Neolithic farming practice: an archaeological response to the Göransson hypothesis
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hypothesen vägr jag ställa - ett troget klientel av arbetare på ång och åker, vid fiskevatten, som utgjorde samhällenas anonyma flertal och som i vördnad inför de gudaborna vapenbärarna utförde det dagliga livets tunga sysslor. Att ingjuta respekt och tillgivenhet hos dessa var religionens uppgift. Religionen var sålunda ett sammanhållande element i samhällsbyggnaden.


Referenser


- 1989 a Hällristningar och hällmålningar i Sverige.


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Neolithic Farming Practice—An Archaeological Response to the Göransson Hypothesis

Rapid developments in palaeobotanical work over the last two decades have created a new platform for understanding prehistoric farming practice. Basically this is due to a significant rise in the number of modern C14-dated diagrams, the sophistication of analytical methods, each diagram including more and more specific information, and an increasing understanding of the interplay between regional, local and on site vegetational changes (Andersen 1992; Behre 1981 and 1986; Birks et al. 1988: part 2). This development, however, has also raised a number of new problems, questioning traditional knowledge. It has become increasingly clear how difficult it is to establish general patterns of development (Groenman-van Waateringe 1988), to delimit cattle husbandry from agriculture (Berglund 1985), or even define the relationship between cultural and natural processes of vegetational change as reflected in the pollen record. Hans Göransson in particular has promoted a number of interesting new hypotheses about Neolithic farming practice (Göransson 1982 and 1988 a) which run counter to traditional interpretation. This has stimulated research (e.g. Bjerck 1988), which is most welcome. The problem is, however,
that these new interpretations largely ignore the archaeology of the period, which in some important aspects may modify or even falsify some of them (Østmo 1988; Jennbert 1993).

The second problem is that Göransson has increasingly come to consider his own theories as established knowledge, rather than as working hypotheses. In a recent article (Göransson 1988b) this led him to reject the traditional understanding of Neolithic farming practice, as originally proposed by Iversen (1941), as a myth.

In order to keep the debate open, I shall briefly discuss the basic elements in the Göransson hypothesis (1982, 1988a, and especially 1988b) from an archaeological (and South Scandinavian) point of view, although I shall allow myself also to comment briefly upon the palaeobotany (see review by Andersen 1989).

Göransson hypothesis is simple and elegant. It presumes

- that Neolithic Man practiced some farming before the Elm decline
- that this will only be sporadically recognized in pollen diagrams due to the forest cover
- that the Elm Decline was basically a natural phenomenon which suddenly opened up the cover and not only allowed pollen evidence of the existing farming practice to spread, but also stimulated further expansion of farming
- that the gradual closing of the forest visible in many Neolithic diagrams consequently did not reflect a decline in farming activities, but rather the closing of the pollen spread in combination with an intensified exploitation of the forest into a developed forest farming practice.

The basic philosophy is that Man was responding to Nature during the Neolithic rather than the reverse. The natural lifting of the forest cover around 4000 B.C. allow us to catch a glimpse of what was really happening, and soon afterwards the curtain was drawn again. Only from the Late Bronze Age/Early Iron Age onwards did Man open the landscape sufficiently to allow for pollen dispersion and a more straightforward explanation of the human impact on nature in the pollen record.

1. Pre-Neolithic farming. There is virtually nothing in the archaeological record of Southern Scandinavia to support such an hypothesis. No domestic animals, no traces of grain. A few late Ertebölle settlement sites (by some considered mixed settlement sites) may, if accepted, suggest import and use of grain from neighbouring tribes south of the Baltic as reflected in grain impressions on pottery (Jennberg 1984, discussion in Journal of Danish Archaeology Vols. 4 and 5). But the evidence is ambiguous and not yet supported by finds from other sites. Moreover the indications from pollen diagrams are still scanty, and most likely due to other factors, as long as they are not supported by more evidence. This is not to deny that late Ertebölle society could have manipulated the forest for hunting purposes.

2. The Elm Decline was due to natural causes (elm decrease/climate). I shall not go into the pollen botanical discussion, but only point out that Early Neolithic sites are often found upon low lying slopes and terraces close to lakes and streams, as well as on higher ground. Only later did higher lands come into full cultivation, and even later marginal lands. This pattern is recurrent in Northern Europe (Sherratt 1981; Kruk 1982; Madsen and Juel Jensen 1982). As the lower slopes and terraces were its typical habitat, it comes as no surprise that elm is always declining in Early Neolithic pollen diagrams, in various combinations with other species, mostly lime (tilia), from high ground. To this could be added that “Elm Declines” are also found in earlier diagrams from the Linear Band Culture of Central Europe during the 5th millennium B.C. where they are linked to agricultural expansion (Kalis and Meurers-Balke 1988; Kalis and Zimmermann 1988).

