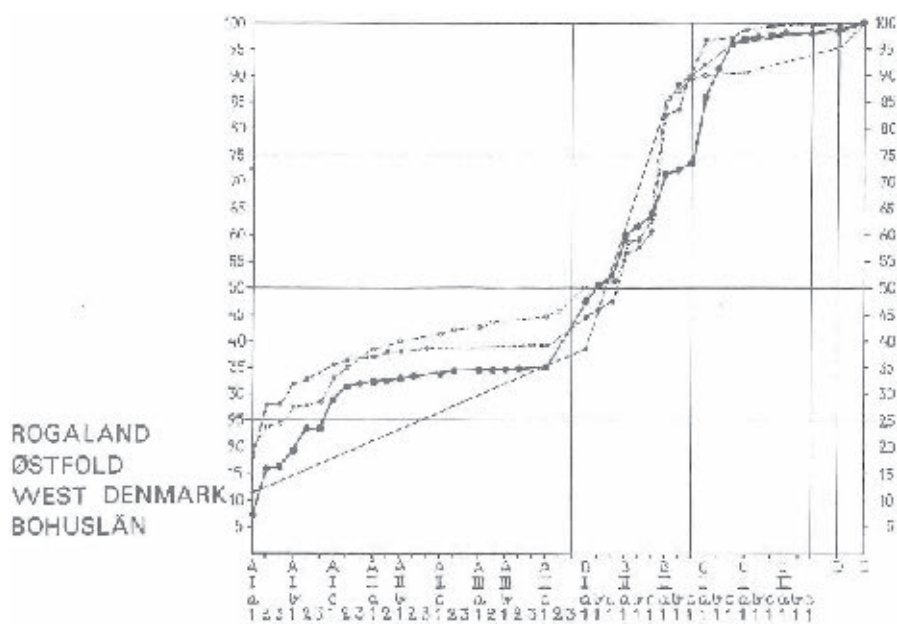


Fig. II:12. Cumulative diagram of the frequency of ship design types in four western areas.

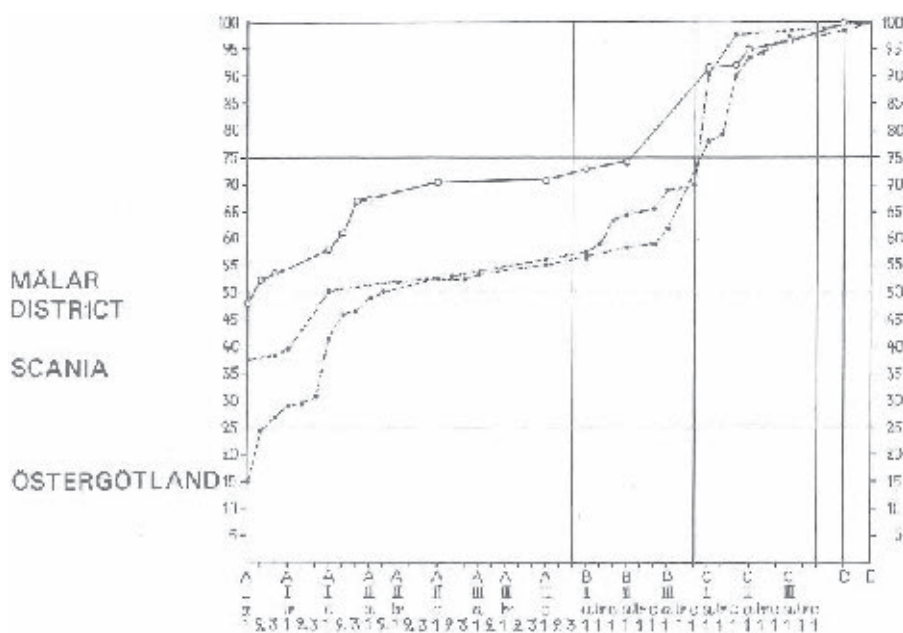


Fig. 11:13. Cumulative diagram of the frequency of ship design types in three eastern areas.

curve would appear virtually horizontal and without any information value, while the whole diagram would be almost twice as big, thus making it more difficult to read.

The percentages are marked off on the vertical axis. Each area is represented by a curve, and the types are marked as points on the curve; the data are based on tab. 11:1a, b. The curves are cumulative: this means that values for each individual type are added together. In Bohuslän, for instance, the earliest type is A1a1 with a frequency of 7%; the next type is A1a2 with 9% which is thus plotted at the position of 16%; next is type A1a3 with 0.5% which is plotted at 16.5%, and so on.

Fig. 11:11 demonstrates that cumulative diagrams of this type can produce visually striking results. The diagram illustrates four geographically distant areas, Bohuslän, the Mälardistrict, Ångermanland and Finland-Karelia, and the curves show conspicuous differences.

Fig. 11:12 illustrates the cumulative frequency distributions of the four richest and best-published western areas, West Denmark, Bohuslän, Østfold and Rogaland. When compared with fig. 11:11, it may be seen that the western areas are indeed a closely-knit unit. The section illustrating the distribution of type A



ship designs shows clearly that the proportion of type A ships increases gradually in areas further north. The curves representing Østfold and Rogaland demonstrate that the former has more ships of type AI, while the latter area has the larger proportion of type A ships when the three types I–III are all included in the comparison.

Fig. 11:13 shows a similar diagram of the richest and best-published areas of the eastern diffusion route: Scania, Östergötland and the Mälär District. The diagram of the eastern areas shows slightly greater differences than that of the western areas, but it nonetheless represents a cohesive unit. The greater differences between the curves in fig. 11:13 is due to the fact that the process, which causes the percentages of type A ships to increase in more northerly areas, becomes more pronounced along the diffusion route in the east than in the west. As far as type A ships are concerned, the relationship between Scania and Östergötland is similar to that observed between Østfold and Rogaland; thus there are more ships of AI design in Scania, but a balance between the two areas is achieved when types AII and AIII are also taken into account.

The facts discussed so far make it possible to outline the main features of the innovation pattern of ship designs. The motif originated in West Denmark at a time approximately indicated by the curved sword from Rørby, i.e. Period I (or at least at a time before the introduction of the Scandinavian spiral ornament); its original form was the double-line ship of type Alal. From West Denmark the ship motif spread fan-wise to other parts of Northern Europe; it is, however, possible to distinguish three main routes of diffusion, a western, an eastern and a south-eastern branch. A common feature of all three routes of diffusion is a strong element of single-line ships (type B). The fact that the proportion of type A ships progressively increases northwards along both the eastern and the western routes would suggest that type B ships also originated in the south, and that their northward spread was inhibited to some extent. There are many type B ships in West Denmark and the origin of the type must be considered to lie in this area. Ships with hammered-out hulls (type C) are peculiar to the eastern branch which consists of the eastern regions of the Swedish mainland: it is unlikely that type C ships originated in West Denmark, as the type is very unevenly distributed along the three routes and only two ships of this type have been found in West Denmark itself. The contoured ships (type D) and ships with three or more horizontal lines on the hull (type E) are peculiar to the Norwegian areas of the western route. It is also unlikely that these types were developed from Danish rock-engravings, but it is possible that they may have their origin in decorated bronze objects of the Danish Late Bronze Age.

## 2. Summary and final discussion<sup>1</sup>

### 2.1. *Point of departure*

The most important aspect of rock art is its *meaning*. All other kinds of archaeological material have at least *some* practical purpose. The settlement serves the living, the graves the dead. The tool must be able to perform its practical task, the weapon be fit for hunting or battle. The field provides subsistence, the ship transport. All artefacts and monuments have been influenced to some degree by ideas of status, solidarity within the community, beauty, magic or religion. We may approach these aspects of the material by making simple calculations regarding the size of the artefact, the scarcity of the material or the quality of the work. In the main, such aspects fail us in rock art. This is a *message* from the prehistoric artist (and/or his patron) to mankind (including ourselves) or to certain particular people: in both cases probably at the same time also to higher powers.

We will never be able completely to understand the meaning of rock art. *If* it were possible to interpret its significance, it is beyond doubt that this could be expressed in words. Rather than speaking of *rock art* we should say *rock language*. The aesthetic aspect is more an expression of our relationship to rock art than our relationship to prehistoric man.

Even if one did not understand a word of a certain language, it could still be used for cultural and social studies. It is not necessary to take so obvious an example as hieroglyphics: without being deciphered they would still provide an exceptionally clear conception of the extent and intensity of ancient Egyptian culture. We may equally well take Hungarian as an example: without any interpretation or understanding of the language, dated texts in Hungarian would still convey to us something important about cultural and social conditions in the Danube basin. Such chorological studies of a (postulated) dead language would actually tell us more than comparable studies of, say, Spondylus shells or Roman *denarii* (that is not to say that these do not deserve study). Language is a more genuine expression of people and their society than any imported objects that may be found.

An axe, or any kind of artefact, can be interpreted in isolation, because its form and function are so simple that it can be understood by reference to the modern observer's own experience. It can of course be better understood if all examples of a type can be studied, providing information about the full range of shapes, sizes and contexts. This complete picture is *necessary* in the study of

1 The following summarizes all of Malmer's 1981 book; see Section VII (SW)

so complex a phenomenon as rock art. An isolated rock surface covered with engravings inspires in the modern observer so many associations of ideas that he finds it difficult to distinguish matters of primary and secondary importance (apart from the strong possibility that his ideas are inadequate and that he has missed the central point of the engraving's significance).

The present study is based on the conviction that in order to interpret rock art it is necessary to survey the whole body of material and its variations in space, time and context. The exposition concentrates on the chorological aspect because a large amount of unexploited material was available. A number of regional collections of material have been published which bear witness to considerable diligence and application, covering at least the majority of the North European rock art areas. There has been less interest in making a survey of the whole of this area: it is the aim of the present study to try and fill this gap to some extent.

## 2.2. *Results*

The differences between the geographical areas with regard to the quantity and quality of rock art motifs are not arbitrary: seen as a whole they show that the innovation process of the farming rock-engravings began in the south, in Denmark and Scania, whence it gradually reached the northernmost part of the area under consideration, Troms-Finnmark. It is possible that these impulses also reached Karelia, but this is difficult to prove because the Scandinavian and Karelian areas which have rock-engravings are separated by the quite distinct rock-painting area of Finland.

It is likely that the innovation centre for the hunting rock-engravings lay in Norwegian Nordland. In any case, the innovation of farming rock-engravings encountered, as it spread northwards, an existing tradition of hunting rock-engravings in a wide zone, and primarily in Nämforsen<sup>2</sup>, the Mälär District, Östergötland and Bohuslän (possibly also Karelia). Seen in the context of Europe as a whole, the exceptional vitality of the rock art of Bohuslän, Östergötland and the Mälär District, as well as of Nämforsen, may be explained by this combination of two traditions of rock-engraving.

In Norway, areas rich in farming rock-engravings are generally also the areas with the best agricultural land: Østfold, Rogaland and Trøndelag. Of the best agricultural areas in South Scandinavia, only the Mälär District and Östergötland are very rich in farming rock-engravings. North Germany, West Denmark,

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2 A major rock-carving site on River Ångermanälven in Ångermanland with hundreds of boats and elks (SW)

Scania, Halland and Västergötland have remarkably few such rock-engravings, considering the extent of good farming land and the substantial quantities of bronze which have been recovered from these areas. The comparative lack of interest in farming rock-engravings in so rich an agricultural area is a phenomenon of the south-west. In other words, rock art is peripheral to the economic and material centre of Bronze Age culture.

The south-north innovation pattern of the farming rock-engravings varies with regard to the different motifs, and indeed also to the various types within the motifs. However, a contrast between east and west is a constant feature. In a number of motifs (ships, human figures and animals) a broad, hammered-out style is characteristic of the eastern areas, and a thin single-line style of the west. This contrast may be due to more important lines of communication running north-south by comparison with those running east-west. We can therefore distinguish two routes along which the innovations emanating from West Denmark spread. The principal areas of the eastern route are Östergötland and the Mälär District, and at a later stage also Ångermanland. The principal areas of the western route are Bohuslän, Østfold, Rogaland and Trøndelag. The position of Scania is somewhat ambivalent, as it is variously linked with both the eastern and the western routes. By comparison with West Denmark, the primary centre of diffusion, Scania always appears to be clearly of the east, while Denmark itself has far stronger affinities with the western areas. The islands of Bornholm, Öland and Gotland may be regarded as comprising a separate diffusion route, which in some respects included Blekinge and Småland (that is, the whole of Småland except Sagaholm<sup>3</sup> in the north, which is linked with Östergötland). North Germany may belong to the primary diffusion centre in West Denmark, but appears in some respects to be secondary to it.

Another chorological grouping which partly coincides with the routes here identified is of a more static character. In the central area of West Denmark, most interest is shown in abstract and symbolic designs: circular designs, hands and feet. Immediately to the east of West Denmark, mainly in Bohuslän, but also in Østfold, West Sweden and Scania, there is a pronounced interest in scenes: the human figure in action – acrobats for example – ships' crews drawn in full, carts, ards and hunting scenes. This may be explained by postulating that cultic ceremonies performed in West Denmark, and well-known in these areas, were here portrayed in stone as there was less opportunity to practise them in real life. In an outer zone around West Denmark (comprising principally Östergötland and the Mälär District, but also to some extent Scania) scenes are less common, but full-scale representations of weapons and clothing

3 A barrow with engraved stone slabs around its periphery (SW)

do occur. One may hypothesise that the myths and cultic ceremonies of the central Danish areas were less well known in these eastern areas; instead we find portrayals of the sacrifices which were deposited in kind in West Denmark and the immediate vicinity, and Scania in particular.

A third chorological grouping has been distinguished: there are further innovation centres other than West Denmark, in particular Scania, Östergötland, Bohuslän, Østfold, Rogaland and Trøndelag. The intermediate, larger areas are secondary, receiving innovations later and apparently by way of these other innovation centres. Typical of areas with a secondary diffusion pattern for farming rock-engravings are South-East Sweden, West Sweden, South Norway, Middle Norway and North Norway. Nordland, which is included in the last-mentioned area, is, however, probably the innovation centre for hunting rock-engravings.

Knowledge of rock art motifs did of course spread mainly through personal contact. We may assume that bronze sculptures like the Trundholm wagon and the Fårdal group contributed to this, along with engraved designs on razors such as ships and related motifs. This is how we must interpret also the beginnings of farming rock-engravings in Sweden; imported designs from the south must have played a part. The design of a chariot on stone no. 7 at Kivik must ultimately be Mediterranean in origin. The imported designs may not have been in bronze – textiles are another possible medium, suggested by the rectangular frames around the designs on the Kivik stones. These frames seem unnecessary, as the carefully dressed edges of the rectangular stone would seem to provide satisfactory frames. They can, however, be interpreted as the borders of cloth with woven, applied or embroidered pictures, cloth which may have decorated the walls of a room or grave. Indeed, there appears to be a rudimentary frame of this kind on one of the stones at Sagaholm, which is closely related to the Kivik cist. No imported textiles have been found from the Scandinavian Bronze Age. However, the clothing which was found in the oak-log graves can best be explained as of Mediterranean inspiration: the cloak, tunic and the cap, which seems to be designed to protect against excessive heat rather than cold.

### *2.3. Economic aspects*

Rock art does not of course simply follow on from the presence of imported designs. The North European farming rock-engravings are as much an independent Scandinavian creation as Scandinavian Bronze Age culture as a whole. The unique quality of this culture within the European Bronze Age cultures has perhaps not been sufficiently emphasised in the literature. Unlike the Continent and the British Isles, Bronze Age Scandinavia had no exploitable resources

of copper, tin, zinc or gold. This must have resulted in certain peculiarly Scandinavian economic and social phenomena: more than elsewhere in Europe, the manufacture and use of gold and bronze objects was the prerogative of land-owners in the richest agricultural areas, mainly Denmark and Scania and, although relatively less rich, Östergötland, the Mälars District, Østfold, Rogaland and Trøndelag.

It seems natural to view rock art motifs in this light. Features signifying wealth and status are emphasised: axes, swords, spears, and bronze shields; shoes, tunics and cloaks of a kind which could not have been owned by the common people. Carts were portrayed, and wheel-crosses representing the most extraordinary feature of these carts: the wheels themselves. Also horses, but not cattle or sheep, which must have been of great importance to the economy of the common people: only horses, with their considerable status value. Ploughing, which must also have been of immediate interest to most people, was rarely shown, and was always accompanied by magic and ritual features. Ceremonies were portrayed, processions with axes or shields, according well with the general pattern. The foot designs may indeed represent an invisible god, although the evidence for this is taken from far distant times and places (Almgren 1962). The very concept of an *invisible* god seems a little strange in the context of the robust symbols of the farming rock-engravings. And the variable sizes of the engraved human figures could rather lead to the hypothesis that some of them were gods. An alternative hypothesis interprets the foot designs as a symbol of the self, the human presence and perhaps the right of ownership (Kjellén & Hyenstrand 1977). It is generally possible to see the rock-engravings as territorial markers, indicative of the rights of possession: this has often been suggested in connection with grave mounds and cairns.

There is no doubt that possible prototypes for the Scandinavian rock art ship may be found in the Mediterranean (cf. Kjellén & Hyenstrand 1977:63, fig. 51), but why then was this motif adopted in Scandinavia? It is of course possible that the ship symbolises an aniconic god, but it could symbolise almost anything. The movement of the sun across the sky, the journey of the dead to another world, water, power over the water, cooperation: the possibilities are almost inexhaustible. Such interpretations are valid at all times and in all countries; specific to the Scandinavian Bronze Age is the fact that all bronze would have to be imported and that all imports came by boat. Starting with this fact of fundamental importance to Scandinavian Bronze Age culture, the ship may subsequently have taken on any other symbolic significance from the numerous possibilities it offers.

The all-important problem which eventually arises from almost any inquiry

into the Scandinavian Bronze Age is this: how was the imported metal paid for? The fact that most bronze is found in the best agricultural areas must not lead one to the conclusion that the metal was paid for with agricultural produce. Such exports would be unlikely, given that the countries exporting the metal were much stronger agriculturally than Scandinavia.

One possibility is that the export was of furs. It can be maintained that such exports would only have been of marginal importance. Against this it can be argued that the importation of bronze and the bronze trade was also of secondary importance by comparison with the predominant farming economy. Bronzes, not furs, have been preserved to the present day; perhaps this is the only difference.

This hypothesis is supported by the evidence of a South Scandinavian Bronze Age culture in the northern part of Fennoscandia where the fur trade must have had its origins: there are Bronze Age cairns along the coasts of Swedish Norrland (Baudou 1968) and Finland (Meinander 1954). Considerable numbers of South Scandinavian bronzes have been found in Finland, even as far away as Savolaks and south Karelia, not least the hoard in Sodankylä in Finnish Lappland which contains four South Scandinavian bronze swords (Meinander 1954:210–230, Abb. 37, Taf. 4c, 9h, 16). Along with this evidence of South Scandinavian Bronze Age culture in the north, one must also consider the significant South Scandinavian features of the Nämforsen rock-engravings. Nämforsen would have been suitable in every respect as a meeting place for the hunters of the north and the southern traders. The fact that the profits from this trade, the bronzes, remained in areas with a strong economy – Denmark and Scania – is not exceptional in economic history: indeed, it is typical.

A few more words may be added on the chorological variations of motifs.

When the ship motif first appears it is highly stylised (A1a1): this does not have the appearance of an artist's first attempts at portraying a ship. It is more likely to be a copy of an imported ship motif (and an imported concept). The ship motif then develops in two directions: many unrelated ornaments and decorations are added, which may indicate that its symbolic significance remained unchanged, while real ships were uncommon in the area; in other areas the ship design becomes more realistic, which may indicate that it took on some other symbolic significance. The realistic ship designs belong to areas in the north: the Mälardistrict, Nämforsen and Karelia. It seems reasonable to suppose that the boats portrayed at Nämforsen were in fact those in use on the river.

The frequency of the ship designs is not unconnected with the importance of sea transport in the various areas. Of 72 ship designs from Denmark, 46 are from Bornholm and 22 from the western islands, while Jutland, being part of



the continental land mass, has only 4, although it forms two thirds of Denmark's surface area; there are none in North Germany. Rogaland is one of the vigorous innovation centres in Northern Europe: situated at Norway's south-westernmost point, it depends almost entirely on sea transport for its imports. In Rogaland the ship designs comprise 72% of the total number of figures, the largest percentage of ship designs recorded in any area.

The two-wheeled chariot is a natural symbol of the warrior and ruling classes. It is doubtful that real chariots were ever seen in Northern Europe during the Bronze Age; it is certainly clear that the earliest, and definitely the best executed, variant of this motif was probably a copy of an imported design. The fact that real carts did not occur, or were at least rare, outside Denmark explains why the cart motif never became popular. It also provides an explanation for its probable transformation into a ploughing scene, an image all could understand. With this transformation the symbolic significance also substantially changes, presumably from the concept of power – wealth – happiness to fertility – wealth – happiness. It is above all in the rock art area of Bohuslän that the creative power to transform and change motifs is found. One can point to its position between four rich agricultural areas, Denmark, Västergötland, Halland and Østfold, and to the ready availability of suitable rock surfaces, but the ultimate reason is irrational; as irrational as the presence of two thirds of all Swedish passage-graves in the Cambro-Silurian area in Västergötland, an area representing only 0.3% of Sweden's total area. It just happened that way.

The circular designs in Denmark are usually a circle with four radii; this was perhaps an example of *pars pro toto*, the wheels of the chariot instead of the whole vehicle. In northern areas, where circular designs take on a large variety of different and irregular shapes, there is perhaps a shift in its significance, from representing the warrior and ruling class to a more general protective and lucky sign.

The only animal portrayed in the Early Bronze Age in Scania (and Denmark) is the horse, which naturally can be interpreted as the symbol of a warrior and ruling class. As the animal motif spread northwards many different species of hunted animals began to be portrayed, and again we can probably note a shift in significance, from the concepts of power and wealth to fertility and subsistence.

#### 2.4. *Approaches to interpretation*

A distinction may be made between *absolute interpretation* and *relative interpretation*. Although never found in a pure form, these two concepts can be seen as polarising present scholarship on the meaning of rock art.

*Absolute* interpretations look immediately to the central significance of the symbol. If in Buddhist India at the beginning of our era a pair of foot soles represent the divinity, the motif is given the same interpretation in Scandinavia a thousand years earlier. If a ship represents the actual divinity among Tacitus' *Suebi* and also in a carnival in Flanders in the year AD 1133, the meaning should have been the same in the Scandinavian Bronze Age (Almgren 1962). For absolute interpretations, chronological and chorological facts are secondary. It is believed that a fundamental significance has been discerned, broadly human and universal, valid at least for a large span in time and space. Other possible interpretations of the significance of, for instance, the foot motif (Mandt 1972:131), are rejected implicitly as of secondary or negligible importance.

*Relative* interpretations reject the possibility of arriving immediately at an understanding of the central symbolic significance of a motif, either by means of intuition or on the basis of material compiled from the realms of ethnography and religious history. The material available for study according to this persuasion is first of all the *variations* of a motif, both geographical and chronological. The major characteristics of a motif are illuminated through its variations and enable us to pronounce with some certainty on the objects represented, and indeed to decide whether or not a representation was in fact intended. It is through these variations and the manner in which an object is portrayed that we may learn something of the ideas with which it was associated.

Interpretations within the spheres of myths and gods, cult and religion lie outside the scope of this study. But our final conclusion must be that future research on Scandinavian rock art and its meaning both could and should concentrate in the first instance on the contemporary cultural context in which it was produced.

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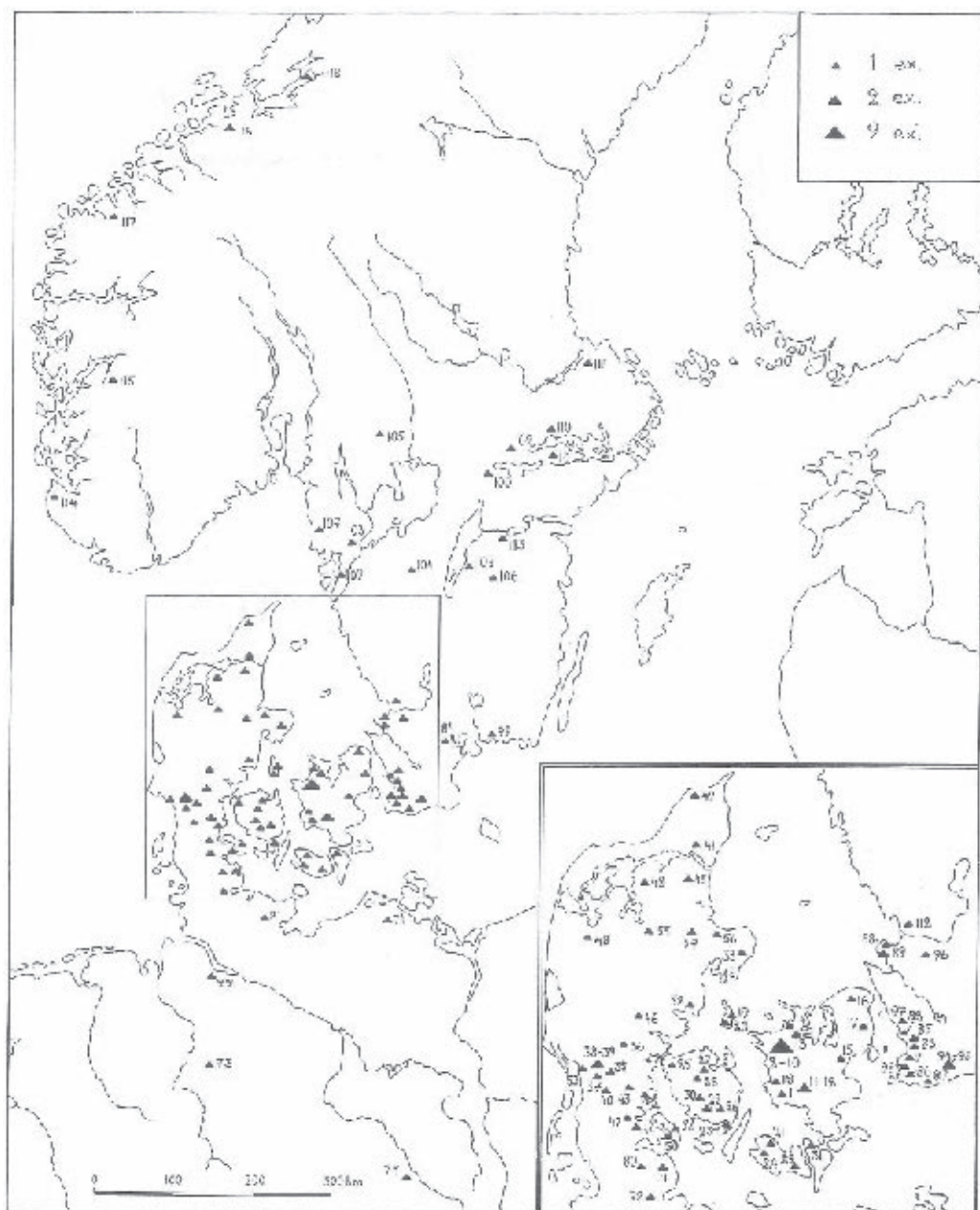
# The metrology and chorology of Fårdrup axes. A preliminary report

1989

ALTHOUGH THIS REPORT must be preliminary, it is based on studies going back to the years around 1970. The collection of material ended in 1972, by which time it had yielded 118 Fårdrup axes from Scandinavia and Germany, which ought to be all or virtually all the axes known then. It has not been possible to consider later finds in this report. Of these 118 axes, I have personally seen, drawn, and otherwise documented 111.

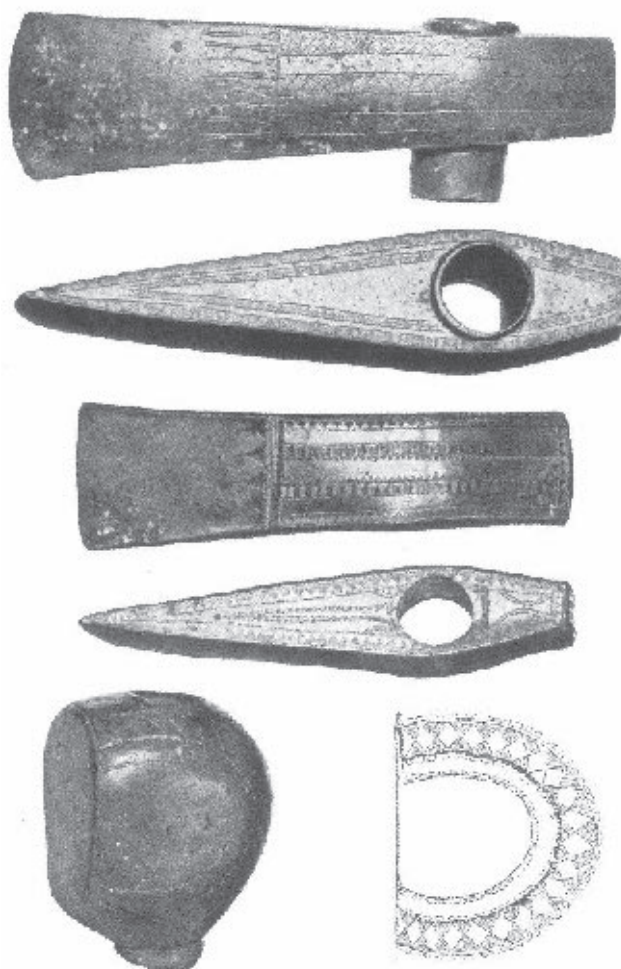
In many researchers' eyes it may seem provocatively old-fashioned to study a single form of artefact. In my defence I would say that the Fårdrup axes are no ordinary artefacts; on the contrary, this is what I would call a *supreme* form. By this new term, a supreme form or type, I mean a form of artefact which is alone and sovereign in its function throughout its time of production, and thus has no contemporary competitors. It is easy to cite examples of competing types. During the Migration Period there are many different forms of relief brooches and equal-armed brooches. During the Viking Age there are many different types of spearheads. A supreme form, by contrast, is the battle axe of the Corded Ware Cultures, especially in the oldest Continental European form. But of all the Nordic types, the Fårdrup axe is one of the most typical instances of a supreme form. There is no shaft-hole axe of bronze contemporary with the Fårdrup axe, and it shows less variation than artefact types usually have. My hypothesis is that a supreme type like the Fårdrup axe is a genuine expression of something central in the essence of the culture.

The map in fig. 12:1 includes all the Fårdrup axes of recorded provenance known to me. It is difficult to imagine a more harmonious distribution picture during the Nordic Bronze Age. The distribution is of exactly the kind that a supreme type ought to have. The centre of gravity is clearly in Sjælland with western Skåne and Fyn with eastern Jutland. The type is sparsely distributed as far north as Ytterøy in Nord-Trøndelag (no. 118 on the map) and Tierp (no. 111) in Uppland. The southernmost example comes from Löbschütz (no. 75) in Saxony. Skåne and Jutland each have one find with two axes: Skurup (nos 94–95 on the map, Oldeberg 1974, no. 719) and Bækbølling, Føvling Parish (nos 38–39,



*Fig. 12:1. Distribution of Fårdrup axes.*

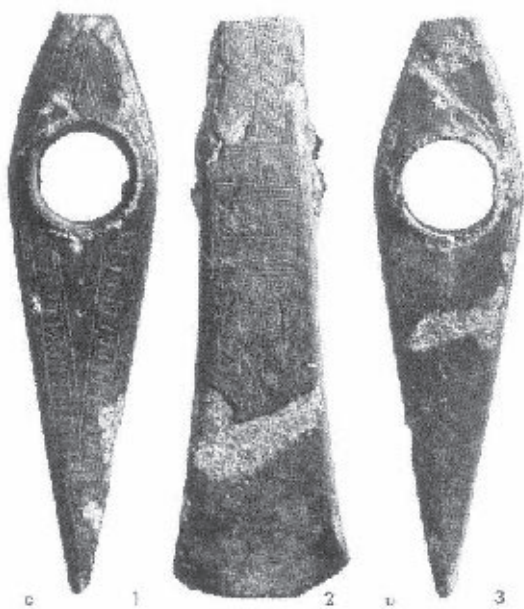
*Fig. 12:2. The Fårdrup  
hoard: Tranderupgård,  
Fårdrup Parish, Sjæl-  
land (after Broholm  
1944, Pl. 6).*



Broholm 1943:210). But Sjælland is the clear leader: this is the site of the eponymous Fårdrup find itself (nos 11–12), which besides the two axes contained a mace head (fig. 12:2; cf. Broholm 1952, nos. 15–17), and the huge Bregninge find (nos. 2–10) with nine axes (NM Copenhagen B. 3092–96). Ten kilometres from Bregninge is Rørby, with the two scimitars. A further two, perhaps somewhat uncertain, hoard finds contain, besides the Fårdrup axe, a flanged axe, namely, the finds from Tierp (no. 111) in Uppland and Vikum (no. 55) in Jutland. Altogether there are thus six hoard finds with Fårdrup axes. But there is little doubt that all the single finds of Fårdrup axes can be regarded as similar deposits; the bog patina and numerous find circumstances point unanimously in that direction. Fårdrup axes thus seem to occur only in deposits, and this is important.

The casting technique displays primitive features. There are often bubbles in





*Fig. 12:3. Axe from Fellingsbro churchyard, Västmanland (no. 102).*

the metal, and this porosity which is so typical of the early Nordic Bronze Age stands out with particular clarity when the patina has been removed from the axe in a heavy-handed way (e.g. NM Copenhagen B. 1383, found “close to the southern boundary of North Jutland”). Occasionally it happened that the axe was cast in a two-part mould, as we can see from the burr on the undecorated underside of the axe from Fellingsbro (no. 102) in Västmanland (fig. 12:3; cf. Montelius 1917, no. 811). The undersides of the axes are usually undecorated, even on the elegant Fårdrup axe itself. On the smaller axe in the find, its undecorated underside has some concavities, the meaning of which can be understood by examining, for example, the axe from Ringgive (no. 46, NM Copenhagen 2868) in Jutland, for here the entire underside is slightly bumpy. The explanation must be that the axes were normally cast in an open mould.

The Fårdrup find is, of course, of very high class, but as Brøndsted observed long ago (1931:III), the objects display differences in quality. The best execution of the ornamentation is seen on the small axe; the decoration is poorer on the big axe, and it is coarse and rather unattractive on the mace head, which also has a clumsy shape. Brøndsted (1931:113) assumes, no doubt correctly, that the mace head was cast in an open mould and that its flat side represents the surface of the molten metal. The question is, however, *why* the casting was done in an open mould. Was it only because this technique was found easier, or were there other reasons? What speaks against the first assumption is that the diameter of



the mace-head is bigger than that of the flat axe. The mould was nevertheless closed in the sense that it was necessary to smash it to take out the mace-head.

*Tab. 12:1. Weight relations of the Fårdrup find.*

	Weight (g)	Percentage of the weight of the big axe
Fårdrup, small axe (no. 11)	1547	49%
Fårdrup, big axe (no. 12)	3170	100%
Fårdrup, mace head	3242	102%
Fellingsbro (no. 102)	1015	32%

The big axe in the Fårdrup find has a shaft-socket, which is highly unusual. Shaft-sockets and shaft-holes form an acute angle against the underside of the axe, exactly as is the case for many other prehistoric axes meant for practical use. What indicates that the Fårdrup axes were not used much for chopping with, however, is their weight. A modern firewood axe, with shaft and everything, weighs less than a kilogramme, while the big Fårdrup axe weighs more than three times as much.

Tab. 12:1 shows the exact weights of the three objects in the Fårdrup find. What is striking is that the big axe and the mace-head weigh almost the same, although they differ so much in form. Moreover, the weight of the small axe is almost exactly half that of the mace-head or the big axe. The hypothesis that automatically comes to mind is that the three objects were made according to a uniform weight system. They were made in the same workshop circle – or on the same magnate's farm – or however one wishes to imagine it, but on different occasions, since the artistic quality, especially as regards the decoration, varies. The hypothesis of deliberate weight norms provides a reasonable explanation for why most Fårdrup axes were cast in open moulds. If the correct weight is more important than the form, it is much easier to weigh the correct amount of scrap metal, melt it and pour it into the open mould, than it would be to calculate and shape a closed mould containing the right weight.

The "right weight" in this case evidently cannot mean a weight that gives the axes the greatest possible practical utility, because most Fårdrup axes have a weight that far exceeds the optimum weight in practice. Moreover, the Fårdrup axes distinguish themselves from all other Bronze Age axes through the impractical consumption of metal. The endeavour to give the axes certain specific weights must instead be due to a clear perception of the value of the metal. One could venture the hypothesis that one of the most important functions of the

Fårdrup axes was to represent certain specific economic values. And that other bronze objects could also have that function is indicated by the mace-head in the Fårdrup find.

The axe from Fellingsbro in Västmanland (fig. 12:3) is one of the few which, like the big axe in the Fårdrup find, has a shaft-socket, now partly broken off. The decoration of the Fellingsbro axe is similar to that in the Fårdrup find. The weight is 1,015 g (tab. 12:1), which is 32% of the weight of the Fårdrup axe. If one envisages restoring the weight lost as a result of the damaged shaft-socket, the axe may have weighed exactly one third of the weight of the Fårdrup axe, which ought to be deliberate. As mentioned above, the Fellingsbro axe has a burr, which is very unusual. A two-part mould was thus used, and the method for achieving the right weight must in this case have been to model the mould using a finished axe.

The weighing of all the Fårdrup axes, which will be presented partially in the following, shows that there was no single weight system in use throughout Scandinavia. There were instead a number of local weight systems, which can sometimes be more or less exactly determined as regards geography, and which can hypothetically be associated with local rulers.

*Tab. 12:2. Geographical distribution of the Fårdrup axes.*

	Number of axes			Percentage
	Decorated	Undecorated	Total	decorated
Sjælland	9	11	20	45
Fyn and the southern islands	6	5	11	55
Jutland	15	11	26	58
Denmark unprovenanced	9	4	13	69
Denmark total	39	31	70	56
Germany	7	4	11	64
Skåne	7	10	17	41
Rest of Sweden	12	3	15	80
Sweden total	19	13	32	59
Norway	1	4	5	20
<b>Total</b>	<b>66</b>	<b>52</b>	<b>118</b>	<b>56</b>

The Fellingsbro axe (no. 102) is linked to the big axe in the Fårdrup find (no. 12) by three typological elements: the shaft-socket, the decoration, and the weight system. These three elements are wholly independent of each other, proving that the two axes are more closely related to each other than Fårdrup axes in

general. The most probable hypothesis must be that the Fellingsbro axe was made in the same workshop circle as the Fårdrup find, that is to say, in Sjælland, and was then transported the 750 km to Västmanland.

Tab. 12:2 shows the geographical distribution of Fårdrup axes. Denmark has 70 axes, two thirds of the total. Germany has 11 axes. Skåne has 17, more than the rest of Sweden together, and Norway has 5. As for the number of decorated axes, it should be noted that the decoration is often very thin and the carving shallow, and in many cases, with the right lighting, I have discovered decoration on axes which are described in the literature and museum catalogues as undecorated. Tab. 12:3 shows the areas ordered according to the descending frequency of decorated axes. The general tendency is clear: the frequency of decoration decreases steadily the further north one comes. The only exception is Sweden north of Skåne, which has the highest frequency of decoration of all the areas, 80%. My hypothesis is that all axes from Svealand and Götaland north of Skåne were imported from the south, from Denmark/Germany.

