

Håkan Rydving

DO WE ALL HAVE TO BE ARCHAEOLOGISTS?

Håkan Rydving, Department of Archaeology, History, Cultural Studies and Religion, University of Bergen, P.O. Box 7805, NO-5020 Bergen, Norway: hakan.rydving@ahkr.uib.no.

In a chapter in the recently published volume *Sacred Sites and Holy Places: Exploring the Sacralization of Landscape through Time and Space* (Nordeide & Brink 2013), the Tromsø-based archaeologist Asgeir Svestad (2013) makes a frontal attack on non-archaeological approaches to Sami 'religious conceptions and practices', and especially on historians of religions and some of the things I myself have written. This means that the text has already been published twice, since it was also (with a few minor differences) included in volume XXVIII of *Fennoscandia Archaeologica* (Svestad 2011). However, the critique cannot stand unchallenged and I therefore find it necessary to make a few comments on it.

By way of introduction, I would first like to mention that I have always held that different approaches enrich one another and that we can therefore learn from colleagues who work on similar themes but within other fields of study. At the same time, however, one has to base one's own investigations on the source materials one has the competence to analyse. Acquiring such competence takes years of hard work, whether the approach one has chosen be anthropological, archaeological, folkloristic, historical, linguistic, or something else. If, after years of study and practice, one has built up skills in the field of archaeology, then it is only natural that one should base one's investigations, into let us say Sami religion, on archaeological sources, not on anthropological or folkloristic ones. This is quite legitimate, even advisable, and I think Svestad agrees. However, in contrast to Svestad I think, for example, that historical or linguistic approaches to Sami religion can also be relevant, even if they are not based on archaeological material, but on written historical or linguistic sources that the historian or linguist is competent to analyse only after many years of hard work.

It seems from Svestad's text that he does not accept other skills as having a relevance equal to those of the archaeologist. Neither does he like it that historians of religions such as Louise Bäckman, Hans Mebius and myself have specialised in the 17th century and the first half of the 18th century, because he thinks the 19th century is more interesting. That would be as to ask of an archaeologist who has specialised in the Bronze Age to study the Iron Age instead. If Svestad wants historical information about the 19th century, why not ask historians who have specialised in that period why they have written so little about Sami religion?

Svestad's reading of texts by historians of religions who have written about Sami religion is very selective and superficial, to say the least. With what justification, for example, does he state that Sami religion has 'quite unambiguously (and asymmetrically) been conceived only in respect to cognitive processes' (Svestad 2013: 114; cf. Svestad 2011: 41), as if no one had studied ritual performances, material aspects (like drums or sacrificial sites), the role of the landscape, etc. The study of Sami religion (by historians of religions and others) during the last four or five decades has been much more varied than Svestad thinks. His negative attitudes towards historians of religions, their interpretations and the sources they use, are even more difficult to apprehend when one notices that his understanding of Sami anthropology and cosmology (Svestad 2011: 41–2, 44–6; 2013: 115–6, 119–20, 122–3) is based largely on nowadays mostly obsolete interpretations by earlier researchers of this category and the written sources that were known to them.

In order to demonstrate that the discussion among historians of religions 'often becomes too narrow and inadequate when addressing ques-

tions of material culture' (Svestad 2013: 113; cf. Svestad 2011: 40), Svestad quotes the following from the conclusions of *The End of Drum-Time* (Rydving 1995a: 161): 'among the three most important interethnic markers that distinguished Saami from non-Saami, only language and dress remained when religion no longer functioned' (as interethnic marker, that is). His comment is that this statement 'demonstrates a limited view of the material culture of the Sámi' (Svestad 2011: 40; 2013: 113), and he continues by giving several examples of objects characteristic of some (but not all) forms of Sami culture. The examples he mentions could very well have been used as markers in the context of a discussion of the Lule Sami around 1700, but only in the cases of the *badjelaha*, the Sami group that migrated with reindeer herds between mountains and forests on the Swedish side of the area, the *njárggaulmutja*, the Sami group that moved with a few reindeer on the mountains on the Norwegian side, and the *vuovdega*, the forest Sami, to use the Lule Sami terms for the groups I analysed. However, my statement about three interethnic markers refers back to the beginning of the book (Rydving 1995a: 42–6), where I discuss how one distinguished between Sami and non-Sami in the Lule Sami area during the period in question. And since the Sami consisted not only of the nomadic groups mentioned, but also of the *meraga*, the coastal Sami whose economy was based on fishing, stock-farming and boat-building, and the *ednambarge*, the Sami farmers whose lifestyle was very similar to that of non-Sami farmers, the 'markers' I mentioned are indeed the only ones common to all these groups. The reason why I did not mention other things as markers is that it was not possible to use them as such, not – as Svestad supposes – because of some kind of neglect or lack of knowledge of important material aspects of Sami culture.

