Priit Ligi

"ACTIVE SLAVS" AND "PASSIVE FINNS": A REPLY

First of all, I would like to thank Leo S. Klejn, Gleb S. Lebedev, Alexander Panchenko, Nicholas Petrov, Adrian Selin and Bruce G. Trigger for their interesting comments, which raise many issues. I agree with them on many points, particularly concerning the theoretical aspects of studies in ethnogenesis, but to others I must make objections. I must also draw attention to two mistakes in my text (Ligi 1993a): on page 34 (line 15, left column) the wording should read: 'were at least one hundred years younger'; on page 35 I erroneously mention arrowheads in this context.

The purpose of my article was to draw attention to the ideological misuse of the paradigm of large-scale Slavic colonization in Northwest Russia, and to point to the inherent methodological weaknesses - not to discuss theories which G. Lebedev himself calls 'marginal'. As for the methodology concerned, Lebedev (e.g. 1977; 1981; 1982; Bulkin et al. 1978) is one of the few to have expressed similar views, and his studies have had a strong impact on me. Since his theoretical article published in 1981, research has made much progress, but the crisis of the paradigm has only been exacerbated. At present, it seems to me that Lebedev (Gerd & Lebedev 1991) has come much closer to the latest version of the paradigm, as he now accepts the agricultural colonization of the Ilmen region already in the pre-sopka period. I am certainly aware of D. Machinsky's article from 1982, but I would much rather point to his article from 1990 in which he expresses ideas compatible with mine in several respects.

I should also note that so far I have not come across any thorough analysis of the paradigm. I would guess that the absence of such critiques is connected with ideological boundaries and the 'policy of science', including the position of the so-called Rybakov school. With respect to V. Paranin's study (1990), I can only add here that it seems somewhat ironic that a researcher 'with no essential historical-cultural practical knowledge and skills' has perfectly grasped some of the obvious weaknesses of the national-romantic paradigm, particularly those concerning 'ethnic indicators'.

I would like to stress that I have the greatest respect for Russian archaeology. After all, I was a post-graduate student in Leningrad from 1984 to 1986. It is also important to note that Russian archaeology was never isolated from international archaeology as was the discipline in Estonia during the years of Soviet rule. Western literature was available, at least in the 'centres'. For political reasons, Estonian archaeology even isolated itself intellectually from the 'East', and gradually in the 1970s became something unto itself (cf. Klejn 1993a, 66-68). It would be difficult for anyone without similar experience to understand what intellectual isolation means to a small scientific community in an occupied country. Under those circumstances, archaeology became inevitably bound to nationalism; or more correctly to 'patriotism'. Even the very existence of both processual and post-processual trends remained unknown.

I am certainly aware of the problems faced by Western archaeologists in trying not to harm their Soviet colleagues. On the other hand, 'sympathetic' Leftist treatments of Soviet reality were always used to best effect by those in power. In view of the Soviet system in general, it was external criticism that helped the internal opposition to break it down. At least in Estonia, Western criticism helped slow down Russification. In my article, however, I refer to literature from the period 1989-1992, a time when such caution was, in my opinion, outmoded.

OBJECTIVITY AND SOCIO-POLITICAL CONTEXT

Positivist theories of science have received a great deal of criticism from both sociological and philosophical points of view. Even the natural sciences

are considered to be dependent on the prevailing socio-political context (Kuhn 1970). Western archaeology saw the emergence of socio-political analyses in connection with the crisis of the 'New Archaeology' (Wylie 1989; Hodder 1992, 3-6). M. Shanks and C. Tilley (1987b, 108) have outlined four hermeneutic circles existing in archaeological research. Two of these are important here: 1) the hermeneutic of living in contemporary society as an active participant; and 2) the hermeneutic of working within the contemporary discipline of archaeology. Much has been written since C. Keller's study (1978) about the misuse of the past in the present (e.g. Trigger 1989; Hodder 1991), and some have described these developments as the discipline's 'new loss of innocence' (Kohl 1985; Kristiansen 1993a).

