RINGS, SECTORS AND BARMOSE I: A REPLY TO STAPERT

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ABSTRACT: This paper comments critically on Stapert's (1992 and this volume) treatment of the Danish early Maglemosian site Barmose I. Basically, he displays an unfortunate mixture of the uninformed use of Maglemosian material on the one hand and methodological inconsistencies on the other.

He fails to consult the excavator's publications and also largely fails to refer to other primary site publications or sources on the Maglemosian, thus ignoring much relevant information. Consequently, his treatment of the site, classification, and hut floors suffers from the lack of knowledge on the nature and specifics of the data.

His ring and sector method has severe theoretical, statistical and operational constraints and has neither been tested on independent ethno-archaeological material for which the behavioural parameters are known, nor matched consistently with other methodologies. It is not proven that the ring and sector method is capable of delivering information of relevance for behavioural interpretation. Thoroughly, his discussions lack balance and consistency.

His conclusion, that Barmose I was an open air hunting camp with a central hearth and only used by two men, remains unfounded and contradicted by the primary data.

KEYWORDS: Intrasite spatial analysis, Barmose I, hut floors, classification, ring and sector method, multivariate methods, ethno-archaeology, Mask Site.

1. INTRODUCTION

In his recently published thesis 'Rings and sectors: Intrasite spatial analysis of Stone Age sites', Stapert (1992) launches a view of the Danish early Maglemosian site Barmose I very much at variance with previous interpretations of the site (Johansson, 1971; 1990; Blankholm, 1991). As will become apparent, this discrepancy seems to be rooted in Stapert's unfortunate mixture of the uninformed use of Maglemosian material on the one hand and his methodological inconsistencies on the other.

In the first instance Stapert completely fails to refer to or consult the excavator's original and latest publications of the site (Johansson, 1971; 1990). Except for Bokelmann (1986; 1989), Bokelmann et al. (1981; 1985), Grøn (1987a; 1987b; 1989) and Blankholm (1984; 1987; 1991) he also fails to refer to any other primary site publication or sources on the Maglemosian. By doing so, much important information is ignored. For instance, it is clearly stated in Johansson (1990: p. 43) that two lumps of resin show the toothmarks of a child of approximately 7-8 years of age, and of a young person not less than 11 years of age, respectively. This effectively refutes Stapert's notion of a men-only hunting camp (1992: p. 157).

In the following I will briefly deal in turn with some pertinent aspects of the Barmose I data; the site, classification, hut floors, and the ring and sector method.

2. THE SITE

From the outset it must be stressed that my 1991 application of the best of spatial analytical methods (i.e. k-means Analysis, Unconstrained Clustering, Correspondence Analysis, and Presab) to Barmose I was not a test, but rather a demonstration (a tutorial if one wishes). Stapert seems to forget this completely. In fact, I (Blankholm, 1991: p. 183, see also the Danish summary p. 233) stated:

> The theoretical potential, practical limitations, and relative power and efficiency of the selected methods should now be readily apparent from the preceding chapters. In fact, all students and professionals with a basic knowledge of quantitative methods and spatial analysis should be able to proceed from here and perform their own analysis. However, in conclusion I will demonstrate how the best of methods may be applied to a purely archaeological example.

The real test, in fact, was on ethno-archaeological material (The Mask Site (Binford, 1978)) for which the behavioural parameters are known. This test clearly proved that intrasite spatial analyses are capable of delivering relevant and important information on spatial data structures for behavioural interpretation (Blankholm, 1991: p. 211).

Since the Barmose I site was already published (Johansson, 1971) and was about to be re-published in extenso (Johansson, 1990), I refrained from going into details in my general summary of the site (1991: pp.

184-186); simply assuming that any analyst naturally would consult the site publications, or, if necessary, the excavator for more detailed information, as I did myself.