Since Svestad does not seem to know my position in these matters, I find it necessary to add that I agree with him that among historians the discussion of religions 'often becomes too narrow and inadequate when addressing questions of material culture'. This is the reason why I have paid much more attention to these questions than most of my colleagues, something Svestad could easily have found out if he had had any intention of giving a nuanced critique. In one of my books (Rydving 2010), which Svestad refers to, three

out of ten chapters deal with sources that give information about different material aspects of Sami religion – places of sacrifice, drums, and place names – and a fourth chapter discusses landscapes. Is that to give inadequate attention to 'questions of material culture'?

One of the many things that irritates Svestad is that in the introduction to another of my books (Rydving 1995b) 'very little space is given to non-verbal primary ethnographic and archaeological sources: half a page on Sámi shaman drums, a [few] lines on sacred places, human graves, and bear graves' (Svestad 2013: 114; cf. Svestad 2011: 40). Nonetheless, he is not interested in trying to understand why that is the case. The book in question is concerned with a careful source-critical analysis of written sources from the 17th and 18th centuries, their interdependences and provenance, a very relevant task for a historian, and the function of the short introduction is only – within the limited space available – to place the written sources I discuss in the context of the source material *in toto*. I do not think an archaeologist would use much more space on written sources if he or she were to discuss different archaeological sources in a monograph.

Another example of a tendentious misunderstanding is when Svestad comments on a text where I complain in passing about the lack of interest that schools and universities in the Nordic countries show in the Sami and their culture: 'There is almost no knowledge about Saami culture and history among the non-Saami speaking majority of the Nordic countries. In the schools, very little – if anything – is taught. And the same is true for the universities' (Rydving 2004: 100). I still consider this assessment generally correct, but Svestad considers it 'out of this world, literally speaking, when compared to the actual research and teaching on Sámi archaeology, history, culture, and language at the universities of Tromsø, Umeå, Oulu, Oslo, etc.' (Svestad 2013: 114, n. 5; cf. Svestad 2011: 41), as if my point were made in ignorance of the important work done by individual researchers and at some departments at these and other institutions. It should be noted that I was writing about teaching, not research, and that the situation has improved considerably since I wrote the article in 1996. How much Sami history, for example, did students of history at even Tromsø, Umeå or Oulu, not to mention other universities, really

get in 1996? And how much Sami history do students of history at the universities of Bergen and Oslo, Gothenburg and Stockholm, or Turku or Helsinki get today? Not very much, if one is to believe the reading lists. At departments and universities other than the few that do offer some kind of Sami research and teaching, there is very little in the curricula to do with the Sami. In my opinion, however, Sami themes should be included in every subject area within the humanities and the social sciences, at universities not just in the north, but throughout Norway, Sweden, and Finland. I find it problematic if Sami themes – as I fear – are taught only at the universities of Bodø, Tromsø, Umeå, and Oulu, in addition to the Sami University College in Guovdageaidnu and at the University of Lapland in Rovaniemi, but only to a minimal degree, if at all, at other universities in the Nordic countries (the universities of Bergen, Helsinki and Uppsala are in this respect the only exceptions). I encounter a lack of even the most elementary knowledge and prejudices against the Sami far too often to think that the situation is satisfactory, despite all the high quality research that has been done for many decades, or in the case of some departments (Tromsø Museum, and the universities of Helsinki, Oslo, and Uppsala) since as early as the late 19th century.