At the same time, however, a similar critical discussion never took place in the Soviet Union - with the exception of critiques of 'bourgeois' science. Now, debates concerning objectivity meet strong resistance in post-communist archaeology, as I can warrant from my own experience (Ligi 1994; cf. Tõnisson 1994a; 1994b; Selirand 1994). Few are willing to accept that research (especially their own) was influenced by ideology and politics, and the very term 'ideology' is given a negative interpretation. In many cases, the collapse of the communist regime has not led to reappraisals of what has been written about ethnicity in archaeology. One reason for this is that, as a phenomenon, the combination of orthodox Marxism and nationalism has not been discussed enough (Ligi, in press). Many wish to forget Marxism while keeping to the main conclusions of ethnic studies.

I myself have felt a certain unease about some of the 'critical' approaches in the Western literature; sometimes the search for socially determined statements has seemed somewhat artificial and violent. Like many archaeologists in the post-communist world, I am also tired of 'politicization'. But it is even more obvious that views concerning the past were and still are misused in the former Soviet Union (cf. Kohl 1994). The study of prehistory was a kind of escape from the reality of the totalitarian regime, and many were willing to subscribe to the call for 'good' archaeology. But I am afraid that in reality we are only deluding ourselves, because we cannot launch 'objective' research from a void. The past of archaeology is still with us, and we as archaeologists have control over the reconstruction of the past, which can be a powerful weapon in the present. Political, ideological and social collisions and clashes and the complex historical background[1] imply that we should have a great deal of social responsibility and that we should take earlier ethnic studies with a grain of salt. L. Klejn has demonstrated such an attitude on several occasions (e.g. 1992).

I remain convinced that whatever our understanding of 'objective' research (e.g. 'neutral' middle-range theory), the question of the origin of the Russians has direct connections with ideology and politics.[2] In my view, this is not only a problem for Estonia with its 'ethnic views and likings', 'national extremism' and 'traditional and slightly obsolete anti-communism'. It is also a problem for Russia and its archaeology, for, as described by Anthony D. Smith, 'all national-romantic mythologies are evolutionist in both form and content and as a rule, science is pressed into the service of poetic constructs' (1986, 191). Politics depends on ideology, ethnopsychology and education, among other factors. So long as the textbooks of history are based on the national-romantic theory of a large-scale immigration of 'active Slavs' into the territory of the 'passive Finns', there is no hope to be rid of the Estonians' 'masochistic' view of history with its image of an 'Eastern enemy'. At the same time, the Russians will continue to find ideological justification for colonialist and imperialist aims, as also for the doctrine of 'racial purity'.[3] In view of present political developments, this accent may certainly seem naive.

I do not think that parallels between the elite of the Novgorod Land and that of the Soviet republics are only a matter of taste. It is rather a question of my own social experience which might influence my view of the past. It is characteristic that, while rejecting my notion about the ideological impact of 'Slavic-Russian' archaeology, A. Panchenko and his co-authors are convinced that there is precisely such an impact on my conclusions. I see this as a typical case of a different social context. Though we all came from the Soviet empire, we experienced its ideology in somewhat different ways. In the 'national' republics, the Communist Party took every effort to combat nationalism, of which all non-Russians were suspected and which was considered to be the main threat to raising new people of a communist type. At least the Russians were never accused of being nationalists, as they were automatically considered to be 'internationalists'. I feel that such an ideology cannot have disappeared without consequences.

It is clear than any discussion concerning past ethnic borders can easily turn into a debate of nationalistic character. I have also criticized the 'ethnic paradigm' in Estonian archaeology (Ligi 1994). But I am perfectly aware of treading on slippery ground. If Klejn is correct in detecting a certain 'vehemence' in my text, one could perhaps at-

tribute it to the 'ignorance' with which the past of the autochtonous population of North Russia has long been treated, and to the much disputed question of the use of language. But of course a critical socio-political reading of my text would probably show that everything can be reduced to the 'ethnocentrism of small nations', and that my nationalism is just a bit crafty. However, great nations usually fail to see that differences in scale and size also matter. What then would be 'objective' criteria for discussing 'nationalism' in this case? What would be the socio-politics of the socio-politics of the socio-politics?