In his short rendition of the site based on my own summary (Blankholm, 1991: pp. 184-186), Stapert (1992: p. 145) seems particularly concerned about the effect of the test pit and test excavation on his analysis and interpretation. He could have spared himself those worries (not least concerning his sector 2 (Stapert, 1992: fig. 1, p. 170)) had he read the excavator's latest publication (Johansson, 1990). There it can be seen from the distribution plans that the large 2x1 m test excavation contained 19 tools, which, to put things straight, is less than half of the 41 tools (or 11.3% of Stapert's total) Stapert nonchalantly excludes from his own analysis and thus consequently ignores the behaviourally interesting areas in the eastern periphery of the site (Johansson, 1990: p. 47; Blankholm, 1991: p. 192 ff.); apparently because he cannot get his rings to fit (Stapert, 1992: p. 147)! I guess this puts his argument (Stapert, 1992: p. 145):

> ..., the disturbing effect of these two test pits on any kind of spatial analysis is taken too lightly in Blankholm's discussion of the results...

into the right perspective. If anyone has taken Barmose I and its data too lightly it manifestly is Stapert. On the contrary, my own analysis and interpretations did fully consider the contents and effect of the regular test pit (Blankholm, 1991: pp. 185-186, 204).

3. CLASSIFICATION

It is of some interest that Stapert (1992: p. 147), without having read the excavator's publications (Johansson, 1971, 1990) and without having seen the material first-hand, relegates the tool class 'square knives' to mere 'splintered pieces' and then goes on to treat them seperately from the rest of the tool classes in his analysis. I wonder how he can justify the following assertion (Stapert, 1992: p. 147):

> Splintered pieces (which Blankholm calls 'square knives') are very numerous, and in fact do not constitute a formal tool class, as the splintering is no intentional retouch but probably the result of some heavy use...

If Stapert really wishes to redefine the material he should do so based on first-hand knowledge of the data, not on sheer speculation. Certainly square knives is a formal tool class in its own right and as such should have been treated consistently with the other artifact classes. He also seemingly is dissatisfied with the term 'micro-burin', which he rather wants to call 'notched remnants'. Although the term 'microburin' may be unfortunate, it nevertheless is a standard and well-recognized term in European Mesolithic typology. At the time of writing, I saw, and see now, no compelling reason to change it.

4. HUT FLOORS

After a discussion of the Presab method for spatial analysis (Blankholm, 1991: pp. 151-65, 199-202), Stapert (1992: p. 145) writes:

> Even more disturbing is the fact that Blankholm's analysis proceeds on the basis of several unproven assumptions, which are not critically tested. The most important of these is the idea that a hut was present at the site, with the hearth located at the centre of its interior. The demonstration that a dwelling was present should be one of the goals of intrasite spatial analysis, not an assumption to start with! It will be realized that the interpretation of any 'patterns' established with the help of whatever mathematical technique, will be very different, depending on whether or not the presence of a dwelling is assumed.

It is self-evident that the demonstration that a dwelling was present should be one of the goals of intrasite spatial analysis, and this also was exactly what was done. The problem is that Stapert confuses matters. In my analysis I used a well-established spatial analytical method, based on intimate knowledge of and experience with preserved Maglemosian hut floors and associated material distributions (Blankholm, 1981; 1984; 1987), for evaluating the Barmose I case. The outline then was modified according to other contextual knowledge (Blankholm, 1991: p. 185). I should kindly recommend Stapert to read Blankholm 1984 and 1987 in detail. None of the multivariate methods (apparently complex methods to Stapert) tested by myself in 1991 were ever designed or geared to demonstrate dwellings per se. Consequently they were not used to this end!

