

Debatt

Paradigm exposed: reply to Ulf Näsman

Background to the Debate

My paper “Scandinavia and the Huns: An Interdisciplinary Approach to the Migration Era” was published in *Norwegian Archaeological Review* 40:1 (Hedeager 2007) and followed by a discussion (in no. 40:2) where Ulf Näsman was requested to contribute. He instead transferred the discussion to these pages with his debate piece in the previous issue of *Fornvännen* (Näsman 2008).

For the reader who is not familiar with my *NAR* paper, it can be summarized as follows. Its aim was to discuss the Early Migration Period as a particular period of “short term history” and its formative impact on the Scandinavian *longue durée* (long-lived/slowly-changing structures) in the first millennium. During this particular period of time, the material culture of Scandinavia demonstrates radical changes in symbolic representation, followed by long-term continuity and social/mental resistance to change. I argued that the Huns, as a historical fact, were present in Scandinavia in the early fifth century. Their impact was to generate an “episodic transition” that opened up a whole new set of social, religious and political strategies, in Scandinavia in particular as well as in barbarian Europe in general, and gave rise to a new Germanic identity in the aftermath of the Roman Empire.

Professor of history, James Howard-Johnston of Oxford University, and professor of archaeology, Frands Herschend of Uppsala University, commented on the article (*NAR* 40:2). Howard-Johnston underlined the power of the Hunnic rule by ascribing it the status of an empire, which explains its immense political and ideological power in restructuring barbarian Europe politically, ideologically and ethnically. His comments provided a stronger historical and explanatory framework for the transformative effects of the Huns on Germanic Europe, including Scandinavia. Herschend contributed

an interesting historical explanation to the lack of graves from the period of Hunnic rule in Europe, which supports their disruptive and transformative presence. He suggested that the missing burials are part of a wider religious change under Hunnic influence. The Migration Period, as the name suggests, was also a period of large-scale warfare, traveling warriors and conquest, which meant that warrior ideology played a dominant role throughout Germanic Europe (Hedeager 2000; 2005). This very same Germanic Europe was strongly impacted by the Huns, including the era’s political and religious institutions. In accordance with this, the Huns in Scandinavia triggered a transformation that enabled Scandinavian chiefs, landowners and warriors to enter into a common Germanic trans-cultural political world.

Scandinavian historians and archaeologists have long neglected any Asiatic or Hunnic elements in the North during the Migration Period. Ulf Näsman belongs to this research tradition. The historical truth of my hypothesis – and of any other hypothesis including Näsman’s – can never be proven, as I stress in the article. Archaeologists must always rely on probabilities and historical plausibility by combining theory and data. Therefore I established archaeological indices and a corresponding historical and theoretical explanation that made such a far-reaching interpretation plausible. However, Näsman makes a simple equation between data and historical fact by ruling out theory. A reply to his critique therefore requires an exposition of the two different academic approaches – or paradigms – involved, his and mine.

Paradigms and the Archaeological Debate

In his classic 1962 book *The Structure of Scientific Revolutions*, Thomas Kuhn coined the concept of a “scientific paradigm”, a research community unified by an (often implicit) adherence to a shared

set of scientific assumptions (Kuhn 1971). He further demonstrated that while accumulation of knowledge took place within the paradigm, a change of paradigm often resulted in a near-complete break with the previous research tradition. Therefore he characterized paradigmatic changes as revolutionary, often leading to fierce but unresolved debates, that lasted only until the new paradigm had won more followers than the old one. In short, there existed a degree of scientific incommensurability between two competing paradigms.

Such a change of research paradigm took place in Scandinavian archaeology during the 1970s with forerunners in the 1960s, when a new so-called “processual”, theoretical archaeology replaced, or rather supplemented, an empiricist, non-theoretical positivistic research tradition (Myhre 1991). With the addition of a so-called “post-processual” paradigm from the 1980s onwards, we are now in a situation where practitioners of three different research paradigms still work side by side. This situation leads to competing and sometimes incommensurable interpretations of the past, and Ulf Näsman’s critical comments on my article exemplify just that.