It cannot be helped that the present case fully justifies, in my opinion, some of the much criticized positions taken by M. Shanks and C. Tilley (1987a; 1987b) in claiming that archaeological texts are not innocent mirrors of an object world. A. Panchenko and his co-authors cannot understand how such a 'convinced anti-communist', as they kindly qualify me, can quote these 'neo-Marxists'[4], but I can only do it again: 'It is important to experiment with ways of writing, ways of seeing, ways of presenting. It is equally important to resist appropriation and incorporation into the sterility of a hegemonic culture which translates everything into its own terms and makes other expressions unintelligible. Consequently, our strategies should be those of polemic and provocation, challenging orthodoxy, working with the unfamiliar.' (Shanks & Tilley 1989, 8).

But unlike Shanks and Tilley, I do not find it necessary to take an ultra-relativist position and to claim that there should be many pasts in the present (yet Klejn justifiably suspects that they also try to attain truth; Kristiansen 1993b, 193). We should try to minimize the impact of ideology - and here I am prepared to join ranks with Klejn and Trigger - but in order to do so we must know where we stand. I understand the problem of choosing questions and answers as relating to the 'ideal' towards which all researchers should be orientated. It is not enough, however, just to declare objectivity, something which is far too often the case in the archaeology of the former Soviet Union (cf. Klejn 1993b, 726).

Not all my Estonian colleagues would agree with the definition of my views as 'anti-communist', nor with suspicions of my being influenced by 'national extremism'. I can only hope that A. Panchenko and his co-authors really know the meaning of these terms, which they use with such ease. Ironically, my views have recently been criticized in Estonia as being close to Marxism-Leninism (Tõnisson 1994a), and some have described my critique of the ethnic paradigm as anti-patriotic. These are exactly the reactions which I predicted (Ligi 1993a, 37). Such extremely polarized qualifications are highly typical of the post-communist world as it confronts unfamiliar ideas and views. Moreover, the misunderstanding that comes about when orthodox Marxism meets so-called neo-Marxism by no means takes last place in this context.

I admit that the last paragraphs of my article are open to different readings. In speaking of the politics of compromise, I meant how my conclusions could be interpreted, and in fact were. I certainly did not look for the justification of any political agenda in the past, and my analysis was not intentionally determined in ideological terms. In fact, I have kept to a position which I already formulated as a student in my final year (1981) and I think that my views were shaped only by the archaeological data. Besides, 'compromise' was not topical at the time. This, of course, is a rather complicated epistemological problem. It would perhaps suffice to refer to the fact that when G. Lebedev claimed the long barrows to be non-Slavic, he could not be suspected of nationalism. But this was not the case when an Estonian archaeologist voiced the same views.

I can only consider remarkable the way A. Panchenko and his co-authors project present ideological prejudices into the past. I am sure that one cannot make Tilley (e.g. 1989) and all the critical theorists more pleased with their statements than by drawing parallels such as the one between our case and the American Indians (cf. Trigger 1980). Furthermore, the concept of the priority of the Slavs in having 'pioneered' Baltic-Volga route is also important in that it presumes that the autochthonous communities were absolutely passive and incapable of adapting to the ongoing major changes in global, political, economic and social structures. Were they really hunter-gatherers?