*Tab. 12:3. Percentage of decorated Fårdrup axes.*

Sweden except Skåne	80
Germany	64
Jutland	58
Fyn and the southern islands	55
Sjælland	45
Skåne	41
Norway	20

*Tab. 12:4. The weight of the Fårdrup axes in the different geographical areas.*

	Total weight (kg)		Average weight (g)	
Sjælland	34.8		1740	
Fyn and the southern islands	14.8		1345	
Jutland	35.2		1353	
Denmark unprovenanced	16.7		1284	
Denmark total		101.5		1450
Germany		17.3		1577
Skåne	21.9		1286	
Rest of Sweden	21.8		1456	
Sweden total		43.7		1378
Norway		5.3		1067
<b>Total</b>		<b>167.8</b>		<b>1422</b>

Tab. 12:4 shows the weight. Many axes have so much patina and damage that the original weight cannot be determined. Germany has the highest average weight, followed by Denmark, Sweden, and Norway. Sweden north of Skåne has a higher average weight than Skåne, and my hypothesis is unchanged: these northern axes were imported from Denmark/Germany.

Fig. 12:4 sums up the decoration of the Fårdrup axes. The “fish motif” is a designation I have chosen in a perhaps misguided attempt at objectivity. The most probable interpretation of the motif is that it depicts a dagger or a sword (Malmer 1970:190). The motif occurs, for instance, on the small axe from the Fårdrup find (fig. 12:2). The same motif occurs with the same placing on coeval or slightly older Hungarian battle axes, and in my opinion the decoration of the Fårdrup axes comes wholly from Siebenbürgen/Hungary.

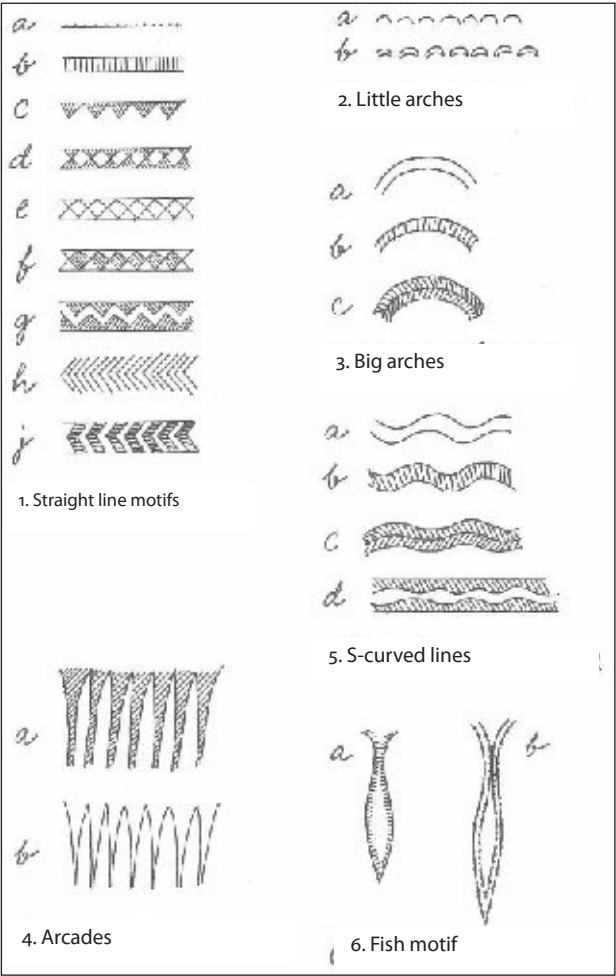
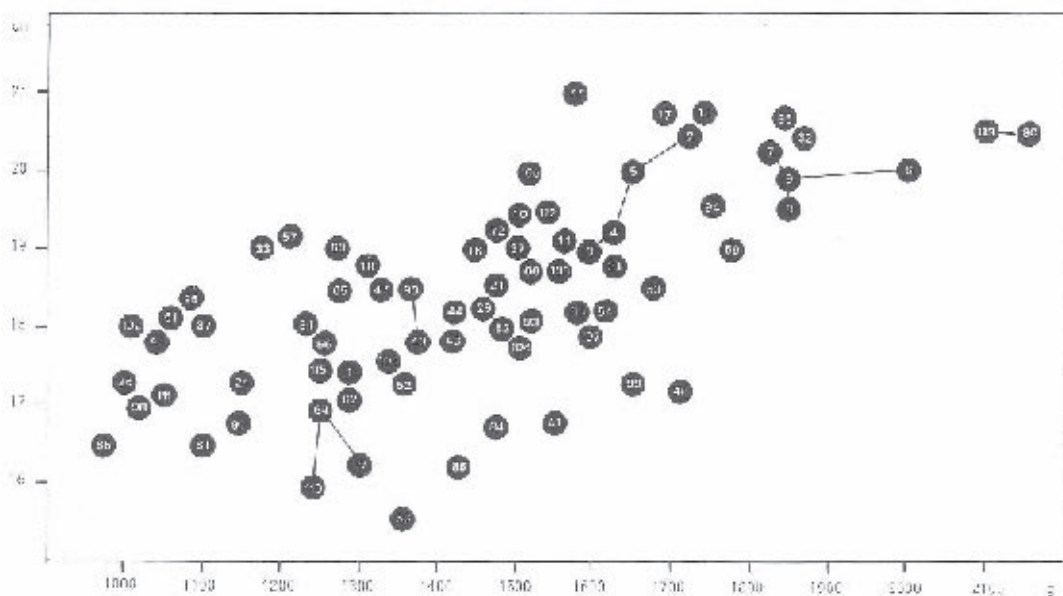


Fig. 12:4. The most important decorative elements of the Fårdrup axes.





*Fig. 12:6. Axe from Sieverstedt, Kreis Flensburg, Schleswig-Holstein (no. 80).*

*Fig. 12:7. Axe from Sjögestad, Vreta Kloster Parish, Östergötland (no. 113).*

find: the weight system was more long-lived than the decoration, and we have two axes which differ in weight by just 2% but are decorated in completely different ways. There can scarcely be any doubt that both axes were made in Schleswig-Holstein. In the entire corpus of Fårdrup axes I know of only three others with round bulges on the shoulders, and they were all found in northernmost Germany. They are no. 72 from Altenhof, Bornstein, Kreis Eckernförde, Schleswig-Holstein (Aner & Kersten 1978, Pl. 62, 2480), no. 74 from Franzburg-Barth, Pomerania (Hachmann 1957, Pl. 30:2; Kersten 1958, Pl. 18, 237), and no. 79 from Schleswig-Holstein, without provenance (Museum Schleswig F.S. 129). I have not been able to weigh no. 74, and no. 79 is badly damaged, but no. 72 Bornstein, which was found less than 40 km from no. 80, Sieverstedt, weighs 1,480 g, which is almost exactly two thirds of the weight of the Sieverstedt axe (the discrepancy is only 3%). It thus seems clear that no. 72 Bornstein, no. 80 Sieverstedt, and no. 113 Sjögestad were all made in Schleswig-Holstein according to the same weight system.

If we compare this Schleswig-Holstein weight system with the one from Sjælland calculated on the basis of the Fårdrup find (tab. 12:1), we see immediately that it is one and the same system. In tab. 12:5 the weights of the seven studied objects are expressed as multiples or fractions of the small axe and of the big axe from the Fårdrup find. The smallest common denominator is a weight

Tab. 12:5. A weight system in Schleswig-Holstein and Sjælland during the Fårdrup period.

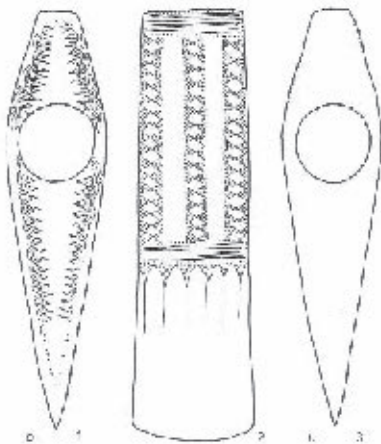
	With the small axe from the Fårdrup find as weight unit			
No. 11	The Fårdrup find, small axe	1,547 g	100%	weight unit
No. 12	The Fårdrup find, big axe	3,170 g	205%	double
	The Fårdrup find, mace-head	3,234 g	209%	double
No. 102	Fellingsbro	1,015 g	66%	two thirds
No. 72	Bornstein	1,480 g	96%	weight unit
No. 80	Sieverstedt	2,150 g	139%	four thirds
No. 113	Sjögestad	2,100 g	136%	four thirds

	With the big axe from the Fårdrup find as weight unit			
No. 11	The Fårdrup find, small axe	1,547 g	49%	one half
No. 12	The Fårdrup find, big axe	3,170 g	100%	weight unit
	The Fårdrup find, mace-head	3,234 g	102%	weight unit
No. 102	Fellingsbro	1,015 g	32%	one third
No. 72	Bornstein	1,480 g	47%	one half
No. 80	Sieverstedt	2,150 g	68%	two thirds
No. 113	Sjögestad	2,100 g	66%	two thirds

of 500–525 g, and the weights of the studied objects are 2, 3, 4, or 6 times this weight unit, which must have been an established system in the time of the Fårdrup axes, at least in the social environment in which these examples were manufactured.

Yet another example of the possibility of using the independent typological elements of weight and decoration to match axes from different geographical areas is provided by nos 40 and 90. No. 40, which was found in Gram, Haderslev amt in South Jutland, weighs 1,375 g. On the top it has a very unusual element in the decoration, namely, long hatched triangles pointing inward from the edge (fig. 12:8). No. 90, which according to a rather uncertain report was found in Skåne “between Lund and Landskrona”, weighs 1,365 g. It has the same unusual decoration, long hatched triangles pointing in from the edge (fig. 12:9, cf. Oldeberg 1974, no. 443 a). Whatever the correct find spot of no. 90 might be, the hypothesis must be that both axes were manufactured in South Jutland. As regards weight, nos 40 and 90 can unreservedly be associated with no. 80, Sieverstedt, since their weight is 64%, or almost two thirds of the weight of no. 80. The distance between Sieverstedt and Gram is about 60 km.





*Fig. 12:8. Axe from Gram gård, Gram Parish, Haderslev County (no. 40).*



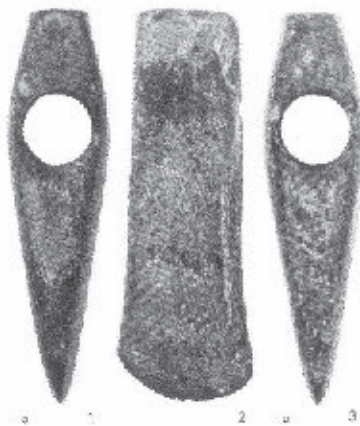
*Fig. 12:9. Axe found "between Lund and Landskrona", Scania (no. 90).*

An example of how it is possible to pair axes even without the support of their decoration comes from nos 64, 97, and 110, far down in the diagram. No. 64 is from Denmark, find spot unknown (NM Copenhagen B 13537, fig. 12:10). No. 97 is from Västra Karaby in Skåne (fig. 12:11). No. 110 was found at Svedvi in Västmanland (fig. 12:12). The three axes weigh 1,263, 1,300, and 1,240 g. The characteristic feature of the axes, as is evident from the pictures, is the edge, which is oblique and slightly concave on both sides. All three axes are described in the museum catalogues as undecorated, but the axe from Denmark and the one from Västmanland have extremely faint carved decoration, which further strengthens the association between them. The difference in weight between them is less than 2%, with the ones from Skåne being slightly heavier. Despite the uncertain provenance of the Danish axe, the most likely hypothesis is that all three were cast in Denmark. The weight shows no association with the weight system of the Fårdrup find.

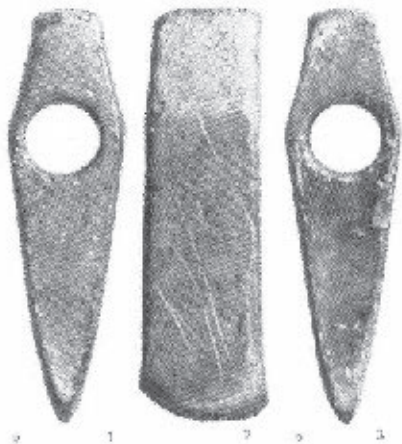
It deserves to be repeated that the weight system of the Fårdrup find, although it seems to apply to a number of axes in the area extending from Schleswig-Holstein to Sjælland (also applying to axes exported from there), it is not followed by the entire production of Fårdrup axes. That this is the case is clear from the big find from Bregninge, less than 40 km from Fårdrup. The nine axes in the Bregninge find can be divided into three natural groups. The four axes nos 2–5 are decorated, clustering in the diagram fig. 12:5 around a weight of about 1,650 g; the difference in weight between the heaviest and the lightest axe



*Fig. 12:10. Axe from Denmark, find spot unknown (no. 64).*



*Fig. 12:11. Axe from Västra Karaby Parish, Skåne (no. 97).*



*Fig. 12:12. Axe from Ekeby, Svedvi Parish, Västmanland (no. 110).*

is 8%. The four axes nos 6–9 are undecorated, all with a weight of about 1,900 g; here too the difference in weight between the heaviest and the lightest axe is 8%. The third and last group among the Bregninge axes consists of a single axe, no. 10, which weighs 1,539 g. Based on these two elements, weight and decoration, one can formulate a hypothesis that the decorated axes, nos. 2–5, were made on one occasion according to one weight standard, while axes 6–9 were made on a different occasion to a different weight standard. The decoration varies quite a lot between axes 2–5, and there are therefore good grounds to prefer the hypothesis that they were made according to a weight standard that prevailed over a certain period of time, rather than that they were cast on the same occasion. The corresponding hypothesis should then be applied to the undeco-

rated axes, and one conclusion is that a possible explanation for the slight variation in the weights of the Bregninge find is that they were produced and collected over a certain period of time.

Without looking closely at the relative internal chronology of the Fårdrup axes here, I must present one result of my studies, namely, that the Bregninge find must be slightly older than the Fårdrup find. One possible explanation for the more varied weights of the Bregninge find compared to the more uniform weights of the Fårdrup find is the hypothesis that the standardized weight system emerged during the time of the Bregninge find, and that the Fårdrup find allows us to study it in a more strictly applied form.

In the cluster of points in the diagram fig. 12:5, it is possible, to some extent, to discern three separate parts. To the right, from no. 6 and to the right, there is a grouping in which the axes from Sjælland – with the Bregninge find and the small axe from Fårdrup – constitute a noticeable element (the big axe in the Fårdrup find is far to the right beyond the edge of the diagram). To the far left in the diagram (from no. 86 to no. 24) there is a grouping in which the majority consists of axes from Skåne and elsewhere in Sweden. In the middle of the diagram there is yet another grouping (from no. 33 on the left to no. 44 on the right) with weights falling roughly between 1,200 g and 1,450 g, the composition of which is worth studying. The grouping consists of 21 axes, and no fewer than 9 of these were found in Jutland and 5 in Denmark with find spot unknown, 2 in Sjælland, 3 in Skåne, 1 elsewhere in Sweden, and 1 in Norway. Even a quick survey like this provides indications of local weight preferences and perhaps local weight systems.

The study of the weight relations of the Fårdrup axes can be continued with the same method as that used hitherto, namely, by contrasting the independent element complexes of weight, find spot, decoration, and to some extent form. An effort like this should not degenerate into a purely numerical exercise, with no regard for source criticism and cultural history, and the numerical evidence should not be pushed too hard to extract information. What may be expected of the continued work is a clearer demarcation of the production area of the Fårdrup axes and a division of that area into workshop circles – or spheres of influence – to which many of the individual axes could be assigned with a fair degree of certainty.

The Fårdrup axes by no means lack aesthetic qualities, but their oddly massive form in relation to other artefact types in the earliest part of the Bronze Age calls for an explanation. The reason cannot be lack of skill in bronze casting, as is perfectly clear from other indigenous work in bronze from the same time. Nor can the explanation be that the Fårdrup axe was a work axe, since it would

have been unreasonably expensive and impractical for that purpose. There are good reasons to assume that the Fårdrup axes had a function in religious ceremonies, especially because all the examples we have can be regarded as having been sacrificed, usually in water or bogs, yet this too fails to explain their massive form. In all probability the Fårdrup axes were status symbols, which would explain the impressive amount of expensive metal. But the fact that the weight was in many cases – perhaps always – carefully calculated shows that the Fårdrup axes also functioned as metal ingots and standards of value. People traded them – or rather perhaps used them in payment – and in this way they ended up outside their area of manufacture, far to the north in the Scandinavian Peninsula. Gifts are a possible but much less probable explanation for this distribution – why would anyone so carefully adjust the weight of an object intended to be given away?

A supreme form ought to be an expression of something central in society, not least if it represented a large symbolic and economic value. In the case of the Fårdrup axes it must be a matter of an emerging class of magnates in the Early Bronze Age in Denmark, including Skåne and Schleswig-Holstein. Local weight systems, which may possibly be discerned in future research, can be envisaged as corresponding to a division into political-economic regions.

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# Weight systems in the Scandinavian Bronze Age

1992

CHARLES SELTMAN FORMULATED the following good definition (1955:1): "Metal when used to facilitate exchange of goods is currency; currency when used according to specific weight-standards is money; money stamped with a device is coin". The possible existence of pre-monetary weight systems has been discussed by archaeologists for a long time. Jacques Briard recently surveyed a number of types, well-known in this connection, such as French leaded-bronze socketed axes and British "ring money" and "currency bars". He concludes that various pre-monetary systems probably existed during the Bronze Age (Briard 1987:742), but is sceptical as regards prehistoric weight systems (1987:731): "On the other hand, it seems that the shape of the object would have played a greater role". If standardized weights did not exist in prehistory our discussion will, however, be very vague. Standardized metal types occur abundantly in all Bronze and Iron Age periods and cultures, and it will in fact be impossible to separate currency from non-currency without guidance from written sources. Our only chance to prove the existence of prehistoric currency or money is, therefore, to find weight systems.

## The Goddess of Wealth

In the Scandinavian Late Bronze Age there is a curious type of female bronze statuette (fig. 13:1), of which 11 specimens are known (tab. 13:1). The majority are concentrated in southern Scandinavia: six from the Swedish province of Scania, and three from the neighbouring isle of Zealand. The remaining two come from the Swedish province of Västergötland and from Pomerania in northern Germany respectively (fig. 13:2).

All the statuettes portray a standing woman, naked but for one or more necklets, with peculiarly looped arms, and hands under her breasts. The modelling is usually poor, the face without beauty, or even slightly intimidating. Some statuettes have a flat face, others a sharp profile, some have wide hips and others no body curves at all: generally they are very uniform (Arne 1909, figs 1-7). All

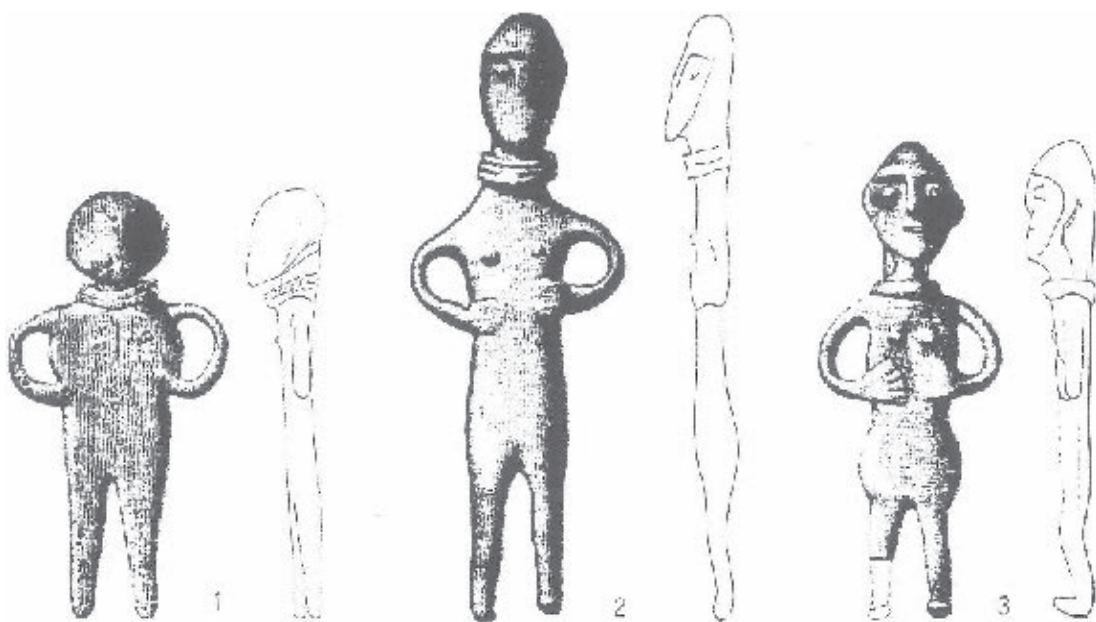


Fig. 13:1. Swedish bronze statuettes. 1. Ivetofsa. 2. V. Ingelstad. 3. Sankt Olof (from Montelius 1922).

are single finds, some found in water, and they are no doubt offerings. This type is usually called the “Goddess with the necklet”.

There is a rich variety of statuettes in the Scandinavian Late Bronze Age (Broholm 1953, fig. 105, 317–323; Brøndsted 1958:224–225), some, such as the famous Fårdal hoard, safely dated to Period V. By analogy with these statuettes, the “Goddess with the necklet” has been dated to Periods V or VI. Further support for this dating is the necklet, or necklets, that the goddess usually wears, since two or more necklets, most often found in bogs or water, constitute the commonest hoards of the Late Bronze Age (Baudou 1960:56). They are usually interpreted as rich offerings by wealthy women to a goddess of fertility, characterized by her necklet.

It has long been noted that these statuettes are artistically inferior to all other statuettes from the late Bronze Age. So the hypothesis has been put forward that these statuettes are cheap mass products, owned by poor people as household gods (Stenberger 1964:300).

I was recently invited to write an article for a Festschrift about one of these “Goddesses with the necklet” (Malmer 1990:78, 198). The statuette in question





*Fig. 13:2. The distribution of bronze statuettes (triangles) and Swedish golden "oath rings" (circles).*

(fig. 13:1:3), which comes from Sankt Olof in Scania, was found more than a century ago, and since all its other characteristics seemed to be adequately known I decided to weigh it. Its weight turned out to be 99 g, but part of its right leg is missing, so the original weight may be estimated at 103 g. Tab. 13:1 shows the weights and lengths of the 11 statuettes (no. 8 is fragmentary, and no. 11, which once belonged to a Berlin museum, may no longer exist).

Five of the statuettes (nos 1–3, 5, 7) vary only a little as regards weight: 102, 104, 110, 103 and 103 g respectively. This may seem unsurprising, since the statu-



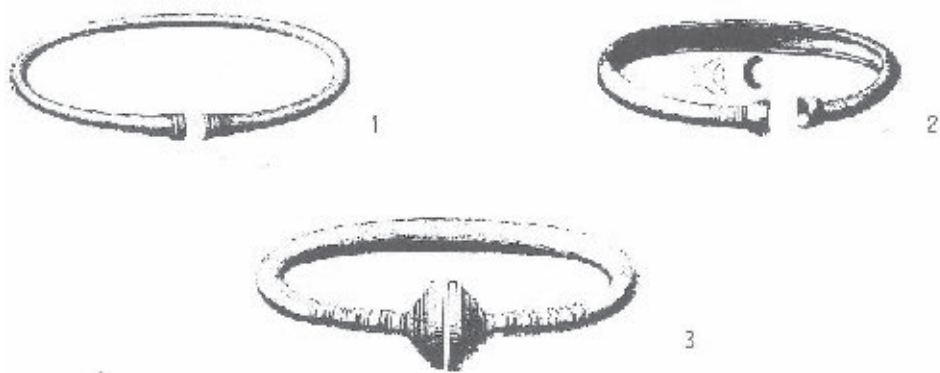


Fig. 13:3. Swedish "oath rings" of solid gold. 1 & 3. Kvistofia. 2. Östra Hoby (from Montelius 1922).

ettes are similar in type. But these five statuettes vary much more in length: they are 100, 96, 137, 109 and 113 mm long, respectively. It seems, somewhat unexpectedly, that the weight of the statuettes was important. This impression is reinforced by statuette no. 4, which is 101 mm long, but weighs only 55 g, about half the weight of statuettes nos 1–3, 5 and 7.

It is easier to compare the weights of the statuettes if they are expressed in percentages. No. 7, from Timmele (see fig. 14:1, left), is patinated, but better preserved than the others, so it is appropriate to start with this one. Its 103 g is consequently given a percentage of 100. Expressed as percentages of 103 g, four of the other statuettes vary only between 99% and 107%, and no. 4 is 53%. This in fact looks like a weight system, where nos 1, 2, 3, 5 and 7 represent the weight unit, and no. 4 half the unit.

Do the remaining statuettes fit in this hypothetical weight system? Nos. 6 and 9 are 128% and 129% respectively of a 103 gramme weight unit; they are close to 125%, or  $1\frac{1}{4}$  of the weight unit. They are also close to 133.33%, or  $1\frac{1}{3}$  of the weight unit. No. 10, however, does not fit in well; its 83% is too far from 75%, three-quarters of the weight unit.

If the statuettes were in fact made according to a weight system, what purpose did they have?

Is it possible that they could have been bronze ingots? No, hardly. It seems pointless to give a small quantity of metal, with a relatively limited value, such an elaborate form. More reasonably they could have been weights for weighing precious goods.

In Bronze Age Scandinavia at least two imported commodities in all likelihood required weighing, namely salt and gold. Of course all Bronze Age salt has

Tab. 13:1. *Bronze statuettes from the Late Bronze Age.*

References	Finding place	Length (mm)	%	Weight (g)	%	Denomination
1 Montelius 1922:1481	'Helsingborg', Scania	100	88	102	99	1
2 Montelius 1922:1480	Ivetofta, Scania	96	85	104	101	1
3 Vebæk 1978:29	Kvistofta, Scania	137	121	110	107	1
4 Salomonsson 1971:115	'Malmö', Scania	101	89	55	53	½
5 Montelius 1922:1478	S:t Olof, Scania	109	96	103	100	1
6 Montelius 1922:1477	Västra Ingelstad, Scania	136	120	132	128	1¼
7 Montelius 1922:1479	Timmele, Västergötland	113	100	103	100	1
8 Broholm 1953:321	Bogø, Zealand	78	69	-	-	-
9 Vebæk 1978:28	Ølstykke, Zealand	120	106	133	129	1¼
10 Broholm 1953:322	Vikso, Zealand	99	88	85	83	?
11 Hoernes 1915:535	Klein Zastrow, Pomerania	140	124	-	-	-

gone for ever, but gold objects are well preserved, and so are suitable to test the hypothesis that the statuettes might be weights.

## The “oath rings” of solid gold

“Oath rings”, a characteristic and uniform gold type in the Scandinavian Late Bronze Age, are traditionally so named because the antiquarians of the early 19th century attributed them to the Viking Age, and because Icelandic literature tells us that oaths might be sworn on a sacred arm-ring (Thomsen 1836:44; Foote & Wilson 1970:403). In fact they are well dated to the Late Bronze Age, Period V, and probably also to Periods IV and VI (Montelius 1922, figs 1304, 1307; Broholm 1953:76, 84; Baudou 1960:66).

They are all found in hoards or as stray finds. The ends of “oath rings” terminate in bowl-shaped knobs (fig. 13:3), which, as Thomsen (1836:44) remarked, must have made them rather uncomfortable to wear. The ring itself may be round or oval in section (fig. 13:3:1, 3), or with a concave inner face (fig. 13:3:2). In some cases the gold is so thin that a bronze ring had to be set inside in order to give the bracelet adequate strength.

In Sweden 11 “oath rings” of solid gold have been found, and five of gold with a bronze ring inside. The eleven rings of solid gold are listed in tab. 13:2, nos 12–22. Is there any relationship between the weights of the gold rings and the weight system of the bronze statuettes? Yes, indeed. Our hypothesis was that the statuette from Timmele with its 103 g represents the weight unit. Two of the gold rings, nos 20 and 21, are very close to this – approximately 107 and 108 g

Tab. 13:2. *Bronze statuettes and "oath rings" of solid gold from the Late Bronze Age.*

References	Finding place	Weight (g)	%	Denomination
Bronze statuettes				
1 Montelius 1922:1481	'Helsingborg', Scania	102	99	1
2 Montelius 1922:1480	Ivetofta, Scania	104	101	1
3 Vebæk 1978:29	Kvistofta, Scania	110	107	1
4 Salomonsson 1971:115	'Malmö', Scania	55	53	½
5 Montelius 1922:1478	S:t Olof, Scania	103	100	1
6 Montelius 1922:1477	Västra Ingelstad, Scania	132	128	1¼
7 Montelius 1922:1479	Timmele, Västergötland	103	100	1
8 Broholm 1953:321	Bogø, Zealand	-	-	-
9 Vebæk 1978:28	Ølstykke, Zealand	133	129	1¼
10 Broholm 1953:322	Vikso, Zealand	85	83	?
11 Hoernes 1915:535	Klein Zastrow, Pomerania	-	-	-
'Oath rings' of solid gold				
12 Montelius 1922:1044	Kvistofta, Scania	54.60	50.99	½
13 Montelius 1922:1307	Kvistofta, Scania	187.20	174.83	1¾
14 Montelius 1922:1304	Östra Hoby, Scania	48.02	44.84	½
15 Montelius 1916, fig. 25	Hunnestad, Halland	80.82	75.48	¾
16 Fornvännen 1922:16678	Tvååker, Halland	83.87	78.33	¾
17 Montelius 1916, fig. 30	Gudhem, Västergötland	85.44	79.80	¾
18 Montelius 1916, fig. 33	Österbitterna, Västergötland	50.51	47.17	½
19 Montelius 1916, fig. 34	Södra Ving, Västergötland	76.44	71.39	¾
20 Malmer 1968:16	Morlanda, Bohuslän	107.07	100	1
21 Montelius 1916, fig. 38	Ljungby, Småland	108.15	101.01	1
22 Montelius 1916, fig. 39	Vänge, Gotland	58.45	54.59	½

respectively. And four gold rings, nos 12, 14, 18 and 22, weigh approximately 55, 48, 51 and 58 g respectively, which is close to half the unit.

Obviously the gold rings are more suitable than the bronze statuettes for calculating the possible weight unit, since they are not patinated, and much better preserved than the statuettes – in fact so well preserved that it is meaningful to state their weights to two decimal places. Ring no. 20, from Morlanda (see fig. 14:2), is an exquisite piece of work, and very well preserved. We shall therefore abandon the statuette from Timmele, weighing 103 g, as the possible weight unit, and use instead the gold ring from Morlanda, weighing 107.07 g.

In tab. 13:2 the weights of all the other gold rings are expressed as percentages of 107.07 g. Ring no. 21, which we thought to be equal to the weight unit, or 100%, is in fact 101.01%, and consequently deviates only just over 1%. Rings nos 12, 14, 18 and 22, considered to be half the unit, or 50% are in fact 50.99%, 44.84%, 47.17% and 54.59%; they deviate only between 0.99 and 5.16 percentage points.

Let us look at the remaining five gold rings. Four of them, nos 15, 16, 17 and 19, are probably intended to be  $\frac{3}{4}$  of the weight unit. Instead of exactly 75% of the unit, they are 75.48%, 78.33%, 79.80% and 71.39% respectively, which means that they deviate between 0.48 and 4.80 percentage points. The last gold ring, no. 13, is obviously intended to be  $1\frac{3}{4}$  of the weight unit. It is not 175%, but in fact 174.84%, only 0.16 percentage points from the expected weight.

To compare the weights of statuettes and gold rings we must express also the weights of the statuettes as percentages of 107.07 g, our new hypothetical weight unit. This calculation is made in the upper part of tab. 13:2. Statuettes nos 1, 2, 3, 5 and 7 are 95%, 97%, 103%, 96% and 96% respectively of the weight unit. None of them deviates more than five percentage points from the new weight unit, 107.07 g. With the old hypothetical weight unit, 103 g, the maximum deviation was seven percentage points, so the new weight unit functions better. This also is the case in respect of the remaining statuettes. No. 4 is 51% of the weight unit, thus obviously  $\frac{1}{2}$  weight unit. Nos. 6 and 9 are 123% and 124% respectively, which is very close to 125%, or  $1\frac{1}{4}$ . Statuette no. 10 caused problems when we thought the weight unit to be 103 g, because its 83% of the weight unit seemed to be too far from  $\frac{3}{4}$ . But the new weight unit brings this statuette well into the weight system, for its 79% deviates only four percentage points from  $\frac{3}{4}$  of the unit.

To sum up, all the bronze statuettes and all the gold rings fit well into a weight system, based on a unit of 107.07 g, and with the following values (fractions or multiples):  $\frac{1}{2}$ ,  $\frac{3}{4}$ , 1,  $1\frac{1}{4}$ ,  $1\frac{3}{4}$ . The deviation from these values varies only from minimum 0.17 (gold ring no. 13) to maximum 5.16 (gold ring no. 14) percentage points. It should be observed that gold ring no. 14 is very worn (Montelius 1916:6).

The weights of statuettes and gold rings is shown graphically in fig. 14:3. If we have in fact identified a weight system the variation range around a value must be smaller than the distance between concentrations. The graph shows that this is actually the case. There is a variation range around value  $\frac{1}{2}$  of 10.43 g, and a distance to the next concentration of 17.99 g. The variation around value  $\frac{3}{4}$  is 9 g, and the distance to the next concentration is 16.56 g. The variation around the weight unit, value 1, is 8 g, and the distance to the next concentration 21 g. The weights at value  $1\frac{1}{4}$  lie close together, and then there is a distance of 54 g to the single gold ring at value  $1\frac{3}{4}$ .

Concerning the lower values there is a characteristic rhythm: the variation ranges are c. 10, 9 and 8 g respectively, and the distances between the concentrations are 18, 17 and 21 g respectively. This strengthens the hypothesis that a weight system is intended, and that the variations around a value are due to technical difficulties and wear.

A weight system must cover a certain region. There is no reason to believe that the region of this weight system should be the whole of Bronze Age Europe. The map (fig. 13:2) shows that the gold rings are concentrated in the same region as the bronze statuettes, south-west Sweden. Sometimes statuettes and gold rings are found not far from each other. Thus statuette no. 3 and gold rings nos 12 and 13 all come from Kvistofta, Scania. There are just a few kilometres between Timmele and Södra Ving, Västergötland, the find places for statuette no. 7 and gold ring no. 19. This may be coincidence – or perhaps not.

The three statuettes outside Sweden, in neighbouring Zealand and Pomerania, could well be of Swedish origin. This cannot, however, be the case with 25 “oath rings” of solid gold found in Denmark, mainly on the islands (Broholm 1953:76). Denmark is of course the centre of the Scandinavian Bronze Age, and there is every reason to believe that the golden “oath ring” as a type originated in Denmark, presumably on Zealand. So far the weight of the Danish “oath rings” has not been studied, and it is impossible to guess whether the Danish weight unit is identical with the Swedish one. In addition to the 11 Swedish “oath rings” of solid gold there are five rings of relatively thin gold with a bronze core. Their find places and weights (gold and bronze together) are the following: one from Österslöv, Scania (65.61 g); a hoard from Simrishamn, Scania, with three rings (40.86, 30.81 and 58.44 g); and one from Resmo, Öland (52.81 g). Expressed as percentages of the proposed weight unit, 107.07 g, they are 61.12%, 38.16%, 28.77%, 54.58% and 49.32% respectively. It is impossible to know if the last three rings are masquerading as solid gold rings with values of  $\frac{1}{4}$  and  $\frac{1}{2}$  respectively. The first two rings do not fit in the weight system. Of course the weight of an unknown quantity of gold plus an unknown quantity of bronze has no interest – unless the intention is to cheat somebody!

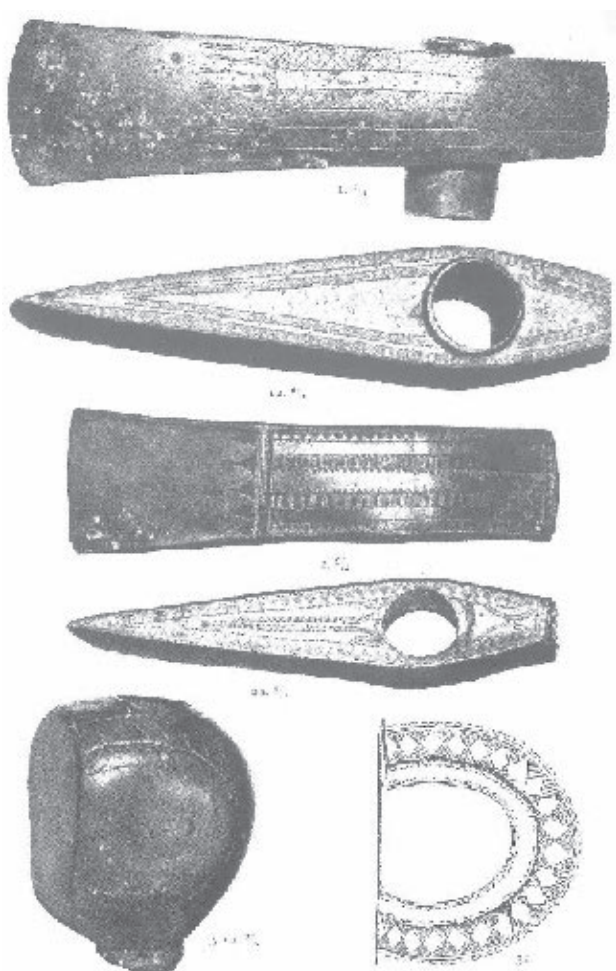
## The massive bronze axes of Fårdrup type

I started weighing the bronze statuettes simply because everything else about them seemed to be known already. But I had earlier studied the weight of the massive bronze axes of Fårdrup type. This study has been published (Ch. 12), and so only a few points will be recapitulated here.

All Fårdrup axes are found in hoards or as stray finds. Their name comes

from a magnificent hoard, found in Fårdrup, Zealand, which contained two such axes and a mace head (fig. 13:4). These axes date from the end of Period I, just before the beginning of the typical spiral decoration style of Period II. About 120 axes of this type are known, concentrated in Denmark and Scania, with a few specimens in the more northerly parts of Scandinavia as well as north Germany (Malmer 1989, fig. 1). Most are, strangely enough, cast in an open mould. The decoration is engraved, varied and elegant, but very shallow. The axes are very heavy – the average weight of the Zealand axes is 1740 g, whereas a modern wood-cutting axe, including its handle, may weigh 900 g. So how could the Fårdrup axes have been used?

The smaller axe of the eponymous Fårdrup find (Brøndsted 1931) is very elegant, but the decoration of the larger axe is not so good, and the mace-head is



*Fig. 13:4. The Fårdrup hoard (from Broholm 1944).*

downright clumsy in form with an ungainly decoration, so the three objects can hardly have been made by the same master. All three were cast in open moulds.

