

The Stratigraphy and Dating of 8th Century Ribe

by MOGENS BENCARD

In Vol. 6 (1987) of this periodical Lene Frandsen and Stig Jensen (hereafter referred to as Fr & J) published an interim account of their important excavation at Nicolajgade 8 in Ribe, which has complemented the results of the previous excavations in the area in many respects. The excavation was situated some 30 metres from the site in *Kunstmuseets Have* which was excavated in 1975, with the present author as leader.

There are certain differences between the two investigations, but stratigraphically they have such great similarities that they can usefully be compared. This became evident even at the trial excavation, and the discussions which were held when Helge Brinch Madsen and I visited Ribe were therefore very worthwhile for all of us. In order to widen these discussions, which have continued since, and to prevent them becoming merely internal "Ribe-talk", it may be of benefit to others to put forward some comments on the article at the present point in time.

In their introduction Fr & J touch on the fundamental problem: "The time dimension is a significant factor. How many years did it take to accumulate the series of layers we have looked at? Did the site have a long or a short functioning period?" This question was crucial also in the excavation in *Kunstmuseets Have*, partly with respect to general interpretation, and partly because certain datings were difficult to reconcile with others.

The article first gives an account of the stratigraphy, then an evaluation of the dating based on the finds made, and finally discusses the function and character of the site.

To illustrate the stratigraphy a simplified section of the order of the strata was given (fig. 6). In addition a map and a clear account of the individual layers were supplied. Discussion is facilitated by the fact that Fr & J have restricted themselves in this way to the fundamental structure of the stratigraphy and have not become lost in its details. In the following commentary I shall focus attention on the distinctions between the individual layers.

1. Immediately above the natural surface there is a layer of refuse (the term "settlement layer" may be slightly misleading, in that the layer presents only indirect evidence of settlement); the finds from this layer, in contrast to those from

the layers above, do not include evidence of trade or crafts. No equivalent layer was found in *Kunstmuseets Have*, but this layer could well correspond, chronologically, with the ard traces in the natural surface which were there designated "Phase 1". The same ploughed surface was also found during the investigation in *Kunstmuseets Kælder* (area 5) in 1973, and in *Dommerhaven* on the opposite side of Nicolajgade in 1974.

2-3. Immediately above this is a layer of turf, or where this is non-existent, the lowest workshop level (VH1). Where both layers are present, VH1 lies directly above the turf. There is thus a direct continuity between these and the underlying layer – a fact to which Fr & J also draw attention.

4. VH1 and another contemporary level, referred to as VH1a, lie on both sides of a "ditch" (G1), which is flanked by the turf-layer in such a way that the turf has to be interpreted as having been laid up to G1, i.e. it must be supposed that G1 is contemporary with the turf and with VH1-VH1a. The ditch is therefore not a dug-out trench, but should rather be seen as an open area between the two workshop levels. The excavators interpret G1, very convincingly, as a trace of an original parcelling-out (tenement division) of the site. Corresponding "ditches", but without the surrounding turf-layer, could be seen in *Kunstmuseets Have*. There they could only be viewed as possible indications of a parcelling-out, whereas the results in Nicolajgade 8 can be taken as confirmation of this theory. Our "Phase 2", which was not found in Nicolajgade 8, is a layer of undecomposed or mineralised manure, recorded in *Kunstmuseets Kælder* and in *Dommerhaven* as well as in *Kunstmuseets Have*. Since the manure could be seen on one side to have been ploughed down into the ard-furrows of Phase 1, and since on the other side it partly made up the workshop layers and partly continued without interruption into our Phase 3 workshop-layers, we could clearly assume that there was direct continuity between these phases. Despite the difference in sequences of layers there is therefore a parallel in that the close chronological linkage is identical.