One of Svestad's comments concerns my summarising statement that, for the Sami who became Christians and hence left the sphere of the indigenous religion, 'one half of the family ceased to exist' (Rydving 1995a: 143), namely the half that was regarded as living in the world of the departed, a world that was not accepted as existing in the new religion. I never stated, as Svestad thinks, that the many Sami who adhered to the indigenous religion also experienced that loss, and I therefore have no problem in accepting his idea 'that this "other half" continued to remain a part of Sámi nature-culture' (Svestad 2011: 51; 2013: 132) for that group of Sami. The desacralisation of space that I talk of, and that Svestad (2011: 52; 2013: 133) criticises, was – I still maintain – an important aspect of the changes this group of Sami went through. I tried to nuance the description and show that different Sami reacted differently during the process of religious change. There was a range of responses; in the decades around 1700, the Lule Sami included not only those who decided to continue to live within the

framework of the indigenous religion on the one hand and those who abandoned the traditional customs because they preferred the new religion on the other, but also groups who adopted various combinations of the two religions. The focus in *The End of Drum-Time* was on change, but that does not mean that I neglected different aspects of continuity (see below).

Just to be clear: before writing *The End of Drum-Time*, I spent long periods walking in the Lule Sami area in all seasons learning to read the landscape (and in that connection, my knowledge of the language and of the place names was an important help). I discussed different aspects of the landscape with local Sami friends, and visited most of the known sacred mountains, lakes and sacrificial sites in the area. Not only had I read about the Lule Sami drums and seen the pictures and drawings in Ernst Manker's monographs on the drums, I also had a first-hand knowledge of them thanks to opportunities to hold them in my hands and study them carefully in the storehouse of the Nordic Museum in Stockholm. My interest in Sami objects and in the landscapes of Sápmi has certainly not diminished since then; in fact it has broadened into new areas. The fact that I have at least tried to take the materiality of Sami religion and culture seriously, despite not being an archaeologist, makes it hard to understand Svestad's attack. I should add, perhaps, that Louise Bäckman and Hans Mebius, the two fellow historians of religions whom Svestad also takes to task in his text, could rightly claim similar experience. In addition, Bäckman has the advantage of knowing Sami culture from the inside.

The generalising accusations in Svestad's text have, as far as I can see, very little to do with the research we three, as historians of religions, have done on Sami religion. The only accusation that really holds is that none of us is an archaeologist, and that we have therefore focused our attentions primarily (but not exclusively, as Svestad thinks) on the analysis of written rather than archaeological sources. But do we all have to be archaeologists?

Finally, a comment on a misinterpretation that I have also seen in texts by other Tromsø-based researchers; in effect, Svestad (2011: 39; 2013: 112) imputes to me the belief that nothing of indigenous Sami religious ideas and practices has lived on after about 1750. This interpretation is

probably due either to a misunderstanding of the title of my dissertation, *The End of Drum-Time*, or to a careless reading of the book. The title was chosen because the Lule Sami concept *goabdesájgge*, ‘drum-time’, is highly appropriate to an important theme in the book: how the authorities used violent methods to force the Sami to abandon the indigenous religion. One interesting point about the concept of *goabdesájgge* is that it does not, as some readers think, mean ‘the period when the drums were still used’ or something like that, but refers instead to the time when one could adhere to the indigenous religion without any risk of punishment. It was that era that came to an end in the Lule Sami area during the period I analysed in *The End of Drum-Time*. The period that ensued was consequently called the period ‘when one had to hide the drums’ (*at da sjaddin goabddájt tjegadit*) (Rydving 1995a: 1). In the book, I analysed how the written sources describing the Lule Sami area from the early 1670s to the late 1740s portray this situation, but in the epilogue I clearly state that those sources do not give us the whole picture. As one of ‘the more tangible tasks’ for future research, I point to investigations of the period after 1750:

