

- CRAWFORD O.G.S. 1922: A prehistoric invasion of England. *Antiquaries Journal* 2, 27–35.
- EVANS E. 1930: The sword-bearers. *Antiquity* 4, 157–72.
- FOX, J. W. 1987: *Maya Postclassic state formation*. Cambridge.
- HAWKES C.F.C. & DUNNING G.C. 1931: The Belgae of Britain and Gaul. *Archaeological Journal* 87, 150–335.
- HAWKES C.F.C. & DUNNING G.C. 1932: The second Belgic invasion. *Antiquaries Journal* 2, 411–30.
- KRISTIANSEN K. 1991: Prehistoric Migrations – the Case of the Single Grave and Corded Ware Cultures. *Journal of Danish Archaeology* 8, 211–25.
- PERRY W. 1924: *The growth of civilisation*. London.
- PIGGOTT S. 1938: The early Bronze Age in Wessex. *Proceedings of the Prehistoric Society* 4, 52–106.
- ROUSE I. 1986: *Migrations in prehistory: inferring population movement from cultural remains*. New Haven.

Research History of the Single Grave Culture – a Commentary

by C. J. BECKER

In the latest number of *Journal of Danish Archaeology* Kristian Kristiansen contributes to the debate whether prehistoric immigrations can be established from the archaeological sources alone, and as an example chooses to renew the old argument about the Jutland Single Grave Culture (SGC). The question of the first appearance of this culture in Denmark has played a central role in our whole conception of the cultural development in the country's Neolithic, and as well as being important for the interpretation of related groups both in other parts of Scandinavia and in much of central and eastern Europe. Its importance can be attributed to a well established internal relative chronology, the large number of finds, and not least to the fact that there is such a rich and well studied material from the other (mainly earlier) group, the Funnel Beaker Culture (TRB). The problems are now, as earlier, the exact dating of the two groups in relation to each other, and the question of their economy and material/mental culture. During the last decades discussion has revolved chiefly around the first question. Did a massive immigration of a new people take place, or were there for some reason only radical changes in the economy, burial customs, and entire material culture of the old population? The immigration theory remained the dominant one until the early 1960's, while subsequently the alternative view received much support especially from archaeologists of the younger generation. The question has still not been finally answered. KK's article is therefore an important contribution to the discussion. My comments on it will be confined to making supplementary points and criticisms of the author's conclusions.

KK divides his comments into three sections. First comes a review of the theoretical models that in the 1960's and 1970's inspired Scandinavian archaeologists to try new interpretations, and were one of the main reasons for the emergence of a different, and to many older colleagues surprising view of the cultural development in especially the Scandinavian Neolithic. Such a review is helpful as a guide through the history of research. One is given among other things an adequate explanation of why "migrations" as a concept do not necessarily involve radical changes in culture, and why they are left unmentioned in new scholarly and popular descriptions of the cultural history of the Neolithic. Even the National Museum exhibition (up to now) follows this line, except with the Pitted Ware Culture.

KK's next section deals with the Jutland SGC, and it is mainly here I have comments to make. The third section deals with related Corded Ware groups in the rest of Europe and beyond, and would be difficult to discuss in brief despite its qualities and interesting viewpoints.

In his earlier writings about the Jutland SGC KK has inclined to the autochthonous interpretation, no doubt owing to his extensive knowledge of and the way he has been influenced by more recent Anglo-American scholarship, the so-called “New Archaeology” (an expression of diminishing applicability!). It may surprise some readers to find he no longer regards social-anthropological or economic factors as the explanation of the SGC’s appearance, but speaks of the immigration of a new people. Like every other serious scholar KK has a right to a new opinion, and it should surprise no one if several of his colleagues were to follow suit. To me this is far less important than KK’s account of his earlier attempts to explain the appearance of the SGC in Jutland. So long as a problem in archaeology has not been solved to everybody’s satisfaction, every possibility should be looked into and evaluated critically; this is the only road forward for scholarship. We may however take note that once again evolutionist theories seem not to have been able to solve the problem.

JDA is an international periodical, and this may lend the question especial importance, for it is not merely an internal controversy for Neolithic research in Scandinavia, but has been regarded as a schoolbook example of cultural historical interpretation applied to a purely archaeological material (Brøndsted 1957, 260). This means that a short account of the background in research history is entirely in its place, but the account should include the main points and refer to the papers where the new material or new interpretations were first presented. It is not enough merely to mention the most recent article reiterating an argument that may have been used several times before; and the main phases of the debate should in all events be stated correctly.