5. "After some time", write Fr & J, without attempting any more precise estimate, G1 was filled in, and then a layer ("VH2") spread over the whole area of the excavation. Here it is important to note that the finds in VH2 do not differ significantly from those in VH1 – or even as a matter of fact from those in VH3 (see the tables of pottery elements, and pp. 179 concerning the relationship between VH1 and VH2, and 180 concerning casting moulds). VH2 therefore does not represent a chronological break.

This continuity is supported by the relationship between G1 and the above "ditch" G2, which clearly respects the course of G1. In other words the original parcelling-out was not forgotten, even if the traces were apparently obliterated. It should be

mentioned that no ditches, either here or in *Kunstmuseets Have*, held remains of posts. There were no traces to be found. In Area 1 (a trial excavation in *Kunstmuseets Have*) the conditions for observation were so remarkably good that even individual spade-marks could be seen. There can be no doubt that this was a genuine ditch, which was dug through Phase 1A down into the subsoil. At that time we cut sections in the ditch in all imaginable directions in order to find traces of either posts or stakes which could have supported a wattle structure, but there was nothing to be found – except traces of the spade. One can therefore only speculate as to how G1 could have been re-discovered when G2 was being established. It is crucial to the present discussion that the area did not change character or sub-division, and that one cannot therefore imagine that there could have been a great difference in time between G1 and G2.

6. Immediately above VH2 (or so one must suppose since Fr & J do not indicate otherwise) the rest of the workshop levels (VH3–6) are accumulated. The fire-places can be seen in the middle, and the layers decrease in thickness in the direction of G2 (and G3 to the other side). We are here dealing with a continuous development of workshop levels (“activity horizons” were what we called them during the excavation in *Kunstmuseets Have*), and intervening layers of sand mixed with varying quantities of refuse (then called “levelling horizons”). The phenomenon is seen in completely parallel form in *Kunstmuseets Have* (“Phase 3”). At the time of the first excavation, and also during the one at Nicolajgade 8, there was much discussion concerning whether there had been breaks (e.g. in the form of vegetation horizons) observable in these layer-sequences, but this does not appear anywhere to have been the case. Fr & J do not mention anything of this kind, and if they have results which demonstrate this one must regret, for the sake of the discussion, that they have not been made explicit.

My argument has consistently been that we are here dealing with an unbroken sequence, and that a break of even a decade would be discernible. In support of this it can be pointed out that an individual activity-horizon could not have had a long life-time – in fact it could not have survived even one winter. The workshops were not protected by a roof, since there are no traces of house-construction. Tents or flimsy windscreens leaving no traces which archaeologists can register are the only possibility, if the activities did not actually simply take place under the open sky. Even the most solid of the examples of fire-places consist of thin, more or less scorched layers of clay, sometimes with an under-layer of stone. Frost and rain would have worn them away if they had not been covered relatively rapidly by a new layer.

This has forced me to conclude that Phase 3 covered a very short span of time – and that in fact a workshop level must correspond to the duration of a market. If there had been a market once a year on the site, a workshop layer (with related levelling) would represent a “year-ring”. The continuity in any case is quite clear. Not only is the parcelling-out of the land respected, but even the fireplaces are sited in the same spot in succeeding layers. Transposed to Nicolajgade 8 this would imply that the shop-levels strictly speaking need not represent more than 5 years, to which should be added the time which elapsed between G1 and G2.

7. The ditch G2 also has a parallel in *Kunstmuseets Have* (as does G3), although not as clear as here. G2 is also significant for the judgement of the time-factor. Again it is important to stress that this is not a matter of a dug-out ditch, but rather of an area which has been open while the surrounding horizons have risen on each side. In the light of this Fr & J also speak of G2 as a “cavity” (p. 179). Apparently they conceive of the filling-in of G1 and G2 as having taken place independently, at different times. On the basis of the finds, however, they also consider the lapse of time between the uppermost workshop levels and the filling-up of G2 to have been of short duration (p. 179).

