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MORE ON FINLAND'S SETTLING MODEL

I thank the commentators as well as those who in one way or another have shown interest in my model (1987). Their essentially positive response has been stimulating and offers me the opportunity of clarifying certain points which have been apparently misunderstood. But before dealing with specific questions, it may be useful to go into the background of the model and the purpose and spirit with which it was written.

The model of Finland's settling developed gradually over a 15-year period. The basic environmental concepts took form quite early (1972), and the mechanism of human expansion into deglaciated territories followed as a by-product shortly after. But it was not until seven years later that the model was resuscitated and the implications of the split-and-short-distance-spread process taken into account: Common traditions, marriage and kinship ties, trade and, of course, language would help preserving contacts between splitted groups. Furthermore, all these would lay the foundations for far-reaching interaction patterns, which in turn would help preserve speech intelligibility over vast areas. Around the same time it was observed that the vast region once covered by the Winsconsin glaciation in North America held only three major language groups at the time of European contact: Eskimo-Aleut, Athapaskan-Eyak and Algonkian. And moreover, the configuration of these territories suggested that each group may have spread ultimately from single areas that were ice-free towards the end of the Winsconsin maximum – a feature paralleled, albeit weakly, by Uralic languages in Eurasia.

After encouragement from semipopular articles on the "Roots" symposium (Itkonen 1980; Meinander 1980; Nevanlinna 1980; Jutikkala 1980), the language corollary finally was incorporated to the model in 1981 (Nunez 1984). It was therefore dissapointing to read the symposium proceedings in 1985. The positivistic attitude of 1980 had been replaced by detached self-centered views within each discipline. As M. Schauman-Lönnqvist (1985:133) put it, the proceedings are "a collection of essays where each author, often irrespective of other researchers' works, presents isolated results within his own field".

This prompted my call for an interdisciplinary dialogue (Nunez 1987). At the time there was the choice of reproducing the relevant chapters of my thesis (1984) or completely writing a new paper that took into account later works. I opted for the first alternative because the model had been developed independently and there did not seem to be contesting issues in more recent literature. The resulting article was a slightly modified synthesis of two chapters of my thesis that proposed a simple general model as a base for discussion and testing.

REPLY TO Dr. P. M. DOLUKHANOV

It was gratifying to read Dolukhanov's positive comments and I have little to add in reply. He is certainly right in stressing the importance of theoretical issues, and I could not agree more with the statement that palaeoethnical reconstructions may be successful on the basis of high ranking empirical units only. This was precisely what I had in mind with my model. Finally, it was certainly encouraging to learn that the model can be successfully tested in the North European plain, particularly when the comment comes from an expert in the region (cf. Dolukhanov 1979, 1986a, 1986b).

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REPLY TO Prof. HEIKKI LESKINEN

Coming from a linguist, the points raised by Leskinen are of special interest. I am pleased to learn that early dates may be acceptable to finnougrists if corresponding changes take place in Indo-European chronology. In recent years the traditional dates of Indo-European prehistory have been increasingly questioned (e.g. László 1985; Dolukhanov 1986a; Renfrew 1987; Sherrat & Sherrat 1988). Although so far the objections have been raised mainly by archaeologists, it is only a matter of time before we see linguist-made revisions.

It should be stressed, however, that my model is not concerned with the various Uralian (U) or Finnougrian (FU) stages and their dates. The estimate of 10 000–6 000 bc for the Samoyed-Finnougrian separation is merely a suggestion based on palaeoenvironmental grounds. It can have taken place later, though probably not after 3 500 bc. Similarly, it is irrelevant to the model when and whence the Baltic loans were introduced to Finnish and Lapp. The Baltogermanic-Corded ware association was accepted by linguists and archaeologists, who felt they were roughly contemporaneous. Later radiocarbon determinations pushed Corded ware dates some centuries back, but many linguists have not made a corresponding adjustment to the Proto-Baltic stage despite pressure from the archaeologists. At any rate, the most likely archaeological event for the introduction of Baltic loans in Finland is undoubtedly the Corded ware episode. It would not only explain their presence in Finnish and Lapp and the lack of Finnic terms in Baltic languages, but the Corded ware connection would also account for the existence of other Baltic loans in Central Russian Finnic languages (Serebrennikov 1957; Sammallahti 1984; Suhonen 1984).