Here we come upon the other hermeneutic circle mentioned above. There is a long tradition in the Russian literature to describe pre-Slavic 'Finnish' communities as primitive and static. The 'Finns' were thought to have been either quickly assimilated or exterminated, and to have taken hardly any part in the state formation process (see Ryabinin 1990). Although this attitude has partly changed by now, the autochthonous population is still treated as something less important in most of the studies that have appeared in the last decade (cf. Noonan 1991). It is highly characteristic to look upon the 'passive Finns' from the Slavic viewpoint ('Slavic-Finnish contacts'), i.e. from outside the society and not from within. Though somewhat masked, the concept of ethnic indicators still plays its misrepresenting role.

Obligatory parallels with 'pure Finnish' cultures are part and parcel of 'objective' research, and woe betide the 'poor Finns' if there are only few or no 'analogues' or specific artifacts of 'Finnish character'! It is of no importance that, for example, in Estonia there is nothing specifically 'Finnish' in the material culture of the second half of the first millennium. Nor does it matter that one should take the same ideological standpoint when trying to prove Slavic immigration. What is specifically 'Slavic', for example, in the 'Sopka Culture', in the large mounds or in the materials from Staraya Ladoga, the Novgorod hillforts and settlements of the 8th-10th centuries? *Quod licet Jovi, non licet bovi?* As regards the term 'Old Russian culture', it should be confessed that in fact it has an ethnic meaning in most studies.

It is perhaps inevitable for Soviet archaeology to appear different from the outside. Trigger makes it clear that Western archaeology was directly connected with the ideology that caused the crimes of the Nazis and praises Soviet archaeology for its humanistic approach. But it is somewhat surprising that he wants to see Soviet archaeology as something separate from the Soviet system. He seems to forget that the latter was based on a Pharisaic ideology which preached an equality in which some groups turned out to be more equal than others and led to the crimes of the Stalinist era such as the extermination of millions of people (which can even be traced archaeologically; see Lõugas 1991). In archaeology, the declaration that all human groups have an equal capacity to develop coexisted perfectly with the concept of the 'active Slavs' (Big Brother). This is what I meant when referring to naive Marxist solidarity.

INTERDISCIPLINARY RESEARCH AND CIRCULAR ARGUMENT

I am still convinced that it is impossible to understand the early history of Russia without addressing the question of whether or not there was a large-scale immigration of Slavs. One of the main problems in research concerning North Russia is the phenomenon of circular argument. Archaeologists refer to the data of other disciplines in order to prove the existence of ethnic borders, while these in turn interpret their data in the light of large-scale Slavic colonization, which is taken for granted. If, for example, the possible impact of the 'Finnish' dialects on the Russian language is considered, parallel phenomena in other Slavic languages are preferred in the interpretation (see, however, Veenker 1967; Birnbaum 1990). The same is true of written sources, physical anthropology, ethnography, folklore and toponymics. We cannot really speak of any explicit ethnogenetic research strategy, as demanded by L. Klejn (1988). Instead, we find everywhere the influence of the Procrustean bed of the colonization paradigm, i.e. a dependency on the above-mentioned second hermeneutic.

There are both Slavic and 'Finnish' hydronyms in Northwest Russia (Popov 1981; Ageeva 1989). It should be remembered, however, that most of this evidence dates from the 16th-18th centuries, when the 'Finnish' dialects survived only in the periphery of the region. Large numbers of Finnic hydronyms were registered in the areas where the 'Finnish' language was still alive when the place-names were written down. As we are dealing with completely different language families, there is a great probability of direct translation, parallel versions (cf. Joalaid 1989; 1990a) and deformations resulting from 'language imperialism'. We must also bear in mind that the language border between the 'Finns' and the Balts still remains unknown. Later historical events are also important, including partial depopulation during the so-called Troubled Times. In the areas where the 'Finnish' dialects survived, the non-Slavic population of the 20th century often did not even know the official Russian hydronyms. We should also remember that, as a rule, the mother tongue of the officials who wrote down the place-names was Russian (see Joalaid 1989; 1990a; 1990b).