To mention some places where these lines have not been followed, Sophus Müller (1898) and P.V. Glob (1944) are quoted at the beginning of the section on the Jutland SGC for seeing this as representing the immigration of “Indo-European speaking peoples”. In 1898 Sophus Müller did not mention Indo-Europeans at all, and in 1944 Glob cites other, mainly German scholars’ theories, and concludes prudently that the immigrants may have been Indo-European peoples without mentioning their language at all (1944, 235 ff.).

Dealing with the history of more recent research KK describes the change to evolutionistic interpretations that resulted from the increasing numbers of ^{14}C dates, and then says that these can support arguments against the immigration hypothesis from a different direction. As it is the situation in Jutland that is being considered, a better starting point might have been Malmer’s work (1962) on the Swedish-Norwegian Battle Axe Culture, which takes a major part of its at that time shocking arguments from new studies of the Jutland group. His book is earlier than the whole ^{14}C debate. Afterwards the role this came to play for the whole discussion could properly be described. A number of younger archaeologists concluded, as KK says, that the chronological objections to the evolutionary theory were now removed. However not all were convinced (Davidsen 1975, 1978; E. Jørgensen 1977). It would also have been possible to refer to Becker (1973, 180), where it was emphasized that scientific and archaeological methods should lead to the same conclusions independently before the evidence can be regarded as

convincing. Most people have failed to see that though there are many ^{14}C datings of the three phases of the SGC, there are still too few of the late TRB. Furthermore the familiar safety margins of the method mean that the two Neolithic groups could have lived side by side for a couple of generations – which is enough to support an invasion hypothesis. Has it been forgotten that “reliable” pollenanalytical and geological studies in the 1930’s and 1940’s let the Ertebølle Culture survive far into the Middle Neolithic?

For KK the debate on immigration versus indigenous development of the Jutland SGC is by and large confined to the last decades. When facing the problem for the first time – for instance as a student – the “old” immigration hypothesis really stands opposed to three propositions, all of which conclude instead that there was an indigenous development. In correct order they are 1) Malmer’s re-interpretation (1962), 2) various contributions connected with ^{14}C dating, and 3) theoretical studies rooted in arguments from “New Archaeology”. KK’s return to the invasion hypothesis may not seem so epoch-making to colleagues with “old-fashioned” views. The discussion does not really seem so new, despite the fact that the date of the invasion in relation to the TRB has been changed twice since 1944.

If the debate as a whole is to be described correctly – for instance because of its place in research history – it should also be said that the discussion took place in the 1930’s and 40’s. This is a serious omission on KK’s part. As a full account is available in print (Becker 1954, 132-37) there is no need to repeat here the arguments that were used to support an autochthonous solution (Åberg 1937 and 1949), nor the repeated opinion of Danish scholars that the SGC’s appearance in Jutland could only be explained as the immigration of a new folk from the south. Also Brøndsted’s considered and cautious views could have been mentioned.

Work from the 1950’s and 60’s on the TRB in central and western Jutland could have been placed in a different light than by KK (p. 212ff.). He writes that no systematic efforts to prove or disprove the immigration theory was made either then or later, and apparently does not lay much weight on the works mentioned (Davidsen 1975, 1978, E. Jørgensen 1977). In my opinion there lay systematic research behind these two important works, and it completely changed the picture of the late TRB in these areas. Firstly a previously unknown final phase of the TRB was identified in 1954 (the Valby or MN V phase – Becker 1954), and it was also present in central and western Jutland. Secondly a new type of burial structure, the stone packing grave, was found in the same regions, and it showed that the TRB culture was present throughout the Middle Neolithic in many of the areas where the SGC made its first appearance. It is hard to understand why KK continues to describe these areas as sparsely populated or deserted. Also he mentions the stone packing graves in a curiously obscure manner. It is true that they are individual graves, but their entire construction (meaning the fixed rituals behind them) are as different as possible from the earliest SGC graves.

This brings us back to KK’s assertion that really no systematic attempt has been made to confirm or disprove the immigration theory. What arguments would KK accept? Presum-

ably if evidence could be obtained from e.g. the examination of skeletal material from the final TRB and the earliest SGC in Jutland, this would be sure enough, but as everyone knows the lime-poor soil is the reason why not a single properly preserved skeleton has been found from either the stone packing graves of the TRB or the earliest Single Graves. For the same reason it is not possible to study the economy of either group properly. Animal bones are absent from the few known settlements. Impressions of cultivated plants (or carbonised material) are still too scarce for any definite conclusions. It may be noted in parentheses that the common view that the cereal crops of the TRB were wheat and barley, but the SGC only had barley, is not correct, as also wheat impressions are found in the pottery of this group (Rostholm 1986a, 231). Finally, it is still unclear whether ¹⁴C dating can answer this particular question. As well as the familiar margin of uncertainty, continued research on calibration curves seems to reduce the possibilities especially at this point of time. What about new systematic excavations? Perhaps, but archaeologists with field experience know how little chance even the best prepared project would have with our present knowledge.