In the sources I have used, it seems as if the change of religion on the part of the Lule Saami was total at the end of 1750s (the indigenous religious customs were regarded to have been abandoned; all the Lule Saamis attended church and took communion), but sources from the nineteenth century show that individual persons continued to use drums and to sacrifice much later. What the clergymen and many of the Saamis did not know, was that in addition to the enculturative and deculturative processes, there was also a process of concealment. Saamis who continued to practice indigenous religious customs made a point of hiding them from the clergymen and from Saamis they did not trust. It is therefore quite logical when the period after the end of drum-time was called the period ‘when one had to hide the drums’, not the period ‘when the use of drums had ceased’ or the like. It is important to be aware of the great difference between what clergymen and others who did not accept the traditional religious customs knew of, and the knowledge about these matters among those who tried to preserve the customs. The latter persons, unfortunately, wrote no sources, but recordings from the nineteenth and also the

twentieth century show that isolated remnants of the indigenous religious customs were still alive. How these elements functioned is, however, obscure. Throughout this book, I have concentrated especially on change, but if a longer period would be studied, it would be possible to put more emphasis on continuity in the midst of change as well (Rydving 1995a: 167–8).

If one has read this, how is it possible to conclude that I consider Sami religion after 1750 to have been totally ‘dead, abandoned, or non-functional’, as Svestad (2011: 52; 2013: 133) maintains that I (and my above-mentioned colleagues) do? In my analysis in *The End of Drum-Time*, I had of course to be faithful to my sources, but that does not mean that I accepted their conclusions, as I made clear in the epilogue.

For nearly four decades I have tried to promote an increased understanding among fellow historians of religions of archaeological and other approaches that take materialities, embodiments and aesthetics seriously, in both research and education. If Svestad had attended any of my lectures, be they on Hinduism, Judaism, Sami religion, or anything else, he would have realised this. As a matter of fact, I am very critical towards approaches to religions that take only cognitive aspects into account (cf. Rydving 2008). In, which is the main target of Svestad’s criticism, I even refrained from discussing cognitive aspects of the process of change, but focused instead on how bodies, things and places were affected, and on ritual and social aspects. Since my whole approach to religions is based on the idea that they are a matter of things done far more than of things thought, I find it very unfair to be accused of opinions I myself have been fighting against.

Having participated in many archaeological seminars, having read and commented on texts by archaeologists, given guest lectures at different departments of archaeology, and even serving as opponent at the public defence of a doctoral dissertation in archaeology, I know that archaeologists in general do not share the negative attitudes towards non-archaeologists (and especially historians of religions) to which Svestad gives vent. However, his text demonstrates that there is urgent need of more – not less – co-operation between archaeologists and historians of religions.

REFERENCES

- Nordeide, S.W. & Brink, S. (eds.), *Sacred Sites and Holy Places: Exploring the Sacralization of Landscape through Time and Space*. Brepols, Turnhout.
- Rydving, H. 1995a. *The End of Drum-Time: Religious Change among the Lule Saami, 1670s–1740s*. Uppsala University Library, Uppsala. 2nd ed.
- Rydving, H. 1995b. *Samisk religionshistoria: några källkritiska problem*. Almqvist & Wiksell International, Stockholm.
- Rydving, H. 2004. Sami responses to Christianity: resistance and change. In J.K. Olupona (ed.), *Beyond Primitivism: Indigenous Religious Traditions and Modernity*: 99–108. Routledge, New York.
- Rydving, H. 2008. A western folk category in mind? *Temenos* 44: 73–99.
- Rydving, H. 2010. *Tracing Sami Traditions: In Search of the Indigenous Religion of the Western Sami during the 17th and 18th Centuries*. Novus forlag, Oslo.
- Svestad, A. 2011. The impact of materiality on Sámi burial customs and religious concepts. *Fennoscandia Archaeologica* XXXVIII: 39–56.
- Svestad, A. 2013. Sámi burials and sacred landscape: aspects of the impact of materiality on Sámi religious conceptions and practices. In S.W. Nordeide & S. Brink (eds.), *Sacred Sites and Holy Places: Exploring the Sacralization of Landscape through Time and Space*: 111–39. Brepols, Turnhout.

Copyright of Fennoscandia Archaeologica is the property of Fennoscandia Archaeologica and its content may not be copied or emailed to multiple sites or posted to a listserv without the copyright holder's express written permission. However, users may print, download, or email articles for individual use.