In explaining the 'archaic Slavic' hydronyms, R. Ageeva (1989) points to different possible interpretations of the evidence (including the role of Baltic and 'Finnish' dialects), but prefers those predicted by the accepted historical background - i.e. Slavic colonization. The predominance of Slavic hydronyms can be interpreted as speaking for, and not against, language replacement. Under conditions of bilingualism, direct translations would have been highly probable. Had there been an 'ethnically mixed' population, many hydronyms of 'Finnish' origin would have survived. This is precisely the situation in areas where 'Finns' and Russians still live, or where 'Finns' lived until only recently.

In connection with the interpretation of the combination of place-names and archaeological evidence, I would also point to the northern coast of Lake Peipus, which was part of Old Livonia. The only archaeological monuments there are barrow cemeteries displaying typical Novgorodian culture, but all the place-names pre-dating the middle of the 16th century are of 'Finnish' origin (Moora 1964; Ligi 1986; 1992).

I certainly presume there was some Slavic infil-

tration, but for the period c. 750-1300 AD I would place most emphasis on centres of power: the towns with their multi-ethnic populations and the local centres of administration with their Russified elite. The keywords for the language replacement I presume would be 'trade', 'handicraft', 'power', 'elite dominance', 'administration', 'social mobility', 'high-status language', and 'bilingualism'. I do not think I can add much to the debates on 'archaeological culture and ethnos' (with respect to 'ethnic consciousness' it is not necessary to refer to I.V. Bromley - I.V. Stalin would be more correct; see also Klejn 1993b, 734). However clever the theoretical constructs explaining 'Slavic-Finnish contacts' may be, they are useless so long as assumed immigration remains unproved. I do not give an ethnic attribution to the long barrows, sopkas or other graves; all I am saying is that they can be interpreted without reference to Slavic migration.

Furthermore, the anthropological evidence from Northwest Russia can be given alternative interpretations. In my opinion, however, the data of population-genetic and odontological studies clearly show that there was no dramatic replacement of population or an assimilation of the sparse autochtonous groups by numerous immigrants. The fact remains that there is considerable physical similarity between contemporary Northwestern Russians and the Balto-Finnic nations (Gravere 1987 & 1990; Bunak 1969; Schneider & Tikhomirova 1991; Heapost 1994). It is a question apart that physical anthropologists also tend to interpret their data within the paradigm of Slavic colonization, and that some of them speak of the strong influence of the Russian genotype on the Balto-Finnic peoples, but not vice-versa.

I have already noted that concentration of power tends to level cultural differences in peripheries (Hodder 1983). The cultural and politico-economic solidarity of the population of the Novgorod Land was the main precondition for language replacement. This would also explain why there was no 'cruelest cultural crisis and conflict'. Serious language conflict would only have been natural in a conquered land amidst the clash of ideologies and interests. In such a situation the defeated ethnic groups would have had to take a passive role. If the indigenous society and its elite played an active part, this conflict would hardly have been so dramatic. 'Social mobility', as well as bilingualism, would have served as a 'soothing' factor. The extinction of language is an epidemic effect of multistate organization with its bureaucracies. Knowledge of the high-status language was the key to power and success in life (Robb 1993; see also Renfrew 1987). With regard to religion and deities, we know that Christianization was met with a certain resistance.

D. Machinsky (1982) has pointed to the possibilities provided by written sources to argue for the southern route of large-scale immigration. But so far I do not see any archaeological evidence for large-scale immigration of an agricultural or other nature (cf. Kirpitschnikov 1985, 17, 18; Machinsky & Machinskaya 1988, 49-52). There are no written sources telling of Slavic immigration in the 8th-9th centuries. Furthermore, interpretations of both written sources and archaeological data with the assumption of Slavic colonization face a number of pitfalls. Critical Theory has had a strong impact on archaeology in the past decade, and it is characteristic that not only 'sinners' like Shanks and Tilley but also eminent processualists such as Colin Renfrew have accepted that the elucidation of written texts is even more perilous than that of material evidence (Renfrew 1993, 250). Moreover, if we subsume the interpretation of archaeological data under the unclear evidence of written sources, the probability of error will only grow.