An open ditch presents in itself a considerable interpretation problem. The VH levels on both sides consist to a large degree of loose sand, which easily shifts out to the sides. To this it should be added that the light sand of the Ribe plain is often stirred up by the wind to end up trapped in a ditch. Blown-sand layers were in fact found in *Kunstmuseets Have*. It therefore follows that the longer the time-span one wants to connect with the establishment of the workshop levels, the more difficult it is to explain how the ditch could have remained empty. Our own observations of the conditions in *Kunstmuseets Have*, together with the presented description of the stratigraphy of Nicolajgade 8, do not give me reason to alter my basic position, which is that we are here dealing with a short lapse of time.

Taking the stratigraphy on its own as departure-point, I therefore have great difficulty in accepting the time-span which Fr & J have assumed, i.e. c. 100 years from VH1 (“first quarter of the 8th century”, p. 182) to G2 (“early Viking Period”, p. 182).

If I am right, there thus appears to be a built-in contradiction, in these two investigations, between the stratigraphy and the current dating of the finds, not least in the layers which contain the mould-fragments of Berdal brooches.

Where I have used an explanatory model which puts weight on the testimony of the stratigraphy, Fr & J have chosen one which accords primary importance to the finds. Their long time-span is bounded at the lower end by the first coining of the *sceattas* found, c. 720, and at the upper end by the dating of the Berdal brooch gripping beasts to the early Viking Age (p. 181), i.e. c. 100 years. They use an examination of the pottery in support of this long development. *Sceattas*, which are a completely dominant basis for dating in both excavations, were found in Nicolajgade 8 in the workshop levels, but not in G2. In the other excavations they occurred in both Phase 2 and Phase 3, and also in the Phase 4, which is probably either contemporary with, or later than, G2. The presence of casting-mould fragments, combined with an absence of *sceattas* in G2, leads Fr & J to conclude that the conventional dating of the Berdal brooches to the beginning of the Viking Age can be sustained. Strictly speaking the absence of *sceattas* need not be anything other than chance, and thus the two mould-fragments mentioned (reproduced in fig. 9) take on special significance. Fr & J themselves in their text are in doubt as to what object has been cast in the fragments. They suggest that the gripping beast of the fragments has long ears similar to the Borre style. The illustration text states, however, that they

come from brooches of the Berdal type, and they are inserted into a drawing of such a brooch. There is no specific reference, but the drawing appears to be identical with Berdal brooch "Type 2" from *Kunstmuseets Have* (see *Ribe Excavations*, vol. 2, p. 46). Mould fragments of this type were found in the two upper concentrations (8 and 9, *op.cit.* pp. 88–89). Concentration 7, immediately below, likewise produced Berdal brooches, but of other types (p. 87). The contents of concentration 7 also included the layer QD, and those of concentration 8 the layer AAE. As is apparent from Kirsten Bendixen's table (*Ribe Excavations*, vol. 1, p. 90) one *sceatta* was found which can be attributed either to layer QD or to AAE. Furthermore one *sceatta* was found in layer CØ which is contemporary with these concentrations, and two come from layers KZ and KÆ of Phase 4. Thus here we have a clear case of contemporaneity between Berdal brooches and *sceattas*. As is made clear by Kirsten Bendixen (*Ribe Excavations*, vol. 1), these *sceattas* were struck in Europe until 755. Bendixen at the same time put forward the theory – a seemingly accepted one – that *sceattas* had been in circulation in Denmark up to c. 800, when they were superseded by the earliest Hedeby coins.

On the basis of the find-frequency (highest in the lower layers, decreasing through the upper ones), Fr & J build up a case that the coins occur most frequently in the layers from the time of their circulation in Europe, while the layers where there are fewer of them are supposed to correspond to the period when they were only in circulation in Denmark – i.e. the second half of the 8th century. This might well be presumed to be the case in a situation where coins occurred in the upper layers which were in common circulation elsewhere in the world. There are no such finds here. One can therefore equally well maintain that all the *sceattas* come from a time when they were only in circulation in Denmark, i.e. the second half of the 8th century.