Tab. 14:2 gives the weights of the three objects of the Fårdrup hoard. They are remarkably heavy – how do you use an axe weighing 3170 g (7 lb)? And what motivated this waste of bronze, which was no doubt very expensive in the Early Bronze Age? Still more enigmatic is the relationship between the weights of the three objects. The small axe weighs almost exactly half as much (49%) as the large one, and the mace head has almost exactly the same weight as the large axe (102%), even though they are completely different in shape.

All these strange facts seem to point in one direction, namely that the Fårdrup axes are not axes but rather bronze ingots with a fixed value. This hypothesis explains why they were cast in open moulds, a technique that makes it easier to attain the correct weight. It also explains their simple form and shallow decoration, and why they are so heavy as to have been unsuitable for practical use, and the apparent fixed relationships between their weights.

The weight system of the eponymous Fårdrup hoard can be traced among the bulk of Fårdrup type axes. A number of axes in Denmark, north Germany and Sweden fit into the same weight system (Malmer 1989:25, tab. 5), and also resemble one another in decoration. However, other Fårdrup axes correspond with other weight systems (Malmer 1989:24, fig. 5). Certain facts indicate that the weight system of the Fårdrup hoard is relatively late among the Fårdrup type axes (but still in Period I), and consequently it can be interpreted as a weight system which in due course obtained recognition in Scandinavian Early Bronze Age society.

The weight system of the Fårdrup hoard seems to be based on a weight unit of 515 to 540 g. This is in fact five times the proposed weight unit of the bronze statuettes and golden “oath rings” of the late Bronze Age, 107.07 g. The small axe of the Fårdrup hoard is not far from 15 times (14.45 times to be precise) that weight unit. The large axe and the mace head are roughly that weight unit multiplied 30 times (29.61 and 30.28 times respectively). The small axe deviates less than four percentage points from 15 times the weight unit of the late Bronze Age; the large axe deviates a little more than one percentage point, and the mace head a little less than one percentage point from 30 times the weight unit. The total weight of the hoard (7959 g) deviates only 71 g or 0.9 percentage points from exactly 75 times the weight unit.

This similarity between the proposed weight system of the Early Bronze Age Fårdrup hoard and that of the Late Bronze Age statuettes and “oath rings” may or may not be pure coincidence. There may be two weight systems, or a single one.



## Discussion

There is strong evidence that there were one or more weight systems in the Scandinavian Bronze Age, applied to a curious range of objects – unwieldy bronze axes, strange female statuettes, and heavy gold rings. But these three types do have something in common. In the first place all three types are found exclusively as stray finds or hoards. Since none of these types is likely to have been dropped by accident we can be fairly sure that all the items come from either offerings or hoards, even if only a single object was deposited. There are other circumstances which connect the three types.

The female statuettes are usually described as representing a goddess of fertility, related to Astarte and her sisters in the Near East (Broholm 1949:267; Stenberger 1964:302). There is, however, a connection between fertility and wealth, and this goddess also wears bronze necklets, perhaps to show that wealth can be expressed and demonstrated by metal. The peculiar looped arms of the statuettes may have been useful for fastening to scales. There is of course no reason to believe that ancient weights should resemble modern ones. In Ugarit a weight had the form of a sitting bull, and on Cyprus a sitting calf (Balmuth 1967:29). In this connection it is also worth while to remember that the Latin *pecunia*, “money”, is derived from *pecus*, “cattle”, and that Old Norse *fé* means both “cattle” and “goods”.

Many scholars claim that the Fårdrup axes are copies in bronze of the Late Neolithic, heavy, simple stone-axes with shaft-holes (Brøndsted 1958:20; Broholm 1944:34). This seems to be very bad typology. Why should the very valuable Fårdrup axes imitate the simplest of stone axes? Even less convincing is the idea that Bronze Age man only gradually realized that this was a very expensive way of making a working-tool, and finally stopped production (Stenberger 1964:171). The simple form of the Fårdrup axes seems best explained by the hypothesis that they are ingots, cast in an open mould in order to give them precise weights.

To modern man it is obvious that gold is more valuable than copper and its alloys. But since gold, copper, bronze and brass are all different yellow metals, and assuming that metals were mainly prestige objects and used for gifts, then perhaps gold was no more valuable in the Bronze Age than copper and its alloys? This is not the case. It has been convincingly shown that the amount of metal in Danish Bronze Age graves is directly proportional to the quality of fields and pastures, that male graves are richer in metal than female ones, and that there is proportionally more gold in male graves (Randsborg 1973; 1974). Consequently gold must have been worth more than bronze, and a golden

“oath ring” much more expensive than the bronze statuette which we believe was used to weigh it.

To sum up, the three types seem to fit well together. According to our hypothesis the statuettes are weights, and the Fårdrup bronze axes and golden “oath rings” were manufactured according to a weight system in order to give them a fixed and known economic value. This hypothesis seems to have consequences for our view of Bronze Age society. During recent years many scholars have tended to think that gift exchange, often in order to strengthen “alliances”, was the main cause for the spread of metal artefacts in the Bronze Age (e.g. Jensen 1979:156; 1982:166; Kristiansen 1981:254; 1987a:36; 1987b:83; Larsson 1986:85). Many of these scholars admit that the gift exchange system that they propose in the Bronze Age did not exhaust the entire range of circulation of goods (Zaccagnini 1987:57), although some scholars speak openly of a “gift economy” (cf. Gregory 1982:18).

Friendship, alliances and gifts must have been important in all periods of human history. But what factual basis is there for asserting that there was more friendship and alliances in the prehistoric economy than in the modern one? What evidence is there about the exchange of goods in prehistoric times? This is provided by weights and weight systems. The only purpose of making gold and bronze objects in accordance with a weight system is to define the economic value of each item. A transaction cannot be called a “gift” when both giver and receiver know the exact weight and value of the metal object delivered, and when the giver expects to get an object with the equivalent value in return. This must be called trade, whatever polite ceremonies accompanied the transaction.

The debate over “gift economy” or normal trade comes when we are dealing with Bronze Age Scandinavia. No copper, tin or zinc ores were known in Scandinavia in that age. All metal had to be imported, and the continental producers did not provide it for nothing. In exchange furs were no doubt exported to the continent (Malmer 1975; 1981:107). It is well known that the best furs come from areas with a cold climate, and trade in furs is a plausible explanation for the great number of south Scandinavian bronzes found in Finland. Two rich hoards have even been found north of the Arctic Circle: one in Sodankylä contained four swords, and another in Inari four necklets and three armlets of south Scandinavian type, as well as a socketed axe of eastern provenance (Meinander 1954, Taf. 16–17). The profits from this exchange between the Arctic region and the continent were no doubt mainly taken by the middlemen in Denmark and Scania.

When discussing the Scandinavian Bronze Age it is sometimes overlooked that for 40 years we have had texts from the European Bronze Age available,

namely the Linear B tablets from Knossos, Pylos and other Mycenaean palaces. These texts tell us nothing about heroes and friends, alliances and gifts: "Their contents are deplorably dull: long lists of names, records of livestock, grain and other produce, the account books of anonymous clerks. Here and there a vivid description of an ornate table or a richly decorated chariot breaks the monotony." Weights of copper, gold and other metals are important, along with food rations or wages for officials and workers (Chadwick 1976:IX, 102–58).

Of course there were heroes in Mycenae, as Homer records, and also in Bronze Age Scandinavia, depicted on the stone slabs in the Kivik grave. But it is unrealistic to claim that Bronze Age Scandinavians built their society on a "gift economy" when the Mycenaeans built it on work, organization and trade. A Scandinavian Bronze Age chief, *mutatis mutandis*, was in all probability not very much unlike a Mycenaean *wanax* or *guasileus* (Chadwick 1976:70).

But is it realistic to compare Bronze Age Scandinavia with distant Mycenae? I believe that it is. The distance from southern Denmark to the hoard of Inari, north of the Arctic circle, is almost exactly the same as that from Denmark to Thessaly, in both cases about 2000 kilometres. Moreover, the ship and chariot pictures of the Scandinavian rock carvings (Malmer 1981:32 f., figs 4, 16:1) give conclusive evidence of their Mediterranean origin.

### *Acknowledgements*

I am indebted to Dr Ulla Westermark of the Royal Coin Cabinet for introducing me to literature on pre-monetary currency of Mediterranean antiquity, and to Mrs Anita Knape and Mr Kent Andersson for allowing me to use their new and exact weighings of gold objects in the Museum of National Antiquities, Stockholm.

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# How and why did Greece communicate with Scandinavia in the Bronze Age?

1999

MY THEME IS *how* and *why* Greece and Scandinavia communicated in the Bronze age, and I shall start with the question *how*. The invitation to this symposium mentions several types of communication, such as ideas, symbols and prestige bronze objects. *Communication* is derived from Latin *communicatio*, meaning “mutual information”, and it may be useful to stick to the original meaning of the word. I shall try to speak about communication of *ideas*, and I shall concentrate on cases where it can be *proved* that information or signals from one side have been well understood by the other side. A prehistoric artefact, sent from one part of Europe to another, may have been accompanied by ideas, but this is not necessarily the case.

I shall illustrate my point by a concrete example. It is a thousand years younger than the Bronze Age, and thus belongs to a period whose ideas are much better known. In the rich Iron Age site Helgö, in middle Sweden, was found an Indian bronze-sculpture of Buddha and an Irish crozier, both from the seventh or eighth century (Arrhenius & Holmqvist 1961:112, Pl. B–C). We can imagine that the people of Helgö looked at these objects with great curiosity, but they hardly got any information about the ideas connected with them. There is no sign of an eighth century conversion of middle Sweden to either Buddhism or Christianity.

How was long-distance information given in Bronze Age Europe? Could people from different parts of the Continent speak to each other? Perhaps it is not quite impossible to discuss this question. After all we know that in Greece the Bronze Age language was simply Greek. We know it from the writing called *Linear B*, used in the Mycenaean palaces. But what languages were spoken in the rest of Europe? Perhaps an original Proto-Indo-European had by then split into three or four European languages, say Greek-Italian, Celtic, Germanic and Balto-Slavic. But many scholars nowadays think that this is a simplistic model. More probable is, they think, that by the second millennium B.C., many languages or dialects existed, most of which died out without leaving any traces, neither in writing, nor in later languages (Drobin 1989:43 ff.).

A borderline between two great languages would probably also mean a cultural frontier, but that kind of rigid barriers would hardly result from many small languages or dialects. So the more probable alternative would no doubt make communication easier. Of course one could partly rely on sign language. And we can imagine that conversation was primarily on two topics, namely safe conduct and goods. The Bronze Age is first and foremost an age of *trade*. Copper and tin ores are much more rare than iron ore, which means that the components of bronze had to be traded, and trade means personal contact. The Bronze Age is also an age of *ideas*. Bronze is much more formable, and so mentally more inspiring than other commodities, such as corn, flint and iron. And since bronze had to be traded very long distances it brought with it ideas across Europe.

Fig. 14:1 shows two small bronze statuettes, only about 11 centimetres long, which portray a standing woman, naked but for a necklace, with peculiarly looped arms, and hands under her breasts. This type of statuette belongs to the south Scandinavian Late Bronze Age, probably Montelius Period V. Thirteen



*Fig. 14:1. Bronze statuettes from Timmele, Västergötland, and Sankt Olof, Scania, Sweden.*



specimens are known, seven of them from Scania and three from Zealand, and there can be no doubt that they were made in that region. On the other hand, similar statuettes are well known from various periods of the Neolithic, Eneolithic and Bronze Age of south-east Europe and the Near East (Müller-Karpe 1974, e.g. Taf. 222, 230, 250, 335, 352, 359–361, 374, 381, 680, 705). It seems quite probable that our south Scandinavian type of statuette has been ultimately inspired from the east Mediterranean region.

But on the other hand, what specific information does this probable south-eastern origin give us? Not very much, really. The mere idea that the Scandinavian statuettes were inspired from the south-east is quite natural, since many important innovations came from there, such as agriculture and the use of metal. The symbolism of the statuettes is of course somewhere in the sphere of fertility. But all farmers, whether Greek or Scandinavian, and whether ancient or modern, are dependent on fertility. Fertility, on the other hand, is a rather wide notion, and the specific myths, which no doubt were connected with these statuettes, were hardly the same all over the east Mediterranean region, not to speak of Scandinavia. So our statuettes seem to tell us only what we already knew, and they don't reveal their special secrets.

There is, however, still another clue to the meaning of the statuettes. The statuettes look rather standardised, only the number of necklaces is different. But the length of the statuettes varies considerably: The longest one is almost twice as long as the shortest one, 78 and 140 millimetres respectively (Malmer 1992:377 ff., fig. 1, tab. 1). It is rather remarkable, then, that the *weight* of the statuettes is standardised (tab. 14:1).

The majority has a weight of a little more than 100 grams – between 102 and 110 grams. But also the rest of the statuettes seem to have a clear weight relation to the ones with standard weight. One has a weight of 55 grams, which is half of the standard weight, and two have a weight of a little more than 130 grams, which is five quarters of the standard weight. (Statuettes nos 4 and 9 are fragmentary, no. 4 also of a slightly different type. Nos 12 and 13, originally in pre-war German museums, may no longer exist).

These statuettes are artistically inferior to other statuettes of the late Scandinavian Bronze Age, and so the hypothesis has been put forward that they are cheap mass-products, owned by poor people as household gods (Stenberger 1964:300). This is indeed a very good hypothesis, supported by all traits of the statuettes that we can see. But a trait that we cannot see by our eyes, namely the weight, makes the hypothesis most unlikely. Poor people hardly own scales, and there is no reason why an idol should have a standardised weight. On the whole, there is no reason why a small and light bronze-object should have a fixed

Tab. 14.1. *The weight of bronze statuettes and 'oath rings' of solid gold from the Late Bronze Age.*

References	Finding place	Weight (g)	%	Denomination
Bronze statuettes				
1 Montelius 1922:1481	'Helsingborg', Scania	102	99	1
2 Montelius 1922:1480	Ivetofta, Scania	104	101	1
3 Vebæk 1978:29	Kvistofta, Scania	110	107	1
4 Larsson 1994:100, right	Kvistofta, Scania	-	-	-
5 Salomonsson 1971:115	'Malmö', Scania	55	53	½
6 Montelius 1922:1478	S:t Olof, Scania	103	100	1
7 Montelius 1922:1477	Västra Ingelstad, Scania	132	128	1¼
8 Montelius 1922:1479	Timmele, Västergötland	103	100	1
9 Broholm 1953:321	Bogø, Zealand	-	-	-
10 Vebæk 1978:28	Ølstykke, Zealand	133	129	1¼
11 Broholm 1953:322	Vikso, Zealand	85	79	¾
12 Wiegel 1892-93:68	Klein Zastrow, Pomerania	-	-	-
13 Wiegel 1892-93:69	Torun, Poland	-	-	-
Gold rings				
14 Montelius 1922:1044	Kvistofta, Scania	54.60	50.99	½
15 Montelius 1922:1307	Kvistofta, Scania	187.20	174.83	1¾
16 Montelius 1922:1304	Östra Hoby, Scania	48.02	44.84	½
17 Montelius 1916, fig. 25	Hunnestad, Halland	80.82	75.48	¾
18 Fornvännen 1922:16678	Tvååker, Halland	83.87	78.33	¾
19 Montelius 1916, fig. 30	Gudhem, Västergötland	85.44	79.80	¾
20 Montelius 1916, fig. 33	Österbitterna, Västergötland	50.51	47.17	½
21 Montelius 1916, fig. 34	Södra Ving, Västergötland	76.44	71.39	¾
22 Malmer 1968:16	Morlanda, Bohuslän	107.07	100	1
23 Montelius 1916, fig. 38	Ljungby, Småland	108.15	101.01	1
24 Montelius 1916, fig. 39	Vänge, Gotland	58.45	54.59	½

weight. In the Late Bronze Age 100 grams of bronze cannot have been a treasure. So the conclusion seems clear. The statuettes are not poor people's household gods, and they are not rich people's treasures. The purpose of the statuettes must be to represent an idea, namely the idea of weight, and to make that idea work. In all probability they served as weights.

What commodities of the Scandinavian Bronze Age needed weighing by a weight of only 100 grams, or the half of that? We can think of many stuffs, but there is only one which permits us to make a test, namely gold.

Tab. 14:2. *The weight of the Fårdrup hoard.*

	Weight (g)	Percentage of large axe
Small axe	1547	49
Large axe	3170	100
Mace head	3242	102
<b>Total</b>	<b>7959</b>	

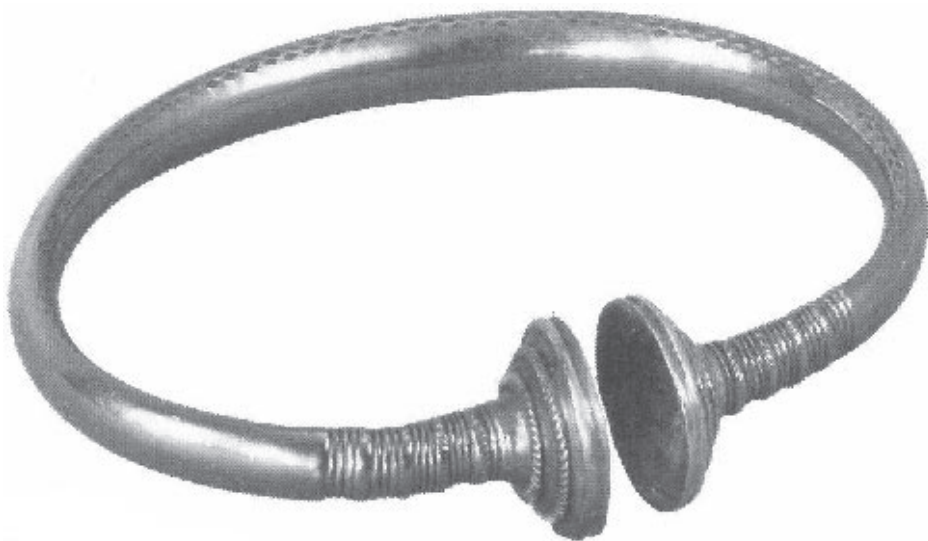


Fig. 14:2. *Gold armring from Morlanda, Bohuslän, Sweden.*

The fine gold ring from Morlanda, Bohuslän (fig. 14:2) is of a type, traditionally but wrongly called “oath ring”. Its weight is c. 107 grams, which fits well to the statuettes which weigh between 102 and 110 grams. Gold rings of this type are safely dated to Montelius Period V, precisely as the statuettes.

In Sweden gold rings and statuettes are distributed over the same south-western region. (See Malmer 1992, fig. 2, for a map. A second statuette from Kvis-tofta should be added in north-west Scania. The statuette from Torun is found on the shore of the Vistula, in the south-eastern corner of the map). The great majority of this type of golden “oath rings” are found in Denmark (but not marked on the map Malmer 1992, fig. 2). Three of the statuettes are found in easternmost Denmark, so the centre of this type is obviously Scania.

The exact weight of the Morlanda ring is 107.07 grams. It is well preserved, and so we can use it as a weight unit for a comparison with the other gold rings

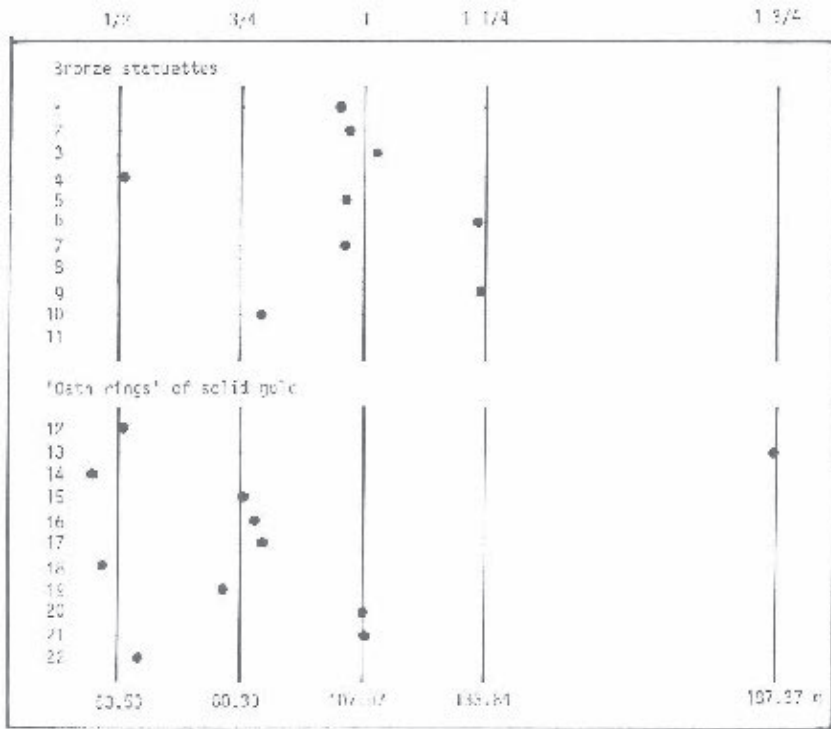


Fig. 14:3. Diagram of the weight of bronze statuettes and golden arm rings.

(tab. 14:1). One of the other rings agrees well with this weight unit. Four rings are close to half, and four others three quarters of the weight unit. The heaviest ring is one and three quarters of the weight unit.

The weight of the bronze statuettes are in good accordance with a weight unit of 107 grams. Five are quite close to the weight of the Morlanda ring, one is half of that, one is three quarters and two are one and a quarter. The diagram fig. 14:3 gives a clear picture of the weight system. So far is represented: half of the weight unit, three quarters, the weight unit, one and a quarter, one and three quarters.

The famous votive-find from Fårdrup on Zealand (Malmer 1992, fig. 7) is dated to Montelius Period I. It contains three objects which are so unpractical that one has to ask what purpose they had. The big axe weighs about 3 kilos and a quarter, and so it must have been very expensive and rather useless. Tab. 14:2 shows that there is a remarkable relation between the three objects in the Fårdrup hoard. The small axe weighs almost exactly half as much as the large one, and the mace head has almost exactly the same weight as the large axe, even though they are completely different in shape. All these strange facts seem to point in one direction, namely that the two axes and the mace head are primar-

ily bronze ingots with a fixed value. Axes of Fårdrup type are quite numerous, distributed over vast parts of Scandinavia and particularly dense in the central Danish-Scanian region. Several facts indicate that all of them are bronze ingots with a fixed value, and of course also status objects (Malmer 1989:20 ff., fig. 1).

Maybe some archaeologists will find it surprising that Bronze Age people were interested in weight, and often calibrated the weight of their metal objects very exactly.

But primitive societies must as a rule have practised strict economy, and all bartering of gold and bronze undoubtedly required weighing. An exception from strict economy was feasting, and a counterpart to feasting was sacrifices. Fårdrup axes, gold rings and bronze statuettes are as a rule found in hoards or in water, in most cases no doubt sacrificed.

It seems natural that the Scandinavian weight systems were taken over from the Continent, since all metal was bought from there. In all likelihood exact weights and prices were more important in foreign than in domestic trade. Erik Sperber (1993:623 ff.) has analysed our Bronze Age weights from a statistical point of view. He applies a method for revealing weight systems in a number of experimental data, such as the weight of gold rings and statuettes. This method uses the analogy between a weight system where the recorded weights appear at regular intervals and a sine curve which moves from  $-1$  to  $+1$ , likewise at regular intervals. A search for a weight system can be made by letting a tentative weight unit vary and plotting the sum of deviations of the sine curve from zero. When the tentative weight unit coincides with the weight unit of the actual set, a minimum appears.

Fig. 14:4 is a diagram published by Sperber (1993:614, fig. 1). Curve A represents the Swedish golden "oath rings", and curve B is the bronze statuettes. Sperber resolves that the only minimum that is statistically significant with a probability of around 98% is at 26 grams for the statuettes and at 27 for the gold rings. If the two sets of weights are combined the minimum will be found at 26.6 grams, and the probability that this is the real weight unit is better than 99%. Curve C is based on 17 gold rings from the Pannonian Late Bronze Age, listed by Eiwanger (1989:443 ff.). The weight unit found in this case is 27.25 grams, which is very close to Sperber's Scandinavian weight unit. This is a hint that the weight system was actually taken over from the Continent.

Sperber's weight unit, 26.6 grams, is almost exactly half of the lowest denomination (53.53 g) in the diagram fig. 14:3. Or, expressed in another way, it is one quarter of the weight unit on which this diagram is built, namely the Morlanda gold ring.

Sperber also made a statistical analysis of the Fårdrup axes, which are much

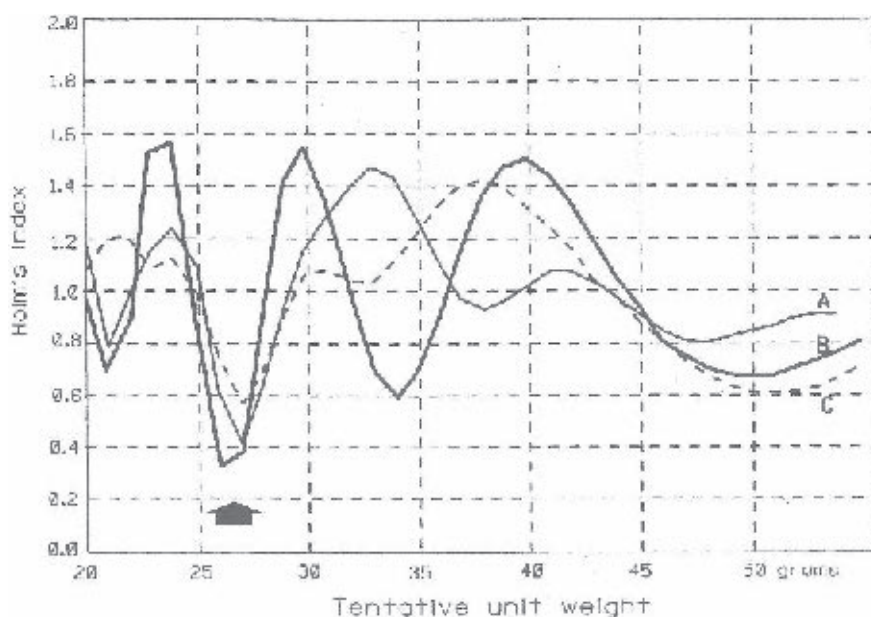


Fig. 14:4. Diagram of Bronze Age weight systems (from Sperber 1993). A. Swedish golden arm rings. B. Scandinavian bronze statuettes. C. Pannonian golden arm rings.

older than the gold rings, since they belong to the Early Bronze Age, Montelius Period I, whereas the gold rings belong to Period V. The axes are also much heavier. The weight of the eponymous Fårdrup axe turns out to be almost exactly 30 times the weight of the Morlanda ring (Malmer 1992:385). This is no coincidence. The result of Sperber's analysis is that the weight unit of the Fårdrup axes with a probability of better than 99% is 264 grams. That is 10 times the weight unit of the gold rings and statuettes, which is 26.6 grams. So there is a possibility that 26.6 grams was not just a weight unit among others, but simply *the* weight unit of the entire Scandinavian Bronze Age.

We saw that a weight unit of about 27 grams is found also in the Hungarian Late Bronze Age. The same weight standard can be traced further away. The weight of the Attic drachm (*drachma*) at the time of Alexander the Great (4th century BC) was 4.37 grams. Consequently 6 drachmas, or a *hexadrachma*, weighed 26.22 grams. But the weight standard is much older than that. Flinders Petrie (1926) showed that in Egypt, at the time of Akhenaten in the 14th century BC, there was a weight unit of about 8.75 grams, called a *stater* or 2 drachmas, a *didrachma* (Sperber 1992:617). Three times that, a *hexadrachma*, is 26.25 grams. Consequently the weight of the Morlanda ring is almost exactly 24 Attic drachmas, 2 *dodekadrachmas*.

Of course we cannot say that it has been proved that the Greek or Near East *drachma* was used in the Scandinavian Bronze Age. The problem is more complicated than that. For instance, a drachma weighed differently in different Greek cities, especially in later periods. However, it is definitely sure that a weight system was used in Mycenaean Greece, for it is abundantly mentioned in *Linear B* texts (Ventris & Chadwick 1956). It is also proved beyond doubt that one or more weight systems were used in Bronze Age Scandinavia. Even if we did not know the weight system of bronze statuettes and gold rings, I think that the three objects in the Fårdrup hoard would be sufficient evidence (tab. 14:2). It is most unlikely that the relation between their weights should be a coincidence.

To sum up: the economy of Bronze Age Greece was very much dependent on trade. In Bronze Age Scandinavia all metal had to be imported. You cannot trade without a weight system. There are weight systems in Greece, in central Europe and in Scandinavia. This is what we know for certain.

I started with the question *how* Greece and Scandinavia communicated in the Bronze Age. The answer must be that they certainly did not communicate directly, but by means of middlemen in Pannonia and elsewhere on the European continent. Metal objects were transported rather long distances, but not from Greece to Scandinavia. However, even if a Greek weight had been transported that far, there had been no proof that the Scandinavians understood the significance of such an object. But the weights of domestic Scandinavian metal objects prove that Scandinavian Bronze Age people used a weight system. In all probability this system was identical with one of the weight systems used in Greece and the Near East. But that is not the main thing. The central fact is that people all over Europe, from Greece to Scandinavia had a common frame of reference when handling commodities. They knew the concept of weight. They could communicate, and they did.

My second question, *why* there was such trans-European communication, has already got an answer. The driving force was obviously trade. We don't need to discuss the question at what point in history trade started. There is no real distinction between trade and gifts, just a formal one. People exchange commodities and services because they think it is to their advantage.

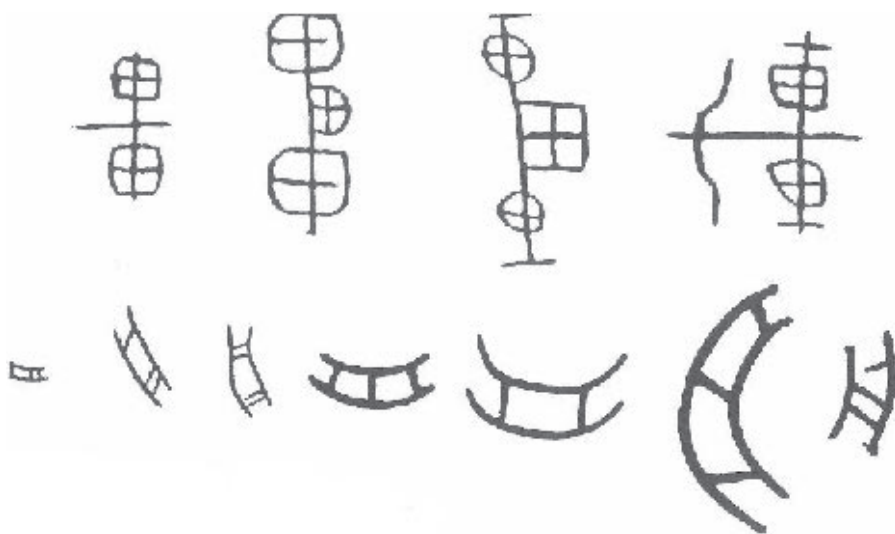
The richest hoard of swords, found north of Denmark, is not from south Sweden, but from Sodankylä in northern Finland, north of the Arctic circle. It consists of four magnificent swords (Meinander 1954, Taf. 16). As is well known the best furs come from regions with a cold climate, and probably furs were exported to the Continent, in exchange for metal. Denmark is 2000 kilometres north of Greece, but the Arctic circle is further 2000 kilometres north of Den-



mark. The people of Central Europe were middlemen between Greece and Scandinavia, and likewise the Danes were middlemen between Central Europe and the Arctic regions. As usual, this situation was no doubt favourable to the middlemen.

Weight systems are certainly not the only instance of intellectual understanding across Europe. The well-known stele from Novilara, at the Adriatic Sea (Brizio 1895:18, fig. 3), probably belonging to a period contemporary with the Scandinavian Late Bronze Age, shows three pictures of ships, much similar to Scandinavian rock art ships. This type of Mediterranean ship picture was very long-lived, and may well have influenced the Scandinavian rock art from its beginning. But I am not suggesting that Scandinavians just copied a picture. The real reason why ship pictures are common both in the Mediterranean countries and in Scandinavia must be that the ship in both regions was a central concept. And a central concept in trade.

Typical of south Scandinavian rock art is, that the motifs are very clearly drawn. They are usually depictions, or standardised symbols. The numerous rock art regions of the world are indeed different in character. But it is obvious that the character of rock art does not really depend on the geographical region, but on cultural stage (Malmer 1993:551 ff.). Fig. 14:5 shows old Chinese graphical signs, meaning "chariot" and "boat". The similarity with Scandinavian rock art chariots and ships is remarkable, but of course there was no communication



*Fig. 14:5. Old Chinese graphical signs for "chariot" and "boat" (from Lindqvist 1989).*

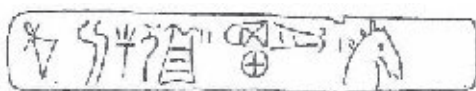
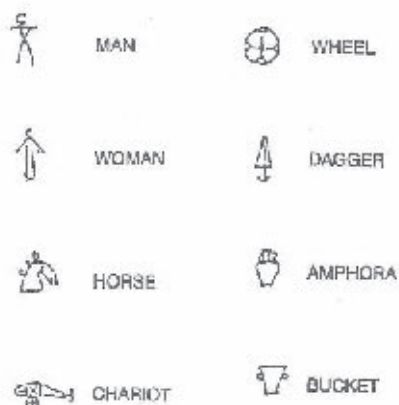


Fig. 14:6. Above: A clay tablet from Knossos. The three signs to the right are "horse", "chariot", and "tunic". Below: Eight Mycenaean graphical signs with translations (from Chadwick 1976).



between China and Scandinavia. Fig. 14:6 shows a clay tablet from Knossos with *Linear B* text. Also in this case one is struck by the similarity with Scandinavian rock art. Man, tunic, dagger, horse, chariot and wheel – that is the same type of depictions as in Scandinavian rock art. Such motifs obviously signify wealth, status and strength.

There can be no doubt that myths and central concepts in life and religion are reflected by South Scandinavian rock art. But the primary sense of the various rock art motifs seems to have been rather analogous to the Mycenaean graphical signs. Of course south Scandinavian rock art motifs are not graphical signs, and in all probability they are not copies of Greek pictures either. But they belong to the same intellectual, cultural and social sphere. And to this sphere also the weight systems belong.

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## IV.

### Fieldwork

THE LUND UNIVERSITY HISTORICAL MUSEUM was the university's teaching institution for archaeology during Mats P. Malmer's early career. Archaeologists there participated in and, from an early stage, directed their own excavations in contract archaeology and other heritage management of ancient monuments and finds in Skåne. It was taken for granted that all excavations should be presented and evaluated in print. Mats P. Malmer's first two publications are two of these printed reports interpreting excavations: one about a Medieval hospital (Ch. 15) and the other about a Bronze Age barrow. While working in Carl-Axel Althin's Mesolithic laboratory, Malmer had the task of publishing one of the first finds showing conclusively that microliths are parts of composite arrowheads (Ch. 16). Through the years there would be many publications of this kind, giving Malmer broad empirical experience (see Section VI of this book).

Malmer's last major fieldwork campaign was the multidisciplinary investigation of the Alvastra pile dwelling in 1976–1980. He never wrote any final summary synthesizing the entire project. The text printed here is an unfinished chapter from a manuscript written around 1995 about the ideas that had guided the plan of the fieldwork and laboratory studies. Marginal notes with question marks and exclamation points in the original manuscript show that it was not intended for publication in its present form. It must be read with that in mind and cited with caution.

One problem for the group that planned the Alvastra project under Malmer's leadership was how to relate to Otto Frödin's excavations of 1908–30. There was extensive high-quality documentation and a large number of finds, although they had never been fully analysed. Malmer recommended digging first and then, using the experience gained from the new excavation, confronting Frödin's excavation reports when it would be easier to understand them. Others in the project group thought they would be able to dig better if they had first made a thorough study of the old reports (Hans Browall, pers. comm. 2013). The manuscript printed here is in many respects a reconstruction after the event. It

contains ideas and questions written after the fieldwork was completed, and they are far from being fully thought through.

The manuscript about the Alvastra pile dwelling is printed here with only slight editing. The two figures are, it is hoped, the ones that Malmer had intended to include in the chapter of the publication that never came about.

Most of the multidisciplinary work by the various participants in the project was completed. A selection of publications in English is presented below together with Malmer's articles in English (see also Section VI in this book):

- Bartholin, T.S. 1987. "Oak and willow". Active and passive periods at Alvastra pole dwelling. A result of dendrochronological and wood anatomical investigations. In: Burenhult, G. et al. (eds), *Theoretical approaches to artefacts, settlement and society. Studies in honour of Mats P. Malmer*. BAR International Series 366 (i) (pp. 123–132).
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- 2002. *The Neolithic of South Sweden. TRB, GRK, and STR*. The Royal Swedish Academy of Letters History and Antiquities, Stockholm (Ch. 3.3.2, The Alvastra pile dwelling).

Hans Browall, Malmer's closest collaborator in the Alvastra project, has published four monographs about the Alvastra pile dwelling. The first was his doctoral dissertation with a lengthy English summary (Browall 1986, 2003, 2011, 2016). At the time of writing the 2011 monograph is the best available summary of the Alvastra project.

It is perhaps apt that while Malmer's first fieldwork of his own, the excavation of the Medieval hospital at Åhus, was performed and analysed in the same way as a Stone Age settlement site, his last fieldwork, the excavation of the Middle Neolithic pile dwelling at Alvastra, was performed and analysed in the

same way as a Medieval town centre and informed by the idea of Harris matrices (Harris 1977, 1979).

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## Sankt Jörgen's hospital in Åhus

1948

LEPROSY HAS BEEN known since ancient times in the Orient, where it is still widespread today. In Europe there were sporadic cases of leprosy in the Early Middle Ages; Carolingian laws from the eighth century include rules for how to deal with lepers (Uhlhorn 1882–1890:251, note 1). In the course of the 12th century the disease was rapidly spread all over Europe by returning crusaders, and as we shall see, it was in a more severe and infectious form than before or subsequently in modern times. It was understood early on that the disease could only be combated by isolating lepers. Through strict and carefully maintained regulations, people with the disease were forced to live in secluded places reserved solely for them, outside towns and villages, or in the more permanently organized hospitals established during the 13th century in every European town or city of any importance (Uhlhorn 1882–1890:261). In 1244 the estimated number of these hospitals in Europe was 19,000, but it should be noted that many of them were quite small; the number of inmates was often twelve, the same number as the apostles (Uhlhorn 1882–1890:251). These establishments, unlike modern hospitals, did not provide any health care or treatment. Their religious character dominated, as they were organized more or less on the pattern of the monasteries. When we read about the “brothers and sisters of Sankt Jörgen's hospital” in the charter that King Erik of Pomerania issued to protect the hospital in Åhus, this is probably a reference to the lepers themselves, regarded as a monastically organized unit.