Over the past decades, much work has been done to investigate settlements in Northwest Russia. But it will be a long time before the available data will be representative enough for any socio-economic or even ethnic conclusions. Nevertheless, settlement data is certainly important, and it already points to the necessity of reinterpreting the differences between the so-called Long Barrow Culture and the Sopka Culture. The site of Zolotoye Koleno revealed ceramics of both 'traditions' (Nosov & Plokhov 1991), and I see no reason why they cannot be explained with reference to different chronology and local socio-cultural change. I would point once again point to the necessity of gathering large collections of pollen data in order to understand the local economy (to my knowledge, such investigations have already been initiated). Otherwise, we will only artificially amputate a certain period from the rest of the past and obscure the nature of local society by denying its capacity to change.

GRAVES, SOCIETY AND MATERIAL CULTURE

As our knowledge of the 'Finnish' and 'Old Russian' cultures is mostly based on grave-goods, we should bear in mind the role of ritual communication in social strategies (see e.g. Bloch 1977; Shanks & Tilley 1982; Tilley 1984; Parker Pearson 1984a; 1984b; 1993; Hodder 1984; 1986; Burström

1991; Hedeager 1992). The sudden emergence of 'cultures', as reflected in the burial rites, specific grave types and grave-goods may well be the product of a certain 'tension' in a society resulting from socio-political and economic structural changes. The absence of such phenomena could, in turn, point to stability. Here I have to note that it was not only the 'Old Russian' culture or its Novgorodian version which appears to have emerged 'suddenly'. Several 'Finnish' cultures also appeared in much the same way. In the Livonian and Ladoga areas we can speak of a different culture only from the beginning of the 10th century. In Ingria and along the northern and eastern shores of Lake Peipus this occurred from the second half of the 11th century to the beginning of the 12th century, and in Karelia around 1100 AD. I believe these phenomena to be connected with local and global structural change in the societal, economic and political spheres.

In most parts of Estonia, very little is known about burial customs and grave-goods in the period from c. 700 to 950 AD, which I have explained with reference to a phase of consolidation in the cyclical change of power strategies. The sudden emergence of a specific local culture in Estonia from the second half of the 10th century to the beginning of the 11th century coincides with profound socio-political and economic change which can also be traced in other areas of the archaeological record (forts, settlements and hoards). These structural changes were paralleled by similar processes in neighbouring countries, including Northwest Russia. Jaroslav's raid on Tartu, which appears to have existed as a trading site already before 1030, is just one logical part of these processes (Ligi, in press).

I regard the emergence of some of the 'Finnish' cultures as phenomena of cultural-political signalling by autonomous groups bordering on the Novgorod lands. We may also speak with due cause of a specific Novgorodian culture, as that of a political unit which signalled cultural and political solidarity and unity (cf. Hodder 1982; Odner 1985). I do not give it an ethnic meaning, nor do I think we can follow the chronology of language change in the archaeological record. If we cannot prove large-scale immigration, we should first and foremost speak about change within the local societies whose native languages were non-Slavonic, although external factors were perhaps crucial for that.

In trying to reconstruct the 'Finnish' societies we should consider some of the basic ideas of post-processual archaeology:

1) the material culture was used in social strategies:

2) the question of whether the burial rites reflect the way society is constituted depends on the situation within that society;

3) only a few symbols may suffice to bear witness to social status (Hodder 1982; 1986; Parker Pearson 1982). Burial rites might not directly reflect social relations. In the case of the 11th-13th centuries we must also remember that the elite might well have been baptized, and accordingly costly mortuary display would have been both unnecessary and even impossible. Such an elite would have been archaeologically 'invisible'. On the other hand, one of the remarkable features of the 'barrow culture' of Ingria and the northern and eastern shores of Lake Peipus in the 11th-13th centuries is the small number of early burials, which has been explained by demographic factors (Lesman 1988; Ligi 1988; 1993b). However, we may interpret this instead by the phenomenon of ritual innovation used by the 'upper middle class' to legitimize its social status. We know that, as a rule, the early mounds are larger in these areas and that the early burials normally contain numerous grave-goods. It might well be that the other members of the communities began to be buried in the mounds only somewhat later (after 50-100 years).