Certain aspects of the distribution of debitage (waste) is essential for delineating hut floors on Maglemosian settlements. However, Stapert excludes waste/flint-knapping debris from his Barmose I analysis altogether apparently for several, in my view, highly dubious presumptions not mentioned in his text on Barmose I proper. Instead they are enumerated in a methodological section of his book called 'Some choices' (Stapert, 1992: pp. 28-29). Also, in his discussion of the beha-vioural meaning of the distribution of cores (Stapert, 1992: p. 146), he does not recognize their conspicuous oval distribution. In my view this distribution is not consistent with the Binfordian toss model neither in static, nor in rotary seating mode. Again, I see no reason to change my (and the excavator's) original view on its significance (Blankholm, 1991: p. 185).

In fact Stapert's treatment seems to have more than its share of unwarrented presumptions, biases and prejudices against prehistoric behaviours, their multi-variate nature and firmly tested methods to their resolution. Unfortunately Stapert does not report the coordinate of the origin for his rings and sectors, so I am unable to redo his analysis including debitage.

Anyway, the partly analytically demonstrated floor on Barmose I is based on much firmer and solid knowledge and facts than the presumption that Stapert wishes to replace it with and then depart from: i.e. an

open air, central domestic hearth. In the first instance he bases his research on two unwarranted assumptions or postulates: a) the spatial organization of a site is defined by a central hearth, and b) that the central hearth (itself a presumption to begin with) was the focal point of the daily life of the inhabitants, regardless of whether it was inside or outside a dwelling. While this sometimes may have been the case, I could enumerate a great number of other things that could have structured or partly defined the spatial layout of a site, including dwellings. Moreover, Stapert's analysis carries with it, or imposes, a series of notions from his previous investigations of Late/Upper Palaeolithic and Mesolithic sites, widely scattered in time and space across northwestern Europe, which may not be applicable to the Maglemosian. In my opinion his methodology clearly would have gained had he tested it on independent, ethno-archaeological material for which the behavioural parameters and internal arrangements are known and then presented a positive result (see below).

Also Stapert, in his assessment of his own method, forgets that some dwellings, such as tents or light structures, might not necessarily leave any archaeologically visible traces, which of course has a bearing on his grouping of hearths into open air and inside hearths.

5. THE RING AND SECTOR METHOD

Perhaps not surprisingly, Stapert starts off his theoretical considerations with a very insufficient and unbalanced critique of mathematical/statistical approaches and methods of others, largely emphasizing their drawbacks and completely forgetting their advantages (Stapert, 1992: p. 144). In fact, one gets the impression that Stapert has felt it necessary to play down the capabilities of other methods in order to advance his own. In this perspective a statement on his own method is interesting (1992: p. 144):

> It should be clear that this method does not claim to detect all possible spatial patterns in sites.

While this certainly is true for the ring and sector method, he fails to report that neither have any such claims been made for any other method (Blankholm, 1991).

Stapert's seeming preponderance for grabbing things out of context and apparent unwillingness to see things in a broader perspective is also reflected in the opening paragraph of his Section 2 (Stapert, 1992: p. 145). For instance, in his rendition of some of the results from my 1991 publication he fails to report that the various methods and techniques were, in fact, rigorously and scientifically tested on ethno-archaeological material (see above). Clearly my aim was objectively to evaluate and find those methods that were capable of delivering

information of relevance for behavioural interpretation (Blankholm, 1991: p. 53 ff.).

Back to rings and sectors. Stapert (1992: p. 144) claims that the main goal of his paper is:

> ..., to explore the potential of the ring and sector method, compared to other techniques of spatial analysis.

Basically Stapert falls short of his aim. Firstly, he opted to try it out on a site (Barmose I) for which he never sought relevant information in the original publications. Secondly, it is surprising that since so much of his argumentation and modelling is linked to Binford's 'hearth model' (Binford, 1983), he does not take the only right and straightforward consequence and test his techniques on the Mask Site data, not least since those data are equally available in my 1991 publication (Blankholm, 1991, Appendix A)! His application of the ring and sector method to Gönnersdorf (Bosinski, 1979; 1981) was not a real, independent, test as the material and structural remains were interpreted prior to analysis. Thirdly, nowhere does he embark on any rigorous procedure for the testing and comparison of the different methods although such procedures were readily available in my book (Blankholm, 1991: p. 55 ff.).