> Germanic speakers [can be traced] to the Corded-Battle axe complex in northwestern Europe; the Baltic speakers to the East Baltic-Central Russian (Fatjanovo) Battle-axe complex... (Gimbutas 1975:292).

I do not claim that the peoples occupying the Baltic-Ural region spoke a uniform language (see also reply to Welinder). What I had in mind was a group of related dialects/languages that were mutually intelligible to some extent and gave eventually rise to the various U/FU languages spoken in that very region in historical times (cf. Hajdú 1987). But let us not underestimate the territories covered by single languages/language families in subarctic-arctic environments. The vast region comprising Canada and Alaska (c. 12 000 000 km^2) was occupied by only three major language groups which included c. 80 languages – a mean density of one per c. 150 000 km^2 (Voegelin & Voegelin 1966, 1977; Rogers 1985). Mutually intelligible Inuit dialects, for example, were spoken from Alaska to Greenland (Rasmussen 1927; Swadesh & Marsh 1951). Or focusing in the northern Athapaskan family, we see that it extended across a territory of over 3 000 000 km^2 where some languages covered areas over 300 000 km^2. Moreover, languages like Ingalik and Chipewyan, separated by a distance of c. 2 000 km, share over 70 % cognates (Fowler 1976). Incidently, the Northern Athapaskan territory roughly corresponds in size to the Baltic-Ural region.

As Leskinen points out, the possible existence of an ancient non-Uralian substratum does not necessarily exclude presence of U/FU elements among the first inhabitants of Finland. In fact, it is quite possible that non-Uralian speakers would have spread into the region in the same manner as U-speaking groups as the ice receded. As indicated in my article, bearers of two apparently independent archaeological complexes seem to converge towards Finland at the end of the Ice Age from the east and southeast: North Russian (Arctic Palaeolithic) and Kunda (Nunez 1987:7–10; cf. Meinander 1984). Regardless of when and whence each language may have come, since one would expect minority languages to be eventually assimilated, the fact that U/FU languages survived would suggest that they constituted the linguistic majority.

Leskinen's gesture of a compromise model is well taken. But I must question the insistence in placing the early stages of U/FU linguistic development elsewhere to bring the later ones to Finland through migrations. Linguists alone cannot be held responsible for this deep-rooted migrationist view, archaeologists are at least as much to blame. But archaeology aside, it is difficult to explain "several waves of migrations directed towards Finland and the Baltic" from central Russia around 3 000 bc. Why? What would have made the country so attractive? Or if we think of a gradual budding off process, what was the reason for such population surplus? And what about the local inhabitants? Why would they let their territory be taken over by peoples who had apparently the same technological level? Or if it was a question of overwhelming numbers, can such surge of extra population be explained sa-

tisfactorily? Or if the population movement was a minor one, would the local inhabitants have changed their language to that of a minority of newcomers with comparable technological level? These and many more questions may be asked. It would be much simpler if at the end of the Ice Age the Baltic-Ural region would have been colonized by peoples speaking languages ancestral to the U/FU languages that came to be spoken in the area in historic times.

Although the possibility of migrations taking place in the past cannot be excluded, it seems to me that a migration that would cause the replacement of a non-FU language by a FU one in prehistoric Finland should be archaeologically evident. And this is precisely the problem. Despite general opinion, there are difficulties with interpreting the Typical Comb ceramic phase (Ka2, c. 3 300–2 800 bc) as the result of a migration. There are few, if any, traits that can be regarded as intrusive and traceable to a distinct outer source (cf. Wiley et al. 1956; Rouse 1958; Trigger 1968). Changes in pottery traits – if foreign origins must be seeked – are best explained as the product of exogamy with neighbouring areas: Ethnographic data indicates that in the great majority of the known primitive pottery societies the pot-makers are women (Murdock 1937; Nunez 1984).

