

individual was placed in a vertical position in the lake and then met with sudden death, which caused it to fall on its right side. As there were no other foot tracks in the calcareous sediment, it can be concluded further that the individual did not walk to the place but was transported there, probably in a boat.

In the original report from 1949, the excavator suggested that the cause of death was a vigorous blow on the left side of the cranium, which produced the large lesion seen on Fig. 13 in Bennike and Ebbesen 1987. The skull itself was not investigated in detail by him, because the cranium was brought to the museum in an intact state and was later examined by others. As mentioned by Bennike and Ebbesen 1987, only a few splinters of bone occurred in the cranial cavity and a large piece of the tempolar region including a part of the cheekbone was missing. Bennike and Ebbesen 1987 conclude that the cranial lesion had been caused by the peat diggers using a fork and that the large missing bone fragment was removed by them. They also found that the edges of the lesion partly follow the sutures and that the lesion therefore was inflicted after death, and they maintain that a displacement of the jaw could not have happened before or shortly after death (p. 94).

At that time peat diggers did not use a fork, but rather spades or shovels, which could not have caused the indentations mentioned by Bennike and Ebbesen 1987. The peat diggers carefully saved all bones found by them including even very small specimens. It is therefore inconceivable that they should have discarded the large bone fragment from the skull. The effect of a vigorous blow on the cranium could easily have caused a dislodgement of a fragment along the sutures, as the individual was still quite young, whereas the indentations mentioned by Bennike and Ebbesen 1987 may or may not have been inflicted in recent time. The missing cranial fragment could have become displaced by water movement further away than other parts of the skeleton, and therefore not recovered during the excavation. The jaw was displaced by pressure of the sediments as its support on the chin-bone was missing. The present author therefore finds no evidence that the cranial lesion was not inflicted during life, but rather, that a blow caused the individual to collapse whilst standing with its feet sunk into the mud.

The present author therefore adheres to his original opinion that the individual was transported to the finding place in the lake and was sacrificed by a vigorous blow on the side of its head whilst standing in the water, probably by the side of a boat, which supported the killer.

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Reply to a Review

by AXEL HARTMANN

As an exception, the editors have accepted a reply to a review. Whether this is fairness or not, the reader will have to decide. We think, however, there is one good reason to publish the following comments by Axel Hartmann: they demonstrate the importance of a close cooperation between the archaeologist and the natural scientist, be it fifteen years ago, or at all times.

In volume 4 of *Journal of Danish Archaeology* H. Thrane has reviewed my book "Prähistorische Goldfunde aus Europa II. Spektralanalytische Untersuchungen und deren Auswertung". I would like to comment on some points in this review.

It is obvious that as a natural scientist I cannot be expected to possess the whole specialized archaeological knowledge necessary to come anywhere near exhausting the potentialities for new insights latent in the results of the analysis of prehistoric gold objects. This is all the more the case when their geographical and chronological range is as wide as it is in the present case. It seemed therefore a very promising start when in 1970 I was able to embark in the company of H. Thrane and K. Randsborg on a study of the gold objects in the collection of the National Museum in Copenhagen. It hardly seems worth investigating today what reservations may later have arisen at the National Museum, but 1½ years after the agreement reached in October 1970, both colleagues abandoned the study of the Copenhagen gold – Thrane for fresh fields in Odense, and Randsborg to devote himself to other researches. Being aware of the difficulties that would arise, I wrote first to E. Lomborg requesting collaboration – unfortunately without success. After the National Museum in October 1974 made the astonishing suggestion that the analyses should be published without any Danish participation at all, I was lucky enough to get P.O. Nielsen to work through and check the already existing list of provenances on a private basis. This happily corrected a number of inaccuracies and mistakes. P.O. Nielsen is also to be thanked for many of the references to publications. As the National Museum was unable to provide either drawings or photographs owing to pressure of work on its photographic laboratory, I was obliged as a last resort to make use of the inadequate private working photographs in the publication – "a poor example of international collaboration" indeed, as Thrane remarks. However it is odd to hear such vigorous complaints about the volume's lack in archaeological weight coming from Denmark in full knowledge of the circumstances.

At this stage, however, it would have been an irresponsible procedure to abandon the project or leave the existing analyses unpublished, so I was forced to the decision of presenting the results attained, linking them together with a conclusion of the more general kind that was all I as a natural scientist without specialist knowledge was capable of. Obviously this is unsatisfactory for experts in Danish prehistory, for it leaves many important questions and problems untouched. The aim, how-

ever, was to make results and ideas available to archaeologists and enable them to make further use of them in their own work.

