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IN WHICH LANGUAGE DID THE ANTREA FISHERMAN CURSE WHEN HE LOST HIS TWINE?

Since the 1970s Finnish archaeology has become increasingly isolated from the rest of Scandinavian archaeology. A growing proportion of its publications are in Finnish, fewer and fewer Finnish scholars are seen at Scandinavian seminars, few guest lecturers and researchers go in or out of Finland . . . The set of barriers bumped into by Milton G. Nunez in his attempt to understand Soviet archaeology is apparent also to a Swede trying to understand Finnish archaeology. Thus, a number of the ideas and arguments put forward by Nunez is impossible for me to take a standpoint to. Instead, I will try to elucidate the types of ideas used by him, and likewise try to guide the reader to some of the literature discussing related ideas. In the process my own viewpoints will, at least implicitly, be clarified.

After 40 or more years of self-inflicted and ashamed silence (cf. Hagen 1986) North-West-European archaeology again discusses ethnicity, language groups, the origins of nations, e.g. Saami ethnicity (Odner 1983, 1985, Næss 1985) and Indo-European origins (Anthony 1986, Kristiansen 1987, Renfrew 1987). The emergence of symbolic and structural archaeology has contributed to this state of affairs. Little of this discussion is seen in the article by Nunez, who instead presents an interesting attempt to build explicit models in the spirit of processual archaeology ("New Archaeology") on a foundation of traditional East-European, and Finnish, ethno-historical archaeology.

Nunez focuses on two problems and constructs two models. The first is on the settling of Finland after the retreat of the ice sheet. The second is on the introduction of Finno-Ugrian languages into Finland. As the title of his article suggests – "A model for the early settlement of Finland" –

the two processes turn out to be the one and same. This is the exciting and provocative core of the article.

To give some perspective to the boldness – or stupidity (?) – of the models by Nunez, it can be mentioned that glottochronology, although controversial (Ehret 1988), suggests from the rate of gradual linguistic change that languages c. 10 000 years apart display only haphazardous similarities. Accordingly, the labelling of languages 10 000 years old, as done by Nunez, with a historically known term is straightforwardly meaningless. So far, the discussion in terms of historically known language groups (or families) has tended not to reach beyond c. 6000 years ago. Examples are the discussion of Indo-European origins in terms of the first wave of migrating farmers by Colin Renfrew (1987) and the split of the common early Mesolithic European breeding population into several breeding populations at about the same time, discussed by Raymond R. Newell and Trinetta S. Constandse-Westerman (1986, 1988).

However, this is of little relevance to Nunez. To him it is of importance to stress that no immigrations of Finno-Ugrian peoples have taken place after the first settling of Finland after the retreat of the ice sheet some 10 000 years ago. This is an extreme standpoint within the Finnish debate, where a number of immigrations have been suggested in the time-span c. 6000–1500 years ago.

Nunez's model of settling of the virgin ice-free areas is an outline of bands of foragers in a step-wise, but slow, process extending over dozens of generations, moving to the north in order to keep to their traditional resources in an ecologically changing world. The model has much in common with the Ammerman & Cavalli-Sforza wave of advance model (Renfrew 1976, 1987), although in comparison with the latter it contains no factor of demographic growth. It has perhaps

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still more in common with my own model for the settling of the subarctic coast of Norway during the Late Weichselian (Welinder 1981). Compared with this model, it is less specific as concerns ecology and thus perhaps neglects to differentiate between terrestrial and marine resources, which is astonishing from a study of Figs. 2, 4–5 in Nunez's article. These maps display Late Weichselian and Preboreal Finland as an archipelago and a narrow coastal area with most Preboreal and Early Boreal sites situated along the coast. Perhaps it is a corollary of this that the Nunez model does not discuss actual settlement sites and exploited territories nor does it distinguish between task camps and base camps. The model seems general and abstract in relation to the activities of living humans beings. It is hard to guess, if this is due to the scanty Finnish Preboreal data, the lack of efficiency with which Nunez has discussed these data, or the lack of efficiency with which he has discussed the ethnography of subarctic foragers. In this environment people display remarkable mobility and flexibility. The work by Erica Engelstad on the Late Stone Age of subarctic Norway may be of interest as an introduction (1984, 1985).

The model chosen by Nunez on a successive northward budding off of new bands of foragers implies the emergence and existence of a network of social contacts in a vast North-East European and North-West Asian area. Such a network may certainly involve the exchange of artifacts, mates, and ideas over enormous distances. Nunez cites a highly relevant Canadian example from the ethnographic record. From quite another ecological environment the mobility of individuals between bands among the Kalahari Bushmen can be noted (Lee et al. 1968). They walk between camps to have a chat with relatives and friends, to take part in rituals and feasts, or just to live somewhere else for a change. On a more general level, networks within band societies have been studied with simulation experiments by H.M. Wobst and from the ethnographic record applied to the European Mesolithic by R.R. Newell (Newell 1986 with refs.).