It was the duty of the superintendent of the hospital to ascertain whether there were any lepers in his district. The rules employed to determine whether a suspected case actually had leprosy or not were the statutes of Mosaic law, Leviticus Ch. 13 (cf. Uhlhorn 1882–1890:255 f.). A person who was proclaimed a leper lost all his civic rights: he was not allowed to administer his property, he could not plead his own case in a court of law, and his inheritance rights passed to his heirs. If he could not afford to build a house in which he could be isolated and pay a large sum to the hospital, he had no choice but to become an inmate of the hospital, which he could then not leave on any pretext. The cer-

emonies conducted to proclaim the civic death of a leper admitted to hospital were, at least in some periods, extremely macabre; for instance, he had to undergo a symbolic burial (cf. Hildebrand 1885–1887:208).

In Scandinavia, as in the rest of Europe, there was usually a hospital in every major town. Hospitals in Medieval Sweden are generally first attested around 1270 or slightly later (Hedqvist 1893:63 ff.). Without exception they bear the name of Saint George, in the Swedish form Örjan or Jörgen. While these earliest hospitals were solely intended for lepers (*hospital* is the source of the Swedish word for leper, *spetälsk*), in contrast to the “houses of the Holy Spirit” (*helgeandshus*) which admitted other sick people and paupers, the later hospitals, such as the one founded in Stockholm at the start of the 15th century, were meant for people with other diseases than leprosy (Hedqvist 1893:91). This also applies to Sankta Anna’s hospital in Åhus, founded in the 1520s. Several of these later hospitals are also named for Saint George, as in the case of the one in Stockholm.

We know from written sources that five towns in Skåne had leper hospitals: Lund, Landskrona, Ystad, Åhus, and Tommarp. In accordance with the regulations, they were all located outside the town and were called, at least in the late Middle Ages, *Sankt Örjans* or *Sankt Jörgens hospital*, even when the church to which the hospital belonged had originally been dedicated to some other saint. The church of Sankt Jörgen’s hospital in Lund, for example, had originally been dedicated to Saint John (Blomqvist 1949a:122). The one in Lund seems to be the oldest, probably founded around 1200 (Hedqvist 1893:128). The hospital in Åhus is first recorded in 1252, the one in Tommarp in 1285, but those in Ystad and Landskrona are not attested until the 15th century (Hedqvist 1893:134 ff.). From this we cannot draw any certain conclusions about the ages of the different hospitals, but it does not seem unreasonable to assume that Åhus, probably the biggest town in eastern Skåne in the Middle Ages, was among the first to acquire a hospital for lepers. Another contributing factor in this may have been the close links between the town and Lund with its archiepiscopal see, after Åhus had been granted in fief to the archbishop of Lund in the mid-12th century. Archaeological excavations have been conducted in recent years at the hospitals in Lund and Landskrona. The excavations at the former confirmed the great age of the establishment; results have not yet been published from either of these excavations.

Some 500 m west-south-west of Västerport, the west gate in the old town wall around Åhus, is the area that has traditionally been pointed out as the site of Sankt Jörgen. In the summer of 1946 an excavation was undertaken here by Lund University Historical Museum, under the direction of the author. [The author would like to express his gratitude to all those who helped with the ex-

cavation or in other ways facilitated the work, particularly Bengt Järre, BA, who took part in most of the excavation and rendered invaluable assistance.] The excavation exposed the badly damaged foundation walls of a church and some other vestiges of walls. The archaeological dating of the structure must be based mainly on the coins retrieved: a total of 197 coins from all the parts of the church. The oldest coin is from the reign of Valdemar Sejr (1202–1241). This is followed by two coins of Erik Plogpenning (1241–1250), nine of Christoffer I (1252–1259), twenty-nine of Erik Klipping (1259–1286), and so forth. The youngest coin is a *søsling* of Frederik I from 1525. The coins form a continuous series from the start of the 13th century to 1525, but the majority come from the 13th and 14th centuries. The numismatic dating of the excavated church thus agrees fairly well with the information in the written sources about Sankt Jörgen in Åhus. The oldest document mentioning the hospital is a charter of Christoffer I of 24 December 1252 in which the king releases all those pertaining to the hospital from paying the naval levy and all fees to the crown. Charters concerning Sankt Jörgen have generally not survived, neither the original documents nor the wording. The relatively good knowledge that we nevertheless have about a number of these charters is due to the fortunate circumstance that a list survives (albeit only in a copy belonging to the Royal Academy of Letters, History and Antiquities, Stockholm) of documents that “Claus Hanssøn transferred to Niels Lauritsøn here at the aforementioned Åhus hospital on Saint Philip and James’s Day 1600” (DA:783). The royal charter of 1252 is one of the few documents preserved in its entire wording (DA:78).

Similar letters of protection or exemption were issued by Erik Klipping in 1268, Christoffer II in 1320, Magnus Eriksson in 1339, Valdemar Atterdag in 1361, Margareta in 1389, and Erik of Pomerania in 1410. A number of archbishops’ letters concerning the hospital are known as well, the earliest being Karl Eriksson’s charter of protection of 1327, the latest Birger Gunnarsson’s letter of 1499. For the sake of completeness we may also mention three wills leaving bequests to the hospital, two bills of sale, and a papal charter of protection issued in 1500 by Alexander VI. Erik of Pomerania’s charter of 18 May 1410 (SD 1298) speaks of “brothers and sisters of Sankt Jörgen’s hospital in Åhus” (*sancti Yrjans hospitali i Åus*).

Within the town of Åhus itself there was an institution, a house of the Holy Spirit, for people with other diseases than leprosy, first attested in 1393. It is seldom mentioned in the sources, but we know that it still existed in the 1520s. It is known that the economic foundation for institutions of this kind was removed through the Reformation. What happened to the two establishments in Åhus is unknown, but we do know that a new hospital was founded in Åhus in

1524, on the site of the old house of the Holy Spirit. The new hospital probably had a different economic structure from its predecessor. The founder and superintendent was the well-known Claus Jenssen Denne (DL; Weibull 1945:49 f.), who set up hospitals in several places in the kingdom of Denmark during the 1520s; they all bear the name Sankt Anna's, including the one in Åhus. It is likely that leprosy was receding all over Europe in the 15th century. The fact that the word *hospital* from this time did not refer solely to a place for lepers may be taken as an indication of this. Combined with the changed economic position that the Reformation may have entailed for Sankt Jörgen's hospital, it meant that it must have seemed natural to combine the old leper hospital with the new Sankt Anna's hospital, which was no doubt financially stronger. This happened through a royal decree, issued by Christian III in 1549. The decree reads (DL:402):

We Christian granted [...] that when Lord Oluf Ipsen, who holds the fief of our and the Crown's hospital Sankt Jörgen's outside the aforementioned Åhus, is dead, then this hospital with all the farmers and servants pertaining thereto shall be and remain with the hospital in Åhus [...] Kollinghus, the Eve of Saint Matthew the Apostle [= 23 February].

Through this decree, the king donated Sankt Jörgen's hospital with all its belongings to Åhus hospital (Sankt Anna's), although with permission for the then fief holder to retain the hospital during his lifetime. Sankt Jörgen's at this time had probably already ceased to function as a hospital.

The written sources thus tell us that Sankt Jörgen's hospital existed for 300 years; it was founded before 1252 and closed in 1549. The oldest coin excavated in the church outside Åhus in 1946 was from 1241 at the latest and the youngest from 1525. This good agreement between the written and the archaeological evidence could perhaps be taken as proof for the assumption that the excavated site is identical with Sankt Jörgen in Åhus. Other facts would support that assumption. We have already mentioned the location, *outside* the wall of Medieval Åhus. The area where the excavated church is located is generally known as *Spetarelyckan*. This name goes back a long way, as we see from the land survey map of Åhus from 1770 (fig. 15:1), depicted in a copy from 1853, where the name is changed in pencil to *Spetalelyckan* ('Hospital Enclosure'). There can be no doubt that it is the memory of a hospital that is preserved in the name Spetarelyckan. To cite just one parallel, there is the name Spetelöv for the site of Sankt Jörgen in Lund. The place that is called Spetarelyckan on the 1770 map of Åhus is an irregular quadrilateral measuring about 140 by 130 m and bordered on one side by "poor sandy land belonging to the town" and on the other by the "west-

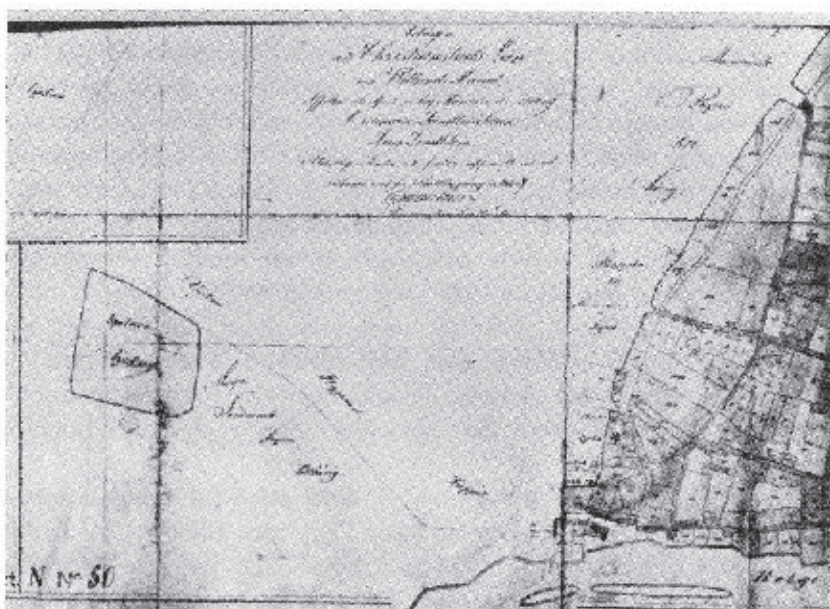


Fig. 15:1. Detail of the land survey map of Åhus in 1770, in a copy from 1853.

ern drift-sand patch". It can further be observed that a somewhat irregular rectangular figure is shown in the north-east corner of the area. This figure is undoubtedly the church which still stood as a ruin, at least in the 18th century. The length of the ruin, according to the map, is 17 m, which corresponds almost exactly to the length of the nave in the excavated church. The reason the chancel is not shown on the map will be considered below. The irregular form given on the map to the northern part of the church could be interpreted as meaning that only a portion of the surviving extension (the "sacristy") remained, in very dilapidated condition.

Spetarelyckan as shown on the map from 1770 was maintained as a single unit until very recently; it was only about twenty years ago that it was first divided. Now seven plots have been parcelled off from the area and five of these have had houses built on them; only the westernmost part is still open arable land. The author has had no opportunity to undertake any detailed studies of the ownership of Spetarelyckan at different times. It has been found, however, that at the time of the *laga skifte* enclosures in 1842 the area belonged to Kristianstad hospital, which would support our theory since the hospital of Saint Anne in Åhus, with which Sankt Jörgen was amalgamated in 1549, was subse-

quently merged with the Kristianstad hospital when Åhus lost its chartered status as a town. Today, as in 1770, the area between Spetarelyckan and Åhus is mostly poor sandy soil unsuitable for tillage. Spetarelyckan itself, on the other hand, has been under the most intensive cultivation for a number of decades, used for growing tobacco. The better soil of Spetarelyckan can only be explained by the Medieval occupation layers covering the area. Over the years a large number of Medieval objects have been found in the soil of Spetarelyckan, but not outside its boundaries. Despite the recent plot divisions in and outside Spetarelyckan, its boundaries according to the 1770 map are still clearly distinguishable. Along this boundary there is a stout fence, supported on the inside by an earthen rampart, and in several places it is drystone walling rather than ordinary fencing. To the east and south, some stretches of the wall have been demolished and replaced by other types of enclosure; at the south-east corner it can still be discerned as a low earthen bank. It is worth noting that the ground surface inside the wall at several places is considerably higher than outside. This wall is evidently of great age. Its character cannot be properly determined until it is excavated, but it seems to be more than just a possibility that we have here the wall that surrounded the site of Sankt Jörgen's hospital in the Middle Ages. A Medieval leper hospital, as a rule, covered a large area surrounded by a wall. In the absence of a description of a Scandinavian hospital we may cite Uhlhorn's words about leper hospitals on the Continent (1882–1890:262):

They lie outside the town and form a yard surrounded by a wall. Inside the wall there was also a church and a cemetery, so that they were completely cut off from the rest of the world.

We cannot attain full certainty about the character of the large area south and west of the church excavated in 1946 and within the walls of Spetarelyckan until some of the parts still without buildings have been excavated. All the circumstances cited above, however, should leave us in no doubt that the church excavated in 1946 really is identical with the church of the *domus hospitalis leprosorum iuxta Aos* – the leper hospital next to Åhus. The Latin words are quoted from Archbishop Peder Jönsson's letter of 27 July 1336 (SD 3240).

\*

As mentioned above, the areas west of Åhus used to be an expanse of drift sand, now transformed into meadow land or under cultivation. On the site of the 1946 excavation, under 25 cm of topsoil, there was a deep layer of drift sand, the



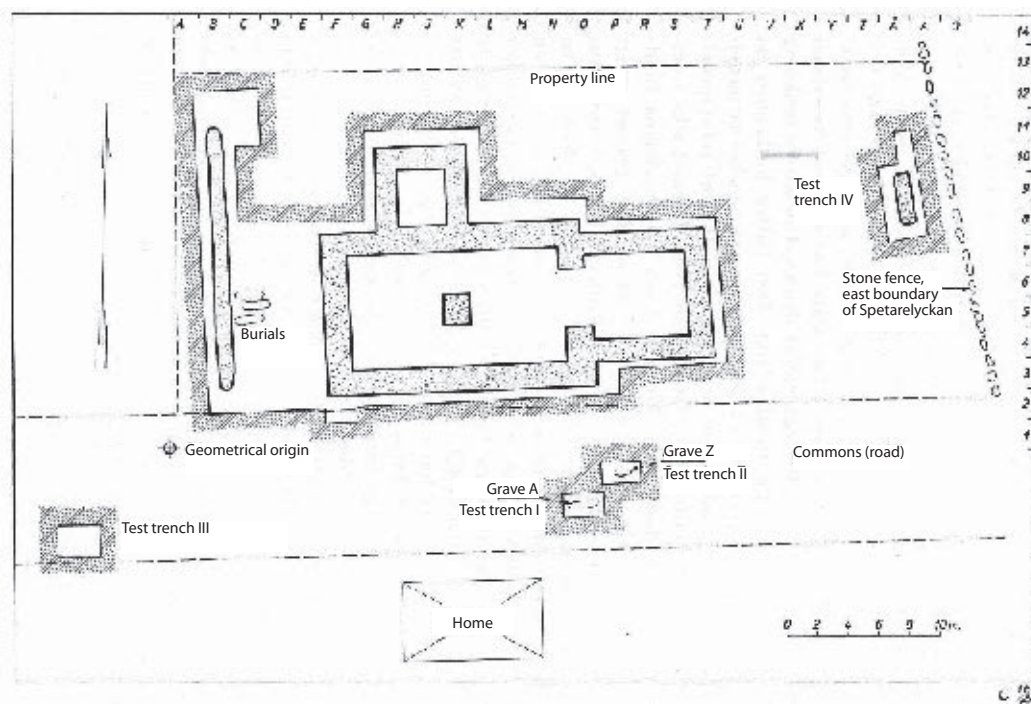
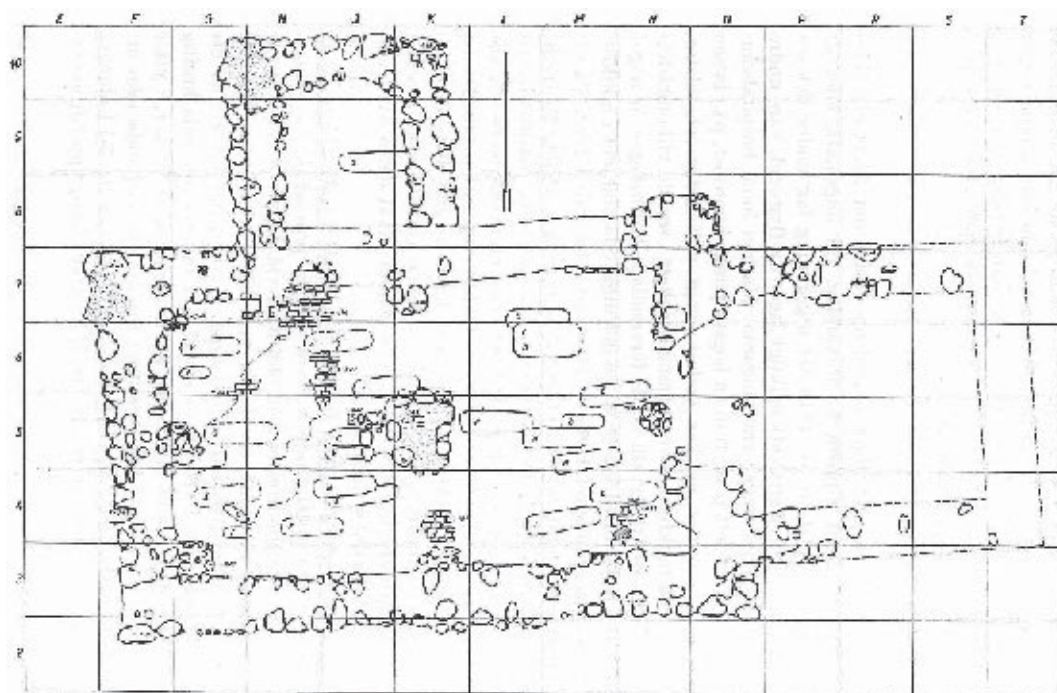


Fig. 15:2. Plan of the excavated area.

bottom of which was not reached by the excavation. The ground inside the hospital area is mostly flat; from the highest point in the north-east, the site of the church, it slopes very gently to the south and west. It is conceivable, however, that some levelling was done in modern times when the area was cultivated, and that the occupation layer which has turned into humus is somewhat thicker to the south and west than in the excavated area in the north-east. The undisturbed sand layer was of a consistent white or yellowish-white colour. At a depth of about 50 cm under the floor of the church, however, there was a 20 cm thick layer of black sand. This layer, which was found at exactly the same depth over the whole excavation site, was completely without finds and probably represents an old vegetation zone. This vegetation period must have been before the church was founded and was followed by a covering by drift sand; it could be observed in several places that the foundation walls of the church were cut through both this upper white layer and the layer of black sand.

The excavation involved exposing an area of approximately 35 by 15 m and digging four smaller test trenches (fig. 15:2). The building remains uncovered were sparse and badly damaged, but sufficient to admit of a reconstruction in broad outline. The church consisted of a nave with external dimensions of

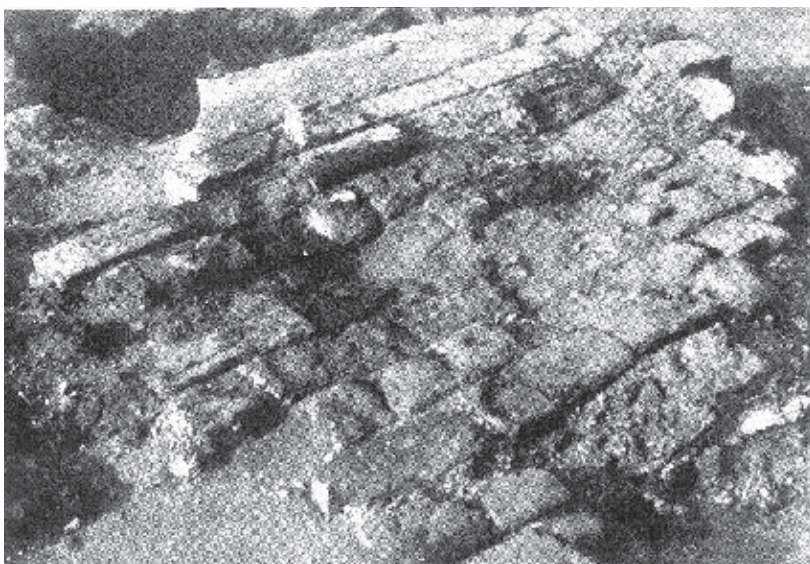




*Fig. 15:3. Plan of the church marking the graves.*



*Fig. 15:4. The church ruin viewed from the west.*



*Fig. 15:5. Part of the brick floor of the nave (in excavation square H7).*

about 17 by 11 m, with a single stout pillar in the middle, and an almost square chancel with a straight back wall, with external dimensions of 8 by 8 m. To the north there was also an extension, perhaps not original, measuring 6 by 5 m (fig. 15:3). Generally speaking, only the foundation wall is preserved, made of large unworked stones, up to a metre in length (fig. 15:4). The only traces of the lowest course of bricks were found at the north-west corner of the northern extension and at the central pillar. At the north-west corner of the nave there was a bonded course of uncut natural stone. Of the chancel walls, only four metres of the north wall and insignificant remains of the south wall survive, adjacent to the nave. It was nevertheless possible to determine the position of the chancel walls with complete certainty, as the demolition had left distinct traces in the form of deep hollows filled with rubble in the undisturbed sand. Small vestiges of the floor of the nave are preserved in three places. The floor is of the very simplest kind: bricks laid in a layer of mortar right on top of the undisturbed sand (fig. 15:5). The floor is at a level of about 40–50 cm above the highest points of the surviving foundation walls.

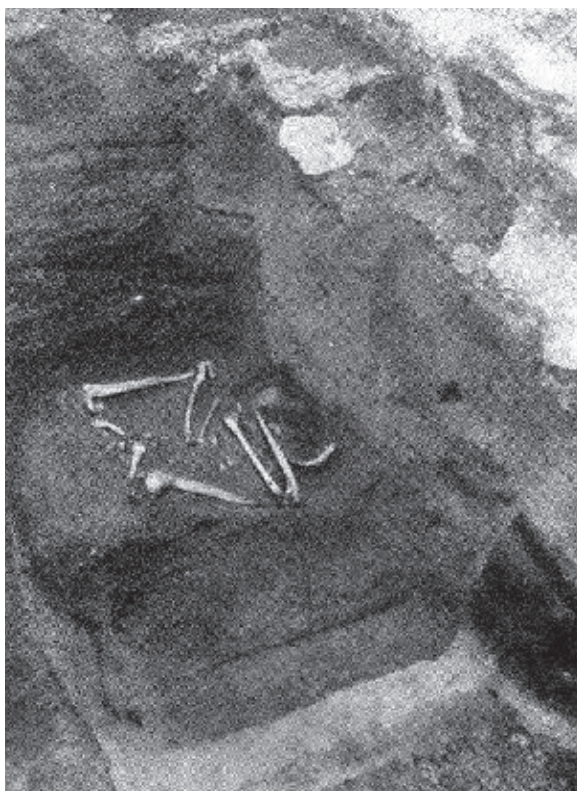
The demolition of the church was done thoroughly. That the chancel suffered most severely is perfectly understandable in view of the location of the church: the chancel is nearest to the road into Åhus. Judging by the 1770 map, the chancel had already been demolished by then. Otherwise the final destruc-

tion of the church probably did not take place until the latter half of the 19th century. The demolition did not only involve tearing the walls down to the foundation and ripping up the brick floor; any material that could be reused was carefully removed, so that the rubble contained strikingly little brick, in all only about 250 measurable bricks and a small number of roof tiles, all in fragmentary state. The very poor state of the church means that it is not even possible to say with certainty what material it was built of. As for the central pillar in the nave and the northern extension, there can be no doubt: they were of brick. But this does not mean that the nave and chancel were also built of brick. On the contrary, one circumstance concerning the west gable of the church suggests something different. West of the west wall the excavation uncovered a large number of big stones, most of them unworked but a few cut to shape (fig. 15:8), lying in an area whose length corresponded to that of the west wall and with a maximum width (at the south end) of 4 m. The stones were generally lying separately, but were occasionally joined by vertical layers of mortar. In most cases they had traces of mortar on their vertical sides, and their maximum length was consistently parallel to the west wall of the church. This accumulation of stones was at a level corresponding to the floor of the church. This is probably a surviving section of the west gable of the church (with its maximum height at the south-west corner of the nave), which collapsed on some occasion or else was deliberately knocked over towards the west. In this section, then, the nave was built of stone, at least to a height of 4 m. The question arising from this is whether the northern brick extension is a later addition, younger than the church itself. A crucial factor for this question is the interpretation of a grave (designated with the letter S on the plan in fig. 15:3) which was found in squares I9–K9. This grave, which consisted of a 2 m long pit with perfectly vertical walls in which the corpse was placed without a coffin, was in the eastern part of the northern extension, located in such a way that its eastern part together with the extremities of the deceased was *under* the east wall of the extension to a length of 20 cm. None the less, the eastern part of the grave was also dug with perfectly vertical sides. One of the finds in this grave was a coin minted in Lund during the time when Magnus Eriksson ruled Skåne, 1332–1360 (no. 126 in the list of coins). The circumstances of the burial cannot be said to prove that the grave is older than this part of the church, but in my opinion it is at least probable that this is the case. It would follow from this that, since grave S is at least a hundred years later than the oldest parts of the church, this would also be true of the extension. Unfortunately, not a single coin was found in the extension apart from this one, whereas there were numerous coin finds in the chancel and the nave. The absence of coins in the extension is nevertheless support for our theory in-





*Fig. 15:6. Foundation of the altar west of the central pillar in the nave.*



*Fig. 15:7. Grave U (partially excavated) and grave V, located under the altar at the central pillar.*

asmuch as the number of coins from the last centuries of the church is much smaller than in the case of the older coins.

As regards the purpose of this extension, only some suppositions may be put forward. It would appear most desirable to assume that it is a sacristy. What is striking, however, is its location so far to the west. As an attempt to explain this location we can only cite one circumstance, although it is an important one. Immediately to the west of the central pillar in the nave, at a level exactly corresponding to the brick floor of the church, there was a foundation built of dressed limestone blocks (fig. 15:6). The north part of the foundation is preserved best, whereas the southernmost part is entirely removed; the original length was probably about 1.5 m and the width 0.75 m. At the north end of the foundation, at the level of the floor, there is a limestone slab which indicates fairly certainly that the brick floor at this place was replaced with limestone slabs. This foundation can scarcely be anything but the podium for an altar. The church's main altar was undoubtedly in the chancel, although no trace remains. The altar by the central pillar is certainly secondary, for under it there are no less than two graves, grave V, which is oldest and is a coffin burial, and grave U, which was dug later and thereby partially damaged the older grave (fig. 15:7). Only the westernmost parts of these graves were excavated, but without yielding any finds to indicate their age. What justifies us in associating the "sacristy" with the altar at the central pillar is that both appear to be secondary additions and that the prolongation of the central north-south line of the presumed sacristy meets the altar precisely. This brings us to a question about which little is known, namely, the use of the church, and what groups of people, besides the lepers, may have used the church for services. One naturally thinks of the functionaries of the hospital, of the population of the surrounding countryside, of wayfarers, and of other categories of sick people than lepers. To put it briefly, when the number of lepers in the hospital probably decreased in the 15th century, the church may have been opened to other kinds of churchgoers, leading to the construction of the altar at the central pillar and of the "sacristy".

Some other details of significance for the history of the church deserve mention. In the north-west and south-west corners of the nave there are square stone settings with sides of more than one metre. They are made with much bigger stones than the other foundation walls and were not dug down as deep (fig. 15:8). They are obviously secondary. Corresponding to these are two similar stone settings, probably rectangular, although now badly damaged, at the mid-point of both the long walls. These stone settings are probably foundations for vault pilasters. Admittedly, there are no counterparts in the north-east and south-east corners of the nave, but this is probably because the eastern part of



*Fig. 15:8. Square stone setting in the south-west corner of the nave.*

the church was more thoroughly demolished, combined with the fact that these stone settings are positioned higher than other foundation walls. It may be interposed here that the state of the foundation walls also prohibits an exact determination of the width of the chancel arch. More enigmatic than these stone settings is the circular arrangement of big stones, measuring 1 m in diameter, which was found in the middle of the chancel arch, shifted so much to the west that its central point is in line with the inside of the wall between the nave and the chancel. It has a counterpart in a semicircular stone setting half-way along the west gable wall (fig. 15:9). Both these stone settings are placed lower than the foundations of the vault pilasters mentioned above, but higher than the other foundation walls. All these stone settings lead us to the question what the roof of the church was like. The fact that the vault pilasters are secondary leaves us with just two possibilities, namely, that the church originally had an open roof truss or that it had a wooden ceiling. In view of the foundation date of the church, the latter seems more probable. At a later stage the vault pilasters were built, presumably to hold up a vault. Although there was no sign in the rubble of any rib bricks, or indeed any kind of specially shaped brick, the occurrence of foundations for vault pilasters makes it unlikely that the work was not completed through the construction of a vault. It seems difficult, however, to deter-





*Fig. 15:9. Semicircular stone setting half-way along the inside of the west gable wall of the church.*

mine the structural function of the round stone settings. It is impossible to imagine a stone pillar placed in the middle of the chancel arch, or a pilaster at the west gable wall since this, as we shall try to prove below, had a window. The sturdy construction of these stone settings nevertheless shows that they were intended to support a considerable weight. The question of their function must remain open, but a hypothesis may be put forward here. It is conceivable that foundations like this could have been used to support wooden pillars. A possible structural task for wooden pillars could have been to support a wooden ceiling, either temporarily during the construction of the ceiling, or permanently, or – most probably – when the roof needed supporting at some time, for example, when a vault was being built.

The fixed points in the history of the construction of the church that we have been able to discover are very few. If we sum up what has been said above, we get the following main periods, and it must be repeated that most of this should be regarded as hypothetical.

1. The nave and chancel were built shortly before 1252.
2. The “sacristy” was added and the altar was installed beside the central pillar, probably after 1360.
3. Vault pilasters were built and a vault was constructed.

We repeat that the nave and chancel were mostly made of stone, probably also with smaller structural details of brick. The floor was made of brick, with lime-



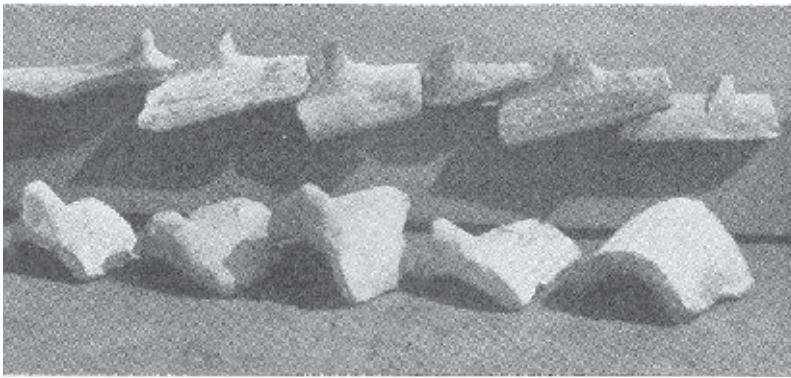
stone slabs in some places. The “sacristy” was built of brick. The decline of the church can be summed up as follows:

1. Fixtures and furnishing were removed, probably at the start of the 16th century.
2. The chancel and most of the “sacristy” were demolished before 1770.
3. The remainder of the church was razed to the ground in the second half of the 19th century.

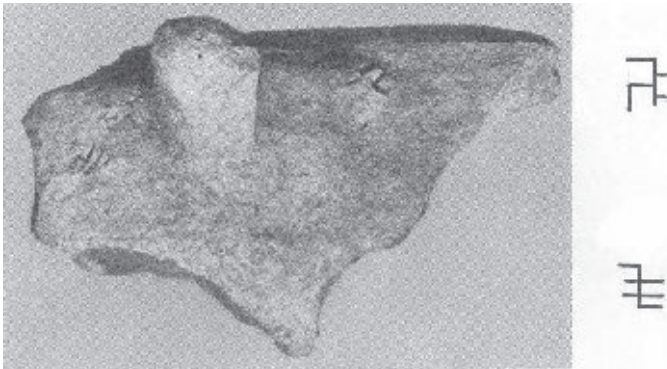
The bricks that were found among the rubble were generally of a deep red colour with mean dimensions of approximately  $25 \times 12 \times 8$  cm. The floor bricks were of a slightly different type: more of a warm yellowish red and larger, on average  $28 \times 14 \times 8$  cm. Some bricks had one broad side with deep footprints of dog and domesticated pig (det. Senior Curator, Docent Hervéd Berlin). Prints like this are not uncommon on Medieval bricks. They arose when the newly cast bricks were laid out to dry. The drying was done on the bare ground, not on a smooth surface, as is evident from the noticeable roughness of the underside in contrast to the other sides. Fourteen bricks were worked in such a way that one end on both the small sides had been chopped to a point, obviously to allow the building of a portal arch or the like. Seven of these pointed bricks were found in squares H4 and I4. This is not without interest, since these two squares correspond to the place where the south portal of the church ought to have been located according to the Medieval pattern. All the roofing tiles that were found were fragmentary. They were of the normal arched Medieval type (“monk and nun” tiles); the variations in form can be seen in fig. 15:10. One of the tiles is of interest in that two marks are stamped on it, one on either side of the knob on the upper side (fig. 15:11). These must be the marks of the brick-maker or the master builder, or both.

The finds reveal yet another detail in the design of the church, namely, the windows. Pieces of window glass were found over much of the church and immediately outside the walls, in total almost 700 fragments. The majority of them were undecorated, slightly green or yellow glass, or completely opaque as a result of chemical changes. Only about fifty are fragments of stained glass. The relatively small amount of glass is explained by the thoroughgoing destruction of the church and by the poor condition of the glass. A great deal of glass must have disintegrated; this applies in particular to the stained glass, which is generally in poorer shape than the undecorated glass.

The location of the windows is of primary interest. If we place all the glass finds graphically on a plan of the church to ascertain this, however, it turns out



*Fig. 15:10. Types of roof tiles.*



*Fig. 15:11. Roof tile with two stamped marks (right).*

that glass was found in virtually every part of the church and in roughly equal quantities in every excavation square. Only the chancel and the westernmost part of the church show larger than average amounts of glass. With another method, however, we can arrive at a more exact knowledge of the location of the windows. Some of the pieces of glass were cut to shape to fit the lead came that held the glass in place. The number of pieces retouched in this way is about 250, while the number of unretouched pieces is almost 450. If we now take the plan of the church and mark the squares in which the unretouched pieces outnumber the retouched ones, and use a different marking for the squares in which the retouched pieces of glass are in the majority, we get a picture like that in fig. 15:12. In most squares (58 in all), of course, the unretouched pieces are in the majority, but a number of squares (15) have a majority of retouched pieces. The location of the areas in which the retouched pieces are in the majority seems to be associated with the placing of the church windows. We find one such area lying like a belt across the midpoint of the west gable, two squares across the

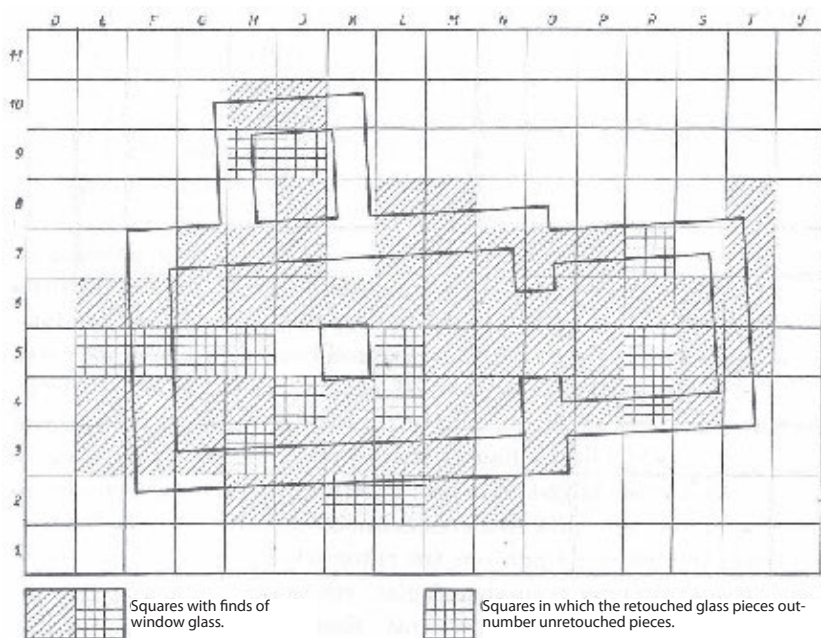


Fig. 15:12. Plan showing the distribution of retouched and unretouched window glass.

south wall of the chancel and one over the north wall, then two squares across the west wall of the “sacristy” and a belt across the eastern section of the south wall of the nave. This seems to agree quite well with the places where one would expect to find windows in a church of such a simple plan: in the middle of the west gable, in the middle of the south and north walls of the chancel, in the west wall of the “sacristy”, and in the eastern part of the south wall of the nave. The grounds for this correspondence are, of course, the following: the pieces used in a stained glass window are retouched all round. “Retouched glass pieces” are thus pieces that were fixed in the lead comes, while “unretouched glass pieces” are parts of a pane which was nowhere near a came. During a period of neglect, when the windows were exposed to strong winds, hail, or other damage, the central parts of the pane were thrown furthest away from the window, while the retouched peripheral pieces fell close to the window. In at least three squares the correspondence is not good, however: square L3 ought to have contained a majority of retouched glass, and we might have expected squares H3 and I4 to have a majority of unretouched pieces. Yet there are many potential sources of error

in the excavation: the amount of glass, 700 pieces, is far too small, and the squares ( $2 \times 2$  m) are far too big to permit a wholly correct result with the method used here.

Apart from the large mass of plain glass there are, as we mentioned above, a number of fragments of stained glass and some pieces of coloured glass without painting. The latter display the colours deep blue, emerald green, and yellow. The grim fate suffered by the church should be obvious from the way the fragments, although they are few, display very different forms of composition and must necessarily represent different paintings and different periods (fig. 15:13). This need not mean that the church had all these stained glass windows at once; one window may have been destroyed and replaced by another. A suspicion to this effect is raised in particular by the few shards of the type in fig. 15:13:1, 2. These two, along with fig. 15:13:10, are very similar in composition to the "Folkunga window" in the Dominican monastery in Lödöse (af Ugglas 1931:266 ff., Pl. II:6–7, 9). In these three shards the glass is a slightly bluish-green colour; the colour of the shards in fig. 15:13:1, 2 can no longer be determined because of their badly decomposed state, but on the better-preserved shard in fig. 15:13:10 it is a dark brownish-red. The Folkunga window is dated by af Ugglas (1931:272) to around 1325; these three shards show such a close correspondence to its composition that one is almost tempted to assume that they are the work of the same master. The agreement in style between the Folkunga window and these shards from Sankt Jörgen seems much greater than that between the Folkunga window and the parallel cited by af Ugglas from Riddarholmskyrkan (cf. M. Olsson 1937, fig. 57). At any rate, the difference in time must be negligible. All the shards of this style, with one exception, lay in the south-easternmost part of the nave. It must be said in general that the scarcity of fragments of stained glass, combined with the extensive destruction suffered by the church, does not allow us to attribute the different composition styles to specific windows in the church. If our hypothesis concerning these shards is correct, namely, that they belonged to a stained glass window which was destroyed while the church was still in use, the matter should perhaps be viewed in a different light. In such circumstances there is no reason to assume that the shards were widely spread in the church, and so we might dare to presume regarding this style that it was found in the window in the eastern part of the south wall of the nave.