For this reason one finds nowhere in the text the suggestion attributed to me by Thrane, that the Trundholm sun-chariot might date from the urnfield period. I am content to leave detailed study of such questions to the archaeologists. My only wish is to point out that gold with added copper is an exception in Montelius II. This is a fact now established by the tables of analyses, which ought not be overlooked in any future examination of the dating of the sun chariot.

Thrane's remark that the 20 wire rings with flat leaf-shaped ends shown in Pl. 28 are incorrectly attributed to Montelius VI and probably are from the Copper Age, is naturally of great value. In the Bronze Age exhibition of the National Museum, of which H. Thrane was in charge when the samples were taken, they were exhibited as Late Bronze Age. Confident in the rightness of this attribution I placed the 20 wire rings in Pl. 28, but am now naturally most grateful for the correction after a delay of fifteen years. It shows how valuable the participation of the National Museum would have been a step further than to the mere taking the metal samples.

Of some of these wire rings, which are now known to be very early, the observation may be made that pairs found together sometimes differ strikingly in composition (incidentally Au 3724 was not found with Au 3737 but with Au 3727). This is somewhat unusual, as in later periods gold ornaments found together in pairs are generally of very similar composition. This observation in the case of these early pieces ought not to confuse "us poor archaeologists", as Thrane opines, but make one appreciate that at that time the objects were not made in pairs simultaneously by the same goldsmith, but more likely at separate times and places. Apparently gold was not yet so abundantly available that pairs of ornaments could always be produced together.

At this stage it already becomes obvious how wrong it is to approach experimental data – in this case the gold analyses – with preconceived notions and fixed expectations, for objective statistics seldom confirm subjective prejudice. Thus Thrane is disappointed to discover how little the gold from the hoard from Råddenkjær bog in central Jutland, with its unambiguous attachment to group N and NC, differs from the gold of other Bronze Age finds in Denmark, although the forms at Råddenkjær suggest an origin far away to the south-east. This disappointment is due clearly to an attitude of expectation, that is unjustified and leads nowhere so long as maintained. One ought instead to adduce from this surprising result that the same gold N was used in the south-eastern area where this object originated, as in Bronze Age Denmark. As all the gold used in Denmark in view of the obvious lack of local occurrences must have come through some kind of trade, this might have given a first clue to the direction from which gold of type N may have been imported. Certainly no occasion for disappointment!

We have H. Thrane's vigilance to thank in the last part of his review for calling attention to various mistakes and printing errors. The incorrect provenances given for Au 3575, Au 3847,

Au 3853–54, and Au 4055 may be attributed to mistakes in the lists sent to Stuttgart. These were prepared under Thrane's supervision by a female student at the National Museum, myself being unable to read the inventories. When Thrane calls attention to the fact that in SAM 5 the Danish place names are not always spelled correctly (e.g. Brønsted instead of Brøndsted, Tjærborg instead of Tjæreborg, Tudved instead of Tudvad, etc.), these mistakes are regrettable, but in some cases spelling variants may have played a part.

Thus in the penultimate paragraph of his review Thrane writes once Skødstrup and once Skydstrup. He specifies the provenance of Au 4085, which does not appear in SAM 5 at all, writes "pl.1" where he must mean "pl.71", attributes Au 3747 to the provenances Hvidbjerg and Toftehøj both, and says Au 4368 comes from Brøndhøj when he means Au 4968 did. And this is all in a single paragraph, whose purpose, of course, is to provide supplementary information to help the reader avoid the confusions arising from my errors! It really is difficult to produce in print a large and difficult text without a mistake. [Translated by David Liversage]

(25th August, 1986)

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Stylistic Analysis

A Critical Review of Concepts, Models, and Applications

by ANNE BIRGITTE GEBAUER

Studies of stylistic variation in prehistoric artifacts have played an important role in archaeological research since the beginning of the discipline. Assumptions about the causes of patterned stylistic variation have always been central to the development of cultural chronologies and to traditional concerns with culture-historical relationships and are equally important in "processual" or "post-processual" studies today. Beliefs about the processes by which stylistic elements have spread through time and space have differed. Despite a rich history of interpretive disagreements the subject has remained poorly understood.

Recent years have witnessed an increase in systematic efforts to identify the forces that create different patterns of stylistic trait distributions. There has been an expansion of archaeological interest in the social conditions that promote and inhibit the transmission of stylistic traits. These studies have produced some interesting results which has renewed the