Had there been a major migration from the Volga region, one would expect new subsistence and settlement patterns, and new tool forms relatable to the source area. Sealing, which was certainly not practiced in central Russia, continued to be an important subsistence activity according to faunal remains (Siiriäinen 1982; Edgren 1982; Nunez 1984, 1986a, 1986b). Intrusive forms such as ambers, flints, worm-like antropomorphic clay figurines and pottery styles can be explained as the result of increased contacts and trade – an exchange of amber, flint and women. As far as I can see there are no traits in Ka2 assemblages that could not be explained as local development under external influence. In fact, the apparently prosperous but short-lived Ka2 period is best explained as an intensification of contacts between Finland and the neighbouring areas. An event which would have developed more readily if, as assumed in my model, interaction patterns based on similar traditions, trade, kinship and marriage ties, and mutual speech intelligibility already existed in the area.

One event should be noted in connection with the Typical Comb ceramic phase which agrees with Leskinen's date for the spliting of Proto-Finnougrian. The unity reflected by the Ka2 phase in Finland begins to crack around 3 000 bc. After this date inland assemblages gradually acquire a distinct local character while soon after, around 2 800 bc, the Typical style gives way to the late style (Ka3) in the coastal zone. A parallel decline is also observable in the Pit-Comb ware complex of the Baltic-Ural region. All this may be interpreted as the result of pressure by Neolithic cultures from the mixed forest zone. On the other hand, a weakened divided region may have invited intrusion from neighbouring cultures (Nunez 1984, 1986a, 1986b, 1987).

I will conclude my reply by commenting that linguistic and archaeological data do not appear to be so far apart after all. Many of the "insuperable difficulties" mentioned by Leskinen stem from the association of archaeological and linguistic change with migrations. Personally I do not think we can get very far that way. But an interdisciplinary seminar would certainly be very useful and the idea is therefore welcome. In the mean time, however, I would like to ask if finnougrists could test their linguistic data within the non-migrationist scenario proposed by the model. I am particularly interested to hear whether or not that is possible from the linguistic viewpoint.

REPLY TO Dr. STIG WELINDER

There is much truth in Welinder's comment about the isolation of Finnish archaeology, though awareness of this very problem now seems to be stirring a counterreaction. I must point out, however, that after five years of publishing in non-Finnish languages, *Fennoscandia archaeologica* is hardly an appropriate forum for Welinder's complaints. Low congress attendance, on the other hand, depends on policy-bound economic support and not on archaeologists themselves. And for this reason, Welinder's comments are welcome: learning about the image we project abroad may have a redeeming effect on our decision makers.

It appears that Welinder has misunderstood my article. He is critical of certain issues, but his own comments show that his opinions do not differ significantly from mine. Moreover, by complaining about the absence of certain topics that, regardless of their importance, are irrelevant to the model, it is obvious that he has missed its very point and purpose. I will nevertheless do my best to clarify the issues in question.

I welcome the complementary list of related

literature published after the development of my model. And while speaking of publications, I would like to remind Welinder that the decades of "ashamed silence" about discussing ethnicity and languages did not affect Finnish publications (e.g. Brønsted 1952; Äyräpää 1953; Meinander 1954, 1959, 1967, 1969, 1973, 1977, 1980; Luho 1968; Salo 1969; Carpelan 1975, 1979), or the many west European archaeologists published or cited in the *Journal of Indo-European Studies*.