Another critical feature of the Elm Decline is its apparent synchronous occurrence over most of Europe between 4000–3800 B.C., suggesting a natural cause. Such rapid spread of new cultural and economic practices, however, are not unusual in prehistory. One need only to refer to the Corded Ware/Battle Axe
Culture (Kristiansen 1989), or the Tumuli and Urnfield Cultures of the Bronze Age, which expanded with similar speed and with significant consequences in pollen diagrams. I am not denying the possibility that elm decrease may have been a feature of Early Neolithic agricultural expansion; thus in Switzerland leaf-foddering was practiced at this time, but with elm as a weak component (Rasmussen 1989 pp. 64 ff). However, the picture is complex and cannot be reduced to a single factor or cause, biological or cultural, but rather to an interaction (Starkel 1992).

3. The Landnam is a myth, since it reflected small scale forest farming, now suddenly recognized, while at the same time expanding in the natural openings of the forest created by the Elm Decline. The forest remained the basic resource, and Neolithic Man consequently welcomed the recovery of the forest and adapted successfully to it. Recent pollen analyses from dolmens and passage graves, however, basically confirm Iversen’s perception of slash and burn practice (in opposition to Rowley-Conwy 1981). They also confirm the existence of pastures, as well as the intentional maintenance of a secondary forest or bush vegetation which corresponds to regional diagrams (birch in the Early Neolithic, hazel from the Middle Neolithic onwards, with secondary forests predominating (Andersen 1988 and 1993)).

Massive archaeological evidence further proves that clearances and cutting of forest for farming and grazing took place from the Late Early Neolithic onwards. Long efficient flint axes were produced and distributed throughout southern Scandinavia in their hundred thousands, as is evident from Mathiassen’s regional studies (Mathiassen 1948 and 1957), as well as later surveys. Field surveys also show that broken, used and sharpened axes are regularly found over large areas of today’s farming land, thrown away in Neolithic fields and clearings after use. To this we may add the recurrence of plough marks under many megaliths, proving the existence of fields and the maintenance of an open landscape in the settled areas (Thrane 1989).

4. The recovery of the forest in many areas during the Late Middle Neolithic (the late 4th and the early 3rd millennium B.C.) reflected more efficient exploitation of the forest rather than crisis and settlement contraction. This hypothesis I consider the most interesting and probable. But modifications are needed here too.

Settlement agglomeration did take place in many regions, as proved by regional field surveys (Kristiansen 1988 pp. 74 and 68). It is a general feature that during the period of the passage graves settlements became larger, reflecting the unification of former smaller dwelling sites. It can also be demonstrated that settlements became more permanent. The frequency of new foundations, i.e. the shifting of settlements, decreased throughout the fourth millennium, as demonstrated in some detail at Langeland by Skaarup (1985).

So although there may be some truth in Göransson’s proposal, settlement concentration and movement, certainly contributed to forest recovery, as in NW Zealand and in Southern Scania, where inland settlements moved towards the coast. This established a pattern of coast/inland dualism which apparently remained a characteristic feature up to the present day in several areas—the coastal region being densely settled, dominated by open land, while the interior remained rather more sparsely populated with a higher proportion of forest.

So perhaps we should abandon both the crisis hypothesis—although it may still be valid for some regions—and the forest farming hypothesis, as the principal explanations, and rather seek to combine them in a pattern of regional variation and interaction emerging from the late fourth millennium onwards. It is beyond doubt that already from the third millennium onwards large regions were heavily deforested, and remained so, while there was forest recovery in others, which would serve as a reservoir for settlement expansion from the more densely exploited regions. Yet elsewhere there appeared changes between heavily and less heavily exploited regions, forming long term cycles of settlement expansion and regression. The nature of such local and re-

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Regional interaction holds a great promise for future research (Kristiansen 1982) as demonstrated in the Ystad project (Berglund 1991, ch. 5 and 6).

I propose then that the major components in the Göransson hypotheses, although highly inspiring, do not stand up against the archaeological evidence. Instead they tend to support Iversen's original hypotheses, as modified by the recent research referred to above. What is needed, therefore, is a closer theoretical and methodological integration of archaeological and palaeobotanical work in a long term perspective, as excellently demonstrated in the Ystad project (Berglund 1991).

References
— 1991. The cultural landscape during 6000 years in southern Sweden—the Ystad Project. Ecological Bulletins 41.
Bebyggelseutveckling i Spånga och på Åland


Jag har tidigare publicerat en recension av Biuws avhandling i Bebyggelsehistorisk Tidskrift nr 24 men jag skulle här vilja utveckla några av recensionens synpunkter när det gäller bygränserna på ett mer konkret sätt eftersom jag anser att Biuws slutsatser om gränserna i området inte är helt berättigade medan å andra sidan materialet som står till buds kan ge underlag för en del andra slutsatser.

Häradskartan är den yngsta kartan med bevarade bygränser över undersökningsområdet och dessa gränser används genomgående