It should be noted, in parenthesis, that precisely the absence of European coins in Ribe after Pippin's coin reform of 755 makes Kirsten Bendixen's theory about circulation of *sceattas* in Denmark in the second half of the 8th century rather difficult to apply. The way *sceattas* were found in Ribe, spread over a large area and at varying depths, makes it impossible to see them as a scattered treasure-hoard; they must be taken as evidence of a coin-based economy. Kirsten Bendixen bases her theory about the prolonged circulation on the appearance of the Wodan/monster as a type of the earliest Hedeby coins. It has a larger flan, which Kirsten Bendixen associates with Charles the Great's coin reform in 794. Much would fall into place in the event that the numismatists could consider it possible to redate the Wodan/monster Hedeby-coin back to the middle of the 8th century, i.e. seeing it as an unbroken continuation of the corresponding *sceatta*. On the other hand this would make the dating of the Berdal brooches even more problematic.

As the discussion appears now it seems best to maintain Kirsten Bendixen's extended circulation theory, according to which both the earlier excavations' Phase 3 and the workshop levels in Nicolajgade 8 are dated to within the period 720–800. The style D mould fragments found mainly in VH2 and VH3 (pp. 180 and 182) can in any case be dated to within that period.

As far as the pottery is concerned, it is worth noting that all the comparative material to which Fr & J refer is dated to the "later Germanic Iron-Age", i.e. the period already mentioned. They use the statistical account of the differences in rimsherd shapes as an argument in favour of a long time-span. Personally I find this difficult to accept, partly because of what has already been mentioned about the dating of the comparative material, and partly because statistics based on 189 pieces from many different types seem to me too flimsy.

There is thus a large gap between Fr & J's find-based time-span of 100 years and my stratigraphy-based contention that the layers in Nicolajgade 8 need not represent more than e.g. a decade. This short period could, as I see it, be placed at any time between 720 and 800, e.g. at the end of the century. The only certain date that has been fixed is a dendrochronological dating of c. 710, which Fr & J also mention. This comes from a well in *Dommerhaven* Phase 1, and is thus probably contemporary with the lowest "settlement layer" in Nicolajgade 8.

How can one view the standard dating of the Berdal brooches if the layer which contains the mould for casting them is dated before the year 800? This question is indeed a significant underlying cause of Fr & J's argument in favour of the long time-span.

While stressing once more that *sceattas* were found in the bronze-casting workshops with Berdal brooches in *Kunstmuseets Have*, I should like to state the following: This is the first time that we have been faced with casting moulds for Berdal brooches, and therefore the first time we have evidence about the actual production of these brooches. Their dating has hitherto depended on the finished brooches, chiefly from grave-finds. It stands to reason that there would be a time difference between these two stages of the life of the brooches. How large a difference would be dependent on how many generations had used the same brooch, and it is difficult to make any meaningful conjecture about such a question.

The dating of the Berdal brooches to the early Viking times, i.e. in the 9th century, which is a fundamental point for Fr & J, is based on a dating of the beginning of the Viking Age to around the year 800. This date, however, is no longer as unquestioned as it once was. In his book on *Ovala Spåmbucklor* (1985), Ingmar Jansson, in the chapter "Den absoluta kronologien" (the absolute chronology) (p. 176 ff.) reviews the discussion and calls attention to the finds from recent years which could contribute to an altered evaluation. Jansson reaches the conclusion (p. 186) that in the light of up-to-date knowledge the beginning of the Birka time (the "archaeological Viking Age") should now in fact be situated before the 8th century.