Let us now look into the subject of glottochronology, which was not mentioned in my article for reasons of space. According to lexicostatistics, after having been mutually isolated for a minimum of 11.7 centuries, two languages with common ancestry will show no more similarity than any pair of completely unrelated languages (Swadesh 1960). This sets of course a limit for the ability of the glottochronological method to detect any relationship between 11 000 year old 'sibling' languages, but it does not deny that two given languages may in fact have evolved from a single parent language 10 000–15 000 years ago. Since this is not the place to discuss the nearly 40 year old controversy about glottochronology, only those points relevant to my position will be touched.

Glottochronological calculations are based on the assumption that the compared languages have been isolated from each other for some time, and the results are supposed to represent the time elapsed since separation. But we are told by Swadesh himself (1960, 1972) that such estimates should be regarded as a minimum figure. Furthermore, my model postulates interaction amongst the groups speaking early Finnougrian languages in the Baltic-Ural region until at least the third millennium bc, which would imply a reduction of the rate of linguistic differentiation and, consequently, a glottochronological bias towards too young dates. Comparisons based on Swadesh's 100-word list have shown a 13–15 % cognate retention between Finnougrian and Samoyed languages (Hajdú 1975, 1987) indicating a minimum of 7 500–8 500 years since the separation of these groups (Swadesh 1960). Since this 5 500–6 500 BC date is to be regarded as a minimum, my suggestion of 6 000–10 000 bc date for the Finnougrian-Samoyed split was certainly not very far off. The linguists' Proto-Uralian stage would by definition precede this hypothetical linguistic separation.

I will assume that by "historically known labels" Welinder refers to protolanguages reconstructed from historically known related languages, even if Proto-Uralian (PU) and Proto-Finnougrian (PFU) have of course no historical reality. In fact, the main reason why discussions of such protolanguages seldom extend beyond 6 000 years is that the oldest known languages barely go back into the fourth millennium BC and, consequently, the protolanguage constructs from which they hypothetically evolved cannot be placed much earlier. As Renfrew (1987) pointed out, 40-year old glottochronology has little to do with this state of affairs, though it imposes limits to the time interval within which the method can be empirically tested. Incidently, it should be pointed out that, despite Welinder's comment to the contrary, Renfrew sets early Indo-European languages some 2 500 years earlier than the 6 000 year magic figure: "The hypothesis that early Indo-European languages were spoken as early as the seventh millennium BC in eastern Anatolia certainly also gains in plausibility in the light of recent work..." (Renfrew 1987:269; for more extreme positions see e.g. Kühn 1934 and Schwantes 1958).

But Welinder is certainly right in questioning my choice of terms to refer to the languages spoken in deglaciated territories around 13 000–6 000 bc. PU and PFU are well-defined linguistic terms and I have used them in a context which does not quite correspond to that of linguistic circles. As mentioned earlier a protolanguage is a hypothetical ancestral language reconstructed from a later group of related languages thought to have evolved from it; for obvious reasons, protolanguages are regarded as the stage immediately preceding the division into 'daughter' languages. When I state that PU or PFU were spoken in a given area and period I mean a group of mutually intelligible related languages that are ancestral to the known, and unknown, U/FU languages. The difference between this and the conventional linguistic concept of the terms is implicit in my model, and it is intimately related to the idea of sequential split and short-distance spread by human groups that continue to interact long after separation.

Perhaps more adequate terms should be sought. While writing my 1987 article I was partial to the prefix eo-; but a precedent has been recently introduced in connection with Indo-European languages:

> Moving further back in time, one can postulate an ancestral language or group of languages which ultimately gave rise to *PIE and its successors (and no doubt to others now extinct). Although beyond the limits of reconstruction, its existence is a logical necessity; and we may recognize its hypothetical status

by a double asterisk – hence **pre-proto-Indo-European (Sherrat & Sherrat 1988:588).

Personally I would have prefered the shorter Eo-Uralian and Eo-Finnougrian alternatives because Pre-Proto-Uralian could also be interpreted as languages that preceded PU in a given area regardless of their relationship to this language group. Eo-Uralian and Eo-Finnougrian, on the other hand, clearly refer only to languages directly ancestral to Proto-Uralian. Since, as the Sherrats point out, there is a need for a stage preceding linguistic protolanguage constructs, it may be wise to adopt this concept in Uralian prehistory as well.