A paradigm, and within that an interpretation, can be compared to a jigsaw puzzle. Each researcher adds yet another piece to its completion, but the process assumes that the bits and pieces of the puzzle are read and understood in a similar fashion. In the positivist paradigm in archaeology 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 this tradition source-criticism is employed as an instrument to sort out false pieces that belong in another puzzle. Thus, when enough pieces are correctly assembled it is possible to directly read and reconstruct the puzzle, despite some missing parts. The assembled puzzle is the interpretation. Theory, interpretation and data are separated, as data are believed to exist without theory (theory is therefore unwarranted), and interpretation is believed to emerge by itself if the puzzle is correctly assembled.

In the processual and post-processual 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. If the pieces conform to the theory, it allows the archaeologists to finish the interpretative painting of the puzzle. The assembled puzzle is thus the starting point for interpretation. Theory, data and interpretation cannot be separated, as they are mutually dependent.

Ulf Näsman’s critique of my *NAR* paper exemplifies this paradigmatic difference of interpretation. This is already evident from the title, where he keeps “Scandinavia and the Huns”, but replaces my subtitle with “A Source-Critical Approach to an Old Problem”. It clearly signals two different paradigms: an objective, positivistic source-criticism is applied in order to deconstruct a theoretical, interdisciplinary interpretation combining history and archaeology. Näsman’s paradigm demands that such bold, theory-based (and thus subjective) interpretations be confronted by restrained, objective source criticism. As a result we now have two completely different interpretations of the same data. One may ask, how wrong is it possible to be? Or is it the very concept of “right or wrong” that should be discussed? From this follows the question: how neutral and objective are Näsman’s source-critical re-interpretations? Ulf Näsman’s critique thus offers a rare opportunity to discuss some of the mostly implicit factors that govern archaeological interpretation, or indeed all historical interpretation.

In the following I shall dissect just one of Näsman’s points of criticism, in order to illustrate its relative and subjective nature, which in fact applies to all of his critique. From this follows that there is no clear “right or wrong” answers to complex research problems in archaeology. In conclusion, I shall situate Ulf Näsman’s contribution within its own research paradigm.

The Arguments of the Debate – Right or Wrong?

Ulf Näsman stresses from the first page that “it is sound scholarly procedure to be critical of

narrative sources” (it is understood that I have been too uncritical), and as to my archaeological reasoning Näsman finds that “her factual reasoning is disappointing”. These are well known (and rather worn out) rhetorical strategies for establishing the upper hand without argument, in the past often employed by older professors against their younger opponents. However, it is all in the eye of the beholder. Thus, waters do not divide between us in the basic empirical conduct of interpretation, but rather in a source-critical fragmentation of knowledge versus a historically structured, contextualized interpretation where the sum of the parts are bigger than its individual elements. But, most fundamentally, while Ulf Näsman believes in a single correct archaeological interpretation, I wish to demonstrate that this objective is neither obtainable nor desirable. I will do this by showing in some detail, that Ulf Näsman’s so-called source-critical re-interpretation of one of my points is as debatable as mine, and in the end it comes down to a subjective evaluation of conflicting opinions on the very same objects and texts. There is no neutral way to evaluate right or wrong interpretations of complex historical problems.

At the core of Näsman’s critique is a re-interpretation of the ten gold earrings found in Norway and Denmark, which in my view represent a possible Hunnic “diagnostic artifact”. To erase any relationship between these rings and Hunnic material culture, Näsman points to the common occurrence of “similar” rings during the Late Viking and Early Medieval period in Scandinavia. To support this interpretation he presents two rings from a Gotlandic hoard find dated to the 11th century AD (Näsman 2008, fig. 3). He also finds support from certain colleagues, who believe that the “Hunnic” rings belong to the Viking and Early Medieval Periods. This leads on to the conclusion that “anyone familiar with earrings and finger rings from c. 1000–1200 would be suspicious” (Näsman 2008, p. 113). However, this apparently self-evident chronological knowledge (the old rhetoric is at work again) was not in place when the finds were catalogued at the National Museum in Copenhagen, and at the Archaeological Museum in Stavanger. At both museums they were

catalogued as belonging to the Iron Age. The ring from Stavanger was found in a Migration Period grave. To get around this information Näsman now argues that “The Vesterbø grave find is not unequivocally closed. The opposite is more probable.” He continues: “In Norway it is not uncommon to find secondary graves placed in Roman Iron Age or Migration Period chambers. To conclude, the Vesterbø ring cannot carry the evidential burden for a Hunnic presence in Scandinavia” (Näsman 2008, p. 114). I would rather rephrase the real content of this exercise as follows: because it supports Lotte Hedeager’s interpretation, the Vesterbø grave must by all means be declared suspicious. In the exhibition at AmS the Vesterbø ring is presented in the showcase with Migration Period burial finds, just as Kent Anderson in his 1993 work on gold from the Roman period includes the find in his catalogue, even if he now in an e-mail to Näsman states that it can be dated to the Viking Period.