The problems surrounding the immigration theory must not be laid aside, they must be capable of a final solution. We must be optimistic and allow ourselves to await one of the surprises that are one of archaeology's most charming aspects.

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REFERENCES

Most references are to works cited by Kristiansen. The following may be added:

- BECKER, C.J. 1954a: Die mittelnololithischen Kulturen in Südkandinavien. *Acta Archaeologica* XXV (reprinted privately 1971).
- 1954b: Stenalderbebyggelsen ved Store Valby i Vestsjælland. *Aarbøger for nordisk Oldkyndighed og Historie*.
- 1973: Studien zu neolithischen Flintbeilen. *Acta Archaeologica* 33, 125 ff.
- BRØNDSTED, JOHANNES 1938: *Danmarks Oldtid*. I. *Stenalderen*. Copenhagen.
- 1957: *Danmarks Oldtid*. I. *Stenalderen*. 2. edition. Copenhagen (reprinted without changes 1966).
- ÅBERG, NILS 1937: *Kulturmotsättningar i Danmarks stenålder*. Kungs. Vitterhets Historie och Antikvitets Akademiens Handlingar 42:4. Stockholm.
- 1949: *Nordisk befolkningshistoria under stenåldern*. *ibid.* 70:1. Stockholm.

Reviews

O.M.C. HAEX, H.H. CURVERS & P.M.M.G. AKKERMANS (eds.): *To the Euphrates and Beyond. Archaeological Studies in Honour of Maurits N. van Loon*. A.A. Balkema Publishers, Rotterdam/Brookfield 1989. 304 pp, 10 diagrams, 41 figures, 5 plates.

This *Festschrift* to Maurits N. van Loon, professor of Near Eastern archaeology in Amsterdam, contains 20 articles and a bibliography of van Loon's publications written by colleagues from his excavations in Eastern Anatolia and Syria and by his former students at the Oriental Institute in Chicago and at the University of Amsterdam. The composition of the book reflects his broad knowledge and interests in prehistory and historic archaeology, in linguistics, palaeography, and iconography, in the collaboration between archaeology and the natural sciences, and in archaeological theory, methods, and techniques. On this background it is understandable that the topics of the articles vary from typological studies of a single group of artifacts to wide-ranging considerations or reviews of archaeological problems, often with a starting-point in van Loon's own excavations at Korucutepe, Tell Selenkahiye, Mureybit, Bouqras, or Tell Hammam et Turkman.

We are led from detailed studies of Neolithic figurines (Erik Lohof), crescent-shaped axes (Friedrich Lüth) animal headed cups at Mari (Sally Dunham), and the seal used by the god Tispak to kill Mušhuššu, the dragon (F.A.M. Wiggermann) – through surveys on the origin and early development of ceramics (Marie le Mière), mortuary practices in the Halaf period (Peter Akkermans), and the beginning of the third millennium in Syria (Hans H. Curves) – to an intriguing reconstruction of the famous battle at Qadesh, outlining step by step the positions and movements of the various units of the Hittite army and the Egyptians under the command of the Pharaoh Ramesses II (M.J. de Bruyn).

Of particular interest for Danish archaeologists are perhaps a microwear analysis of borers from an Early Neolithic site in the Jordan Valley published by Johannes Bueller and three studies with a wider methodological perspective.

In the first of these studies, entitled "Ground plans and archaeologists: On similarities and comparisons", D.J. Meijer explores the criteria used by archeologists, and the conclusions they draw, when they compare the lay-out of an architectural complex or the plans of houses from sites that are sometimes far from each other in time and space. The main question regarding similarity is obviously the extent of the identity of buildings, i.e. in this case of plans. Do we require congruence or a simple superficial likeness? Do we compare measurements? In his introduction Meijer states that in his opinion there are four aspects involved in an analytical classification: form, location, utilitarian function, and symbolic function. Any priority of one of these aspects or variables depends on the particular theory with which one approaches the ancient buildings. In his study he shows how archaeological comparisons often – and for obvious reasons – depend on the form of houses, as represented by