It is irrelevant whether or not speakers of a Finnougrian language migrated to Finland after the initial colonization. What I maintain is that the archaeological data do not indicate any migration event of a magnitude that could have brought about a non-FU to FU linguistic change. Since Finnish and Lapp were spoken in Finland at the beginning of the historic period, it follows that the first Mesolithic settlers would have spoken languages/dialects ancestral to the modern ones. Consequently, if there was archaeologically ambiguous migrations of FU speakers to Finland after the seventh millennium bc, the newcomers would have met speakers of related languages. It should be added that my ideas are neither new nor unique, similar thoughts were entertained long ago by Ailio (1909:112–113, 1912, 1917, 1931).

I am surprized at Welinder's complaint about the failure to "differentiate between terrestrial and marine resources" allegedly obvious from my palaeogeographical maps for 13 000–9 000, 8 000–7 000 and 7 500–6 500 bc (1987 Figs.2,4,5). What marine resources? There was obviously no marine environments in the study area during the glacial period, even if rapid evaporation of certain proglacial lakes may have temporarily created saline environments during deglaciation. Most of the southern Baltic basin, including the Gulf of Finland, was glaciated until around 10 000 bc, after which the basin was partly filled by proglacial lake(s) that gradually grew in size as the icesheet retreated (1987 Figs.1,2). By 9 000 bc the ice border was static at the Salpausselkä zone in southern Finland, and the Baltic basin held an immense proglacial lake, the Baltic Ice Lake, though most of the Botnian Gulf remained glaciated until the seventh millennium bc (1987 Figs.2,4,5). Admittedly, there was the Yoldia Sea stage around 8 200–7 500 bc (1987 Fig.4) but, according to sediment data, the marine nature of this phase is rather dubious on the Finnish side of the Baltic (Eronen 1974; Alhonen 1980). This is not surprizing since the Baltic basin had experienced a c. 30 m regression in the century preceding the Yoldia phase (Donner 1982; Synge 1982). The intense erosional processes that ensued in the newly emerged areas (cf. Nunez & Alhonen 1974) and the abundant meltwater from the once again retreating ice border (1987 Fig.4,5) would have certainly hindered, if not impeded, marine influence from reaching Finnish shores.

In any event, it is not until after the onset of the Ancylus Lake stage, that man reaches Finnish soil – it could not have been much earlier since practically all Finland lay then under ice and water (Nunez 1987 Fig.4). This 'great lake' conditions (cf. the US-Canada border) lasted in Finland until around 6 500–6 000 bc, after which a very tenuous marine influence was felt in Finnish shores: the slightly brackish Mastogloia phase around 6 000–5 500 bc (Eronen 1974; Alhonen et al. 1978; Hyvärinen 1982). After this brackish episode true marine environments developed in the form of the Litorina Sea; and it is not until after this event that we can observe a rise of maritime adaptation in Finland (Nunez 1984, 1986a, 1986b, 1987). For these reasons I only speak of exploitation of aquatic environments before the onset of the Litorina Sea (1987:7–8).

There are indeed great difficulties in reconstructing the subsistence and settlement patterns of the early Finnish Mesolithic (c. 7 500–6 500 bc): few and small sites, minimal archaeological faunas, and unstable environmental conditions. Although the frail fish and fowl bones are certainly underrepresented, there are good indications that these resources were exploited at an early date. The oldest known find, the c. 7 300 bc Antrea net, points to fishing, and the associated artefacts of elk and swan bone to the exploitation of both land mammals and water fowl. Similarly, the slightly younger icepick of elk bone from Kirkkonummi points to land hunt and winter fishing. Ringed seals, a relic from the ocean-connected Yoldia stage, were also exploited according to faunal remains. However, the proportion of seals in Mesolithic refuse is less than one fourth of that in the subsequent early Comb ceramic period (Ka1). An increase in the importance of sea mammals may be correlated with the onset of the Litorina Sea stage and its related marine fauna (Siiriäinen 1982; Nunez 1984, 1986a, 1986b, 1987), but this does not entirely

explain the extremely low Preceramic figures.

