SCIENTIFIC COMMENTS AND COMMUNICATIONS

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COMMENTS TO THE PAPER:

»On Vohtenkellarinsuo, a bog in Paimio, SW Finland with a cultural origin» by Irmeli Vuorela, Bull. Geol. Soc. Finland 55, 57—66.

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With increasing applications of palynological data in the interpretation of agricultural activity, the complexity of the technique, its limitations and weaknesses should be discussed. As I shall attempt to demonstrate below, the results must be carefully examined, because as the literature shows, generalized deductions have a tendency to be established as facts rather than conjectures. This cannot be useful for future research, especially in the context of archaeopalynology, as most users of cross-disciplinary data are not experts, with the ability to evaluate the given data.

It is my view that Vuorela's conclusions with respect to the events of the formation of the bog are premature; they may well be correct, but they are insufficiently documented and hence somewhat uncritical. The inferential bias — stated in the introduction of the paper — in favour of human influence without unambiguous supporting evidence or, just as essential, without accounting for alternative »natural» interpretations — must be guarded against.

My objections are threefold. First, my criticism concerns methodological considerations. Unfortunately, there is no precise model for deducing patterns of long-distance transport and related phenomena, nor for distinguishing between the local and regional, or air-borne and water-borne components of the incoming pollen. Vuorela writes that since the study site »is located in a forested area 1 km from the biggest fields in the principal river valley, the present pollen data mainly reflect human activity in the area and its influence on the origin and development of the bog itself and its surroundings.» The forested area around the study site may have varied considerably in the past, and this fact raises several questions.

The extent of hypothesized woodland clearance around A.D. 300 provides an assumption of the size and change of the pollen catchment area. How far did the fire extend? (Vuorela, op. cit. p. 63 »a wide area»). What was the composition of the forest? Were there any trees left after the fire between the river valley (the

seashore) and the study site? Or was there more or less open landscape from the west to the southwest with prevailing winds towards upland environments? In the latter case, the major source of pollen (this especially affects wind transport of herb and shrub pollen) must have been regional pollen rain. However, the assumption that changes in the abundances of preserved pollen reflect abundance changes in the plant populations is not valid, because differences in the pollen production rates of a plant (formation or removal of the tree-canopy layer) cannot be distinguished from pollen data presented as percentages of the total pollen count. Although this gives a general picture of the major habitat, the validity of the indicator species is questionable.

My second criticism is of ecological factors. The author could have stated more clearly that because of the topography, the retreat of the sea closest to the bog was slow around 600 B.C., and that a relatively broad and shallow bay (breadth 2.0—2.8 km) (at 15 m asl) at a distance of 1000 m from the study site, with reed swamp vegetation, may suggest the presence of ecotonal areas with unstable vegetation succession (cf. Groenman—van Waateringe 1968), instead of her stating (without any reference) that the distance to the shoreline at that time gives »no reason to interpret the above apophytes as part of the sea-shore vegetation». At what distance or at what time would a site like Vohtenkellarinsuo be out of the range of seashore influence? I do not know and I suspect the author does not know either. Relevant experiments are required on the topic.

Vuorela states that »the profile reflects *continuous* human interference from the Kiukainen culture (4000—3300 B.P.) onwards», though »no ash layer or *other* clear evidence of human activity can be detected». Fragments of charcoal are present almost universally in the peat and soil profiles of coniferous forests. Microscopic charcoal occurs abundantly in pollen profiles, and several studies have verified its validity as an indicator of past fires (see Huttunen 1980 and references therein). But, the presence of charcoal (either microscopic or macroscopic) cannot be taken as proof of intentional clearance without other documented proof.