The style represented by the shard in fig. 15:13:3 likewise has good parallels from both the Dominican monastery in Lödöse and Riddarholmskyrkan in Stockholm. Both Olsson (1937, fig. 57:i) and af Ugglas (1931, Pl. II:11, fig. 82) date this style to the 14th century. As is evident from the fragments from these two churches, the decoration here consisted of large, symmetrical, flatly spread

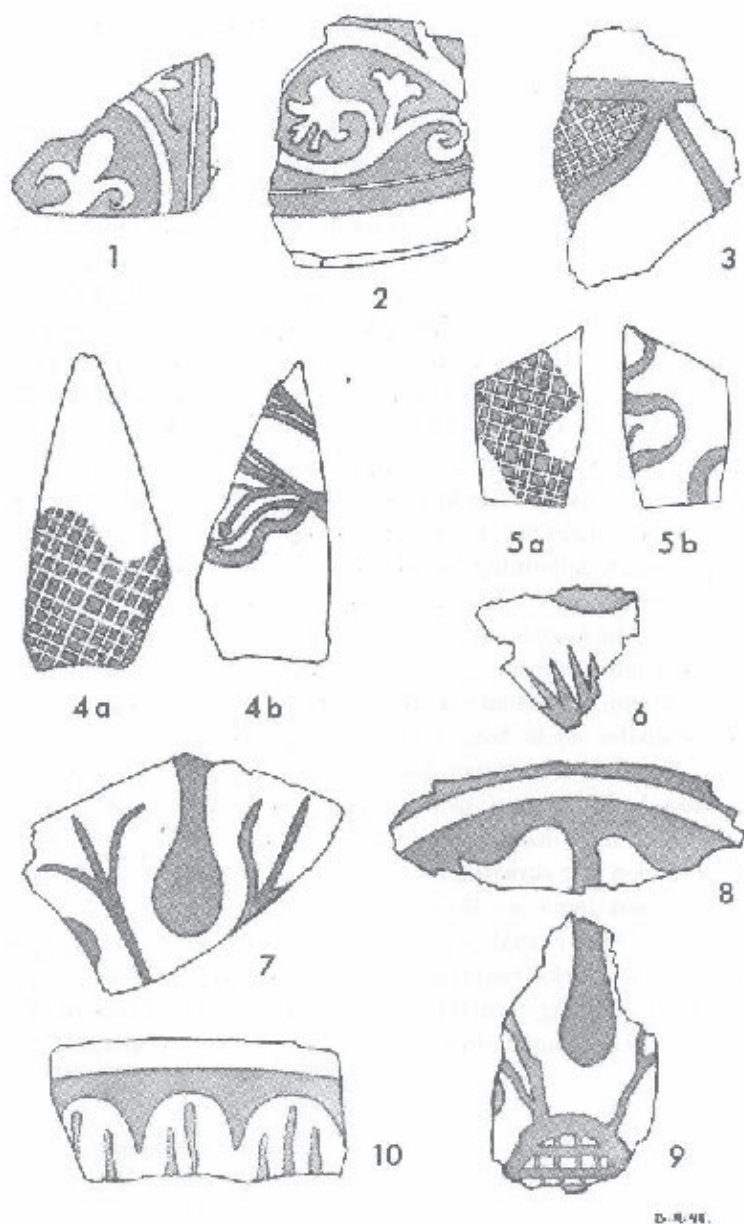


Fig. 15:13. Fragments of stained glass. Tinted drawings by Brita Alenstam.

leaves with the edge cut into several large lobes. The sections outside the leaves were hatched, which was done by adding an even layer of paint in which a fine mesh was then made using a sharp tool. The colour of the decoration is red; the



colour of the glass cannot be determined. The hatching on the numerous shards of the type in fig. 15:13:4, 5 was done in a different, highly unusual manner. Here the hatching was done on one side of the glass (most probably the outside) while the figures were drawn on the other side (the inside). The retouching of the glass pieces was done solely from the outside, which has allowed us to determine which way the glass faced. In contrast to the almost geometrically stiff composition type in fig. 15:13:3, this is a real figure drawing, which takes the form of naturalistic branches on all the surviving fragments. That it really was a plant motif is fairly obvious from the small leaf rosette that terminates one of the branches on the shard in fig. 15:13:4. The hatching and figure drawing were consistently combined so that one of the branches follows and half-covers the edge of the hatching; it should be noticed here that the edge of the hatching is a straight line that is softened by the gentle curve of the demarcation line on the other side of the glass. The shards of this type have by far the best-preserved glass; its colour is light blue while the paint is red.

The last group that occurs in any numbers is represented by the strangely drawn shards in fig. 15:13:6–9. The shards in fig. 15:13:8–9 can probably be refitted as they are placed in the drawing; it is difficult, however, to determine the motif. Here too the colour of the decoration is red.

In connection with the stained glass, it may also be mentioned that there were murals on the walls of the church. We know nothing about their location or form; only a small fragment of plaster with red lines in lime paint reveals the existence of these paintings.

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The hospital's cemetery was mainly located south of the church. When the foundation was dug for the dwelling house 12 m south of the church, shown at the bottom of the plan in fig. 15:2, it is said that a number of graves were found. Test trenches I and II, dug south of the church, likewise revealed several graves. At a distance of 6 m from the west gable of the church, our excavation uncovered a wall running north–south for about 17 m, of which only the foundation stones were preserved. A number of graves were found east of this wall. The wall probably bounded the cemetery to the west; it should possibly be regarded as a cemetery wall. The digging of test trench IV exposed a counterpart to this wall, a fairly well preserved three-metre stretch of a metre-wide wall running NNW–SSW, 13 m east of the eastern wall of the chancel. The most likely interpretation is that we have here a fragment of the east perimeter wall of the hospital grounds, which simultaneously bounded the cemetery to the east. This frag-

ment of wall is only 2 m inside the eastern perimeter of Spetarelyckan, which at this stretch has the character of a fairly ordinary fence, although the ground level inside it is much higher than outside. It is conceivable that, when the church was demolished, a piece of the boundary wall was removed to allow free access to the demolition site. Rubble and sand were then tossed over the old wall, after which the existing fence was erected a few metres east of the older wall. No excavation was done to ascertain whether the area east of the church was used as a cemetery. The graves found outside the church were left untouched, with two exceptions, graves A and Z.

Twenty-five graves were found within the walls of the church. Their locations could be determined without difficulty, because after the removal of the sand down to the black layer mentioned above, the graves stood out as light-coloured rectangles about 2 m long and 60 cm wide. The corners of the pits were generally rounded, the sides always perfectly parallel. In cases where the body was buried in a coffin, its sides were visible as dark lines in the light sand. It should be noted, however, that some form of coffin occurred in only seven graves (J, K, O, V, Y, Ö, and in grave Z outside the church). As for the graves in which there was no form of coffin, it appeared from the artefacts retrieved that the deceased was buried in his ordinary clothes. Such finds did not occur in any of the coffin graves. The coffins were of a simple kind, consisting of four straight sides and tapering towards the feet. Only in one grave, Y, was any of the wood preserved, probably oak. The only surviving traces of the lid or the bottom were the nails with which they were attached. Graves O and Z contained no traces of the coffin sides. As regards the location of the graves in the church, it should be noted that not a single grave was found in the chancel. In the nave east of the central pillar there were 11 graves, west of the pillar 13; in the western part of the church there were also scattered skeletal remains from graves which had been damaged by digging at some time. In the "sacristy" there was a grave too, but as mentioned above, this grave was probably dug before the "sacristy" was built. In most cases it is impossible to determine how the graves were marked in the floor of the church. Concerning grave L, however, exactly above one edge of the pit there is a join between the brick floor, of which a small part is preserved here, and a fragment of a limestone slab that covered the grave. The slab did not reach all the way to the regularly laid brick, so the join was filled with small pieces of brick cut for the purpose. The grave slab was evidently flush with the floor; the surviving fragment has no trace of any decoration or inscription. The dead were placed in their graves in the conventional way: on their backs, with their arms crossed, usually over the chest.

Of the twenty-seven excavated graves, no fewer than five (G, O, P, R, and X)





*Fig. 15:14. Cranium, grave S.*



*Fig. 15:15. Crossbow arrowhead, found inside the cranium, grave S.*

were child graves in which newborn babies or infants were buried. A number of other graves contained individuals who had probably not reached full maturity. Not a few of the skeletons also had deformities evident even to a layman's eye. Unfortunately, it has not been possible to have the skeletons examined by a physical anthropologist.

Two of the graves aroused particular interest because the individuals in them had died a violent death. It may seem surprising to find, in the secluded area of a leper hospital, remains of individuals who obviously died in battle; yet it is indisputable. The skull of the person buried in grave K, probably a male although the sex of the individuals in these two graves has not been confirmed by physical anthropology, was split from the forehead to the neck by a violent blow from a sword or an axe which hit the right-hand side of the head. It must be even more certain that it was in warfare that the man buried in grave S met his death. This grave, already mentioned, contained the skeleton of a probable male whose skull had a circular hole diagonally above the right eye, from which there were deep cracks leading over the bone of the face (fig. 15:14). There can be no doubt as to what caused this injury, because there was an arrowhead inside the skull (fig. 15:15), which had thus penetrated the bone of the forehead and stopped inside the head. The arrowhead is of the type used for crossbow bolts, 8 cm long, with a socket for the shaft and square in cross-section towards the

point. There are good parallels, for example, from the mass graves at Korsbetningen (Thordeman 1939:24, fig. 134). Grave S, as we have seen, contained a coin – unfortunately not *in situ* but undoubtedly belonging to the grave – minted in Lund during the time when Magnus Eriksson ruled Skåne, 1332–1360 (no. 126 in the list of coins). Because the grave has thus been dated, through a fortunate chance, to the middle decades of the 14th century, the question that arises is what specific act of war left this mark in the secluded grounds of Sankt Jörgen in Åhus. The end of Magnus Eriksson's reign was a time of unrest for Skåne and the whole of Scandinavia. There may have been campaigns and battles of which the written sources tell us nothing. Some events nevertheless stand out particularly sharply, in connection with which the town of Åhus is explicitly mentioned. In October 1356 there was a rebellion of lords against Magnus Eriksson, in which the king's oldest son, Erik, also took part; it was Erik who issued the first call to rebel from Kalmar. On the rebels' side was the then archbishop of Lund, Jakob Nilsson Kyrning. Dissension arose, however, between Prince Erik, who now called himself king, and the archbishop, and in February 1358 there was a dramatic event which we have reason to consider. In the early hours of 5 February, Erik mounted a surprise raid on the archbishop's palace in Åhus, where Jakob Kyrning was staying, and captured the archbishop (Carlsson 1946:459 ff.). Nothing is recorded about any combat taking place on this occasion, but there is no reason to suppose that the raid happened without bloodshed. In the following years Skåne was likewise at the centre of events in which the town of Åhus played a part. The Danish king, Valdemar Atterdag, forged an alliance with Magnus Eriksson against the latter's rebellious son and conquered Skåne, after which Erik soon regained the province. In June 1359 the rebel prince died suddenly, the alliance between Valdemar Atterdag and Magnus Eriksson was broken, and Valdemar began the war to recapture the province, the war that would culminate in the conquest of Gotland and the Battle of Visby in 1361.

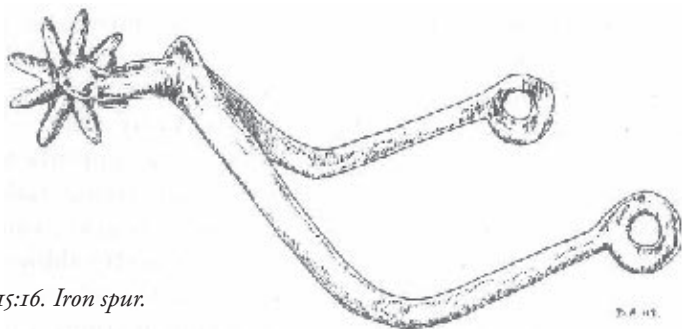
For our purpose it is sufficient to refer to the agitated events in Skåne around 1360. This is enough to explain plausibly why a man in a grave, with finds which date it to the middle of the 13th century, died a violent death. If we nevertheless pause at the events of 5 February 1358, when Archbishop Jakob Kyrning was taken prisoner, it is because these events suffice to explain yet another strange circumstance concerning grave S: that the body was buried north of the church. To be buried north of a church was a fate that at this time befell people who were of lesser worth in the eyes of the church (Rydbeck 1942:63, *passim*). The fate of the man buried in grave S is particularly striking if we compare him with another unfortunate, the man in grave K, and assume that they met their death at the same time, although there is no way we can prove this. The latter man, in

grave K, was buried in a coffin on a splendid spot in the middle of the church, near the chancel. The dead man in grave S, on the other hand, was stuck in a grave without a coffin and buried on a spot reserved for suicides and criminals. We must be permitted for a moment to give free rein to our imagination. If the man buried in grave S was one of Erik Magnusson's followers and took part in such a wicked and treacherous deed as to capture the archbishop, then it is easily understood that, when killed by the archbishop's men, he was considered deserving of the fate due to the most heinous of criminals: to be buried north of the church.

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When we now turn to look at the scattered finds from the church, readers must be warned that the treatment of these is not intended to be exhaustive. Many finds must be ignored, and most of the objects considered here will be treated in summary fashion. The author hopes to return to some of the finds in a later publication. Primary consideration is given to all the objects found in graves – there are not many of them – and other scattered finds associated in some way with the grave finds.

By the outside of the north wall of the chancel we found the only object that could conceivably be connected to the martial events outlined above, namely, an iron spur (fig. 15:16). The spur, which is well preserved, has a maximum length of 13.5 cm, a yoke of thin shanks almost ribbon-shaped and bent up sharply, both ending in a round eye. The star-shaped rowel, sitting on a short neck, has eight points and measures 2.5 cm in diameter. Spurs of this type, with a small rowel on a short neck and with few spikes, are typical of the mid-14th century (H. Olsson 1937:100 ff.). Two good parallels to our example are known from the finds at Korsbetningen (Thordeman 1939, figs 127–130). The spur thus belongs to the same time as grave S above, so it could easily have ended up in the earth at the same time as the burial in grave S.



*Fig. 15:16. Iron spur.*

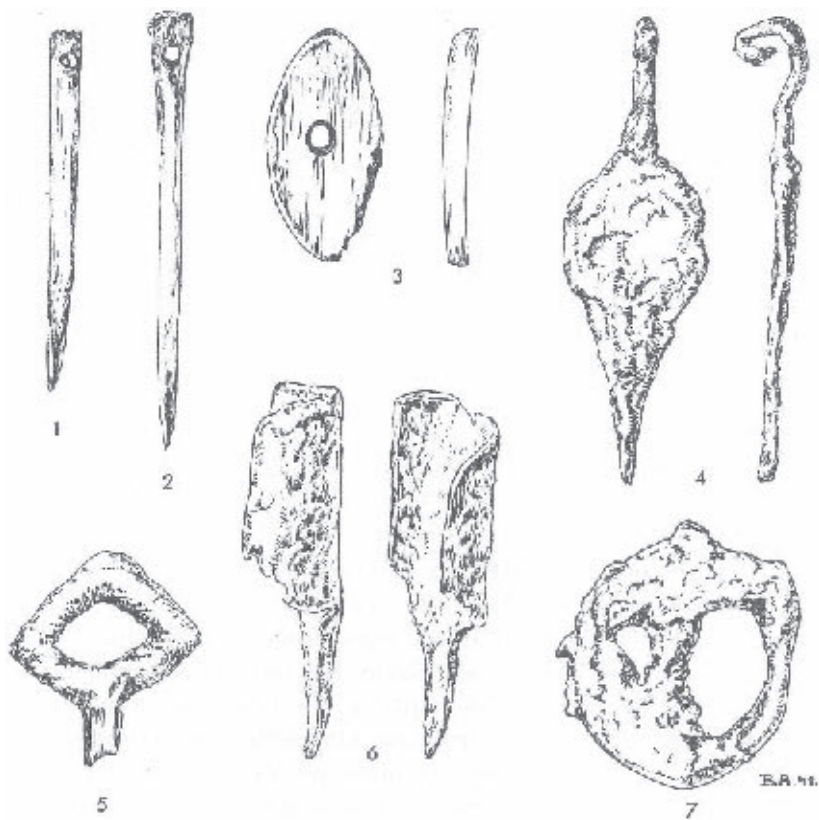


Fig. 15:17. Objects of bone (1-3) and iron (4-7). Drawings by Brita Alenstam.

A simple iron buckle (fig. 15:17:7) was found *in situ* in grave M, by the right side of the pelvis. Tiny remains of textile or leather fragments were preserved under it. The front side of the buckle is slightly arched and furnished with simple decoration in the form of shallow grooves scratched across the arch. The width of the buckle, measured at right angles across the tongue, is 4 cm. Buckles of this form have been found in large numbers in the mass graves at Korsbetningen (Thordeman 1939, fig. 120:35 ff.).

During the excavation some other buckles of bronze or iron were found, and three of the bronze ones should be mentioned here. The circular buckle in fig. 15:18:7 has a tongue in the form of a flat rod. The decoration, which is distributed asymmetrically in that it covers only the half of the buckle above the tongue, consists of shallow incisions. Similar circular buckles have been found at excavations in Lund; they were probably also made there. A good counterpart

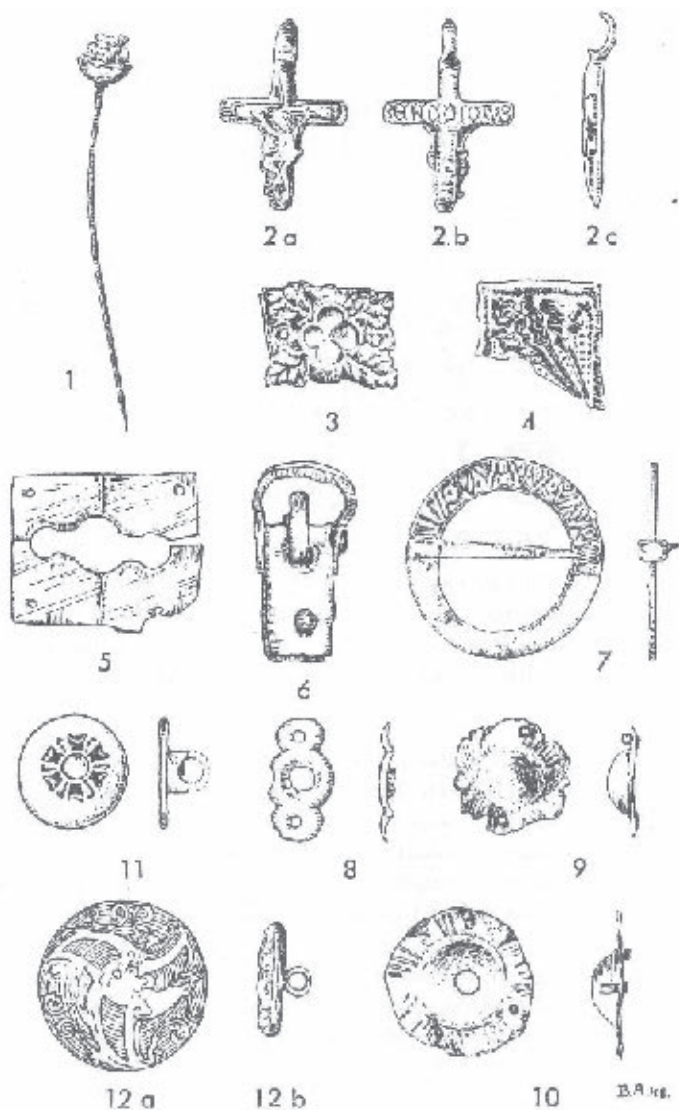


Fig. 15:18. Objects of silver (1–2) and bronze (3–12). Drawings by Brita Alenstam.

to our buckle has been found together with Western European 13th century pottery (Blomqvist 1949b:134, fig. 23:1). On the buckle from Lund the decoration has the following form: triangular fields enclosing three dots point outwards from the inner edge of the buckle frame; the spaces between these triangular fields, which are themselves triangles pointing in from the outer edge, are hatched. A buckle of that kind was undoubtedly the model for our example.

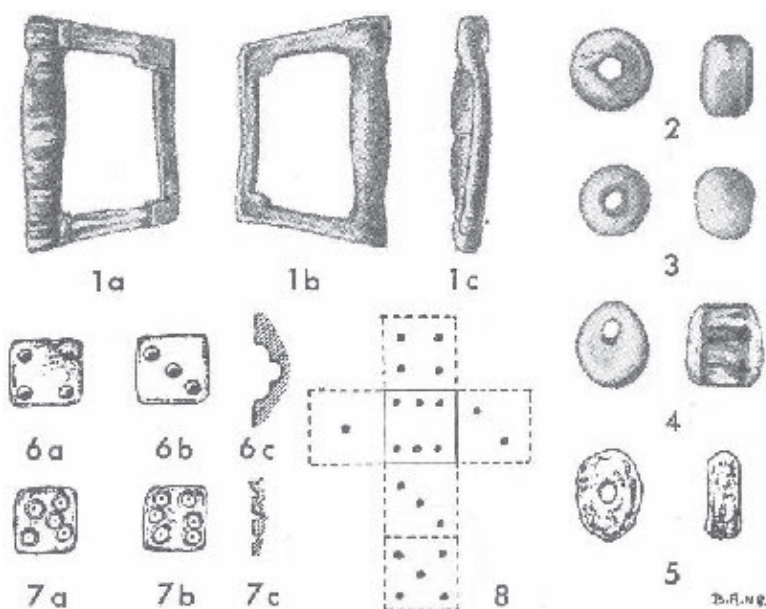


Fig. 15:19. 1. Bronze strap buckle. 2–5. Beads, respectively bone, blue glass, pottery, and amber. 6–7. Bone dice (6c and 7c, schematic sections of eyes). 8. Schematic extension of dice 6. Drawings by Brita Alenstam.

The triangles pointing in from the outer edge of the frame have been retained unchanged, but the dots enclosed in the other triangles could not be done by carving, so the craftsman contented himself with lines which cross each other at three points.

The buckle in fig. 15:19:1 probably also belongs to the 13th or 14th century. Of the four straight sides of the frame, separated from each other by decoration, the front one has stripes running across it. A large number of comparable buckles have been found in Lund, but none exactly the same (Blomqvist 1949b:149 f., figs. 34–35). The small, extremely artless buckle in fig. 15:18:6 is assembled solely of flat bronze rods of different widths. The folded plate that held the belt contained remains of a narrow leather strap.

The small bronze mount in fig. 15:18:10 was found *in situ* in grave C on the right-hand side of the pelvis. It has a bulge in the middle with a hole in it; the edge around the bulge is decorated with irregular groups of radially placed lines and perforated by three irregularly positioned holes with small, short bronze rivets still in them. Under the mount there were remains of some organic substance, probably leather. Another similar mount, but without the extended edge, was



found, not associated with any grave; the rivets, here two in number, are attached to the bulge itself. A mount similar to the first-mentioned one is depicted in fig. 15:18:9, but it has no hole in the middle; the extended edge, in which two rivets are fixed, is divided by shallow incisions into six rounded “leaves”. A similar mount has been found in Skanör castle (Rydbeck 1935, fig. 106:22). Since the first of these mounts was found *in situ* in a grave, it is likely that mounts of this kind were fastened to the dress, probably the belt. In cases where the mount has a hole in the middle, it must have had a practical task, as an attachment for a braid or cord of some kind. With a knot tied on the inside, the cord would be prevented from escaping through the hole. What was attached to the other end of the cord must remain uncertain, probably a tassel or bell or the like, but not a heavy object which the small rivets on the mount, with their diminutive heads, would hardly have been strong enough to hold in place. The mounts with no hole in the middle may have had an exclusively decorative purpose, or else they were intended to conceal a rivet or something else attached to the belt for some purpose. The mount in fig. 15:18:8, for which there are parallels in Skanör castle (Rydbeck 1935, fig. 106:27, 29), appears to have had the task of lining a hole in the belt through which a cord was threaded from the inside and fastened with a knot on the outside. It is very likely that mounts like this were also attached to belts, as we see from a rectangular belt mount from Gotland which has a reminiscence in the form of an ornament exactly the same as that of our simple mount (Hildebrand 1884–98, figs 294–296). The thin rectangular bronze plate in fig. 15:18:3 should doubtless be interpreted as a spangle sewn to a garment (Hildebrand 1884–98:390 f.). The flower-like hollow surrounded by four naturalistically rendered leaves may have been filled with a gem of paste, enamel, or the like. The object in fig. 15:18:4 may also be a spangle, although the surviving fragment has no hole by which it could have been fastened.

Either grave C or D – these two graves which were presumably dug at the same time had been badly damaged by later digging – held the silver pin in fig. 15:18:1, which originally had a spherical head filled with lead, of which only the lower part survived. The excavation uncovered a number of beads of different material. Four of them are depicted in fig. 15:19:2–5; they are made respectively of bone, blue glass, pottery, and amber. The pottery bead, whose Medieval date is uncertain, was made in an unusual way. Someone evidently made a ceramic rod with green glaze, which was then cut into pieces through which holes were drilled. The cut ends thus lack colouring. Another possibility is that the bead was made from some part of a pot, for example, the handle of a jug. Amber, bone, and blue glass were commonly used materials for rosary beads (af Ugglas 1941:63 ff.).



Another object that should probably be counted as a grave find is the small silver crucifix pendant (fig. 15:18:2). It was found south of the church in the area of the cemetery. On the reverse, across the arms of the cross, there is an inscription in Gothic capitals: BENEDICTVS.

Grave E had a small die of bone (fig. 15:19:6). The eyes on this die were made simply with deep gouges, as can be seen from the schematic cross-section of an eye in fig. 15:19:6c. Another die, found west of the church, is quite similar, but the eyes here are made as shallow-carved circles with a dot in the middle (fig. 15:19:7abc). When dice like these are mentioned in the literature it is only to say that the sides, as with modern dice, are marked with eyes from 1 to 6. It should be noticed, however, that the placing of the eyes on a Medieval die usually follows a different principle from modern dice. On today's dice the sum of the eyes on opposite sides is always 7, so that 1 is opposite 6, 2 is opposite 5, and 3 is opposite 4. On the two dice from Sankt Jörgen, however, the placing is quite different, as shown in fig. 15:19:8. Here two opposing faces are always marked with an odd number and the following even number: 1 is opposite 2, 3 is opposite 4, and 5 is opposite 6. Dice with the numbers arranged in this way may be called – not accurately but for the sake of brevity – the “Medieval” type. On dice made in ancient times the eyes were placed as on modern dice, as is evident from the two dice in the Vimose find (Engelhardt 1869, fig. 12, Pl. 3:6). We call this type “classical-modern”. During the Iron Age in Scandinavia there were also dice of a completely different type which does not concern us here, elongated and marked in quite a different way (Petersen 1914:75 ff.). It may thus seem possible to divide dice into specific groups according to the placing of the numbers, but that would require far more detailed studies than there is room for here. Let us nevertheless consider some of the dice in the Lund University Historical Museum. In the rich find material from Skanör castle (LUHM no. 17300) there are no fewer than 68 identifiable dice; 64 are of the “Medieval” type, only 4 are “classical-modern”. It should be noted that two of the “classical-modern” dice are of elegant form, made of walrus ivory, while the “Medieval” dice are artless, made of cattle bone, and, judging by the unfinished ones, were made on site (Rydbeck 1935, fig. 108:17–20). Of the others, up to a dozen in the museum's collections, mostly found in Lund or elsewhere in Skåne, all but two are of the “Medieval” type. One of the two “classical-modern” dice is extremely elegantly made of amber (LUHM no. 13119). In addition there is a die unusually marked with 3 opposite 6, 4 opposite 5, and 4 opposite 5 (LUHM no. 13250). Perhaps it was influenced by the Viking Age type which excluded the low numbers. Yet another die (LUHM no. 13120), very clumsily made, is numbered, perhaps by mistake, with 1 opposite 6, 2 opposite 3, and 4 opposite 5.

If one should dare to draw any conclusions about the mutual relationship of the two types of dice from the meagre evidence presented here, it would be that, during a period that included the High Middle Ages and in an area including Skåne (and probably the whole of Scandinavia), the prevalent type of dice was the one here called “Medieval”, whereas on the Continent the “classical-modern” type has been used from antiquity right up to the present.

The excavation also unearthed a number of utility objects, although they are naturally few in number. There was some pottery, mainly on the periphery of the excavation trench, that is to say, outside the walls of the church. We may mention in particular a number of shards of one and the same large jug with glaze shifting from red to green and applied star-like ornamentation in dark blue. The jug should probably be dated to the 13th or 14th century. West of the west wall of the cemetery, two stout bone pins (fig. 15:17:1, 2) were found together in one place, along with an equally stout iron pin. Probable church fittings were found in the form of a fragmentary cylindrical candleholder of iron (fig. 15:17:6) and a leaf-shaped drop from a chandelier (fig. 15:17:4). According to af Ugglas, drops like this were common in the Mälaren valley and southern Norrland, but not in western Sweden; they occur sporadically in Medieval Denmark (af Ugglas 1931:308). Finally, we may mention the small engraved piece of slate (fig. 15:20). It has a picture of a large bird sitting in a tree. Carved above it in mirror image are the letters M. C. S. This is no doubt a seal. The reverse has been scratched horizontally and vertically to make it easier to glue the seal to a wooden handle. The object is almost certainly not Medieval; the author has not been able to find any functionary of the hospital with the initials M.C. or M.C.S. in the written sources.



*Fig. 15:20. Seal stamp of slate.*

\*

By far the strangest of all the grave finds came from grave B. This grave was immediately to the north of graves C and D, both of which appear to have been dug at the same time, probably later than grave B. Like most of the graves, grave B was a cut with perfectly vertical sides and no coffin. A curious feature, however, was the modest length of the grave, just over 150 cm, which indeed proved quite insufficient: the skull was bent forward, almost at right angles to the body, while the foot bones were pressed hard against the other end. It seems as if the deceased – probably a woman – was violently squeezed down into a grave that was far too small. This careless burial made an odd contrast to the

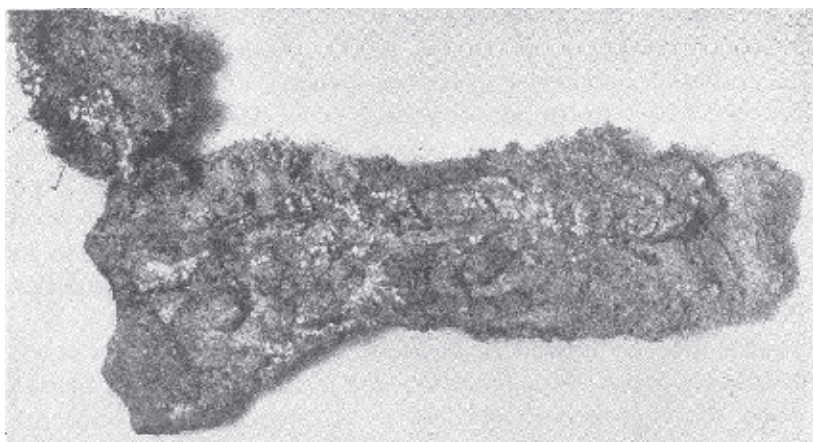


Fig. 15:21. Bead-embroidered silk ribbon lying *in situ* on the breastbone and collarbone, grave B.

splendid location of the grave near the chancel, and it contrasted no less strangely with the finds in the grave. Across the breastbone and collarbone of the deceased was a silk ribbon embroidered with beads of red coral, gold-foliated glass and pearls; the ribbon ended in a small silk bag containing an image of the Madonna, only 24 mm long, carved of deer antler.

The photograph in fig. 15:21 shows the silk ribbon as it lay *in situ* on the breastbone. A length of about 12 cm of the ribbon is preserved. A small piece is also preserved on the end of the left collarbone leading to the breastbone (top of picture). The ribbon is folded in two lengthwise so that only half the width is visible; the full width is probably nearly 2 cm. Letters are embroidered on the ribbon with a single colour used for each letter, either red coral, gold, or pearls. A comparison with the X-ray photograph in fig. 15:22 shows that the underside is also embroidered with letters in the same way. There are thus two lines of text on the ribbon.

Most of the breastbone was covered by a thin layer of a black, felt-like substance. Where the actual bone is exposed in some places it has an intense green colour, quite surprisingly. The preservation of the ribbon naturally requires the presence of some preservative, and in all probability it is this substance that has stained the bone green. It should be noted, however, that the silk ribbon itself is not green but brownish-grey. No chemical analysis has been performed to ascertain the substance that turned the bone green, but in all likelihood it is verdigris from some copper salt; copper salts have a conserving effect on silk. Yet it is a mystery how this fortunate combination arose: a preservative present at precisely the same place as a pearl-embroidered silk ribbon. No other bones were



*Fig. 15:22. X-ray photograph of the bead-embroidered silk ribbon in fig. 15:21.*

stained green, nor was any green stains observed in any of the other excavated graves. Although the rubble from the demolished church contained numerous pieces of sheet bronze and other fragments of bronze and copper, it was never in such quantities as could cause such staining. Moreover, the staining could only have happened if the copper fragments had been right beside the silk ribbon; in the area of Grave B, however, the nearest copper fragments were at a level more than 120 cm higher, that is, at the level of the church floor. In addition, the grave was completely undisturbed, so it can be said with certainty that no copper object was anywhere close to the grave after it was dug. We are therefore forced by logical necessity to assume that an object with preservative properties lay close to the silk ribbon when the body was buried, and that it is actually still there. In such circumstances, there seems to be only one possible explanation for the phenomenon. The thin layer of a black, felt-like substance mentioned above, covering the breastbone, was a piece of cloth, probably linen. This cloth had presumably been soaked in a substance containing copper salt, for example, an ointment of some kind, and thus exerted a preservative effect on the silk ribbon and gave a green colour to the breastbone, the collarbone, and the image of the Madonna.

Attached to the lower end of the silk ribbon was a little silk bag containing the 24 mm long image of the Madonna. Unfortunately, we cannot say how the bag was attached to the ribbon. The bag had become separated from the ribbon; it was found lying crosswise, wedged between the bones of the ribcage. This damage was undoubtedly caused by the careless way the body was stuffed into the grave and the distorted position of the skeleton. The silk bag was likewise



*Fig. 15:23. Madonna effigy from grave B.*

1.

2.



3.

4.

5.

embroidered with beads, but only in two colours: red and white coral (all beads determined by Erik R. Ygberg). The bag was the only place where white coral beads were found still attached to the silk. The appearance of the silk bag after the uncovering of the Madonna image can be seen in fig. 15:23:1. There were beads on every side of the bag, placed in groups, white and red separately. These groups of beads did not form letters but some kind of indeterminable decoration. At several places, however, there were acute angles, formed by 9 or 10 beads of the same colour; see e.g. fig. 15:23:1, bottom left. On the upper part of the Madonna there were no detectable fragments of cloth. It thus seems as if the bag had an opening on the front so that the image was partly visible.

The fragment of ribbon embroidered with four letters, found on the collar-bone, shows that the ribbon continued over the shoulders and in all probability round the neck. Here, however, the ribbon was beyond the preservative effect of the presumed copper salts impregnating the linen, and so the silk had disinte-



grated entirely. About 140 loose beads were picked up in the region of the collarbone; a larger number may undoubtedly have been crushed or disappeared among the sand and not been observed during uncovering. The pearls are all very small, spherical, and about 1 mm in diameter. The gold-foliated beads also measure about 1 mm in diameter; the coral beads, on the other hand, often more or less flattened, occasionally reach a diameter of 3 mm.

The Madonna image in the bag, after cleaning, looked as in fig. 15:23:2–5. The material is probably red deer antler, possibly roe deer or elk (determined by senior Curator, Docent Hervéd Berlin). In its present state it has the same intense green colour as the breastbone, but originally it was no doubt uncoloured. The image shows the Madonna seated on a throne with a crown on her head and an orb in her right hand. The baby Jesus is sitting on her left arm, seemingly holding an indeterminable object in his right hand. The work is not of high class; the face of the Madonna is crudely carved and the baby's face and arms are only cursorily hinted at. The carver elegantly avoided the difficulty of delineating the Madonna's feet and lower legs by letting her cloak reach the ground; the folds in the cloak are carved with delicacy and assurance. The coarse impression conveyed by the upper half of the image can partially be explained by heavy wear here. The lower part, by contrast, shows hardly any traces of wear. The image can probably be dated to the second half of the 13th century (pers. comm. Dr. Monica Rydbeck). The back of the image is completely smooth (fig. 15:23:4). It is cut off square at the base so that it can be placed upright; since the lower part of the image has much more mass than the upper part and the centre of gravity is low, the image stands up safely.

A similar little image of the Madonna – the only parallel known to the author – was found during the excavation of Lagaholm castle (fig. 15:24; Salvén 1929:89 f., fig. 6). The Lagaholm Madonna is 32 mm high and made of bone or antler. The portrayal is the same as on our image from Sankt Jörgen but executed with more artistry. The reverse is fully carved as well. The base, as on our Madonna, is smooth so that the image could stand upright. It is dated to the latter half of the 13th century (Salvén 1929:89); the two small images are thus contemporary. Salvén's assumption that a Madonna image like the one from La-



*Fig. 15:24. Madonna image from Lagaholm castle.*

gaholm was worn as an amulet is correct, as the find from Åhus confirms. One might nevertheless dare to presume that the way the images were designed so that they can stand alone is not only due to the obvious fact that they were modelled on the large Madonna images in churches. It does not seem too bold to imagine that the small images were intended to be taken out of the bag in which they were kept and placed in front of their owners so that they could say their Hail Marys to the image. As mentioned above, the upper part of the image from Åhus is heavily worn. The only reasonable explanation for this is that the owner grasped the image with her fingers to pull it out of the bag, where it was probably a tight fit, and used her fingers again to put it back.

Before we leave this find, a few words should be said about the text on the silk ribbon, with an attempt to interpret it. We have seen that there are two lines of text. The ribbon was thus divided lengthwise into two rather stiff halves, but it would have been easy to fold it in two along the middle. This was indeed done, and thus the upper line of text ended up facing downwards. What is now visible is thus a part of the bottom line. The X-ray photograph in fig. 15:22 shows that the letters on the underside are well preserved, but the photograph does not permit a definite reading. We must try to gain some idea of the content of the text by studying the letters preserved in the bottom line. There seem to be eight letters. Reading them is facilitated by the way in which each letter is composed of beads of just one type, and that the regular recurrence of the colours according to a certain system. Every other letter is thus embroidered with red coral beads, every fourth letter with gold-foliated beads, and every fourth letter with pearls.

–	N	A	–	O	M	I	–
red coral	pearls	red coral	gold	red coral	pearls	red coral	gold
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

The first letter is an illegible fragment. Letter no. 2 is certainly an N, and no. 3 is certainly an A. No. 4 is illegible. No. 5 is an O. No. 6 is either an M or an N, but since it differs in shape from the previous N, we presume that it is an M. No. 7 is certainly an I. No. 8 is difficult to read.