With both data sets (The Mask Site and Barmose I) ready at hand, it should have been no problem for Stapert to match his method in a more appropriate fashion. Instead he defers to what appears to me an unbalanced and inconsistent discussion.

As to the ring and sector method proper it suffers from a number of constraints. Being a feature oriented approach (which is fine and commendable (Blankholm, 1991: p. 25)), Stapert (1992: p. 144) states:

> It is directed at describing and interpreting global spatial patterns that relate to the domestic hearth.

This requires that a domestic hearth be demonstrated and not simply assumed in the first place, and the method then applied subsequently. He (Stapert, 1992: p. 144) also states:

> It is essentially a way of partitioning space (in two related ways: rings and sectors), which seems more suited than any regular grid structure to analyse sites where the global spatial structure is determined by the presence of a central hearth.

This again would require the demonstration prior to analysis that the global structure is determined by a central hearth.

In contrast, none of the multivariate methods make such extremely narrow assumptions. In fact, the latter are developed and operate under a minimum of constraints according to modern concepts of past human behaviour, in which activity areas are expected to be of widely differing size, shape, density, composition and internal patterns of covariation and association, and in which the above characteristics are treated as variables (Whallon, 1984, Blankholm, 1991). This, indeed, is far

from Stapert's simplistic notion that they essentially are cluster procedures.

Operationally and regarding interpretation, the ring and sector method is linked to the work of Dekin (1976) and Yellen (1977) and in particular to Binford's (1983) 'hearth model' and the associated concepts of toss and drop zones in a very narrow fashion, ignoring the fact that people need not always arrange themselves or do things in semi-circles around hearths.

The use of the ring and sector method constrains the association and covariation of piece-plotted artifact classes and consequently much information is lost for interpretation.

The position of the rings and sectors clearly is arbitrary, as is the selection of radii for the rings and size of the sectors. This is a draw-back. For instance, the selection of radii can be made so as to reveal, respectively mask, important variability, just as in the case of selecting intervals for histograms (also frequently used by Stapert) and the selection of contours for distribution plans (Blankholm, 1991: p. 79). Moreover Stapert's (1992: p. 158) statement:

> It contains no inherent assumptions of a statistical nature, which encumber many of the more complex approaches to intrasite spatial analysis.

is not correct. In fact, the statistical analysis and significance tests Stapert applies to the ring and sector method minimally require that comparisons are made over units of equal size. Where this is not possible, areal extent of the pertinent units must be accounted for in the calculation of the expected values (e.g. Dalton et al., 1972). Merely consider that the area of a circle grows exponentially relative to r, while areal increase between circles drawn with equal increments of r grows linearly. For example, some of Stapert's calculations are based on 2 rings of 2 m width (Stapert, 1992: pp. 153, 159). In these cases the area of the inner and outer rings are 12.56 and 37.68 sq.m, respectively, which of course must be considered in significance tests.

Talking statistics, it is also of interest to note that no mention is made of the very large standard deviations and their possible analytical consequences for his distance measures per tool class (Stapert, 1992: table 1).

Some general problems of Stapert's treatment of Barmose I was dealt with above. A number of more specific aspects will be dealt with in turn below. On p. 145 and again on p. 146 Stapert comments critically on the use of my own method, Presab, and related interpretation of Barmose I. His critique merely reflects his uninformed use of the data (already commented upon) and seeming unwillingness to understand modern multivariate approaches to spatial analysis generally and specifically. In the first place, the Presab procedures are clearly described in Blankholm, 1991: pp. 151-164). As to his statement (Stapert, 1992: p. 146):

> A general problem with this kind of approach seems to be that there are no guidelines for interpreting the results of such rather mechanical mathematical operations. These do not seem to be directed at answering specific questions, and we are essentially left in the dark as to what the outcomes might mean.

