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Näsman, Ulf

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Debatt

Paradigm misused: a de-railed debate

In 2007 Lotte Hedeager published a paper in *Norwegian Archaeological Review* titled “Scandinavia and the Huns: An Interdisciplinary Approach to the Migration Era”. It is a well-written paper with several interesting ideas. However, I found one of her main ideas questionable: on the basis of the famous Priscus fragment 8, she concluded that “the Huns’ supremacy included parts of Scandinavia” (p. 44) and that “Southern Scandinavia constituted part of Attila’s sphere of dominion” (p. 48). I still find my reading of the same text more convincing: Scandinavia was not part of Attila’s realm (1984: 99f.). So of course I was interested to see what in the archaeological record she used as support. Among finds that Joachim Werner (1956) considered to be Hunnic, only five ‘magic’ sword beads are from Scandinavia (*Fundliste V*). Five beads cannot support any notion of ‘supremacy’ or ‘dominion’.

Another of Werner’s ‘Hunnic’ types is the simple penannular earring made from metal rods with tapered ends (“...schlichte Ohrringpaare ... aus zusammengebogenen, gegen die Enden sich verdünnenden Metallstäben ...” Op. cit. p. 24). Hedeager’s contribution is the suggestion that eight similar rings in the National museum of Denmark in Copenhagen can be identified as belonging to Werner’s ‘Hunnic artefacts’. Unfortunately, all are stray finds without context. Since similar rings are found in a number of well-dated Viking Age and High Medieval contexts, I realised that Hedeager had probably made a mistake. In my opinion her other arguments for a direct Hunnic presence in Scandinavia are not stronger, but of course open for discussion. A serious weakness in her paper is that she does not give references to alternative views by other prominent scholars who have interpreted the same material very differently. Without customary reservations her story appears as original, stream-lined, and convincing. So when she reproduces Werner’s map of earrings and supple-

ments it with the assumed ‘Hunnic’ earrings from Denmark (*NAR* 40/1 fig. 4), the result is in my opinion serious misinformation. But I agree with her (uncontroversial) conclusion that the Huns were “at least well known, to people of the North” (p. 51).

Because of the expected reaction, I hesitated at first to publish my opinion. Instead I awaited the comments in *NAR* 40/2 (2007). Unfortunately the commenters did not evaluate the archaeological material of Hedeager’s Hunnic story. So I manned myself, realising that someone had to point out the weakness of her narrative. I published a paper in *Formvännern* 103 (2008).

Lotte Hedeager rejects my criticism and, as expected, she uses rhetoric excellently in her reply (2008). Her purpose is to convince readers that criticism delivered by such a primitive square of a scholar as myself need not be taken seriously. I have decided not to repay Hedeager in the same coin.

An attentive reader, who has read all three papers, will notice that Hedeager’s reply does not contain any new arguments concerning the question about the Huns and their relation to Scandinavia. Consequently, the substance of my criticism stands unchallenged. I note with satisfaction a small progress. Hedeager is now “open to the possibility that some of the Scandinavian ‘Hunnic’ rings could be of later date...”.

Unfortunately, she finds “it unproductive to go through all of Näsman’s examples, because it is trivial to state the obvious: we are working with two different historical models, and therefore interpret the evidence differently...” Is this an acceptable answer? I find it hard to believe that her readers would find it unproductive to learn more about her arguments.

She rejects my criticism with reference to Thomas Kuhn’s 1962 classic *The Structure of Scientific Revolutions*. According to Hedeager, my research belongs to an “old” paradigm and can

thus be dismissed without further ado. Instead of taking my objections seriously and arguing against them one by one, the trick is to lump them together and dismiss them as belonging to an old-fashioned paradigm.

Anyhow, I cannot understand why Hedeager places me and herself in different Kuhnian paradigms. We both believe that there once existed people whom others – and maybe themselves – called Huns. We both have a European perspective on Scandinavia during the Migration Period. We both work in an interdisciplinary manner and study historians' analyses of the Migration Period and the Huns. We both use written sources. We both use typochronological methods. We both prefer contexts and closed find combinations over stray finds. To be precise: the methods to date gold rings are the same irrespective of whether we use traditional, processual, or post-processual approaches in our interpretations.

I cannot accept paradigm as an argument. I have consulted papers on the issue in Scandinavian archaeology by Bjørn Myhre (1991, p. 164, 171), Bjørnar Olsen (2003, p. 64), and Evert Baudou (2004, p. 20, 231) and conclude that Hedeager and I both work in the same paradigm. We use the same way of reasoning. Her interpretations are more speculative, while I take a more cautious position. And that is all right as long as we respect our right of mutual criticism.

Hedeager's attempt to explain her paradigm and mine has to be quoted: "an interpretation can be compared to a jigsaw puzzle. ... In the positivist paradigm...it is believed that the pieces of the puzzle were painted in the past and are thus still readable if analyzed and assembled correctly. ... In the processual and postprocessual paradigms, on the other hand, the pieces of the puzzle are seen as unpainted: only their shape allows us to fit pieces together, but there are numerous options. It is therefore the ability of the researcher to critically "select" and contextualize them according to a theory of their historical meaning that enables an interpretation of which individual pieces belong to the same jigsaw puzzle." This metaphor is unconvincing.