The interpretative deconstruction, or rather manipulation, continues. To further support his criticism, Näsman now illustrates what he considers to be a close similarity with Viking Period and Early Medieval rings, by comparing my figure of the Scandinavian so-called “Hunnic” rings with a figure of the two Gotlandic rings, but without showing the figure from my article with East European and Asiatic rings (assembled from Bona 1991 and Werner 1956). The reason is clear: they show a much stronger typological resemblance with the Scandinavian rings than the Gotlandic rings. Especially the characteristic faceted ring from Åbenrå (no. 10/27) is typologically identical – and of the same size – as the Ukrainian ring from Radensk (Hedeager 2007, fig. 3).

In the same vein Näsman finally presents a Frankish silver ring from the Migration Period, with little or no similarity either to Werner’s Asiatic diagnostic type, nor to the Scandinavian rings. This ring is put forward with the argument that the Scandinavian rings cannot be of Hunnic origin. The conclusion is: “if they reflect any foreign influence, it is Slavic” (Näsman 2008, p. 114). The whole argument is irrelevant, and based upon Ulf Näsman’s typological blind-

ness in conjunction with his desire to undermine my interpretation.

I wish to demonstrate two things with this example. Firstly, it is neither wise nor fruitful to claim higher insight into archaeological and historical truth, when this truth is not obtainable. Probability is at the heart of all archaeological and typological reasoning, and when there is more than one interpretative option, we should remain restrained. Is the glass half empty or half full? Consequently, I am open to the possibility that some of these “Hunnic” rings could be of later date, but I maintain that they should be taken into consideration as a possible “Hunnic” diagnostic type in the Migration Period of Scandinavia.

The second point I wish to make is that when there is no consensus as to the interpretation of evidence, the strength of the arguments depends on the degree of correspondence with a clearly formulated theory or hypothesis. Thus, I find 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. Näsman mobilizes his supporters and I mine, which does not bring us forward. Is Birgit Arrhenius’s interpretation of a Gothic/Hunnic mirror from Gamla Uppsala correct, or is Wladyslaw Duczko’s rejection correct? Was animal art influenced mainly by Asiatic or Roman art? Do the shamanistic elements in Norse religion go back to an eastern influence, or are they indigenous? Are the masks in Nordic animal art a reflection of Asiatic people or not? Are the gold bracteates mainly an emulation of Roman ideas, or do they reflect a new symbolic system of eastern origin too? Right or wrong depends on where one wishes to put the stress, which is depending on the historical models at play. I have put forward a new historical model for understanding Migration Period Scandinavia and its cosmology, and other interpretative aspects, now brought forward by Ulf Näsman, were therefore downplayed. If my interpretation leads on to new research and debate I consider it fruitful. At present it is not possible to say who is right or wrong, but the model that prevails must be able to mobilize additional and future evidence.

The Theoretical and Pedagogical Value of the Debate

In conclusion I wish to argue that Ulf Näsman’s apparently ‘neutral’ source-critical comments are implicitly embedded in a larger historical understanding and theoretical model of post-Roman Scandinavia, one that sees Roman and Nordic traditions in art and religion as more formative than Hunnic influences. There is in fact a long-standing historical tradition for denying the impact of the Huns in the formation of Scandinavian art and mythology, based upon traditional historical source criticism (and, I suspect, some ideological baggage against barbarian Huns). It is this previously dominant model Ulf Näsman defends, against my alternative reading of the archaeological puzzle, which is based upon a new and different historical model. It is therefore his adherence to the traditional historical model that implicitly links together and subjectively frames his interpretation of each element in the critique. My contribution implies that there is no neutral standard by which empirical interpretations can be judged individually, as long as they comply with sound scholarship. All interpretation assumes an underlying theoretical and historical model that gives meaning to each element.