It is obvious that the sequence of letters N A . O M I can appear in many texts. Without being deterred by this fact, we may nevertheless choose a text that may seem reasonable for a place such as our embroidered silk ribbon. Let us choose the Ave Maria. The first part of this prayer – the angel's greeting – has the following wording: *Ave Maria, gratia plena, Dominus tecum*. We find immediately that the sequence of letters N A . O M I occurs at one place in the text,



namely, in the words *plena Dominus*. Based on this correspondence we may examine letters 1, 4, and 8 which we left undeciphered. Letter 1 could be an E, but so little of this letter survives on the ribbon that it cannot be determined whether it really is an E. We are in a better position as regards letter 4. The glass in the gold-foliated beads making up this letter is mostly decomposed, but fragments remain of the gold foil, and these tiny fragments describe a more or less regular circle. There is thus nothing to prevent letter no. 4 from being a D. Letter no. 8, according to our suggestion, ought to be an N. Yet is obviously not. The fairly well-preserved gold-foil beads making up letter 8 form a figure that is nothing like the N that is letter 2. Yet if we study letter 7 more closely, we see clearly that there is a short stroke over the top of the I, which is not a necessary part of the letter but could be an abbreviation mark for the following N. Letter 8 could then be a U or V, and if we look at the surviving gold-foil beads we see immediately how they should be read. We have in fact two letters squeezed together, a V and an S, thus a common sequence of two letters embroidered in the same colour. This gives us the following reading:

–	N	A	D	O	M	Ī	V	S
red coral	pearls	red coral	gold	red coral	pearls	red coral	gold	gold
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)

We now believe we have corroboration for our hypothesis that the text is the Ave Maria; to achieve further certainty we must consider how the words of the prayer could have been distributed in the two lines of text. This is the most probable arrangement:

First line	A	V	E	M	A	R	I	A	G	R	A	T	I	A		
Second line	P	L	E	N	A	D	O	M	Ī	V	S	T	E	C	V	M

Here we surely have the explanation why it was necessary to abbreviate the N in DOMINVS, and why the V and S in the same word were squeezed together: the bottom row, when written in full, consists of three letters more than the top row. After the abbreviations already noted, all that is needed now is that one more letter – for example, the M in TECVM – is shortened so that the lines will be of equal length.

To achieve full certainty that our reading is correct, all we need to do is to be able to read one of the letters in the top row on the X-ray photograph and ascertain that it is in the place where it should be according to our suggested distribution of the words in the two lines. One letter is easy to find. Opposite the

N in the bottom row we can see a distinct M on the X-ray photograph. Furthermore, opposite the A in PLENA, the first A in MARIA is distinct.

If we may consider it proven that the text on the ribbon by which the Madonna image was suspended reads *Ave Maria, gratia plena, Dominus tecum*, it is much more difficult to prove that the ribbon round the neck also had a bead-embroidered text, and if so what. The many loose beads found around the collarbone indicate that there was such a text, like the fragment of silk ribbon with four fragmentary letters found lying on the left collarbone. This is a matter that cannot be demonstrated. All that we can say here is that the first two of the four fragmentary letters can probably be read as C and T, and we may point out that the combination *ct* occurs no less than three times in the second part of the prayer, the blessing of Elizabeth: *Benedicta tu in mulieribus, et benedictus fructus ventris tui, Jesus*.

\*

A total of 197 coins were found during the excavation. They have been examined by Senior Curator Nils Ludvig Rasmusson, who has drawn up the list below. Some comments should be made about this catalogue, mainly concerning the distribution of the coins across the site.

As regards the general character of the coin finds, to begin with, the circumstances in which they were found provide some information. When coins were found *in situ*, it was always where the demolition rubble met the untouched sand, in other words, at a level corresponding to the now removed brick floor. Although the vast majority of the coins were found by sieving, even with this method it could be observed on several occasions that the coins came from a level corresponding to the floor of the church. The most likely explanation for the occurrence of the coins in the church is that they were lost, at many different times, and found their way into cracks and cavities in the floor, which was no doubt far from perfect. It may seem daring to explain such a large quantity – 197 – as having been lost. Yet even if we reckon the time when the church was used as only 200 years, from 1250 to 1450, which is surely too low an estimate, it would mean that only one coin a year was lost. This estimate presupposes that all the lost coins were found at the excavation. The method followed was that *all* the rubble and sand was sieved, even material which had already been dug out with small tools, and even rubble which appeared to lack finds. If we nevertheless calculate that only half of the lost coins were found by the excavation, all this means is that the number of lost coins each year was on average two. This too seems like a low figure; even if the real number of lost coins at certain times

– for example, in the last phase of the church – was less than two per year and in other times was more than that, the figure would still never become so high that it would need some explanation other than the one stated here: that the coins were simply dropped.

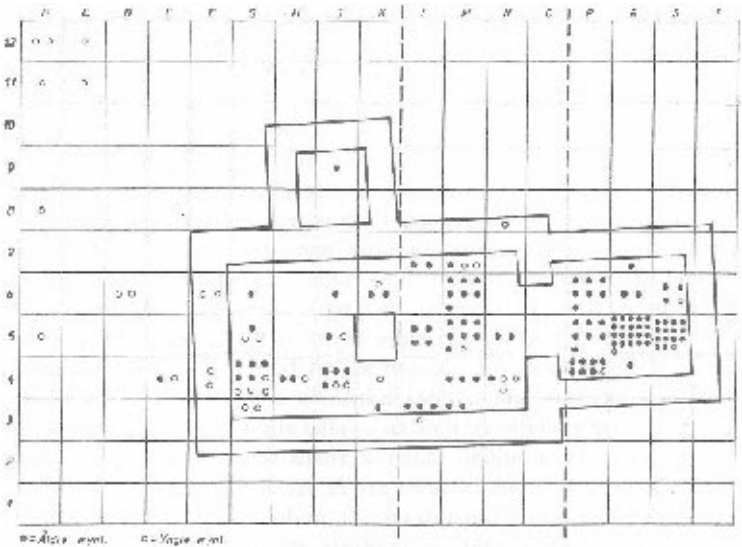
If we place the coins graphically on a plan of the church, we find that the distribution over the excavated area is by no means even. The spread can be seen most clearly in tab. 15:1.

*Tab. 15:1. The spread of coins in the church.*

Chancel	69
Eastern nave	65
Western nave	41
"Sacristy"	1
Outside the church	21
Total	197 coins

The boundary between the chancel and the nave has been drawn along a line between excavation units P and O, as in fig. 15:25, and the boundary between the eastern and western parts of the nave is drawn along the line between units L and K.

We thus find a heavy concentration of coins in the chancel; the eastern part



*Fig. 15:25. Plan showing the distribution of earlier and later coins in the excavated area. Only coins securely recorded to one square are included in the plan. A solid circle denotes an early coin, while a ring denotes a late one.*

of the nave, which represents a larger area, has slightly fewer coins, and the western part of the nave, an even larger area, has even fewer coins. To examine this problem in more detail, let us divide the coins into two groups, earlier and later. The boundary chosen to divide these groups is the year 1361.

To the earlier group we assign coins belonging to the following sections of the coin catalogue: Danish kings 1202–1332, Magnus Eriksson as lord of Skåne 1332–1360, the Swedish coins nos 1–7, the Norwegian coins nos 144 and 197, the French coin no. 189, identifiable civil war coins nos 109–III, and the unidentifiable coin no. 191.

The younger group consists of all the other coins: Danish-Norwegian kings 1361–1525, the German coins nos 146–188, the Visby coin no. 143, the Dutch coin no. 190, and the unidentifiable coins nos 192–196.

This division may have favoured the younger group in that all the German bracteates, which are difficult to identify, have been assigned to the younger group even though some of them may have been struck before 1361. The older group, on the other hand, has no coins other than those certainly minted before 1361. If we assume that the church was in use during the period 1240–1500, then the older period comprises 120 years, the younger group 140 years. Despite this, the older group has 142 coins, the younger group only 55. Let us now distribute the coins from the older and the younger group in the different parts of the church. The result is summed up in tab. 15:2.

Tab. 15:2. *The distribution of coins in the church.*

	Outside the church	Western nave	Eastern nave	Chancel	"Sacristy"
Older coins	5	22	49	65	1
Younger coins	16	19	16	4	–
Total	21	41	65	69	1

The distribution of older and younger coins is shown graphically in fig. 15:25. Only coins securely placed in *one* square are included on the plan, however. For technical reasons it was sometimes necessary during the sieving to combine two, and occasionally four, squares; it was not considered suitable to include the coins – 43 in number – from these amalgamated squares. However, no such group of combined squares is intersected by the boundaries between the different parts of the church, which means that the distribution shown in the table is exact.

When considering this distribution of older and younger coins, one is struck primarily by the fact that the chancel as the primary location for coins is due almost entirely to the older coins: 65 older coins were found there but only 4

younger ones. In the eastern part of the nave the distribution is somewhat more equal: 49 older coins, 16 younger ones. In the west of the nave the two groups are of almost the same size: 22 to 19. Outside the church, finally, the younger coins are by far in the majority: only 5 older coins were found here as against 16 younger ones. If we stick to our opinion that the coins were dropped by churchgoers, there can only be one conclusion: people attending service – or otherwise visiting the church – must have been in a different part of the church in the later period than earlier. It would be very interesting to obtain clear proof of this, since we observed above that, in the later phase of the church – and probably after 1361 – a new altar was set up just west of the central pillar in the nave. It is precisely here – west of the altar at the central pillar – that the younger coins reach their maximum. In the absence of any other changes observable in the actual remains of the church, it is doubtful how the changed distribution of the coins can otherwise be interpreted. We may nevertheless dare to assume that the chancel was closed off to churchgoers in the later phase of the church. As for the fact that younger coins are in the majority outside the church, it should be noted that the area described here as lying “outside the church” mostly consists of the large area exposed to the *west* of the church. Now it is known that, whereas burials in the early Middle Ages mainly took place east and south of the church, people in the later Middle Ages also started to use the area west of the church (Rydbeck 1932:246). The only explanation for the phenomenon that younger coins are in the majority outside – west of – the church that it seems possible to adduce is that the area west of the church was not used for burials until the late Middle Ages, with the result that a number of coins ended up in the earth.

As we have seen, the estimated first 120 years of the church are represented by 142 coins, and the estimated last 140 years by only 55 coins. Assuming that the amount of circulating coins was roughly constant, this would mean that the church in its last phase was visited much less than in the first phase, or in other words, that the number of lepers decreased significantly in the 15th century. This makes it difficult to compare the time before 1361 with the time after. Throughout the period 1202–1360 there was fairly regular minting in Lund, but in the later period the domestic coins, to the extent that they are among our finds, were to a very large extent replaced by foreign coins, especially from northern Germany.

If we calculate the number of coins represented in our finds per regnal year for kings during the period 1202–1360, we obtain the figures in tab. 15:3 for comparison.

Tab. 15:3. *The number of coins in the church per regal years for kings.*

	Regnal years	Number of coins	Number of coins per regnal year
Valdemar II (1202–1241)	39	1	0.33
Erik Plogpenning (1241–1250)	9	2	0.22
Christoffer I (1252–1259)	7	9	1.29
Erik Klipping (1259–1286)	27	29	1.07
Erik Menved (1286–1319)	33	46	1.39
Christoffer II (1319–1332)	13	10	0.77
Magnus Eriksson (1332–1360)	28	31	1.10

All the rulers of Skåne in the years 1202–1360 are represented among our coins, with the exception of King Abel, who reigned for only two years (1250–1252). We see from the table that the comparison figures for the first two kings, Valdemar II and Erik Plogpenning, are very low. From 1252, however, the year when Sankt Jörgen in Åhus is first recorded in writing, the annual average varies only slightly from one ruler to the next. Each king is represented by roughly one coin per regnal year. Only the troubled reign of Christoffer II – he was expelled from the kingdom in 1326–1329, for instance – is an exception. If the amount of circulating coins was roughly constant in the years 1252–1360, we can conclude from the consistency of the comparison figures that the number of people living at Sankt Jörgen was fairly constant during the period. We must leave aside the question of whether the variations in the figures that do exist are a result of defects in Hauberg's classification system or reflect real variations in the number of lepers. The very low comparison figures for the time before 1252, on the other hand, could be taken as evidence that Sankt Jörgen's hospital was founded shortly before that year.

# The coin finds

By Nils Ludvig Rasmusson

## CATALOGUE

The designation in parentheses, e.g. (M6), indicates the excavation square. If there are two or more such designations, e.g. (B9–C9), this means that the coin was sieved from material from two or more combined squares. An asterisk (\*) shows that the coin is depicted here.

The catalogue contains abbreviations referring to the following literature:

Chautard = Chautard, J. 1871. *Imitations des monnaies au type Esterlin, frappées en Europe pendant le XIIIe et le XIVe siècle*, 1–2. Académie de Stanislas, Nancy.

Dbg = Dannenberg, H. 1893–1896. *Münzgeschichte Pommerns im Mittelalter*. Weyl, Berlin.

Haub = Hauberg, P. 1886. Danmarks Myntvæsen i Tidsrummet 1377–1481, *Aar-bøger for nordisk oldkyndighed og historie* 1886 (pp. 135–189).

Haub = Hauberg, P. 1906. *Danmarks myntvæsen i tidsrummet 1146–1241*. Det Kongelige Danske Videnskabernes Selskabs skrifter. Historisk og filosofisk afdeling, R. 6, 5:3.

Hd = Hildebrand, H. 1887. *Sveriges mynt under medeltiden*. Norstedts, Stockholm.

Jesse = Jesse, W., 1928. *Der wendische Münzverein*. Quellen und Darstellungen zur hansischen Geschichte, N.F. 6.

MB = Mansfeld-Büllner, H.V. 1887. *Afbildninger af samtlige hidtil kendte danske Mønter fra Tidsrummet 1241–1377*. Copenhagen.

Oertzen = Oertzen, O. 1900–1902. *Die mecklenburgischen Münzen des grossherzoglichen Munzkabinetts*. Schwerin.

Schive = Schive, C.J. 1865. *Norges mynter i middelalderen*. Christiania.

Schou, H.H. 1926. *Beskrivelse af danske og norske mønter 1448–1814*. Copenhagen.

Th. = Thordeman, B. 1936. *Sveriges medeltidsmynt*. Nordisk kultur 29 (pp. 1–92).





*Fig. 15:26. Coin types from Sankt Jörgen in Åhus.*



*Fig. 15:27. Coin types from Sankt Jörgen in Åhus.*

SWEDEN?

1. Th. group XII? Crowned  
dragon head. (B9–C9)

SWEDEN

2. Th. group XII. Mid 13th  
century. Barbarous  
crowned lion head. (M3–  
M4)  
\*3. Th. group XVII (L). Hd  
607. *Ca.* 1290–1320. (M6)  
4. Th. group XX. *Ca.* 1360.  
(R4–S4)  
5. Th. group XX. *Ca.* 1360.  
(G5)  
6. Th. group XX. *Ca.* 1360.  
Comparable to Hd 579.  
(I4)  
7. Th. group XX. *Ca.* 1360.  
Comparable to Hd 579.  
(G4) (fragm.)

DENMARK

Valdemar II (1202–1241)

*Lund*

- \*8. Haub. 15 (L7)

Erik Plogpenning (1241–1250)

*Lund*

- \*9. MB 4 (G6) 10.

10. MB 6 (G4)

Christoffer I (1252–1259)

*Lund*

- \*11. MB 62 (P4)

12. MB 64 (G4) (fragm.)

13. MB 64 (G4)

14. MB 64 F (O3–O4)

15. MB 70 (C7–C8)

16. MB 71 (M6)

17. MB 71 (R6) (fragm.)

*Roskilde*

- \*18. MB 82 (R5)

*Ribe*

19. MB 93 (I5)

Erik Klipping (1259–1286)

*Lund*

20. MB 104 (S5)

21. MB 105 (O3–O4)

22. MB 108 (O3–O4)

23. MB 106–108 (P4)

24. MB 109 (P6)

25. MB 111 (S5)

\*26. MB 111 (S5)

27. MB 111 (P5)

28. MB 111 (S5)

29. MB 111 (G4)

30. MB 112 (L5)

31. MB 112 (M6)

32. MB 113 (P5)

33. MB 113 (I6)

34. MB 117 (S6)

35. MB 119 (P5)

36. MB 119 (P4)

37. MB 121 (P4)

\*38. MB 121 (O3–O4)

39. MB 123 (P5)

40. MB 125 (P6)

41. MB 127 (R4)

42. MB 127 (R5)

43. MB 132 (P5)

*Halland*

44. MB 138 (R4–S4)

*Roskilde*

45. MB 141 (P6)

\*46. MB 153 (R5)

47. MB 170 (S5)

*Schleswig*

48. MB 274 (S5)

Erik Menved (1286–1319)

*Lund*

49. MB 283 (L3)

50. MB 284 (R5)

\*51. Cf. MB 285 (M3–M4)

52. MB 285 (R5)

53. MB 287 (N5)

54. MB 288 (R5)

55. MB 289 (P5)

56. Cf. MB 291 (M6)

57. MB 292 (R5)

58. MB 295 (M4)

59. MB 295 (R5)

60. MB 299 (M7)

61. MB 300 (S5)

62. MB 302 (R5)

63. MB 305 (R5)

64. MB 310 (P4)

65. MB 312 (O3–O4)

66. MB 312 (I4–K4)

67. MB 313 (P4)

68. MB 313 (I4–K4)

69. MB 315 (I3–K3)

\*70. MB 315 (R5)

71. MB 315 (K6)

72. MB 315 (M6)

73. MB 316 (L5)

74. MB 316 (K3)

75. MB 317 (M5)

76. MB 317 (S5)

77. MB 317 (M5)

78. MB 317 (P5)

79. MB 318 (I4)

80. MB 318 (S5)

81. MB 322 (M5)

82. MB 322 (L5)

83. MB 322 (S6)

84. MB 322 (P6)

85. MB 323 (L4–L5–M4–M5)

86. MB 323 (R5)

87. MB 324 (M3–M4)

88. MB 324 (L3)

\*89. MB 324 (R5)

90. MB 292 (R4–S4)

*North Jutland*

\*91. MB 434 (M5)

92. MB 435 (S5)

93. MB 438 (I4)

*Ribe*

94. MB 463 (S5)

Christoffer II (1319–1332)

*Lund*

95. MB 532 (P6)

96. MB 532 (P6)

97. MB 532 (B9–C9)

98. MB 532 (P6)

99. MB 533 (I4)

100. MB 533 (E4)

101. MB 534 (M6)

102. MB 534 (L4–L5–M4–  
M5)

- \*103. MB 536 (O3–O4)  
*Roskilde*  
104. MB 554 (S6)  
Erik of Pomerania (1396–1439)  
\*105. Penning like Haub. p. 32, fig. 10 (“crown bracteate”). (G4)  
Christoffer of Bavaria (1439–1448)  
*Malmö*  
106. Hvid (F4)  
Hans (1496–1512)  
*Malmö*  
\*107. Hvid. Schou 141 but Dacie. (N4)  
Fredrik I (1523–1533)  
*Ronneby* 1525  
\*108. Søsling with Søren Norby’s coat of arms in the circumscription on the obverse. Schou no. 52. (L4–L5–M4–M5)
- UNIDENTIFIABLE CIVIL WAR COINS  
109. (P4)  
110. (K6)  
111. (M6)
- SKÅNE UNDER SWEDISH RULE  
Magnus Eriksson (1332–1360)  
*Lund*  
112. MB 637 (M3)  
113. MB 642 (R5)  
114. MB 642 (R5)  
\*115. MB 642 (R5)  
\*116. MB 643 (but obverse with reversed P (L4–L5–M4–M5)  
117. MB 644 (S5)  
118. MB 649 (M3–M4)  
119. MB 649 (M5)  
120. MB 649 (O5–O6)  
121. MB 655 (R5)
122. MB 655 (L3)  
\*123. MB 655 (L7)  
124. MB 655 (L5)  
125. MB 655 (R5)  
126. MB 655 (variant with a star before the R) (I9)  
127. MB 657 (M5)  
128. MB 657 (I4–K4)  
129. MB 657 (R5)  
130. MB 658 (O3–O4)  
131. MB 661 (B8–C8)  
132. MB 661? (S5)  
133. MB 666 (O7–O8)  
134. MB 669 (H4)  
135. MB 670 (S5)  
136. MB 670 (H4)  
137. MB 672 (M4)  
138. MB 677 (N7–N8)  
139. MB 677 (N5)  
140. MB 677 (M5)  
141. MB 677 (M4)  
142. MB 677 (R7)
- VISBY  
143. Gote. First half of 15th century (G5)
- NORWAY  
Erik the Priest-hater (1280–1299)  
\*144. Schive pl. IX:41 (R5).  
Belongs to the group “nigri coronati”.  
Hans (1496–1512)  
*Bergen*  
\*145. Hvid. Schou 171 ff. (M7)
- GERMAN EMPIRE  
*Anklam*  
146. Pfennig (A-bracteate).  
13th–14th century. Dbg 92 f. (R5)  
*Hamburg*  
147. Pfennig (bracteate).  
Second half of 14th century. Jesse 174. (N8)
- \*148. Pfennig (bracteate).  
First half of 15th century.  
Jesse 175. (G4)  
*Mecklenburg*  
Bull’s-head bracteates Pfennig  
149. With radiant circle; possibly like Oertzen 151 and if so mid 14th century. (G5)  
150–\*151. End 14th century–start 16th century.  
Oertzen 182. (M5; I5)  
152–154. End 14th century–start 16th century. Cf. Oertzen 182. (L3–M3; E4; C7–C8)  
155. End 14th century–start 16th century. Cf. Oertzen 183 (C11)  
156. End 14th century–start 16th century. Cf. Oertzen 182, but very different. (Test trench III)  
157. End 14th century–start 16th century. Cf. Oertzen 183. High copper content. (B8–C8)  
\*158. End 14th century–start 16th century. Cf. Oertzen 183. N.B. 2 pellets on the radiant circle. (G4)  
159–177. End 14th century–start 16th century. Cf. Oertzen 183. (Test trench III; \* G4; N4; I4; L4–L5–M4–M5; M3–N3; B8; \* L6–L7–M6–M7; D6; L6–L7–M6–M7; I3–K3; B5–C5; G3; N4; B9–C9; F6; H4; K6; K4)  
178–180. End 14th century–start 16th century. Cf. Oertzen 183. (S6; D6; F4)  
181. End 14th century–start 16th century. Cf. Oertzen 182–188. (B12)

<i>Oldesloe?</i>	<i>Stargard</i>	194. Unidentifiable fragment (B11)
*182. Pfennig (bracteate). End of 14th century. Cf. Jesse 262. (G3)	188. 14th century. Dbg 237 b (L6–L7–M6–M7)	195. Unidentifiable fragment (B5)
Parchim	COUNTY OF PORCIEN	195. Unidentifiable fragment (S5)
183. Viertelwitten before 1379/81. Jesse 334. (M7)	*189. Gaucher II 1303–1329. Neuf-Chateau. Sterling. Chautard Pl. XIX, Fig. 4. (M3)	*197. NORWAY? Bracteate with U or N. (R6). This coin is previously un- known and so far highly enigmatic as regards the date and the country of origin, as well as the meaning of the image. The shape of the N on Nordic coins in the late Middle Ages would seem to suggest that the image on this new type should rather be interpreted as a U.
*184. Viertelwitten after 1389. Cf. Jesse 400. (F6)		
<i>Duchy of Pomerania-Stettin</i>	NETHERLANDS?	
185. Kasimir VI 1413–1434. Denar like Dbg 353 a. (B12)	190. Two-sided pfennig with illegible inscriptions. Obverse lion, reverse long cross. Probably first half 14th century (L3)	
<i>Duchy of Pomerania-Stolp</i>		
186. Bogislaw IX 1418–1446. Pfennig (bracteate). Cf. Dbg 370. (O5–O6)	UNIDENTIFIABLE	
187. Bogislaw IX 1418–1446. Pfennig (bracteate) Dbg 370 a. (I4)	191. Cf. MB 432 (N4) 192. Unidentifiable fragment (C12) 193. Unidentifiable fragment (P4)	

## Commentary

In the list of finds the coins are grouped by chronology and mint according to the “system” published by P. Hauberg (1886, 1906) which arranges all the numerous coins from the civil war under the reigns of specific kings and mints. It is obvious that the Hauberg system claims to know more than can be known, as there are no good grounds for any such specific attribution except for a very small percentage of these coins. Hauberg’s chronological stratification is mainly based on closed finds, and it is evident that a chronological chain of such finds – where an older find has some element in common with a younger one, and this younger one has some other element in common with an even younger one, and so on – can be used to date the individual coin types, but naturally not in such a way that they can be assigned to specific reigns; at most they can be grouped by approximate dates, for example, *c.* 1250, *c.* 1275, *c.* 1300, *c.* 1325, *c.* 1350. One might wish that it were possible to group the coins from Åhus in this way, but it is impossible at present without tackling the whole problem anew, an extremely laborious task. As regards the geographical distribution, matters are somewhat different. Here Hauberg has primarily built on the find locations,

but has mixed hoard finds and stray finds in a highly questionable way. Since hoard finds, by all appearances, are a poorer source than stray finds, one cannot guarantee Hauberg's classification without a reappraisal. All experience shows, however, that the situation is more favourable for the series of coins from Lund. It is the same types that are constantly found in scattered discoveries from Skåne, types that Hauberg has by and large managed to distinguish, partly through the study of such finds, partly through a study – which to a certain degree was also of assistance to him in the chronological division – of style, fabrication, image types, weight, and fineness. Yet even if we are on firmer ground here, a renewed analysis of the material is called for.

Among the foreign coins, the Swedish group of bracteates with a radiant circle from the time around 1360 has previously been observed in finds from Skåne (cf. Rasmusson 1934; 1935a; 1935b; also Wilcke 1930:7–21). On the other hand, 13th century Swedish coins must surely have been an uncommon occurrence in the circulation of money in Skåne. The majority of the non-Scandinavian coins are from the 13th century, the period when the Danish coin system was in serious disorder, domestic coining ceased for a long time (c. 1330 to the end of the century), and the country was filled with foreign coin types. Although coining was maintained in Skåne throughout the time of Swedish control and even continued to some extent after Valdemar Atterdag had seized power east of the Sound, the amount of foreign coin increased there too. Some of the coin finds are also assigned to the 14th century. Their dating, however, is not so certain that their occurrence invites further comment (Hauberg 1884; Galster 1908).

The Mecklenburg bracteates pose a particular problem. The variants of the bull's-head type that occur in Swedish and Scanian-Danish finds and are also represented in the Åhus finds cannot be securely dated. A fairly wide chronological latitude is usually assumed for them, from the end of the 14th century to the start of the 16th. What is certain, at any rate, is that the kind described above with reference to Oertzen 182 was in circulation at the beginning of the 15th century (Oertzen 1900–02). The other variant, which is usually described with reference to Oertzen 183, I have been unable to identify in any closed finds, possibly because the finds that have been published in both Germany and Denmark are described far too briefly. It is curious, however, that this variant, so frequent in Scandinavian finds, is not attested at all in Oertzen's detailed description of the stock of Mecklenburg bracteates in the Schwerin coin cabinet. This state of affairs has raised the question whether these bull's-head types could possibly be of Swedish origin (Rasmusson 1937:200).



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## On the material in the Madonna image from Grave B

*By Herved Berlin*

It is always a very difficult and delicate task to pronounce a verdict on the material in a find like the small image of the Madonna from Sankt Jörgen's hospital at Åhus which, apart from the fact that it appears to have been worked all round without leaving any intact surface, is very small and covered with a coating of something like verdigris, and the difficulties are of course exacerbated when one cannot scratch the statuette or take a sample from it in order to ascertain the nature of the material. I must therefore emphasize that the identification has been undertaken with certain reservations, since it could only be done by means of an inspection of the small specimen with a magnifying glass and consequently cannot be as certain as if it had been possible to perform a more penetrating study of it.

There are a couple of guidelines which can lead to a fairly probable identification of the material. Judging by the weight of the Madonna statuette in relation to the volume, I take it as certain that both metal and wood are entirely ruled out, and the former also for the simple reason that signs of working are clearly visible.

The surfaces of the statuette have been compared with the inner structure of walrus ivory, but it has been found that it could not possibly have been made of that material. Nor does the structure appear to be consistent with the look of diaphyses of the humerus, radius, femur, or tibia of either elk, red deer, or domesticated cattle.

Because the little statuette has a compact zone on its base, and inside that a zone with fine lacunae, and between these two zones small bubble-like elevations, which are probably fine lacunae which were sealed in some manner, my thoughts on examining the specimen were led to a part of an antler of either red deer, roe deer, or elk, and I have settled on this probability.

In the Zoological Museum in Lund there is a sawn-off piece of a red deer antler which comes from foundation digging in the town, including a small collection from the Middle Ages. These sawn-off pieces of antler show that people in the past used red deer antler for some purpose. It is therefore conceivable that someone in those early days could have experimented with making an image of the Madonna from this or some very similar material.

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# Microliths as arrowheads. A find from Lilla Loshult bog, Loshult Parish, Skåne

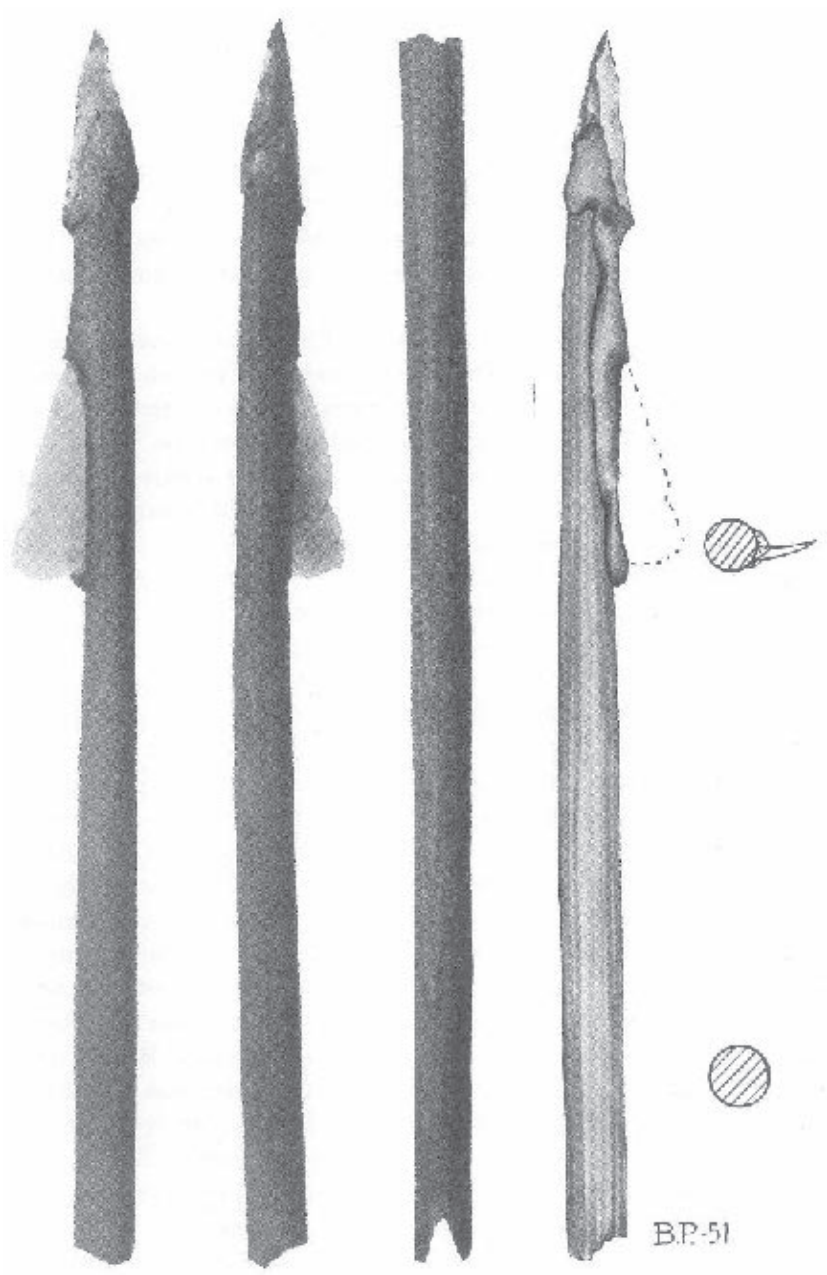
1951

IN MAY THIS year a bog find of a highly unusual character was uncovered in the bog at Lilla Loshult, Loshult Parish, in northernmost Skåne, about 50 km north of Kristianstad. The find consists of a number of fragments of wooden arrow shafts, including the front part of an arrow to which two retouched flint points – microliths – were attached by means of resin, one of them as the point, the other on the side of the arrow shaft, serving as a barb. The find also included two other microliths.

The find spot is in the southernmost part of the bog, about 70 m north of the former lake shore. The discovery was made during peat cutting by the farmer Hjalmar Andersson and his helpers, and the fragments were retrieved without expert help, but with great care. In exemplary fashion, the finders covered the delicate wood fragments with moist peat until the author arrived two days later. The finders also kept a piece of peat with an impression of the arrow-shaft fragments. Thanks to this, and with the help of the information provided by the finders, it is possible to state, with relatively high certainty, the location of the arrows in the bog. They were lying at a depth of almost 2 m under the present surface, in a roughly 15 cm thick layer of *Phragmites* peat. This peat layer overlies a layer of gyttja 15–25 cm thick, resting in turn directly on the bottom sand. When the author visited the site, an untouched peat section was preserved only about 2.5 m from the find spot. Since the peat-cutting trench was filled with water, no pollen series could be taken on site at the time. After it was later drained of water, Assistant Professor Tage Nilsson visited the bog and took the samples necessary for dating by pollen analysis.<sup>1</sup>

The number of arrow fragments retrieved was 17, with a total length of 146 cm. The cross-section is always perfectly circular with the exception of some smaller fragments which were almost certainly damaged at the time when they were found. The diameter varies only slightly – with the exception of the fragments depicted in fig. 16:1, top and bottom end – between 0.85 and 1.0 cm. The arrow shafts were made from larger pieces of wood, not from thin twigs or the

1 The pollen analysis was eventually published: Nilsson 1968 (SW)



*Fig. 16:1. The find from Loshult, top end (1-2, 4) and bottom end (3) of the almost complete arrow (1-3 from photographs, 4 from a drawing.)*



*Fig. 16:2. The find from  
Loshult. Microliths.*

like. The wood is probably pine. The surface is perfectly smooth, without traces of cut marks, which means that we must assume that the shafts were sanded smooth. The wood is very well preserved and it has been possible to refit several pieces. The top end depicted in fig. 16:1 could thus be joined to four other fragments into one piece with an approximate length of 43 cm. The bottom end also depicted in fig. 16:1 could be refitted with another fragment into one piece with a length of about 45 cm. Furthermore, it was obvious, partly because of the grain of the wood, that these two pieces, one 43 cm long, the other 45 cm, belonged to the same arrow. The missing piece in between, which was not found, was probably not very long. The arrow thus had a total length of slightly more than 88 cm. Besides this virtually complete arrow, the find comprised ten shaft fragments with a total length of 58 cm, of which four could be refitted two by two. The common feature for the majority of these fragments, compared with the almost complete arrow, is that the incomplete fragments are about 1 mm thicker. According to the finder, the shaft fragments were discovered in two groups, about 2.5 m from each other. One group contained some longer fragments, all of which belong to the almost complete arrow, including the two fragments depicted in fig. 16:1, the lower end with the nock, the notch for the bowstring, and the upper end with its two microliths, one of which was still attached to the shaft, while the other had come loose. The second group contained various shorter fragments of arrow shafts and the two microliths in fig. 16:2:2–3. It may thus be said with great probability that the find was originally two complete arrows of similar design.

The top end of one of the arrows has a maximum diameter of 0.8 cm. It tapers towards the tip, where the diameter is only 0.55 cm. Attached to the tip is a stout microlith, the visible part of which is triangular: the retouched side and the sharp cutting edge come together at a needle-sharp point. The tip of the shaft has a globular lump of resin which is also the base enclosing the microlith; only the cutting edge is free in its entire length. The microlith undoubtedly fitted into a slot on the arrowhead (cf. fig. 16:1:1, 4), although the slot is completely concealed by the resin. Running from the ball of resin along one side of the arrow shaft is a streak of resin, 6 cm long, 0.5 cm wide, and 0.2–0.3 cm high. At

the bottom end of this strip of resin is the site of the microlith that was found detached from the shaft. (In fig. 16:1:1, 2 the microlith is shown in its original position; in fig. 16:1:4 its outline is drawn in.) It is noteworthy that the microlith was not inserted in an incision in the shaft but only attached by means of the resin. It thereby sat at a tangent to the shaft. This means that it did not sit in exactly the same plane as the microlith at the tip; instead their planes formed an acute angle in relation to each other. The microlith has a lanceolate, or almost triangular, form (fig. 16:2:1). One of the long sides is retouched along almost its entire length, while the other forms a sharp cutting edge. The percussion bulb, which is not completely retouched away, is at the tip. This microlith is much thinner than the one that served as the arrowhead. It is 2.9 cm long. It is of the utmost interest that the microlith is attached to the shaft by its retouched long side. The main aim of the retouching was, of course, to give the microlith the shape appropriate for a barb. However, the retouching can also have had the consequence that the microlith was easier to set in the resin. Otherwise it is natural that the unretouched side should face outward because a barb is not only expected to have the effect of sticking and making extraction difficult, but also has to have a cutting edge. When the barb microlith was broken off, the strip of resin on one side of it followed it (cf. fig. 16:1:1) and was lost. Otherwise the resin on the shaft is completely intact, and it can be said with certainty that the arrow never had more than these two microliths: one as the point, the other as the barb.

The almost complete arrow has a thickness at the middle of 0.95 cm. Towards the bottom it tapers somewhat, but not as much as at the top end. The bottom end depicted in fig. 16:1:3 has a diameter at the fracture of 0.85 cm. For a stretch of about 2 cm at the bottom end the cross-section is elliptical, with the diameters 0.8 and 0.55 cm. The nock for the bowstring is cut perpendicular to the flattened sides. The nock is V-shaped, with a depth of 0.6 cm and a width of 0.4 cm.

Of the two microliths that were found lying loose (fig. 16:2:2, 3) one is 3.1 cm long, the other 2.9 cm. On the one in fig. 16:2:2, a short part of one edge is retouched at the point and a longer part on the other side at the base. The percussion bulb has not been retained. On the microlith in fig. 16:2:3 only a short section at the base on one side is retouched, and otherwise it gives the impression of being an unworked blade, with the percussion bulb retained. Fig. 16:2:2 is the thicker and stouter of the two microliths. If it sat on an arrow shaft in a way corresponding to the other arrow, it seems natural to assume that fig. 16:2:2 was the point and fig. 16:2:3 the barb.

The find published here is remarkable not only for the rarity of the artefact,

but also by virtue of its type. Two arrows found in one context – this combination may lead one's thoughts to a votive deposit. Yet the type of artefact, the microlith, seems so obviously Mesolithic that this idea must be rejected, as no votive finds from the Mesolithic are hitherto known from Scandinavia except the reindeer sacrifice in Stellmoor (Rust 1943:216 f.). But even if the pollen analysis were to show that the find, contrary to all expectation, is Neolithic, arrows and arrowheads do not appear to be the kind of objects used in Neolithic votive deposits (Becker 1947:270 ff.). Does this mean that it is a settlement site find? In the peat section beside the find spot there were several stripes of charcoal, but the deepest of these was at a level some 20 cm above that of the find. There were no other cultural remains in the section, and according to the finder no cultural objects had been discovered during peat cutting, whether of bone or wood or flint. It is thus most likely that the two arrows ended up here by pure chance.