In my opinion the past left us only one puzzle;

some pieces are painted, others are not, some have their shape preserved, others are more or less seriously damaged, and most are lost. Of course all of us will lay the puzzle in our own way. In that way several puzzles exist as modern constructs.

When the archaeological source material is rich and well preserved ("painted"), the number of reasonable interpretations is limited by the quality of the sources. In other parts of the archaeological record a lot of pieces are missing or poorly preserved ("unpainted") and a large number of different interpretations are possible. In both situations, I argue that source criticism has to be integrated into all interpretive speculations.

Hedeager discusses my opinion that "it is sound scholarly procedure to be critical of narrative sources." I do not understand her point: is she of the opinion that we should not be critical of narrative sources? I guess that she agrees that we have to be critical, so what is wrong with my statement?

The arguments to counter my dating of her "Hunnic" rings to the Viking and High Middle Ages are curious. Hedeager offers no comment on the fact that there is not one good Migration Period context for such a ring in Scandinavia, and that similar rings have been found in a number of good Viking Age and High Medieval contexts in Northern Europe. Instead she argues that this "chronological knowledge ... was not in place when the finds were catalogued" and points out that the rings "were catalogued as belonging to the Iron Age" in the museum. I am astonished. I believed that her arguments were based on knowledge available when the paper was published in 2007. It is true that curators at the museums in Copenhagen and Stockholm catalogued some stray finds of rings as Iron Age, but also that similar rings are catalogued as Viking Age or Medieval. In my opinion we must not accept classifications found in catalogue entries at face value.

In her reply, Hedeager refers to Werner's rings as "Werner's Asiatic diagnostic type". This is misleading. It implies that the ring type had followed the Huns on their long ride from China. She cannot have checked Werner's text recently.

In fact he wrote that this simple type probably originated in East Roman workshops in the 4th century. In the 5th century it spread to the West, North and East from the Bosporian towns on the Black Sea (Werner 1956, pp. 24 f). So in Werner's opinion they are not "Asiatic".

The presentation of new hypotheses, interpretations, and theories is almost always interesting. Sometimes such exercises generate new understanding and knowledge. But interpretations and hypotheses are never stronger than their weakest link. Consequently it is important to compare interpretations with alternative attempts by others, to evaluate strengths and weaknesses. Only then can the relative strength of different approaches be evaluated. In this respect, Hedeager's NAR 40/1 paper fails.

To mention just two examples: the eminent scholar Günther Haseloff explicitly rejected the idea that "Germanic" animal art was born of Nomadic animal art. Why is Lotte Hedeager's opinion about its origin more likely? I mentioned that Claus von Carnap-Bornheim has published a contrasting view on the human masks on Nomadic and Scandinavian finds. Why not demonstrate where he is mistaken?

In my opinion we must not ignore results presented by other competent scholars. If we, as scholars, only refer to views that fit our purpose and conceal the existence of other opinions, we will end up like monks in contact only with our fellow faithful. One purpose of my paper was to point out that the literature contains comprehensive studies that contradict many of Hedeager's ideas.

I agree with Hedeager when she writes "Probability is at the heart of all archaeological and typological reasoning, and when there is more than one interpretative option, we should remain restrained." I leave it to other scholars to judge whether she is restrained when she attributes a number of Danish stray finds of gold rings

to the Huns of the Migration Period, and whether I am unrestrained when suggesting that securely dated contexts in Denmark and Sweden indicate that these finds are actually from the Viking and High Medieval periods.

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References

- Baudou, E., 2004. *Den nordiska arkeologin – historia och tolkningar*. Stockholm.
- Hedeager, L., 2007. Scandinavia and the Huns: An Interdisciplinary Approach to the Migration Era. *Norwegian Archaeological Review* 40/1. Pp. 42–58.
- 2008. Paradigm exposed: reply to Ulf Näsman. *Fornvännen* 103. Pp. 279–283.
- Howard-Johnston, J.; Herschend, F. & Hedeager, L., 2007. Scandinavia and the Huns. Comments and reply. *Norwegian Archaeological Review* 40/2. Pp. 199–207.
- Kuhn, T.S., 1962. *The Structure of Scientific Revolutions*. 2nd enlarged ed. 1971. Chicago.
- Myhre, B., 1991. Theory in Scandinavian Archaeology since 1960: a view from Norway. Hodder, I. (ed.) *Archaeological Theory in Europe. The last 3 decades*. London & New York. Pp. 161–186.
- Näsman, Ulf 1984. *Glas och handel i senromersk tid och folkvandringstid*. Uppsala. Summary.
- 2008. Scandinavia and the Huns. A source-critical approach to an old question. *Fornvännen* 103. Pp. 112–118.
- Olsen, B., 2003. *Från ting till text. Teoretiska perspektiv i arkeologisk forskning*. Lund.
- Werner, J., 1956. *Beiträge zur Archäologie des Attila-Reiches*. München.

Ulf Näsman

Humanvetenskapliga institutionen
Högskolan i Kalmar
SE-391 82 Kalmar
ulf.nasman@hik.se