Studies in the 1920s and 1930s by Lukkala (*e.g.* 1922, 1933) convincingly demonstrated the role of forest fire in paludification and its close relationship with the impoverishment of the vegetation type and hence *Sphagnum* bog formation, not least on young coastal soils recently emerged from the sea. He suggests that in nutrient-poor vegetation, burning resulted in still more oligotrophic types, for example: sedge pine fen → cottongrass pine fen (bog), → *Sphagnum fuscum* pine bog. In more mesotrophic and eutrophic mires, fire did not accelerate the formation of *Sphagnum* peat as it did on poorer sites, but maintained the original composition of species. Numerous other investigations have shown that »natural» forest fires were often an important factor initiating peat formation, *i.e.* paludification of mineral soil sites in northern Europe (Haglund 1908; Auer 1921; Huikari 1956; Salmi 1959; Koponen & Nuorteva 1973).

Thus, the linking of the two phenomena *i.e.* the fire and the beginning of cultivation, at c. A.D. 300, calls for independent, not circumventional, evidence. A layer of ash or carbonized wood remains is not proof of cultural origin alone any more than sporadic pollen grains of Cerealia are inevitably from *local* cultivation.

The hydrosere of Vohtenkellarinsuo was largely regulated by small streams flowing downhill through the basin. Moist reed swamp seems to have been the dominant vegetational stage in moister areas on a long time scale (for instance 2500 years), and waterlogged flooded areas could easily have formed after fire(s). Thus a combination of edaphic and hydrological change, species migration, competition and fire determined vegetational changes throughout the sequence. *Sphagnum* started to grow gradually after the fire, or according to Glückert

(1976, 1977), even before it, at least in small areas. To link the events with human activity requires more appropriate indicators independent of the pollen record.

The author uses the term »local», but she does not base her usage of this areal expression on any criteria. In her study the bulk of the biotic evidence — whole pollen assemblages — may show changes in an indefinable source area in the reconstruction of the past environment. An indicator species approach with individual taxa can only be used by species »with a well-defined narrow ecological tolerance not changed in the long run» (Birks and Birks 1980).

My third point concerns the more general question of documentation of pollen identification and, thus the lack of formalization within the paleoecological interpretation. If and when the identification is not documented (by a reference or note) (see *e.g.* Berglund 1979) for the reader, there is not much point in expanding conclusions on the ecology of many problematic species of plants. Vuorela presents no arguments regarding her pollen types such as *Rumex, Polygonum,* Cerealia (palynologically c. 20 possible species) and Cruciferae (c. 40 genera). Identification of pollen to species level is likely to remain impossible for some plant families. It should not be forgotten that »Cerealia» may include other grasses besides cereals (*cf.* Andersen 1979).

In the present context macrofossil evidence of the »local» cultural community is thus highly desirable. Upwelling air currents are evidently capable of carrying pollen grains that ordinarily travel short distances, including cereals, to high altitudes (Aartolahti & Kulmala 1969, O'Sullivan 1976). Leaving aside the question of seashore vegetation, *local* cereal cultivation may thus still be wrongly deduced from the pollen diagram in some circumstances. The resolution apparent in peat (or lake) profiles together with temporal and spatial inference, discussed in detail by Edwards (1979) and others, should be dealt with. The limitations of pollen analysis should be emphasized.

The author should have commented on the differences in the stratigraphy of the profile in relation to that published by Glückert (1976).

In summary, I counted that the suggestion that the bog formed through human impact rather than hydrological and pedological change, with pollen analysis as the only tool of explanation, is not documented in Vuorela's paper and that her conclusions are uncritical; the available evidence clearly places hydrological and pedological factors as an equal possibility. The pollen data should not be over-interpreted, and the complexity of these methods should be emphasized by reference to the use of replicated data. It would be a most valuable addition if the pollen analytical data were supplemented by chemical and other palaeoenvironmental information.

In the above I have attempted to show with some examples — more could be given — how the uncritical use of the palynological method has resulted in unverified conclusions that do not stand up to criticism of the source. Thus, as stated in Birks and Birks (1980), »a paleoecologist uses evidence from as wide a variety of sources as possible, both biotic and abiotic, to provide the fullest and most coherent reconstruction of past environments».

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