Arrow shafts, and to an even greater extent arrows still with their points, are extremely rare finds from the Scandinavian Stone Age. For a long time all that we knew of were two Danish finds of transverse arrowheads with a short bit of the shaft-end (Déchelette 1908, fig. 178:4; Müller 1917:148 f.; Mathiassen 1948, fig. 104), in addition to a few similar German finds. None of these finds has had any pollen analysis done, which makes an exact dating impossible. A wooden artefact, in all probability an arrow shaft, was found at Dyrholmen I, dated to the middle of the Atlantic period (Mathiassen et al. 1942:28). Only at two Scandinavian settlement sites, chronologically very far apart, have arrows been found in such large numbers that one can obtain a clear picture of the characteristics of the type, namely, in the Ahrensburg stratum at Stellmoor, which is assigned to the Late Glacial period, and at Holmegaard, from the end of the Boreal. Even a cursory glance at the arrow shafts from the two finds shows that, despite some obvious differences in design, there are such great general similarities that it is impossible to date the arrow shafts from Loshult on the basis of typology (Rust 1943, Pl. 91–96; Becker 1945, fig. 4).

A typological date for the find from Loshult, if it is possible, should be sought in the microliths. The three microliths in the Loshult find which can be examined in their entirety (fig. 16:2:1, 2, 3) all belong to the lanceolate type, and this is probably also true of the fourth one still attached to the shaft. The form of this microlith can only be established by X-ray photography. Of the lanceolate forms it may be said that they are made from small blades much more frequently than is the case with triangular and trapezoid microliths, which cannot as a rule be described as microliths (Mathiassen 1948:15, fig. 3). Without a doubt this difference is mainly due to the fact that they belong to different times and cultures. This is best illustrated by a comparison of Klosterlund with Sværdborg. In



Klosterlund, dated to the beginning of the Boreal period, people did not use microblades (Mathiassen 1948:15), but they did use lanceolate microliths (Mathiassen 1937, fig. 27). In Sværdborg, dated to the later part of the Boreal, there are masses of microblades, whereas lanceolate microliths, although they exist, are a tiny minority compared to the triangular and trapezoid ones: roughly 30 lanceolate microliths versus, for example, over 800 triangular microliths (Friis Johansen 1919:141 ff.). The Loshult microliths evidently belong to the form of lanceolate microliths which were not manufactured from regular microblades. This is clearly seen in the microlith in fig. 16:2:3, which is retouched just a short distance, so that the original form of the blade is retained. The characteristics of a microblade are absent: the blade is not the same width all the way, the ridges are not straight and parallel. The latter is also true of the three other, more heavily retouched microliths, with the possible exception of the one still attached to the shaft, but this is far too coarse to have been the raw material for a regular microblade. If we now compare the microliths from Loshult with those from Klosterlund, we find great agreement. The example in fig. 16:2:1 especially, with its broad base, seems almost identical to certain forms from Klosterlund. Was this form of coarse microlith, not made from a microblade, used only in the Preboreal? By no means. If we continue to confine ourselves, as hitherto, to settlement sites that are dated by pollen analysis, a quick survey gives the following results. In Mullerup, in the middle of the Boreal (Mathiassen et al. 1942:193 ff.; Mathiassen 1943:162, fig. 83), there are no regular microblades but there are lanceolate microliths (Sarauw 1903:208, fig. 14; Mathiassen 1948:15). Lanceolate microliths made from microblades occur in Sværdborg (Friis Johansen 1919:139, 142, figs. 18, 22–23). In Revelmose N., from the Late Boreal, the same age and later than Sværdborg, we find lanceolates of the coarse type (Mathiassen 1937, Pl. I), as we also do at the other Gudenaå settlement sites not dated by pollen analysis (Mathiassen 1937, fig. 14). Kildegård, finally, with one settlement in the middle of the Atlantic period and one on the boundary between Atlantic and Subboreal, has lanceolates of both the coarse type and the type manufactured from microblades (Mathiassen 1943:47, fig. 18:1–3). This ought to be sufficient to show that there is no possible way of dating the Loshult microliths by means of typology. The only thing we can say about them with certainty is that they do not belong to the Sværdborg phase of the Maglemose culture. Yet that says nothing about the dating: there is a great distance between Sværdborg and Loshult, and microliths of the type in the Loshult find *may* be just as old as those at Sværdborg, as shown, for instance, by Revelmose N.

The only possibility of dating the Loshult find is by pollen analysis. The same

ought surely to apply to most stray finds from the Mesolithic. Generally speaking, the possibility of establishing a tenable chronology for the Mesolithic culture on a typological basis is seriously limited. The lanceolate microlith is just one example of many showing that Mesolithic artefacts could have a very long life, but they could show highly varied forms of development at any given point in time. At all events, any comparison between settlement sites separated by great distances seems out of the question, unless based on pollen analysis.

Much more important than the dating of the Loshult find within the Mesolithic period, however, is the insight that the find gives into Mesolithic culture as a whole. Microliths are one of the most common types of artefact from the period. The Loshult find provides the first incontestable evidence that they were used as arrowheads.

Sarauw, who published the first microliths found in Scandinavia, the ones from Maglemose, declared without hesitation that they were arrowheads (Sarauw 1903:207 f.). Since then this view has been frequently stated, despite being an unproven hypothesis. The longer the expected find of a shafted microlith failed to appear, the more were microliths described as “enigmatic” (e.g. Nordman 1936:82), and new theories about their use were put forward. The find from the Tardenois settlement site of Ensdorf in Bavaria, a microlith attached to a small bone handle, gave rise to the theory that microliths had been used as tattooing needles (Gumpert 1933:196 f., fig. 18:30; also Clark 1936, fig. 69:27). On the other hand, the theory of the microliths as arrowheads gained support from the fact that a microlith from Duvensee was found covered with resin on one side (Schwantes 1939:97), and even more from the find in a Mesolithic grave at Téviec, Brittany, of a microlith in the spine of a human skeleton (Nordman 1936:84). In his publication of the Sværdborg find, Friis Johansen slightly modified the theory of microliths as arrowheads. The lanceolate forms, he argued, could have served as real arrowheads, but the triangular microliths were best envisaged as barbs; yet they were probably not attached in pairs (Friis Johansen 1919:152 ff.). It may be said that this thirty-year-old theory has found confirmation with the find from Loshult. Although the barb on the Loshult arrow (figs 16:1, 16:2:1) is formally not a triangular microlith, it has an unmistakable triangular shape. Clark accepted the theory of microliths both as arrowheads and as barbs, or at least cutting edges attached to the sides, and he found support for the latter theory in a find from White Hill near Huddersfield, where no fewer than 35 microliths were found lying in a row (Clark 1936:203 f.). The theory that microliths were employed as barbs on arrows was also corroborated by the arrow shafts from Holmegaard, with slots for one or more flints (Becker 1945, fig. 4:a–b).

It seems fully obvious that microliths could never be used unshafted. Despite the theory that they were used as tattooing needles, which cannot be considered definitely proved by the find from Ensdorf, and despite all the other suggestions that have been put forward – use as fishhooks, as small knives for fine work with skins or sewing (Mathiassen 1937:100), to say nothing of other hypotheses – it seems fairly self-evident that the theories propounded by Sarauw and Friis Johansen are correct: the overwhelming majority of microliths, if not all of them, were used as points, barbs, and side blades on arrows and light spears of various designs. There are tens of thousands of microliths in our museums, but only once have microliths been found attached to a wooden shaft, namely in Loshult. Mesolithic wooden artefacts are rare as a whole. On the other hand, there is a large stock of Mesolithic implements of bone and antler. If we maintain that microliths were always shafted, it must logically be concluded that the shaft was of wood. And shafts for arrows and spears are normally, if not exclusively, made of wood. But we have a well-known form of artefact in which microlithic flints were set in a bone shaft, the slotted bone point.<sup>2</sup> These points as a whole were shafted, and the shaft was naturally of wood, whether it was an arrow or spear. The slotted bone point is thus nothing more than a reinforcement of the point of an arrow or spear, a reinforcement that was of course ideal for the purpose, partly because carving slots for so many flints in a wooden shaft would have considerably weakened it. How do the flints on a slotted bone point relate to those on the arrows from Loshult? According to the usual view, there is a highly significant difference. The flints on the Loshult arrows are retouched; they are microliths. In the slotted bone point, by contrast, according to a frequently expressed opinion, microliths were never used, but only ever unretouched microblades (e.g. Friis Johansen 1919:153; Clark 1936:122 ff.; Nordman 1936:84). This view, however, is untenable. The outward-facing cutting edges of the microblades on slotted bone points not infrequently have retouches of varying lengths (e.g. Indreko 1948:273, fig. 74:2). In the Lund University Historical Museum there is a slotted bone point where no fewer than five of the six surviving microblades are retouched along their entire length (LUHM no. 12318; Lidén 1942, fig. 29:3). Lidén's view (1942:81), that the retouching was done to straighten the cutting edge of the microblade, can hardly be correct. The microblades are so regular that their edges would scarcely have needed straightening. And even if the outward-facing edges of the microblades on a slotted bone point are not retouched, the ones facing inwards could have been, although mostly this cannot be observed. The example of the Loshult arrow shows that microliths could be attached in that way, and common sense tells us that this kind of attachment

2 Malmer used the old Swedish term *fågelpil*, "bird arrow" (SW)

must have been the most usual method. Should we now describe the flints on the slotted bone points as microliths? There are very high-class, regular microblades which satisfy the strictest requirements for a microlith. They are also very carefully retouched along their entire length. Should the mere fact that the percussion bulb was not removed by retouching disqualify them as microliths? In my judgement such a view would be absurd. The fact that two microliths from Loshult still retain the percussion bulb in whole or part (figs 16:2:1, 3), in contrast to the other two microliths in the find, cannot possibly justify a refusal to call them microliths. There can be no better definition of the term microlith than the one given by Mathiassen (1948:15); according to this definition, both the Loshult microliths and the retouched microblades of the slotted bone points should be counted as microliths. It is quite a different matter that the sharp flint edges of the slotted bone points never have the typical geometrical form, triangles or trapezoids, or even just lanceolate shapes such as the type in the Sværdborg find. These merely show that these geometrical forms had a different use, namely, as points and barbs of the types found on arrows and spears of wood, which must have existed, but about which we know so little.

The arrows in the Holmegaard find represent two main types: those with a club-shaped head (Becker 1945:66, fig. 4:d), and those with a pointed top end and a groove on the side of the shaft near the tip (Becker 1945:66, fig. 4:a–b). These grooves were definitely intended for slotting in flint edges, as is shown by remains of resin and, in one case, a small splinter of flint. The groove can be short, obviously meant to hold one flint, or long. The lower end of the arrow has a nock similar to the one on the arrow from Loshult (Becker 1945:66, 4:c). Traces of resin on the bottom end are interpreted by Becker as the places where feathers were attached. The length of the arrow was evidently almost the same as the Loshult arrow; one example, from which only the tip is missing, measures 86 cm, to be compared with the 88 cm of the almost complete Loshult arrow. Becker reconstructs the type with the short groove at the tip by fitting it with a barb; he envisages a triangular microlith being used (Becker 1945:70, fig. 8:a). Reconstructed in this way, the appearance of the Holmegaard arrow is strikingly similar to the one from Loshult. The main difference is that the Holmegaard arrow has no flint at the top end, but the shaft itself is shaped to a very sharp point, while on the other hand the arrow from Loshult has no groove for the barb; it is held in place solely by resin.

Among the numerous arrows from the Ahrensburg layer at Stellmoor, Rust distinguishes four forms (Rust 1943:190, fig. 20): forms 1 and 2, which at the tip have a slit or a wider notch for attaching an arrowhead, of which the broken base was still in place in a few cases; form 3 with a wide V-shaped notch at the

top end; and form 4, with the tip conically sharpened. The nock for the bowstring, according to Rust, has a strange appearance; it is V-shaped, narrow, and can be up to 5.5 cm long (Rust 1943:191 f., Pl. 92–93, 95–96, especially Pl. 95:5). The length of the arrow likewise varies in a curious manner; an undamaged example of form 3 measures 73 cm, two likewise intact examples of form 1 are only 15.5 and 16.5 cm long (Rust 1943:190, Pl. 91–92). Becker modified Rust's typological classification of the Stellmoor arrows on just one important point (Becker 1945:70). The incisions that Rust took to be nocks for the bowstring, up to 5.5 cm long, are far too narrow and deep for this purpose according to Becker. Instead Becker sees the shallow V-shaped notches as being intended for this use, but Rust describes these as the top end of form 3 (Rust 1943, Pl. 93:5). The very deep cuts are taken by Becker to be devices for joining a top and bottom end to create a complete arrow (Becker 1945:70, fig. 8:b). If the Stellmoor arrows thus consisted of two pieces joined together, it would explain the great variations in length of the seemingly complete, undamaged examples: the short pieces about 15 cm in length are in reality the top ends of the longer 70 cm pieces which were the lower ends. Becker is undoubtedly right in this. It deserves to be noted that the Stellmoor arrows, when comprehended in this way, had a length of almost 90 cm, in other words closely corresponding to the arrows from Holmegaard and Loshult. The thickness, 1 cm or slightly less, is also the same in all three finds. The nock for the bowstring has the same shape in all three finds. The conically pointed tip without a flint, exemplified by the Holmegaard arrow, can be assigned to form 4 from Stellmoor, while the Loshult arrow agrees with forms 1 and 2 from Stellmoor in that both were fitted with flint points. All these correspondences lack chronological value; they are similarities of the kind that are found among all primitive tools in all ages. The differences could, as Becker hints, be significant, but they cannot be assessed in terms of chronology in the absence of much more material than we have at present. It would be tricky to draw any conclusions from such a small sample as the current stock of Stone Age arrows. Yet these arrows are sufficient to make us consider the immense amount of material that is lost. The number of arrows and spears must have amounted to close on the total number of microliths, blade arrowheads, transverse arrowheads, etc. The large number of different designs in use is evident from the material we have, small as it is, and it is highly likely that the wealth of variation was even greater. We cannot imagine the appearance of the arrow types of which we have no examples except in the form of broken-off flint points. Yet all the evidence suggests that arrows with numerous variations in design existed within the same period and the same culture.

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# The Alvastra pile dwelling. Theory and method in the 1976–1980 excavations

c. 1995

## 1. Starting point: the results of the 1908–30 excavations

### 1.1. *The macrostructure of the site*

The planning of the 1976–80 fieldwork campaign was of course based on the results of the excavations of 1908–30. Among other methods, these were documented by vertical photographs, put together to give a photographic plan covering most of the excavation trench (fig. 17:1). More detailed information is provided by the plans at a scale of 1:10, usually covering 8 m<sup>2</sup> each. The plan in fig. 17:2 is composed of III such plans, showing the uppermost structural level, documented in 1928–30. It also shows the maximum extent in the horizontal plane of the part of the pile dwelling that was excavated up to 1930. The remaining plans show lower levels, covering only parts of the area seen in fig. 17:2.

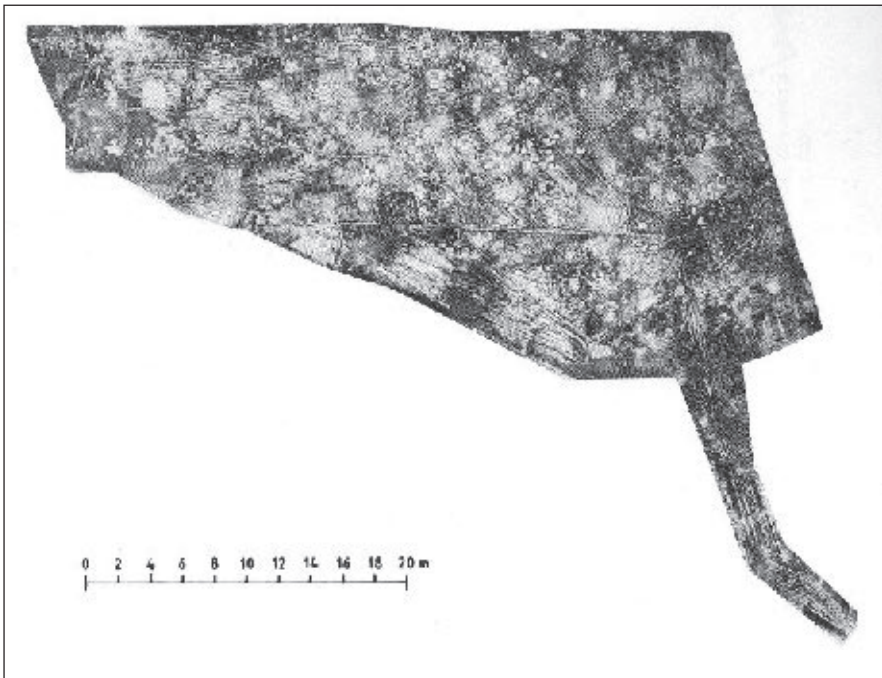
The prehistoric structures visible in fig. 17:2 are horizontal timbers, the tops of vertical stakes, stone-built hearths (mostly of limestone), and other stones of a certain minimum size (down to about 5 cm in length). The structures make up a totality with a maximum length in roughly NW–SE direction of about 65 m.

An attempt at an interpretation of the plan must proceed from the structural differences that are obvious between different parts of the excavated wooden structure.

The logs to the south (bottom right corner of fig. 17:2), mainly oriented N–S, can be perceived without difficulty as a *footbridge*, of which roughly 25 m was exposed (from square F3 in the north to DX in the south). It is a reasonable hypothesis that this footbridge linked the pile dwelling with the area of till soil east of the spring fen. Its total length would thus have been about 75 m, of which roughly a third was excavated.

On the plan it is also possible to discern an *inner part*, characterized by logs laid carefully in parallel, often in such a way as to form a fairly regular rectangular area – or what may be assumed to have been a regular rectangular area before damage and decay set in. The inner part starts in row 4, with its southernmost





*Fig. 17:1. Photographic plan from a single negative of the timber and stone structures in the 1908–30 excavation trench (from Browall 1986).*

point in square J4. Square V16 is the northernmost of the squares excavated up to 1930 that contains logs laid parallel to create rectangular surfaces. In that square the surface of parallel logs is crossed by the northern boundary of the 1908–30 trench. This is also the case in P14 and G12. The other trench boundaries, on the other hand, do not intersect with the inner part of the pile dwelling. To the east, south, and west this inner part is surrounded by more irregular structures. To the west there are mostly stouter and longer logs as well as stakes, to the south mostly stakes (including a highly regular row of stakes from J2 to B4), and to the east a more obscure assemblage of stakes, stout logs, and smaller, irregularly placed branches or sticks. It is clear that the external boundary of this outer part of the pile dwelling was not reached by the 1908–30 trench.

In the inner part of the pile dwelling it is possible to observe two distinctly separate parts merely from the main orientation of the logs. In the south-eastern part the logs mostly lie NNW–SSE (or exceptionally at right angles to this, for example, in squares K8 and H5). In the north-western part, by contrast, the logs mostly lie WNW–ESE (or exceptionally at right angles to this, as in square



Fig. 17:2. Composite plan of the uppermost construction level, documented in 1928–30 (from Browall 1986).

Q12). As a natural boundary between the north-west and the south-east part of the pile dwelling one can regard a log that stands out clearly on the plan, running roughly south-west from a point in square N12. In square N10 its other end meets the ends of two other logs, one of which is in the longitudinal direction of the inner north-west part, the other in the direction of the inner south-east part. The angle between each of these logs and the “boundary log” is the same, about  $110^\circ$ , and the remaining angle, which is thus identical with the angle between the two inner parts of the pile dwelling, the north-west and the south-east parts, is about  $140^\circ$ .

The south-eastern short side of the *inner south-east part* has a length of 10–12 m. Its south-western long side is about 15 m. The width, measured from the middle of the long side, is about 12 m. A total of some 225 m<sup>2</sup> of the inner south-east part had been excavated up to 1930. The excavated part of the north-eastern long side is about 15 m. The excavated part of the boundary line between the north-west and south-east inner parts is about 8 m. If these two latter lines are extended we find that, of the inner south-east part, there ought to be a triangular area of about 25 m<sup>2</sup> north of the 1930 trench boundary which was not excavated.

It can generally be said about the inner south-east part that the logs are more regularly and completely laid to the south and north, and most incompletely to the north-west, at the boundary with the inner north-west part.

In the *inner north-west part* the log laying is correspondingly most incomplete to the south-east, at the boundary with the inner south-east part. This reinforces the impression that the division into a north-west part and a south-east part is relevant, possibly with a functional significance.

The southern long side of the inner north-west part measures about 17 m. Roughly parallel to it is the northern boundary of the 1930 trench, and the width of the excavated section is about 7 m. This means that some 120 m<sup>2</sup> of the inner north-west part had been excavated by 1930. To estimate the size of the section of the inner north-west part located north of the 1930 trench boundary, there is nothing to go by except the calculated northern point of the inner south-east part. If this is identical with the north-east point of the inner north-west part, then the width of the unexcavated section is about 5 m, which means that the north-west part would be the same width as the south-east part, namely, about 12 m. The area of the unexcavated section would thus be around 75 m<sup>2</sup>.

The whole of the inner part of the pile dwelling could thus be hypothetically estimated at approximately 445 m<sup>2</sup>, of which some 345 m<sup>2</sup> – just over 75% – had been excavated up to 1930. Of the 445 m<sup>2</sup>, about 250 m<sup>2</sup> constitutes the south-east part and 195 m<sup>2</sup> the north-west part. Of the south-east part, according to

these calculations, about 90% had been excavated by 1930, and some 60% of the north-west part.

The *outer part* of the pile dwelling is, as already pointed out, more difficult to define than the inner part. There is probably a more or less sparse spread of worked timbers quite far beyond the inner part of the pile dwelling. At any rate, the periphery of the outer part was not certainly reached anywhere in the 1908–30 trench. The relative lack of irregular timber spreading around the footbridge to the south-east does not say much, as the trench dug to investigate the footbridge was very narrow. The area of the section of the outer part of the pile dwelling excavated up to 1930 can be estimated at about 445 m<sup>2</sup>. An outer part of on average equal width around the hypothetically calculated unexcavated inner part of the pile dwelling could be roughly estimated as covering about 160 m<sup>2</sup>.

The figures thus calculated are summed up in tab. 17:1. The total area of the pile dwelling has been estimated at 1,050 m<sup>2</sup>, of which 75% was excavated up to 1930. The inner part can be estimated at 445 m<sup>2</sup> and the outer part at 605 m<sup>2</sup>. Up to 1930, 78% of the inner part had been excavated and 74% of the outer part.

The plans from 1930 show a total of some 900 stakes. Not all of them are marked in fig. 17:2. They are grouped in distinct rows at three spots in particular: to the far south-east (from B<sub>4</sub> to J<sub>2</sub>) and to the far north-west (from U<sub>11</sub> to S<sub>14</sub> and from Y<sub>12</sub> to V<sub>15</sub>). Both the first and the last of these three, it should be noted, are in the outer part of the pile dwelling. The lines of stakes thus seem to suggest a stricter organization of this outer part than might appear from the ostensible disorder of the horizontal timbers.

*Tab. 17:1. The area of the pile dwelling, calculated on the basis of the documentation from 1930.*

	Excavated	Not excavated	Total
Inner part	345	100	445
Outer part	445	160	605
<b>Total</b>	<b>790</b>	<b>260</b>	<b>1,050</b>

Apart from the rows of stakes, the documentation from the 1908–30 excavations hardly shows any hints of any observed order among the stakes and other vertical timbers. The function of the rows of stakes has not been investigated, and the function of the seemingly more irregularly placed stakes has scarcely even been discussed.

### *1.2. The microstructure of the site*

The best information we have concerns the hearths, although there are section drawings only for a small proportion. Otherwise there are few sketches of stratigraphical conditions, which means that the layers can be assessed solely from the plans. While it is often clear which horizontal log systems are older and which are younger, it is impossible to know whether intervening occupation layers or any artefact-free layers indicate any significant difference in time.

With such a large area as that covered by the pile dwelling, one must assume that different parts had different functions. From the older documentation, however, it is difficult to detect whether certain areas were used for craft activities, for example, and others were places for dumping waste. The functional difference that must have existed between the inner and outer parts of the pile dwelling is likewise unclear.

The location is carefully stated for a good many artefact finds, and also finds such as human bones, but what is lacking in the old documentation is a sufficiently large number of three-dimensionally plotted points. Without an adequate amount of such data it is impossible to solve problems concerning details in the structure of the dwelling, and impossible to successfully tackle the problem of the function.

The older documentation makes it effortless to date the pile dwelling to the Middle Neolithic. On the other hand, the dating methods they had back then naturally made it impossible to judge the duration of the settlement, counted in years. Preserved organic material allows radiocarbon dating, although the long time it has been above ground, and any conservation measures that have been taken, could be a source of error. Yet even if a large number of radiocarbon dates could possibly, in principle, give us an idea of the duration of the structure, the surviving organic material is by no means sufficient to allow a dating of different layers or parts of the pile dwelling. These restricted dating possibilities apply just as much to the study of macrostructure and microstructure, macrofunction and microfunction.

### *1.3. The natural environment and the ecofacts*

Those responsible for the excavations of 1908–30 showed great foresight in engaging experts in natural science. Cooperation began in 1909 with Lennart von Post, whose studies in the spring fen and the adjacent Dags mosse bog went hand in hand with the development of pollen analysis (von Post 1916:235, Pl. XI). Botanical examinations were performed by Gustaf Lagerheim, Hjalmar

Nilsson, and Thorild Wulff, zoological specimens were identified by Ludvig Hedell and Adolf Pira, and petrological specimens by Axel Gavelin and Carl Wiman. Of the scientific works about the Alvastra pile dwelling published after the death of Otto Frödin in 1953, we may mention especially Ernest Magnusson's pollen-analytical study (1964:22–23) and those by Hakon Hjelmqvist (1955) and Greta Berggren (1956).

It is thus evident that data collection and evaluation from the older excavation is richer and more efficient as regards the natural environment and ecofacts than as regards the structure itself and artefacts from the pile dwelling, and the use of this evidence for studies of macro- and microstructure and function. It also goes without saying that all the natural sciences relevant to the pile dwelling have progressed since the time of the older excavations.

This section has outlined the main features of the macrostructure, microstructure, natural environment, and ecofacts of the pile dwelling that we find in the documentation from the 1908–30 excavations together with Otto Frödin's article in *Fornvännen* (1910). The above account is intended only to provide a background to the deliberations when planning the 1976–80 excavation.

## 2. Planning the 1976–80 fieldwork campaign

### 2.1. *Macrostructure and chronology*

Since probably as much as 75% of the total area of the pile dwelling was excavated in 1908–30, it ought to be within the bounds of possibility to determine with full certainty whether the plan of the structure is the one suggested in fig. 17:2: two almost rectangular areas, modified to the shape of a parallel trapezium through a common diagonal boundary, and shifted to form a 140° angle between them. The question may seem banal but it is not, since it gives us a rare opportunity to determine the exact form and extent of a Neolithic settlement site, or at least a structure which, in essential respects, functioned like one. By virtue of the location in the fen, we are more certain than otherwise to be able to distinguish cultural products from natural formations. Also, wood and other organic material are preserved particularly well, whereas such material has mostly been obliterated where settlement sites are on gravelly soil. The exact form and extent of a Stone Age site is in turn important for virtually any aspect of life and society one chooses to study. Many hypotheses about social and economic organization, for example, can be tested only on material with the quality of that from Alvastra pile dwelling.

One possibility would have been a *total excavation* of all the area untouched by the 1908–30 excavation of the pile dwelling. Two weighty reasons, however, spoke against that. The careful excavation method that was expected to be necessary would have made a total excavation into an unreasonably protracted and expensive undertaking. The crucial reason, however, was a desire to save a part of the pile dwelling intact, dictated by the fact that many methods and techniques in field archaeology have been developed between the end of the older excavation in 1930 and the start of the new excavation half a century later: a corresponding improvement in methods could be expected in the future. The 1976–80 excavation thus came to cover only about 50 m<sup>2</sup> (not counting test trenches west of and at a distance of 10–35 m from the 1930 trench). This amounts to only about a fifth of the area which, according to tab. 17:1, could be estimated to be still unexcavated.

A total excavation of the pile dwelling would have given optimal potential for exact quantification. For example, the amount of animal bones could have been stated with an accuracy otherwise unattainable. The result could then have been used for a highly exact calculation of the quantity of meat consumed. When a total excavation was deemed unsuitable for other reasons, the solution instead was three smaller trenches, northward extensions of the 1908–30 trenches, named the Eastern, Middle, and Western trenches. The idea behind this division into three smaller trenches was, of course, to make the excavation as representative as possible. The Eastern trench would give an opportunity to study a small section of what little remained unexplored of the inner south-east part of the pile dwelling; moreover, part of the boundary between the inner south-east and north-west parts would fall within that trench. The Middle trench was intended to enable investigation of a central part of the inner north-west part of the pile dwelling. The Western trench, finally, would, if possible, capture the two rows of stakes clearly visible on the plans in the westernmost part of the pile dwelling, and the test trench extended towards the north would, it was hoped, make it possible to fix the location of the north side of the inner north-west part.

If the excavated 50 m<sup>2</sup> had been assembled in a single trench, we might possibly have gained a better overview of the structure and function of that particular area, but through the chosen solution it would likely be possible to shed light on more details of significance for the pile dwelling as a whole. If we were unfortunate, a single 50 m<sup>2</sup> trench could have ended up in a spot that yielded no new information worth speaking of. Or it could have ended up in a place with an abnormally high frequency of animal bones or some other specific category of find. Or it could have ended up in such a way that an all-round picture of a certain functional unit, for example, a rectangular floor of parallel logs, could



be expected to be achieved by extending the trench, which would have meant partly failing in the ambition to limit the excavation area – perhaps without achieving the goal. A dilemma of this kind arose when the test trench was dug west of the 1908–30 trench, a test trench with the aim of establishing the location of a presumed footbridge in the direction of the fen's western edge. The excavation here – because of the relatively meagre quantity of finds and the uncomplicated sequence of layers – cost less labour and expense and therefore the several extensions to the trench were unsurprising. Yet it was not possible to achieve full clarity about the western footbridge.

The choice to excavate three smaller areas instead of a single large one meant that the analysis of the macrostructure of the pile dwelling – the broad outline and history of how it was built up – could be based essentially on the data from the big trench of 1908–30, whereas the analysis of the microstructure – structural details and everyday functions – could mainly be done with the aid of three somewhat randomly chosen samples: the Eastern, Middle, and Western trenches. The choice between one big trench and three small ones was nevertheless difficult, and only time will tell whether the choice we made was optimal.

One aspect of the macro-analysis of the overall structure and function of the pile dwelling is the chronology. Dating possibilities for the Alvastra pile dwelling include, of course, the radiocarbon method, since there is a particularly good supply of organic material: primarily wood, charcoal, hazelnuts, charred grain, and apples. The finds from the 1908–30 excavation include much material of this kind. In the old trench, however, nothing remained of horizontal structures and occupation layers. What did remain was the most important material for radiocarbon dating of the construction history, namely, a large proportion of the vertical stakes. Yet there was little hope of being able to obtain radiometric dates for the parts of the pile dwelling in relation to each other – the margin of error would be too great unless it turned out that the pile dwelling had an extremely long construction history.

It was clear at an early stage that the main method for establishing the internal relative chronology of the Alvastra pile dwelling would be dendrochronology, which was introduced to Sweden in a stringent scientific form in 1974 by Thomas S. Bartholin. An absolute dating of the pile dwelling by the dendrochronological method will probably be possible in the future (Bartholin & Berglund 1975), but in 1984 the standard curve for southern Sweden still did not reach beyond AD 578, and in 1976 work had only begun on it. Yet even a successful relative chronology would naturally be of crucial significance. Dating the vertical stakes would give the length of the construction period, and also data about any rebuilding, extensions, and other changes. Many other findings

in wood anatomy and dendrochronology could moreover be expected to yield information about activities within the pile dwelling, about which more below.

As an important complement to the investigation of the new smaller trenches, the planning of the excavation thus included a re-examination of the big trench from 1908–30, with the aim of finding as many as possible of the stakes left when the older excavation ended. The function of the stakes was a central problem which could raise many questions. Some of them will be intimated here.

Do the stakes – not counting the three rows visible on the plans, two to the north-west and one to the south-east – stand in any order that can be determined by dendrochronology or some other means? If, so, is this order related in any way to the log floors? Is there any association between the order and the depth of the stake points under the former ground surface? Between the order and the species of tree? Between the order and the diameter of the stakes? Is there any sign that the stakes supported something, and if so what? Is the placing of the stakes associated in any way with their inclination, when they are not vertical? Is the diameter associated with the inclination? Are the rows of stakes palisades? If so, why do they not seem to go round the entire pile dwelling? What alternative function could the rows of stakes conceivably have had? How should a hypothesis to that effect be tested? What is the function of the stakes that do not stand in regular rows? Did some of the stakes belong to houses of types that are otherwise known from prehistoric times? Can many of the stakes conceivably have belonged to structures of other kinds than houses and palisades, and if so, what?

These questions about the function of the stakes are naturally connected with similar questions about the log floors. The plans from the 1908–30 excavation seem to show that rectangular areas are the normal shape for the log systems, but these plans are so summary in places that an important task for the 1976–80 excavation was, if possible, to find out the purpose of the log systems. Should they be viewed as floors, or as house foundations? Were all the non-rectangular log systems in the inner part of the pile dwelling originally rectangular, with secondary disturbance? Why are there no rectangular log systems in the outer part of the pile dwelling, which covers a larger area than the inner part? Are the sparsely laid logs in the outer part of the pile dwelling functionally associated with the stakes that often stand right beside them? Do logs which are roughly uniform as regards the direction in which they are laid, parallel to the south-west side of the pile dwelling (north-west and south-east of square O6), possibly have the same function as the presumed footbridge to the south-east? Did a corresponding footbridge lead from the north-west end of the pile dwelling to the

west bank of the spring fen? If the rectangular log systems in the inner part of the pile dwelling are to be interpreted as floors, why then are only some of the hearths on the floors, while just as many others are outside? Are the log floors possibly on surfaces where the layer of peat and gyttja in the bog is of a particular character, and if so, what? Why is the layer of logs sometimes double, with the upper layer of logs laid at right angles to the bottom layer? Do the log systems consist of a random selection of tree species, or was there a deliberate selection – and if so with what purpose?

Although the analysis of the macrostructure of the pile dwelling, as already stated, mainly had to be based on the large trench of 1908–30, the excavations in 1976–80 naturally had to be planned so that they would best serve both macro- and micro-analysis. This aim could be achieved by exercising more systematic care than was possible or aimed for in the 1908–30 excavation, which was otherwise extremely meticulous for its time, and perhaps even more by focusing on specific problems of detail which were not resolved finally or convincingly by the older excavation. Some examples may be cited. Hearths are often placed so closely together (e.g. the hearths in K7/L8) that one must inevitably ask whether they were built simultaneously or on different occasions – but the documentation from the older excavation usually gives no explicit answer. The main way to resolve such problems is through sections, and consequently the 1976–80 excavation was planned so that there would be a much larger number of section drawings than in the older excavation. Sections are evidently also the essential aid to ascertaining the relationship between two log systems overlying each other, with or without intervening occupation layers or peat. In quite a few cases the stakes in the older documentation seem to be covered by log systems and hearths, but the relationships are rarely perfectly clear, and even less so the chronological consequences. An enigmatic element as regards both chronology and function are the lines of stones over a metre wide which frame the inner part of the pile dwelling in several places (e.g. J3 to L5, P11 to X13, C7 to F12); at least the latter row of stones seemed like it might touch the Eastern trench. If we presume that the vertical stakes supported horizontal structures, raised above ground level, we are left with a question not answered by the older excavation concerning the nature of these horizontal structures: were they platforms, house roofs, or something else? This exceedingly difficult problem, it goes without saying, cannot be solved except by an extremely careful excavation technique, geared to finding remains of these raised structures, preserved because they collapsed into the damp earth.

The planning of the 1976–80 excavation thus included an improvement of the chronology as an important goal. Chronological data are essential basic

knowledge under any circumstances, necessary for optimal use of the rich, varied, and unusual stock of other data from the pile dwelling. This is true regardless of whatever problems one chooses to study: concerning function, economy, social organization, or something else. The abundant animal bones from the pile dwelling can be used for the study of meat consumption, and hence also of population – but only on the condition that the chronology is established. The cost in labour of building the pile dwelling – and hence also the character and strength of the social organization – can only be assessed against the background of the time it took. The function of the hearths, the log floors, and the stakes can be discussed only if we know which of them are coeval. Many more examples could be mentioned. Or contrariwise, we could also confine ourselves to the observation that fixed chronological points are always of value in archaeology. Of course, the value varies from one case to another: in the case of the Alvastra pile dwelling the value is unusually high.

## *2.2. Microstructure and function*

Alvastra pile dwelling is unusually suitable for the stratigraphic method in its classical archaeological form since the strata mostly consist of deliberate and systematic structures of an unambiguous kind: hearths built of limestone slabs laid in clay, floors of parallel logs, and other layers of a less stable kind but still undoubted products of culture: beds of twigs, layers of hazelnuts, and layers of clay. As an aid to a relative chronology there ought at least to be pottery, in fortunate cases. It ought not to be beyond the bounds of possibility to find a number of sherds, demonstrably from the same pot, which could serve as a chronological horizon and also help to date structural elements in relation to each other even if they are not directly included in a stratigraphic sequence. Such considerations lay behind the choice of a meticulous excavation method that is sometimes suggested in the literature but rarely practised in reality (Troels-Smith 1953). Nothing in the occupation layer would be dug with a spade, and not even with a trowel, but only with small tools. The diggers would never set foot on the surface being excavated, but work exclusively from purpose-built platforms; it was understood right from the beginning that the only practical solution to this principle would be to lay planks over the trench at such a low height that the work of excavation could be done without difficulty. This practical aspect was one contributory factor in the decision to limit the size of the individual trenches. Larger trenches would inevitably have led to increased difficulties, would have taken more time, and would also have sacrificed part of the area of the pile dwelling to the ground supports for the excavation platforms.

During planning it was easy to understand that dendrochronology and wood anatomy offered great potential apart from giving the relative chronology of the wooden structures of the pile dwelling in relation to each other. It ought to be possible to determine during which season the timber was felled or branches and twigs were cut, and at least in some cases to ascertain whether the timber was stored for any length of time above ground before being used. It was also clear that the identification of species by wood anatomy would supplement the knowledge obtained through pollen analysis and fruits, seeds and other microfossils. The identification of the type of wood in stakes and logs could give information about which tree species were considered suitable for use among those available in conveniently located woods, but the corresponding identification of charcoal from the hearths, for example, ought to represent a less selective choice and consequently give a richer picture of the bushes and trees growing in the neighbourhood. Another easily understood aspect of an examination of the wood evidence was the potential to find out about felling methods and chopping techniques – an area not hitherto studied much in Scandinavia, but one that is of crucial significance for our assessment of the function of flint and stone axes and their importance to Stone Age society.

The microstructure of the pile dwelling, as it could be seen in the documentation of the 1908–30 excavation, raised at least as many questions as the macrostructure. Some of these questions, the answers to which were to be sought in the planned excavation, can be hinted at here.