In the end it therefore comes down to the question of which model is most convincingly able to mobilize the evidence, to frame it into a coherent historical whole. The validity of an archaeological and historical interpretation is thus to be judged by theoretical standards of empirical and interpretative coherence. Which demonstrates that there is much more than neutral source-criticism and “sound” empirical conduct of research at stake in this debate.

To summarize: all of Näsman’s “neutral” and “objective” interpretations of the empirical evidence are as solidly anchored in a subjective historical research paradigm as are mine. The main difference between us is his lack of theoretical reflection (or consciousness), and consequently his lack of insight into his own theoretical paradigm. While we share a fundamental respect for and knowledge of the empirical data base of the Iron Age, we approach the interpretation of that very same database in a funda-

mentally different way. In this our discussion highlights and exemplifies basic mechanisms of Kuhn's concept of research paradigms, not least their incommensurability.

References

- Andersson, K., 1993. *Romartida guldsmede i Norden 1. Katalog*. Uppsala.
- Bóna, I., 1991. *Das Hunnenreich*. Stuttgart.
- Hedeager, L., 2000. Migration Period Europe. The formation of a political ideology. Theuvs & Nelson (eds). *Rituals of Power. From Late Antiquity to the Early Middle Ages*. Leiden.
- 2005. Scandinavia. Fouracre, P. (ed.). *The New Cambridge Medieval History I, c. 500–c. 700*. Cambridge.
- 2007. Scandinavia and the Huns: An Interdisciplinary Approach to the Migration Era. *Norwegian Archaeological Review* 40:1. Oslo.
- Howard-Johnston, J.; Herschend, F. & Hedeager, L., 2007. Scandinavia and the Huns. Comments and reply. *Norwegian Archaeological Review* 40:2. Oslo.
- Kuhn, T.S., 1971. *The Structure of Scientific Revolutions*. Second edition, enlarged. 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.
- Näsman, U., 2008. Scandinavia and the Huns. A source-critical approach to an old question. *Fornvännen* 103.
- Werner, J., 1956. *Beiträge zur Archäologie des Attila-Reiches*. Bayerische Akademie der Wissenschaften. München.

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Ett kungligt dop: Olof skötkonung och Bruno av Querfurt

Kring ett aktualiserat problem i svensk historieskrivning

Sveriges omvändelse till kristendomen är en av de viktigaste händelserna i landets tidiga historia. Inte att undra på att allt som hade att göra med detta blivit ett viktigt forskningsämne för generationer av historiker. Ämnet är starkt engagerande och attraktivt inte bara för professionella utövare av historiska discipliner utan även för en intresserad allmänhet. Den senare tar inte passivt emot forskningsresultat utan lägger fram egna tolkningar, vilka ofta presenterats som mer sanna än dem som erbjuds av akademiker (se till exempel artikeln i *Fornvännen* 2000:4 om bysantinsk mission inom Västergötland i anknytning till de så kallade liljestenarna).

En av de historiska personer som dragit till sig ett brett intresse är Olof Eriksson *skötkonung*, den förste kristne kungen i Sverige. Olofs liv och verksamhet är ytterst litet kända och praktiskt taget endast från sena källor. En av dessa är *Gesta Hammaburgensis ecclesie Pontificum*, en krönika

författad under 1070-talet av Adam av Bremen. Annat kommer från de medeltida isländska sagorna som förtäljer de välkända händelserna, sådana som kungens konflikt med uppsalatinget, hans flyttning till Västergötland, hans ovänskap med kung Olav av Norge och hans dottrars äktenskap. Den rådande uppfattningen om Olof som kristen är baserad inte bara på Adam av Bremens uppgifter utan också på kungens mynt. På dessa första svenska mynt, kopior av engelska pennies, finns sådan kristen symbolik som kors, hand, fågel, lamm, samt även religiösa inskriptioner – *in nomini domini*. På de tidigaste mynten presenterar sig Olof som kung i Sigtuna, på de något senare som svearnas kung.

Förutom mynten finns inga tidiga källor som direkt talar om under vilka omständigheter Olof blev kristen. Den mest anlitade källan, Adams *Gesta*, lämnar inte något besked om kungens dop utan talar om dopet av hans två söner,