It can be argued that the lack of Mesolithic sealing traditions may be responsible for the apparent neglect of the ringed seal potential. But could the reason be the resource itself? The unstable shoreline may have resulted in unfavourable conditions during the Ancylus period. In addition to glacier-melt induced erosion, a transgressive event up to c. 10 m took place in southern Finland (the only ice and water free portion of the country) during c. 9 500–9 000 bc and a 14–4 m regression followed during the next 200–500 years (Eronen 1976; Saarnisto 1981; Eronen & Haila 1982; Glückert & Ristaniemi 1982). These events could have lowered the productivity of the Finnish litoral, affecting in turn the density of the ringed seal population until the stabilization of shore displacement after c. 6 500 bc. These are a few of the many complex problems concerning the reconstruction of subsistence and settlement patterns in Finland.

But those difficulties are not the only reason for merely outlining the main subsistence-related developments in my 1987 article. Although subsistence and settlement patterns play an important role in prehistoric reconstruction, they are at this stage of little relevance to the model. Welinder seems to forget that we are dealing with a general model proposed as a basis for interdisciplinary dialogue, not the answer to all the problems of Finnish prehistory. It is in fact meant to be abstract, general and simple so it may be easily modified, or refuted, in the light of anthropological, archaeological, ethnohistorical, linguistic or palaeoenvironmental data. As Welinder himself admits sometimes it is necessary to adopt simple/simplistic models.

I could not agree more with Welinder: if a common model is ever found by archaeologists and linguists, it will not be the traditional family tree. But what gave him the impression that I advocate that model? That it was not explicitly criticized in my article? Frankly, I do not find it necessary or wise to attack the family tree. Let us have a look at it – a strange upside-down tree that has grown backwards, from its younger branches to the roots. The first represent historically known languages and, consequently, have a real basis. These concrete younger branches 'float' over the rest of the tree, which is a hypothetical construction based on a series of accepted assumptions and somewhat circular arguments. I doubt that many linguists believe that languages evolve in the form prescribed by the tradional family tree model. But despite its shortcomings the tree serves a purpose as a basic framework for historical linguistics that illustrates the similarity, if not necessarily genetical relationship, amongst the various U/FU languages; e.g. Finnish is closer to Estonian than to Lapp. Why then chop the tree down? Even the traditional tree-derived linguistic sequence can be adapted to my model. Once this is done, however, the restrictions imposed by the tree aproach vanish, leaving the door open for the creation and testing of numerous alternative non-tree scenarios.

It seems to me that Welinder ought to read both Renfrew's (1987) book and my 1987 article again. Perhaps if he does it thoroughly, he may find that our models have actually much in common. While we are at it, maybe Hajdú (1987) should be added to the list, since to state that he "challenged" the family tree model is sheer exageration. He simply proposes a more realistic, though still tree-like approach (cf. Hajdú 1987:313):

> Trotz dieser Schwierigkeiten kann das uralische Stammbaum-Diagramm zur Orienterung beibehalten werden, weil es im grossen und ganzen einem ausreichenden Einblick in die Entstehungsphasen der uralischen Sprachen und die urgeschichtlichen Zeitalter gewähr, wenn auch die Entstehung der Sprachen und die urgeschichtlichen (ethnohistorischen und ethnogenetischen) Zeitalter einander eben typischerweise nich adäquat decken. Das Stammbaum-Diagram rechnet weiterhin auch nicht damit, dass auch PU und die folgenden Grundsprachen ihre Mundarten hatten, oder z.B. auch nicht mit der Möglichkeit, dass die Grundsprache eigentlich nichts anderes war als eine areal zusammenhängende Kette von einander nahestehenden Sprachen und Sprachinseln, aus welcher sich in der Endreihe aufgezählten heutigen Sprachen teils durch direktes Ausbrechen, teils – ja, setzen wir hinzu: meist – in einem langere oder kürzere Zeit dauernden bzw. einmal doch abreissenden Prozess gegenseitiger Kontakte, bei gleichzeitigen Kontakten auch zu anderen Sprachen, zu ihrer heutigen Gestalt herausformten. (Hajdú 1987:310).