I would also regard the large barrows of the second half of the first millennium as a means of legitimizing power, as symbols of ideological and socio-political manifestation. I am not even sure that there was such a long interval between the earliest long barrows and the sopkas. This has a direct connection with G. Lebedev's remarks concerning the large mound at Rep'y. Firstly, demographic estimates pointing to the extremely low population density of the 'long-barrow people' (Lesman 1985; Ligi 1989) most probably indicate that only part of the society concerned was buried with such mortuary display. Secondly, the tradition of building long barrows might not even have begun in the 5th and 6th centuries, as has been assumed, but slightly later. The earliest cremations in the large barrows might well have been initially flat graves, those in the 'houses of the dead', or in the small barrows. Only later (perhaps after 1-2 centuries), they might have been covered by an earthwork extension or a high mound, which in turn was used for new burials.

This would explain some of the chronological and regional differences between the long barrows. The idea behind the erection of the large barrows was perhaps to legitimize the higher status of the leading families by 'referring' to their earlier 'roots', i.e. their ancestors. Some of the large mounds were used ('reinterpreted') over a considerably long period. The long barrows without early

burials (e.g. ones with only cremations in the top part) might reflect a process of 'inner colonization', during which the ideological manipulation of the earlier burials was not possible. These were simply missing in the cemetery (Ligi, in press). The small round mounds of the 9th-10th centuries and those of earlier date can be understood as reflecting status differences, and not as 'sound evidence' of the presence of the Slavs.

I would thus regard the appearance of the large barrows as a reflection of local social change and as an active means of transforming society. In my opinion, this rite reflects certain structural changes within local society. The socio-economic background could have been one of internal tension and changes in the relations of land ownership. This tension, or even crisis, may first have appeared in areas dominated by lighter soils, while in the areas of heavier soils the 'ideological marking' of differences in land ownership and status did not become topical until somewhat later. The sopkas can be comprehended within the complex social, political and economic context of the Viking Age, and the process of state formation. Such an interpretation leaves no place for the assumed 'passiveness' of the autochtonous population and requires no *deus ex machina*, i.e. the 'active Slavic pioneers' with or without their ploughs. I do not deny their role completely, but I do not believe they would have met with any success had they not followed the local 'rules'.

Finally, I would like to attempt something truly illegal. Let us suppose for a moment that history would have taken a different course and that there would now be a separate state in the former Novgorod Land with a predominantly 'Finnish-speaking' population. I wonder how the archaeologists of this hypothetical state would interpret the long barrows, the sopkas, and the Novgorodian culture of the 11th-13th centuries. I think I know the answer.

NOTES

[1] One could, for example, establish 8-10 periods of different socio-political background for Estonian archaeology in this century, all of which had a strong impact on research (Creutz 1994; Ligi, in press).

[2] It is interesting to note that during the heated political debates in January 1990 Mikhail Gorbachev directly referred to the colonization paradigm in an interview, claiming that the Slavs have 'always' lived in the present territory of Estonia.

[3] All over the Soviet Union, including Central Asia and the Caucasus, schoolchildren could read in their textbooks that Russians were beautiful tall people with blond hair and blue eyes.

[4] I think that the whole use of the term 'neo-Marxism' in archaeology is misleading. At least in the case of Shanks and Tilley, I would not use this label even in connection with their earlier studies (cf. Trigger 1989, 340-347). At present, it is certain that the term 'post-structuralism' is much more justified (e.g. Tilley 1990; 1991; Shanks 1992).

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