From the point-of-view of style Signe Horn Fuglesang (Proceedings of the Tenth Viking Congress, Universitetets Oldsaksamlings skrifter 9, 1987, p. 219) makes a clear distinction between the Ribe beasts and Oseberg: "The Ribe excavations also reactivate the question of dating the early types of Viking brooches. This should not be confused with a re-dating of the Oseberg style, since one must distinguish between the various types of gripping beasts and the Oseberg style proper. The Ribe moulds found so far do not include examples of the latter." These quotations should suffice to demonstrate that the dating of the Ribe moulds is not so uncontroversial, as Fr&J

seem to think. Thus they cannot be used as a solid basis for a 9th century date.

As the reader may have observed, there is much to discuss concerning the exciting finds in Ribe, even as far as dating alone is concerned. Without doubt there will be much more discussion to come before we reach agreed explanations. In conclusion here there is one factor to be mentioned which should be taken into consideration: are there chronological differences between the workshop layers that have accumulated on the “stall-sites” at Nicolajgade 8 and the bronze-casting layers at the stall-site furthest distant in *Kunstmuseets Have*? This can only be decided when a detailed study and publications of the finds and stratigraphy of *Kunstmuseets Have* is available. On the other hand, it is of great benefit that Lene Frandsen and Stig Jensen have presented their deliberations as clearly as they have done.

The above has been produced so that others can be aware of what the Ribe discussion is about, and so that the general lines of debate do not crystallize prematurely. It can also be made known that a grant from the Research Council has made possible continued publication of *Ribe Excavations 1970–76*. Vol. 3 is now ready for immediate printing. Vol. 4, which will contain an account of the stratigraphy of the excavations, is now in preparation.

Translated by Joan Frances Davidson

Mogens Bencard, The Danish Royal Collections at Rosenborg, Østervoldgade 4A, DK-1350 Copenhagen K.

The Dating of Ribe's earliest Culture Layers

by LENE B. FRANDBEN and STIG JENSEN

It is with great interest that we have read the comments by Mogens Bencard on our article about the excavations in Nicolajgade 8 in Ribe. We agree with Mogens Bencard that it would be best to avoid internal “Ribe-talk” about the chronological questions relating to the earliest culture layers in Ribe, and it was actually for that reason that we prepared the article under discussion only 5 months after the close of the excavation. In Danish archaeology, taken as a whole, it is unusual for the same important site to be excavated by two different archaeological teams with an interval of so few years between. As can be seen, this has already given rise to fruitful discussion.

We were glad to note that there is no overall disagreement between Mogens Bencard and ourselves concerning the basic stratigraphy in Nicolajgade 8. Mogens Bencard states that the sequence of layers is unbroken, and on that point we are in complete agreement with him. The disagreement relates – as far as we can see – exclusively to the question of the time-period which the build-up of layers represents, and is therefore fundamentally a matter of the extent to which one should base one's work on an interpretation of the excavated layers or should instead rely on a chronological analysis of the objects found in those layers.

Since the previous article in JDA was written, we have had the opportunity to go through the *entire collection of find-objects* from the excavation, with a view to future publication (1). This has not changed our opinion about the chronology, but has made it possible to enlarge the basis for discussion of the dating. In order to make this evidence accessible we have worked out a schematic presentation of a number of different types of object and their siting in the layer-sequence.

Before we discuss this table in detail, we would however like to add some clarification on one point where Mogens Bencard has evidently misunderstood us. Our subdivision into workshop levels (VH1–6) does not represent six separate workshops, but on the contrary is simply a practical subdivision of the sequence of layers – a chronological work-tool. To stress this point it can be mentioned that even taking only the span between the top of VH2 and the top of VH6 there are 142 recorded layers, of which 52 lie directly one on top of another.

Let us begin by looking at the *domestic pottery*. As is apparent from the table, pottery vessels with everted rims dominate in the sequence in VH1. In VH2 the inverted rim (semi-spherical pot) occurs, and thereafter it occurs with increasing frequency throughout the layer-sequence to become completely dominant in G2. Correspondingly, flat bases are replaced by globular ones. In addition, the semi-spherical pot develops so that pots with a groove on the outer side under the rim appear in