The provocative core of Nunez's second model is that all members of this social network spoke the same Proto-Uralian language some 10 000 or 12 000 years ago. The model furthermore contains a description of how this proto-language split into several languages and underwent changes through time due to long distances between people, the establishing of environmental and social barriers, and in various contacts with people speaking other languages. Much more

explicit suggestions on how processes of this kind may be modelled have been put forward by Knut Odner (in Næss 1985), but Nunez is on the right track when he hints at social processes at work in linguistic change rather than migrations, which have dominated the Finnish debate until recently as I know it from archaeological publications printed in congress languages (cf. Sammallahti in Kristiansen 1987). Perhaps it is even wise to construct indifferent models at this stage of trying to combine prehistoric, i.e., mute, archaeology with linguistics.

An amazing development within modern archaeology is the growing optimistic view that it will become possible to understand the immense variety of human material culture as a meaningfully constituted world of symbols, a code for human interaction, but also a means for conceptualizing the human world by the human mind. Material culture and speech are different human characteristics, perhaps contrary to each other, but nevertheless there is a growing hope that archaeology and historical linguistics eventually will form a common view of the early history of mankind (Renfrew 1987). This will hopefully be possible when archaeology and historical linguistics have found some common basic view of how humans, groups of humans, and societies are structured and how they change along the time-axis. In my view, this common basis will not be the linguistic family-tree model of languages adopted by Nunez and referred to by him as the model generally accepted among finnougrists.

The tree model states that a group of related languages has a common ancestral proto-language in a homeland, definable in space and time. From this homeland the language has spread in space, and along the time-axis it has successively split into several diverging languages corresponding to the branches of a tree. Thus, all the Uralian languages and dialects of today are derived from a single language once spoken in a homeland situated somewhere in Eastern Europe. Many of the branches of the trees, also of the Finno-Ugrian tree, are dead ends corresponding to languages and dialects not spoken and today known only from written historical sources.

The latter point is of importance. It must be inferred that dozens of Finno-Ugrian languages and dialects today are both extinct and unknown. Supposedly this is what Pekka Sammallahti had in mind, when he proposed that the tree should be a mangrove tree (Sammallahti in Kristiansen 1987). This certainly is more in

agreement with the archaeological record: there is immense variation in material culture, be it ornaments, grave-rituals, or ways of depositing garbage. All prehistoric humans cannot be moulded as a small number of discrete groups of stereotyped humans speaking a small number of proto-languages. Certainly the prehistoric linguistic variation was as extensive as the variation in material culture and as extensive as the linguistic variation among band and tribe societies of the ethnographic and historical record.

In the first chapters of his book on Indo-European origins Colin Renfrew (1987) has summarized a number of ideas on linguistic change that are not derived from the family-tree model. By abandoning the tree model and its inherent view of language change by migrations modern linguistics have managed to model language change as a dynamic process embedded in social interaction. In these pages Nunez and other archaeologists will find more interesting ideas on languages and language change among prehistoric people than those inherent in the tree model with its simplistic view of human variation. It must, however, be admitted that not even Colin Renfrew in the latter part of his book has managed to grasp these modern ideas on speech and languages as social phenomena. In the explicit processual models on language change presented by him, major population movements play an astonishingly prominent role. By the way, to an expert linguist the ideas are perhaps not modern, but to an archaeologist brought up with the family-tree model they are refreshing. The impressive textbook "Die uralischen Sprachen und Literaturen (Hajdú et al. 1987) displays how some ten years ago the family-tree model of linguistic development was challenged by new ideas specifically within the field of Finno-Ugrian linguistics. The continuation of the story is found in Veenker (1985).

Milton G. Nunez has managed to combine a processual model of population movements into a virgin area with the family-tree model of historical linguistics. He has in the "Concluding remarks" of his article identified himself as an "*advocatus diaboli*" presumably because of

- (1) his suggestion of the presence of a Proto-Finno-Ugrian language from the very beginning of the prehistory of Finland, and
- (2) because of his statement that race, culture, and language often show positive correlations.

In my view Nunez's article instead actualises the following two points:

- (1) When are unspecific, processual models of interest in understanding the archaeological record and human variation?
- (2) Is the family-tree model of historical linguistics of interest to archaeology?

It is characteristic for processual archaeology, sometimes also necessary and even suitable, to adopt simple – and simplistic – models.

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