the answer is that multivariate methods, such as k-means Analysis, Unconstrained Clustering, Correspondence Analysis, and Presab, based as they are on modern concepts of past human behaviour, in fact are so versatile that they can be applied to answer or elucidate a wide range of questions, and not simply one single specific one. To me this is a great advantage. As to the results and as a comment to Stapert's appraisal of 'old-fashioned' ways (Stapert, 1992: p. 145), I can only refer to my own conclusions (Blankholm, 1991: p. 203):

> The Barmose I example has minimally shown that all the methods have proved useful for gaining a quicker and much clearer overview of the pattern of variation and covariation between and among the involved item categories than would have been possible from visual inspection.
>
> With all due respect, a very generalized interpretation such as Johansson's (1990), for instance, which suggests only two activity areas, would be quite common among professionals these days as well.
>
> My own preliminary and visual inspection indicated that there would be more areas of immediate interest, and possibly also different uses, both inside and outside the dwelling.
>
> However, the spatial analytical methods instantly revealed an even greater number of interesting areas and patterns of variation and covariation, and also through the aggregate statistics allowed for quick and precise descriptions and assessments of their contents, centres, sizes, etc., such that different areas could be readily compared. That this is all desirable need not be emphasized.

As to the application of the ring and sector method, it is based on the partitioning of space around the 'central hearth'. In this case one is left wondering what constitutes the centre of the roughly 2.4x1.5 m large hearth area. Stapert does not report, either that it is uncertain that the whole area ever was in use at one and the same time (Johansson, 1990: p. 14), or why he selects a fairly asymmetrical position within the hearth area as his centre for calculations. His results might well have been different had he chosen a more standardized and replicable way of defining a centre. In fact, one gets the impression that it was not the centre of the hearth that governed the imposition of rings and sectors, but rather the desire to fit in as many circles as possible within the constraints of the excavation outline, plus the desire to have the most of the test excavation within one single sector (sector 2, Stapert, 1992: fig. 1, p. 170) that determined Stapert's 'centre' of the hearth!

Moreover Stapert is on very shaky ground as to his interpretations of sexual division of labour and prevailing wind direction. In the first place we have no micro-wear determinations and consequently no firm functional determinations of tools or tool classes for Barmose I. Also the strict caution by many a micro-wear analyst that (Dumont, 1987: p. 88):

..., no single tool type can be confidently correlated to either a single manner-of-use or worked material on a scale greater than that of the individual site.

is ignored, quite apart from the fact that we have no way of relating specific tasks or tools to specific sexes nor can we assess prehistoric wind directions. Simply, the answer to such questions is blowing in the wind!

The conclusion to his analysis, that Barmose I was an open air hunting camp with a central hearth and only used by two men, remains unfounded and contradicted by the primary data, i.e. the two lumps of resin show the tooth-marks of a child of approximately 7-8 years of age, and of a young person not less 11 years of age, respectively (Johansson, 1990: p. 43).

6. CONCLUSION

Some years ago I wrote in a comment to the ring and sector techniques (Blankholm, 1991: pp. 25-26):

Stapert is to be commended for his concern with a feature-oriented approach, yet being global in nature, and limited in scope as to their applicability, they are neither geared to handle local variability, nor versatile.

I see no reason to change my position. Also, I see no reason to change my opinion of Barmose I. The analytical result and interpretation of activity areas would have been almost exactly the same, even if a hut floor had not been defined. Stapert has chosen the wrong context for presenting his case and has been largely uninformed about's the nature and specifics of his database. It remains Stapert's task to demonstrate on independent, ethno-archaeological material, for which the behavioural parameters and structural setting are known, that the ring and sector method is capable of delivering information of relevance for behavioural interpretation.

As to his treatment of Barmose I, I find it eminently unsatisfactory and unconvincing.

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