True challenges to the traditional family tree theory are nearly as old as the tree itself (e.g. Schmidt 1872; Trubetzkoy 1939; Demoule 1980). But although today most linguists are aware of the flaws in the traditional family tree theory, there is still a general acceptance of some sort of tree-like genealogy among U/FU languages. This modern modified tree view, as we can see the above quotation, places major importance in the phenomena of dialect differentiation and population contacts, and it is in fact very compatible

with my model (cf. Aalto 1965; Swadesh 1972; Hajdú 1975:47–50, 1987:306–315; Janhunen 1981; Leskinen 1989).

After the discussion above it is easy to appreciate the beauty of Sammallahti's mangrove metaphor; even though the term tree is somewhat misleading – I never saw a solitary mangrove plant during my 16 years in mangrove country. Moreover, the mangrove (forest) is very appropriate in connection with my model: After the beaching of a seed, mangrove gradually spreads and takes over a broad coastal band forming a tight vegetation net, where every plant is somehow related to the others. Not only because they all ultimately descend from the same seed, but also because each plant is linked (through the roots) by both ascent and descent to its immediate neighbours. Another characteristic is the hardiness of mangrove vegetation, which is seldom affected by the strongest hurricanes.

What words the Antreans swore

I enjoyed Welinder's joke which, like this heading, is apparently paraphrased from the "what song the sirens sang" of Sir Thomas Browne 330 years ago (in Renfrew 1987:2). On the basis of my model the Antreans would have spoken an early dialect ancestral to Finnic languages. But the question that arises is was swearing known in Mesolithic Finland? Taking the linguistic evidence literally (cf. Pigott 1950:246; Renfrew 1987:81) we would have to conclude that the awful habit was introduced in Finland by speakers of a Baltic language much later – possibly by the Corded ware people in the third millennium bc. This is supported by the most common Finnish swear-word, *perkele* (devil, demon), which is actually a Baltic loan (Lith. *Perkúnas,* Latv. *Perkon*) (Gimbutas 1973).

Seriously speaking, it should be pointed out that the Antrea find consisted of the remains of a 30×1.5 m seine-net and several artefacts of stone and bone. If fishing twine was also involved it is not known. The net was made of willow bast cord with stone sinkers and pine bark floats (Pälsi 1915). It has been suggested that the artefacts sank to the bottom of an Ancylus Lake bay when the boat/canoe that contained them capsized. The occupant(s) would have recovered the vessel and reached shore safely. An alternate possibility may be advanced on the basis of the ages of 7280±210 and 7369±140 bc (Hel-269, 1303) yielded by two bark floats. Since these dates fall within the period of Ancylus trangression in the area (c. 7 500–7 000 bc) the artefacts may have lain at an abandoned beach site. In which case, incidently, there might have been no swearing at all.

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Addendum

I would like to mention two recent publications not available to me when writing my reply. I am referring to Greenberg's book *Language in the Americas* (Stanford, 1987) and to an article published by Cavalli-Sforza et al. last year *(Proceedings of the Academy of Science of the U.S.A* 85: 6002-6006) which, though controversial, imply that languages may have a greater chronological depth than traditional historical linguistics seem prepared to accept: "Genetic data indicate that most human linguistic phyla known today must have arisen approximately 25,000 to 8,000 years ago" (Cavalli-Sforza et al. *Science* 244:1129, 1989).