What is the purpose of the assemblages of branches and thinner twigs that occur at several spots in the pile dwelling, both in the inner part (e.g. F8 and P11) and in the outer part (e.g. C9 and M6)? Can identification of the species and studies of the position tell us anything about their purpose? Do the collections of branches and twigs have the same distribution of species as the log systems and stakes? And if so, can they be interpreted as by-products of systematic trimming of tree trunks on the site? Or is it the opposite, that the branches and twigs show a species composition different from that of the logs and stakes, possibly suggesting that they were selected with a view to making it easier to walk on the surface of the fen, or to make the log floors more comfortable? Can it be demonstrated that they were cut in the summer when the leaves were on them? Do the twigs normally lie parallel, indicating that they were bunched to give the best possible construction material? Or does the species composition reveal a third alternative, namely, that they were used, possibly with their foliage, as fodder for livestock? And if so, is there anything in the location of the twigs that might confirm this hypothesis?

How should we interpret the layers of bark that are noted as occurring in sev-

eral spots on the plans from 1930? Can a species identification confirm that the bark was collected specifically to facilitate living and moving on the log floors and the surface of the fen? Or does an agreement in species with the log systems and stakes indicate that the bark was peeled, deliberately or otherwise, from the logs? Could the bark have been used to roof some kind of timber structure which must surely have been raised above the surface of the log systems and the fen? Could twigs and brushwood have been used to make wattle walls? Can some of the horizontal timber be shown to have been used for upright structures?

In what way can traces of working on wood be utilized? Is the pointing of the vertical stakes exclusively a result of the mode of felling? Or were the ends of the logs deliberately sharpened further after felling? Are there any signs that horizontal logs were cut across and then pointed? The things that look like horizontal logs on the 1930 plans – might some of them actually be split logs, or even planks? Are there traces of any other working apart from tree felling, pointing, and possibly splitting? Are there traces in the form of wood chips?

By what method were branches and twigs removed from trees? Are there marks showing that they were chopped off, or were they knocked off with the aid of a club or something similar? Does the bark provide any clues about the way it was detached from the tree?

What is the meaning of the traces of fire noted in several spots on the 1930 plans? Was the pile dwelling ravaged by accidental fires, and if so, can the extent of the fire be ascertained? Was the outer layer of the wood deliberately charred to prevent rot? Or can all the fire damage to the wood be attributed to controlled fire in the hearths? Or was the wood close to the hearths charred by the heat from the fires? Is there any guidance for assessing whether any of the charred branches were directly associated with boiling or roasting food, such as parts of a raised structure over the fireplace?

Even though wood, twigs, and bark constitute the material for most of the structures in the pile dwelling, both large and small, stone and clay also play significant roles. We mentioned above the mysterious rows of stones over a metre wide which frame the inner part of the pile dwelling in several places. Their extent seems to suggest that they played a part in the macrostructure, but another possibility might be that they are some kind of consumed material removed from the inner part of the pile dwelling so that it ended up outside its limits. A clearly microstructural feature appears to be the irregularly rounded assemblages of stones at several spots in the inner part of the pile dwelling. The limestone ones could be the remains of destroyed hearths, or unused material ready for the construction of hearths. But since these collections of stones were

usually of other material than limestone, judging by the existing documentation, several different explanations are conceivable: perhaps primarily that they were used in connection with the fireplaces, as cooking stones. A correct interpretation evidently presupposed that the type of each stone was noted, along with any traces of fire or other use.

From the documentation of the 1908–30 excavation, the hearths appeared relatively uniform, inasmuch as the predominant construction material was flat limestone slabs. The variations in the form of their circumference – rounded, oval, or more irregular – might perhaps seem insignificant. More importantly, however, it appeared that the hearths sometimes had a periphery consisting of a circle of vertically placed limestone slabs (e.g. a hearth in H5) whereas in other cases it consisted solely of horizontally placed limestone slabs, or of a large horizontal central slab surrounded by smaller slabs slightly inclined inwards, roughly like the edge of a plate. Yet another phenomenon for which an explanation had to be sought was that some hearths seemed completely intact whereas the irregularity of others hinted that they had been damaged during the time the pile dwelling was in use.

In several places, the documentation from 1908–30 indicates clay layers, in most cases adjacent to, around, or underneath the hearths. An explanation for this had to be sought too. Was the sole purpose of these clay layers to facilitate the construction of the limestone hearths and stabilize them? Or should the clay layers be regarded as floor surfaces intended for special activities? To answer these questions requires determining more exactly the extent of such clay layers both horizontally and vertically, and indeed more exact stratigraphical observations in general.

### *2.3. Artefacts and ecofacts. Chronology and function*

The location of the pile dwelling in a fen entails other problems and opportunities than can be expected at a settlement site on gravelly soil. The fen preserves organic material well, such as bone, antler, fruits, and seeds (but not leather, for instance). Through careful documentation of the position of artefacts and ecofacts one ought to be able to gain a much more rounded picture of activities in the pile dwelling than at an ordinary settlement site. At a site on gravelly soil there has been constant digging, mixing the occupation layers, but this does leave pits which constitute closed functional and/or chronological units. Pits of this kind were a priori improbable in the damp fen, and nor are any noted in the older documentation.

Another form of disturbance, however, seemed probable or even certain,



namely the driving of stakes through the occupation layers. An action like this must have had the effect that cultural remains got stuck to the points of the stakes, although nothing of the kind is mentioned in the older documentation. These cultural remains could be of assistance in clarifying the relationship between vertical stakes and horizontal layers. Such observations would obviously require great care and attention during excavation.

During the habitation period the fen was filled with great fen sedge (*Cladium mariscus*) and common reed (*Phragmites communis*; von Post 1916; Magnusson 1964), possibly other vegetation as well. The fen has thus gradually become clogged with vegetation, which ought to have increased the value of the artefact and ecofact stratigraphy.

A careful documentation of the artefacts might be expected to yield results of the following kind. Since the layers can be assumed to be only insignificantly disturbed, the pottery ought to be able to date hearths and other horizontal structures. If the amount of pottery in the excavated areas is not too large, it might be possible to distinguish individual pots, and also the horizontal distribution of sherds, which could possibly shed light on their function. As for the flint and stone artefacts, no contribution to the chronology of the pile dwelling could be expected, unless it turned out to have been used for a very long time. On the other hand, these artefacts ought to be able to yield information about people's behaviour and the function of the pile dwelling. If the pile dwelling had a defensive purpose, this ought to be confirmed by the distribution and quality of weapons, perhaps arrowheads in particular. To the extent that bone, antler, wood, and leather were worked in the pile dwelling, tools for these materials, especially of flint, ought to be found in special activity areas. If, on the other hand, the flint and stone objects were grave goods, cult deposits or trade stocks, this too should be detectable through microchorological observations. The relative proportions of finished tools and flakes/waste of flint ought to be able to say something about the extent to which flint objects were manufactured at the pile dwelling, and above all, a careful study of the distribution of flints should yield detailed information about manufacturing processes.

The distribution of bone and antler objects should be an even more rewarding object of study. Since there is an abundance of animal bones in the pile dwelling, and they are very well preserved, they should be able to show, more clearly than flint and stone, whether craft activities took place in the pile dwelling, to what extent and in what forms.

Similar views to those expressed above can be applied, of course, to any form of artefact. Ornaments, for instance, ought to be in different locations depending on whether they were sacrificed or simply lost.

The animal bones – which can be perceived as artefacts or ecofacts, whichever you like – appeared to be highly informative by virtue of their sheer mass. There would be no need to draw conclusions from one or two isolated units; there would instead be high statistical certainty. It ought to be possible to study how butchering, cooking, and/or consumption took place. One requirement for being able to consider these aspects, however, is that the bones could be identified by an osteologist in the field, so that data on the animal species and the part of the animal could be incorporated in the documentation. An osteologist therefore had to participate in the fieldwork.

As regards carbonized apples and grain, it could be asked on the basis of the older documentation whether the carbonization was deliberate or not. A careful study of the find circumstances ought to be able to provide clues. Small fruits and seeds – also carbonized – are of course difficult to distinguish from the peat during the digging. It was judged that the only chance of retrieving a sufficiently large number was to wet-screen the material excavated from the occupation layer.

For the study of the microstructure of the pile dwelling it was considered essential to excavate using only small tools. The necessity of this is even more obvious when it comes to artefacts and ecofacts. Yet these also require particularly frequent and meticulous documentation. It was therefore planned to have standing sections along certain stretches, in which the position of *all* artefacts and ecofacts would be plotted three-dimensionally, including stone, since stone could only end up in the fen through human activity.

#### *2.4. The planning in broad outline*

It should be clear from the above that the documentation from the older excavation did not permit any certain hypotheses about the main purpose or purposes of the pile dwelling, and scarcely about any individual activity there during the time of habitation, apart from the most elementary: lighting fires and consuming food.

The older scientific studies might justify a hypothesis, that the pile dwelling could only be occupied in the warmer times of year. An attempt could be made to verify or falsify this hypothesis with both scientific methods and archaeological excavation methods. If the pile dwelling was occupied only seasonally, this must inevitably have left traces in structural details.

On the other hand, it was impossible to formulate any hypothesis about whether the pile dwelling had any defensive purpose, unless we take into consideration that the location – in a fen filled with a fire hazard like sedge and

reeds – might make a defensive function somewhat less probable. It was clear that the inhabitants' economy was based in roughly equal measure on agriculture and hunting/fishing. But could the pile dwelling itself have served as a resource or a base for tillage, for animal husbandry, for hunting, for fishing, for collecting vegetable food? The older documentation had no facts on which to base or test any hypothesis in this field. Nor could hypotheses be formulated or tested concerning general craft activities, which might of course have served all these sources of livelihood, but which could also in themselves say something significant about the technological level of the pile dwelling and its cultural relations with other areas. Moreover, should the human bones in the mass of older finds be interpreted as remains of burials, or in some other way? Some facts in the older documentation could be said to enable hypotheses about ritual activities in other spheres – for example, the mere existence of carbonized food – but the chances of testing these hypotheses were minimal.

The conclusions of all the facts and questions presented above in sections 2.1–2.3 were as follows. It is perfectly clear that Alvastra pile dwelling belongs to the Middle Neolithic and to a population with a mixed economy, quite naturally, with components of agriculture and hunting/fishing. The older documentation, on the other hand, could not answer the question whether this economy was in any way supported by the pile dwelling as such, or what purpose it might otherwise conceivably have served.

The planning of the new excavation thus had to seek to enable the testing of all the hypotheses mentioned above concerning the purpose of the pile dwelling, plus all the hypotheses that might be found justified in the course of the work. The way to achieve this had to be to conduct the excavation with the greatest care possible with the available means, and enlisting adequate help in the natural sciences.

Yet even the limitation to three small trenches described above gave this generally formulated programme so many possibilities that it was found most proper to permit certain variations in excavation technique between the different trenches: the strategy would be the same, but the tactics would depend on the individual character of the structures discovered, and on the particular hypotheses that the excavation leader in each trench wished to test.

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## V.

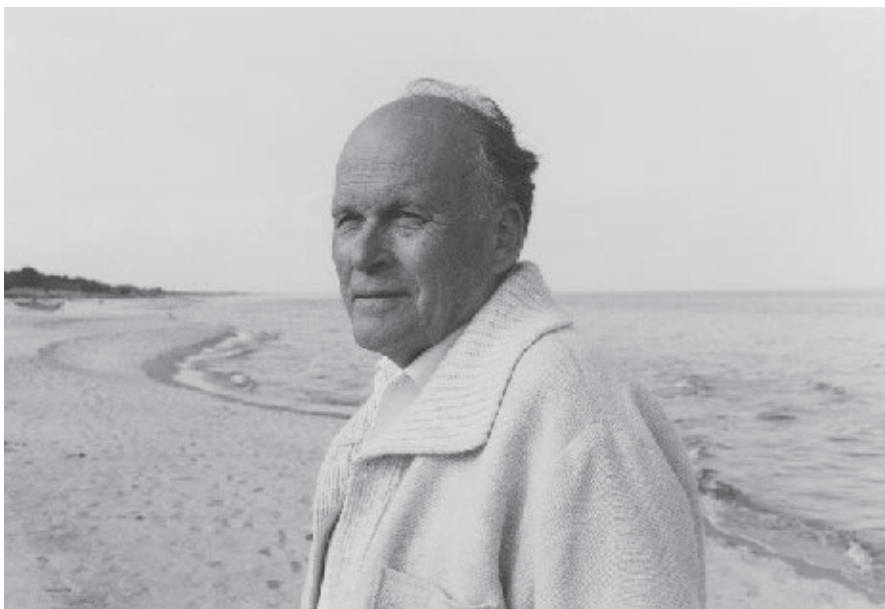
### An archaeological life

MATS P. MALMER (fig. 18:1) was born in Höganäs in southern Sweden in 1921. He completed his university studies in Lund in 1962 by defending his doctoral dissertation, *Jungneolithische Studien*. At that time he had been head of the Stone Age and Bronze Age Department at the National History Museum in Stockholm since 1959. This was followed by twenty years as a chaired professor, first at Lund University 1968–1973, then at Stockholm University 1973–1988. In 1949 he married Brita Malmer (*née* Alenstam). They had one daughter, Elin (fig. 18:2). Mats P. Malmer died in 2007 (Andrén 2008).

In 1995 Malmer published an account of how he lived his life in archaeology (Ch. 18). He writes that “archaeology is tremendous fun” and about archaeology being like an intellectual journey, about people he has met and who have made an impression on him, in both a positive and a negative sense. This piece can be complemented by the final section about “harmful sectorization” in a 1984 debate with Arne B. Johansen about “Priorities in Swedish Archaeology” (Malmer 1984:273 f.). Here Malmer writes about the ideal organization of archaeology in Skåne at the Lund University Historical Museum in his archaeological youth. This, Malmer felt, was an environment that had nurtured experienced archaeologists with wide all-round expertise.

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*Fig. 18:1. Mats P. Malmer on the long sandy shores of Hanö Bay in eastern Skåne, near the Haväng dolmen.*

## The foundation of my life in archaeology

1995

I HAVE BEEN asked to speak on the subject of *Archaeological experiences: academic and private*. And I think I have suffered rather unfair treatment. Other speakers here have been allowed to talk about interesting people, about famous archaeologists like Thomsen, Sehested, Hanna Rydh, and Gudmund Hatt. But I have to talk about myself! It would have been much more interesting and enjoyable to be allowed to say something about, for example, Christian Jürgensen Thomsen – something I would really like to have done.

But since there is no escape for me, I have to decide how one can talk about one's own life in archaeology. Obviously, one can speak about one's rare successes – and claim with false modesty that I don't deserve the credit, that it was mostly a matter of good luck and help from others. And people see through you at once: *Listen to his boasting! He's just saying that to show off how good he is*. Alternatively, one can choose to speak about one's failures and disappointments. And then people react – quite rightly – like this: *I can't bear to listen to this! What has he got to complain about? He's done all right for himself. But look at how unfairly we have been treated!*

This, of course, is the dilemma of every writer of memoirs. My way of solving the problem is to abstain from presenting a catalogue of either pleasant or unpleasant events in my archaeological life. Instead I shall say something about my outlook on today's and yesterday's archaeology. And I shall attempt to consider whether these views of mine are based on my personal experiences.

I shall begin with a declaration that you may find shocking: *I think that archaeology is tremendous fun*. We must all have met engineers, dentists, computer operators, and others who say that they regret not choosing another career, for example, that of an archaeologist. I, however, have never regretted my choice; since I started in archaeology in 1945, I have never wanted to work with anything else. As everyone knows, it is rare for people to be happy in their occupation, and if you meet a person who is happy, he will say that he is fortunate to have his occupation as a *hobby*. But I think it is completely wrong to put it like that, at least in my case. I do not pursue archaeology as a hobby; it is as a *science*





Fig. 18:2. Mats P. Malmer with his daughter Elin, summer 1967.

that it is fun. I also believe that archaeology is important, and I would not have done it if I hadn't found it enjoyable as well.

After the famous year of 1968, at the start of the 1970s, you made a fool of yourself if you said that archaeology was fun. Traditional archaeology was regarded as petty fiddling with beautiful or curious or boring artefacts, and it was definitively not *comme il faut* to think that that was fun. Science was not important or committed unless it could be used in politics, in the cause of a major transformation of society. I presume there are a few people here in this room who still believe this, but probably not as many as in the 1970s. For my part, I think that the findings of archaeology can be used for a lot of things – including a discussion of today's society. Archaeology is important precisely because it is human and multifaceted, and that is what makes it fun to work in archaeology.

Archaeology is indeed multifaceted. In 1946–48 I and many others took part in Carl-Axel Althin's Mesolithic excavations in the Ageröd bog (fig. 18:3). We unearthed thousands of microliths, and they shone like diamonds when we dug them out of the wet peat. But no one knew for certain what function a microlith had. Then it happened on 23 May 1951 that Brita and I were summoned to a bog at Lilla Loshult in northern Skåne – peat cutting had turned up something, but no one could explain what it was. When the man lifted the wet peat that he had wisely placed on top of the find, we saw a wooden arrow shaft with



*Fig. 18:3. Section digging at Carl-Axel Althin's Mesolithic excavations in Ageröd Bog, Skåne, in 1946. Each find was plotted in three dimensions. The excavators are, from left to right, Herbert Salu, Mats P. Malmer, and Gad Rausing.*

two microliths as point and barb. We were the first people in the world who knew for sure how microliths were used!

Every archaeologist has no doubt had a similar experience, but it appears that we judge them differently. In the last twenty years it has often been claimed that it is not worth excavating any more – it is through new ways of *thinking* that archaeology will progress. Of course we cannot do without thinking, but in my opinion it is a complete misconception to say that fieldwork, the digging, no longer has any part to play. The growth of factual knowledge about the ancient past has mostly taken place through new finds and new excavations – in the last twenty years as well. It is enough to take a few examples to remind us of what I am talking about. Large Mesolithic cemeteries such as Bøgebakken and Skateholm. Neolithic cultic sites such as Sarup and Stävie. Bronze Age houses like those at Ridtoft and Fosie. The Eketorp ringfort. The Viking ships in Roskilde Fjord. The warship Vasa. No one can deny that these and many hundreds of other excavations have significantly increased our knowledge.

But new finds do not just mean a growth of knowledge. They are something else as well, which is usually dismissed as romantic treasure hunting. And treasure hunting is something that ordinary people might possibly be forgiven for, but definitely not serious archaeologists. Yet I believe that the romance of treasure hunting can be expressed in more positive terms. The new find removes a person, psychologically speaking, from mundane everyday existence to a point located above and beyond. And the result is that you see everyday life, including the old familiar archaeological material, in a new way. It is generally known that an *idea* can have that effect. But what I claim here is that a new archaeological find can also have a liberating effect.

This brings me quite naturally to how my life in archaeology began. I came to Lund in 1943, in the middle of the war, to study history and Scandinavian languages. My studies were not particularly effective – not because I did not find them enjoyable, but because social life was considered more important, by myself as well. Professor Gottfrid Carlsson lectured four terms in a row about the history of the archbishops of Lund – a pleasant causerie once a week. For the autumn semester of 1945 someone told me that a propaedeutic course in Nordic and Comparative Archaeology was given down at the Historical Museum, and if you took that you had a better chance of passing the history exam with distinction. I went to the Historical Museum for that purpose, and I stayed there for fifteen years.

The propaedeutic course was fine, but what captivated me – literally captivated me – was a seminar chaired by Associate Professor Carl-Axel Althin. He told us about John-Elof Forssander's excavation of a gallery grave at Gislövs-

hammar in Österlen – a highly unusual gallery grave, under a barrow with a surrounding wall. Althin thought that archaeology was fun, and he was very good at conveying his enthusiasm to listeners. It was during that seminar that I decided on archaeology – of that there is no doubt.

For several years Carl-Axel Althin was a crucial person for me. Among other things, I took part in his Mesolithic excavations in Ageröd bog. It was an intellectual excavation, with a high degree of theoretical and methodological awareness. Moreover, the good and cheerful atmosphere within the large excavation team was unmatched. Our accommodation was exceedingly primitive, old military huts on a green meadow, miles from the nearest house, with the starry sky above our evening parties, and ice in the washbasins on the autumn mornings.

I should mention a few other archaeologists who were important to me in the very first years. Holger Arbman, who became professor in Lund in 1945, had very wide and deep cultural interests: literature, music, theatre, and politics. One might even suspect that these interested him more than archaeology. He also had another highly unusual characteristic: although he wrote a great deal, and was profoundly involved in many fields, he always had time for people. I can't understand how he managed it, but his door was always open to everyone. He had a lively interest in people, all kinds of people.

This was of crucial importance to me. I wrote my self-willed doctoral dissertation, *Jungneolithische Studien*, which differed in every respect from what Arbman had expected – theoretical basis, methods, and results. He read the manuscript bit by bit, and he was astonished but he never tried to get me to work differently. One could say that he not only tolerated research that contradicted his own, he actually took pleasure in the fact that it was different, and encouraged students to continue in this way. This was, I say again, wholly crucial to me. Many professors believed, and still believe, that dissertations in their department must agree on every point with the professor's own views. With a professor of that kind, I would never have been able to write my dissertation, and I am eternally grateful that I ended up with Arbman. I have always had the same perception as he about the freedom of research and the conditions for research, and since I myself have attained his position I have tried to apply it in practice. To paraphrase a well-known expression, I have always said that *in my department everyone can go to heaven after his own fashion*.

A third archaeologist whom I was fortunate to meet during my first years was Gordon Childe. He came to the Ageröd excavation in 1947, and in the following year, 1948, he arranged a trip for me to England and Scotland. Apart from visits to Denmark, this was my first time abroad – I was 26, but it had not been possible to travel during the war. Childe let me attend the annual general meet-

ing of the Prehistoric Society in the Lake District in Cumberland, and I also met him at the Archaeological Institute in Regent's Park in London. In between these occasions I was in Oxford and Cambridge and Edinburgh and Sussex and East Anglia. And one sunny summer's day I walked along Hadrian's Wall, heard the larks in the sky, and saw for the first time the boundary between Roman and non-Roman.

Childe was even more of an original than he appears in photographs – usually wearing a battered hat and a creased suit, often with an orange shirt and red tie, which nobody else wore at that time. He had bushy eyebrows and a moustache, and he looked more like a Neanderthal man than the Oxford don he actually was. He grunted rather than spoke, and he had a good command of several foreign languages, including French and German. The only problem was that no one could tell for certain *which* language he happened to be speaking – it all sounded much the same. Yet one could understand quite well what he meant. His knowledge was enormous, and he could answer any question about the Stone Age and Bronze Age in Europe. He would snap viciously if a colleague said anything stupid, but he was kind to students. He liked to sit and talk with them, with or without a glass of beer in his hand. He was kind to me too, and put a lot of thought into the organization of my visit to England. I later read most of what he wrote, and I admired his broad grasp and his interpretation of cause and effect. On the other hand, I could not reconcile myself with his lack of precision when describing material in words or pictures.

A fourth archaeologist of crucial significance for me is Brita. I met her in 1946, when she was 21, and we got married in 1949 (fig. 18:4). We met during the Ageröd excavations, and worked together there and on many other field campaigns. Brita was an archaeologist at the time, just like me. She did not become a numismatist until the mid-1950s, and there was a calculated decision behind that. We thought it might be difficult for two archaeologists to obtain positions in the same city. The calculation worked in 1959, when I got job at the Swedish History Museum and we moved to Stockholm. Between 1970 and 1973, however, I was professor in Lund and Brita was director of the Royal Coin Cabinet in Stockholm, so I had to commute between Lund and Stockholm, fourteen hours of train travel every week. But I would be ungrateful if I did not admit that fourteen hours of reading with no telephone interruptions is both pleasant and educational.

Brita has read the manuscripts of virtually everything I have written, just as I have read what she has written. For me at least, this has been essential. It is difficult to criticize a manuscript constructively, and it is difficult to accept criticism in the right way. You are sensitive when you have written something, and





*Fig. 18:4. Mats P. Malmer met his future wife when they were both studying archaeology in Lund. Brita Malmer later became the first holder of the Gunnar Ekström Chair of Numismatics and Monetary History at Stockholm University. During excavations at Bedinge in the summer of 1950, Brita and Mats had been married for just under one year.*

we have not found it possible to carry on such discussions with anyone but each other.

Being a couple with writing as their profession has another side. Writing is creative work, and everyone knows that a person engaged in such work needs domestic support. This means that we have never been able to write any large works at the same time. One of us writes, the other provides domestic support – this is how it must be. And there is nothing to complain about. On the contrary, it feels proper and healthy to switch between different tasks.

Up to now I have illustrated my declaration that I think archaeology is fun. Now I shall turn to my declaration number two: *I believe that the duty of science is to seek the truth. Consequently, I believe that the duty of archaeology is to seek the truth about the ancient past.* Not much that I have said or written has been criticized as much as that declaration. And of course one would be justified in asking me whether I have found the truth. My answer to that would be no, I have not. And worse, I never will find the truth, not the whole unadulterated truth. But I believe that there is a priceless value in searching for the truth, even if you never reach it.

When I began studying at university, there was nothing suspicious about believing that the task of science is to seek the truth. On the other hand, it was rather naïve, jejune, to say something so self-evident – and therefore, of course, it was not something you said outright. But the basis of our thinking was obviously, whether we knew it or not, positivism or logical empiricism, as outlined by Wittgenstein, Bertrand Russell, and others. Their idea of research, as we all know, came from the natural sciences, making a sharp distinction between facts and values, and between science and personality. And they believed they were looking for the truth. An important element was the strict distinction between science and metaphysics, including the kind of metaphysics expressed in fascism and national socialism. Those were living concepts for people born in my year, 1921.

This brings me to my childhood environment. I was born in north-west Skåne, near the Sound. You could see Denmark across the water, and I was in Helsingør countless times – I believe it cost 35 öre to take the ferry across. For me Copenhagen was the *big* city, and Hamburg the *very big* city. I think I visited Copenhagen before I was three years old, but Stockholm and Central Sweden were quite hazy concepts – I was 20 before I got there. I was thus oriented towards Denmark and the Weimar Republic; my father was in Germany occasionally and had a great deal to tell us about it.

And now I face a serious dilemma which the organisers have set up for me. One can hardly talk about one's life – at least not one's *private* life – without





*Fig. 18:5. Mats P. Malmer (the boy nearest to the camera) grew up in Höganäs in Skåne. Here he is seen together with his parents, Ingeborg Malmer Petersson and Sigurd Petersson, both school teachers, and his siblings Ingemar and Anna.*

mentioning one's parents (fig. 18:5). But *how* can you talk about your parents? There is a private and a public language, and they can scarcely be mixed. I have chosen to cut out a couple of silhouettes.

First of all, there are only two alternatives in relation to parents: you either join them or you rebel against them. I chose the first pattern.

Father had a natural talent for mathematics, and I have often had reason to regret that I did not inherit it. It was easier for me to share his interest in cultural history. Mother was well read; one of her favourite pursuits was to read French and Latin authors in the original. Both were teachers, and they had no ability whatever to fulfil themselves in any kind of career. Instead I remember my childhood as a constant intellectual discussion, where parents and children took part on equal terms. And that is surely the most important foundation of my life as an archaeologist.

The conversations at home also included politics, of course – a lot of politics. And the politics was to the left, sometimes quite far to the left. In January 1933

I was in hospital after an operation, and one of the first things my mother said to me when I woke up after the anaesthetic wore off was: "Do you know, Hitler has now become Chancellor!" That affected her so much that it was the most important thing she had to say to her eleven-year-old son.

After the war there were many who had never heard of the German concentration camps. But Gerhart Seger described his experiences as a prisoner in a book entitled *Konzentrationslägret*, in German *Oranienburg*, which was published in 1934 (English translation, *A Nation Terrorized*, Chicago 1935). The book was in our house and I read it. Another now forgotten man was the National Socialist president of the Senate in Danzig, Hermann Rauschning. He left the party and in 1939 he wrote a book entitled *Gespräche mit Hitler* (English translation, *Hitler Speaks*, London 1939). It is a fairly accurate prediction of what would later happen during the Second World War. The book appeared in no fewer than five Swedish editions, all of which were confiscated by the Swedish government. It was not until the sixth edition that it was possible to buy it in a Swedish bookshop, but that was in 1945, of course. Mother could not accept that situation, so she sent for the French edition, *Hitler m'a dit*, printed in Paris in 1939.

And now it is time to go back to the old positivists. Their famous mistake, as everybody knows, was to believe that with collected data as the sole basis they would be able to find their way to an explanatory hypothesis or theory. Carl Hempel's objection – actually very simple but nevertheless world-famous – in *Philosophy of Natural Science* (1966) runs as follows: "Scientific hypotheses are not *derived* from observed facts, but *invented* [by scientists] in order to account for them." And this brings us to our old favourite Thomas Kuhn and his *Structure of Scientific Revolutions* (1962). It does not help to test a hypothesis, says Kuhn. It is verified or falsified on its own terms anyway – and so can a diametrically opposite hypothesis be as well. No one hypothesis is better than another. They are just different ways to perceive reality, different *paradigms*, as people said for a time.

From Kuhn it is a short step to Christopher Tilley and pure relativism. Tilley says, as most of you have probably noted, that one can never arrive at an objective and uncontroversibly true understanding of the archaeological record. Consequently, we are entitled to interpret it as we like – and use it for any subjective purpose whatever, for example, as political propaganda. In *Social Theory and Archaeology* by Shanks and Tilley (1987), for instance, we can read these words: "Choosing a past, constituting a past, is choosing a future. The meaning of the past is political and belongs to the present." Tilley's stance is the exact opposite of mine. We agree, it is true, that one can never arrive at an absolute

truth. Yet I believe that we must nevertheless try to work our way towards the truth, otherwise what we are doing is not science. Tilley, on the other hand, sees other possibilities. For him the point is to search the archaeological material for evidence to substantiate the Marxist theses that are the foundation for the socialist society he wants to see realized.

Tilley's present stance would no doubt have been inconceivable without the famous year of 1968. I shall not get into politics any further than I have already felt forced to. Yet I must go one step further. For me, a great deal of 1968 and the early 1970s can be summed up in one word: *conviction*, an unshakable faith. They were *convinced*, they even *knew*, that conditions in the Soviet Union were ideal in terms of freedom, equality, and the fair division of material resources. When this conviction was rendered impossible through objective information, they became *convinced* that it was instead China that had the ideal conditions. And when this conviction was no longer tenable, Albania became the ideal country.

I have come across people with such firm convictions once before, in the 1930s. They too had many fine ideas. They wanted to abolish the class society. They wanted to preserve nature, plant and animal life, and they were anxious to keep themselves in good physical condition and abstain from toxic substances. I am referring to the National Socialists. There were years when I was almost alone among my contemporaries in arguing in favour of left-wing political views. All those people of any importance, all the active people who were leaders by nature, automatically adopted the wonderful ideas of the new age, and were profoundly scornful of conditions in Sweden under the first Social Democratic government.

Once, not long ago, I was talking about old times with a famous archaeologist of the older generation. I valued him highly, and still do, as a scholar and a human being. And this wise old man said: "Can you imagine anything as remarkable – all my relatives were Nazis, I was the only socialist!" And I replied: "Yes, that was indeed remarkable – life can be so strange!"

I said this, however, only in order not to sadden the old man. What I actually thought was: "No, old fellow, it's not at all remarkable; on the contrary, it's perfectly normal". It is a matter of temperament whether you choose a political line where the most important thing is a fervent belief, or if you try to base your politics on facts and strive, to the best of your ability, for the truth.

I am, as you will notice, interested in politics, and I respect all forms of serious political commitment. Moreover, it can be said that politics and archaeology have the same goal, namely, a knowledge of the human condition, and hence the possibility to improve people's lives. Nevertheless – or perhaps precisely for that reason – they should not be confused. Politics is a will to power,

and research is a quest for knowledge, and the two are incompatible. In the seventies, as we know, people said that there was politics in everything, and if one did not follow a conscious political line in one's research, one was simply unaware of which political goals one furthered. I do not believe that. I am willing to make yet another declaration: *I believe that research without political ties is both possible and necessary.*

Someone might think that I am exaggerating the danger of mixing archaeology and politics, but I would ask you to remember the international archaeological congresses. I have attended quite a few, ever since the extremely fine little congress in Zurich in 1950. There were only a couple of hundred delegates, but they included Abbé Breuil, Louis Leakey, Kurt Bittel, Johannes Brøndsted, Hugh Hencken, Robert Braidwood, Gordon Childe, Grahame Clark, Sean O'Riordain, Albert Egges van Giffen, Jozef Kostrzewski, and Emil Vogt.

It was different at the latest congress. The English arranged a congress in Southampton in 1986, which was boycotted for political reasons by the International Union for Prehistoric and Protohistoric Sciences. Then the Germans organized a conference in Mainz in 1987, that too with a political slant causing many people to absent themselves. Very few archaeologists attended both Southampton and Mainz; Brita and I were there, as was David Wilson. My idea in attending both events was, of course, to see whether there was any chance of preventing the world congress from being split into two politically coloured parts. I thought it was my duty to try, since I was the Swedish member of the *Conseil permanent*. The attempt was a total failure, of course. In Mainz they even refused to elect Sir David Wilson to the committee revising the constitution of the congress, as he was considered far too politically radical. Instead they elected the now deposed head of all archaeologists and historians in the German Democratic Republic.

The only result of my efforts was that many people said to me: "How could you get involved with those repulsive left-wing radicals in Southampton?" And just as many said: "How could you get involved with those repulsive reactionaries in Mainz?" The lesson of this is that it is safest to move around in a herd. But I knew that long before, needless to say.

Now I shall take the liberty to go outside the topic on which I have been asked to speak. I am going to talk about my relationships to museums and museum material. Having worked in museums for 28 years, half my adult life, I think I have a right to do so. For ten years I was head of the Stone Age and Bronze Age Department at the Swedish History Museum, and for eighteen years I served at the Lund University Historical Museum, the last three years as its director.

I don't think any job has felt so easy for me, or given me such great satisfaction, as demonstrating a museum exhibition. In recent years people have been desperately asking themselves what an archaeological exhibition should be like. But only a person with a specific message, usually a political message, can give an unambiguous answer to that question. I think that the best understanding of an archaeological exhibition can be obtained from a guide who can interact with the visitors and deal with their many different interests and questions.

In the last twenty years, many people have spoken about museums as if their most important task were to mount exhibitions for the general public. It is not, of course. No one expects that the National Archives should work mainly with the exhibition of documents for the general public. Archaeological museums are national archives for nine tenths of our history. Their most important task is to serve research. In keeping with that conviction, I did my best to organize the Stone Age and Bronze Age stores at the Swedish History Museum and to keep them in order.

Thus speaks an archaeologist with a true material fixation, you might be thinking, a traditional archaeologist who has understood nothing of the new, up-to-date archaeology. Not at all. I think, on the contrary, that the explicit theoretical discussion in the last thirty years has been the greatest progress made in archaeology since Ch.J. Thomsen. But there are two great misconceptions associated with the New Archeology. One misconception is that the New Archeology has added huge amounts of knowledge about prehistory. I would say that that is *not* the case. The New Archeology has produced new and important knowledge about the archaeological research process – but relatively little about prehistory.

The other great misconception is that earlier archaeology was extremely fixated on material, devoting itself exclusively to artefacts while completely ignoring other aspects. On the contrary: the fact is that most of our knowledge about ecology, economy, people, and society comes from the *old* archaeology.

The reason why many people now believe that the old archaeology had a material fixation is embarrassingly simple: some representatives of the newest trends stubbornly and ignorantly claim that it is so. This claim is completely false. On the other hand, it is true that the old archaeologists were highly diffident about jumping to conclusions. It therefore often happened that they published a large corpus of material but only hinted at conclusions. Everyone who was there at the time knows *why* they did so. The old archaeologists were concerned with the truth. They told themselves, "If I publish the material now, some other scholar will come along soon and publish precisely the additional material required to formulate a really well-founded hypothesis. And other re-

searchers who read my book will discover new angles and new interpretations that I may not have noticed. I therefore show restraint when formulating my interpretative hypotheses.”

It is not strange that the old archaeologists thought in this way, precisely because this is how scholars thought and still think in all sciences – except in parts of the social sciences, especially social anthropology and archaeology. Archaeology has not shown sufficient independence but has instead clung to anthropology, and anthropology is very much a part of present-day politics.

Archaeology must separate itself from politics to be able to approach the truth. Can one really learn anything from archaeology alone? Of course one can. For humanities scholars, every narrative about human beings is of interest. Natural scientists are increasingly realizing that the most interesting knowledge about nature is the knowledge that is most closely linked to the human situation. And of all the sciences, archaeology is the one that seeks most consciously to view time and culture, nature and mankind, in a holistic perspective.

## VI.

### Mats P. Malmer's bibliography

MATS P. MALMER'S oeuvre up through 1986 is listed in B. Malmer 1987, and then up through 2001 in Trotzig et al. 2001. After 2001 he published the following two works:

Malmer, M.P. 2002. *The Neolithic of South Sweden. TRB, GRK and STR*. Royal Swedish Academy of Letters History and Antiquities, Stockholm.

Malmer, B. & Malmer, M.P. 2005. Om forntida färdvägar och samiska offerplatser. In: Engelmark, R. et al. (eds), *En lång historia. Festskrift till Evert Baudou på 80-årsdagen*. Archaeology and Environment 19 (pp. 321–330).

Malmer's bibliography consists of 144 titles, which includes five monographs and 92 articles in reference works.

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Malmer, B. 1987. Mats P. Malmer's published works 1948–1987. In: Burenhult, G. et al. (eds), *Theoretical approaches to artefacts, settlement and society. The archaeologist Mats P. Malmer*. BAR International Series 366 (i) (pp. 541–547).

Trotzig, G. et al. 2001. Mats P. Malmers bibliografi 1987–2001. *Fornvännen* 96 (pp. 191–194).





## VII.

### Sources of the original articles and book chapters

THESE ARE THE original titles and sources of the papers and book chapters reprinted and, where necessary, translated in this volume.

1. Malmer, M.P. 1963. The methodological grounds of material culture. In: Malmer, M.P., *Metodproblem inom järnålderns konsthistoria*. Acta Archaeologica Lundensia, Series in 80, No 3 (pp. 248–266).
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MATS P. MALMER (1921–2007) was Professor of Archaeology at the universities in Lund and Stockholm from 1968 to 1988. Throughout his career he influenced generations of students and colleagues as a researcher, author and debater.

This book collects book chapters and journal papers by Malmer, most of which have never appeared in English before. They range from the publication of his excavation of a Medieval hospital in Scania in 1948 to his monumental overview of Sweden's Neolithic from 2002. In between are writings where Malmer elaborated the views of archaeological method and theory first formulated in his likewise monumental dissertation *Jungneolithische Studien* from 1962. In its contemporary context, Malmer's work was innovative and controversial.

The pieces collected here were largely selected by M.P. Malmer's wife, Brita Malmer, Professor of Numismatics. A number of never previously published photos from the family album have been provided by their daughter Elin Malmer.

STIG WELINDER, Professor Em. of Archaeology, defended his doctoral thesis under the aegis of M.P. Malmer in 1972. He has commented on the texts and written an extensive introduction.



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