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Some final remarks on collaboration between archaeologists and ethnographers[1]

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It is necessary to quote two statements which appear to paradoxically conflict with each other:

1. "Much archaeological research has been dependent on such analogy [ethnographic. — *O. A.*], especially in trying to explore the social lives of the first farmers and their direct hunter-gatherer ancestors, the Natufians. Such an over-reliance on ethnographic analogy leads archaeological discussion into a re-invention of the present in the past, and makes archaeological researcher a consumer of ethnographic data, rather than a contributor to anthropological thought" (Finlayson, this volume).
2. "As an undergraduate in an American four-field anthropology department, I came to regard the study of human origins as part of archaeology or physical anthropology. In my subsequent career in British social anthropology, little has changed. The people with trowels and callipers do human origins, and 'we' do ethnography. However, the fact is that archaeology and biological anthropology (as the old physical anthropology has become) have little to say about the social or the cultural. Of course, I exempt the archaeological concern with specifically material culture, and I also exempt some rock art studies, with their concern with the richness of symbolic culture. I recognize too the odd archaeologist with an interest in music and human origins, or mathematics and human origins, and so on. These, though, are not the 'bread and butter' of their field. If you want an expert in ritual or symbolism, in kinship or reciprocity, or in political organization, or even in the utilization of resources and communication of

[1] The topic is absolutely inexhaustible and has been spurring unending debates ever since the end of the 19th century, let alone ethnoarchaeology which has become a separate and very specific discipline with its own goals and extremely complicated methodological problems. Here I will only say a few words pertaining to the concerns of this volume. The term 'ethnographers' has been chosen deliberately, because it seems to be incomprehensible why we are ethnographers while working in the field, but then, when thinking about the outcome of our fieldwork, we become social or cultural anthropologists.

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environmental knowledge, why not turn to a social or cultural anthropologist? These are our areas of expertise" (Barnard 2011: XI).

Years ago, I was ridiculed by one of my colleagues for considerations similar to Barnard's assessment cited above. I wrote that the way of collaboration between ethnography and archaeology should be opposite to the common one: not the one of extracting or selecting (from various publications) ethnographic information ("consuming ethnographic data" — see Artemova and Finlayson, this volume) by archaeologists who need to interpret the evidence obtained in the course of their excavations, but the one of investigating and analyzing by ethnographers, who have holistic coherent knowledge about live cultures and people's life, information specifically prepared for them by archaeologists (Artemova 1999: 184). How would archaeologists do that? What kind of interest in doing so could archaeologists have? Archaeologists would dig and ethnographers would think? What prevents Madame Artemova and other ethnographers from applying their "holistic, coherent knowledge about live cultures and people's life" to the analysis of archaeological data right now, without delegating tasks to archaeologists? (Girenko 1999: 189–190).

Well, what prevented it then and still prevents it now is the inability to understand archaeological texts full of special information and vocabulary inaccessible to one without prior training. It is the concrete data about particular findings, detailed descriptions of sites and all the circumstances of excavations that interest an ethnographer most of all. Reading raw archaeological reports about new discoveries, whether published or unpublished (in Russian academia, they are often kept in archives for years before being released as a publication), is like trying to read medical files containing various epicrises and diagnoses. After a few pages of reading, an ethnographer has nothing left to do but jump straight to the conclusion of the text and take the author's word for what is said. Of course, a great amount of archaeological publications do synthesize numerous investigations and present generalizations in a form suitable for wider audiences. But in such cases, the actual line of authors' deductive reasoning and conjectures not infrequently stays behind-the-scenes or raises many questions. Thus, it is almost a common place in archaeological publications that, since the Late Paleolithic up to the Pre-Pottery Neolithic B in particular regions of South-West Asia, social relations were getting increasingly complicated, spheres of economic exchange were getting wider and wider, religious practices were getting more and more effort- and time-consuming, *etc*. However, in the perception of an ethnographer, transition to the sedentary way of life could in fact narrow down the range of exchange and simplify social structures, for it is precisely mobility that may have determined an extreme complexity of kinship systems and social interactions in frames of extended networks, as well as may have facilitated the exchange of things, materials, and various ideas over vast areas among hunters and gatherers which were studied relatively recently. Certain hunter-gatherer cultures — those of the Australian Aborigines, for example — are famous for the great amount of time and energy that they invested in cult activities. The crucial point is that no significant part of that effort went into creating long lasting material paraphernalia for their cult practices, which is something that could be observed among the Epipaleolithic and early Neolithic inhabitants of Levant, who began doing that, perhaps, mostly in connection with their settling down. Social relations may be very complicated and links of exchange may be very intensive, yet they may function in ways that do not leave obvious material traces, or those traces tend to get lost when people are highly mobile and exploit large areas of land. In other words, issues like these deserve a special comprehensive dialogue

between archaeologists and ethnographers — a dialog in which the ethnographer, as much as one may regret it, is often relegated to the role of stuffy skeptic.

Thus, being greatly impressed by Bill Finlayson's material presented in his paper in this volume and favoring the idea of social equality developed anew in the Early Neolithic, I could not help thinking, however, that integrative architecture, including communal storage buildings, described by Finlayson as common in the PPNA sites might well have been an outcome of activities of some totalitarian power structures aimed at levelling lifestyles of all the subjects rather than the evidence of an egalitarianism deliberatively promoted by the citizens themselves. But having scrutinized the sources, both those that Finlayson cites and some others, I actually do not know what to think, except that the data on social relations of the inhabitants of PPNA sites look quite obscure, though experienced archaeologists working at the sites certainly feel and know best.

It also appears to me that mortuary practices which concealed individual identity by creating plaster masks cannot be unambiguously interpreted as evidence of egalitarian social values or reduction of individualism in everyday life or even absence of individuality as "a concept" in early Neolithic. Can we really forget Lazarus raised from the dead? "The dead man came out, his hands and feet swathed in linen bands, his face warped in a cloth" (John 11: 44). Or about crucified Jesus himself with "the napkin which had been over his head" (John 20: 7).

Nina Braginskaya, an expert in Antic and Hebrew studies, has recently quoted to me (personal communication) the words of 'Aqht, a hero from the Myth of Anat, Ugarit, Eastern Mediterranean, 6000–2000 B.C.: "What does mortal man get as his inheritance? Glaze will be poured out on my head, even plaster on my pate, and the death of all men will I die…" (Grey 1965: 258).

A custom of covering faces of the dead with a cloth, clay or any other material was and still is extremely widely spread all over the world, being observed in cultures with quite different styles of social relations, and the reasons for practicing it were and are extremely varied. In many cases, as it is of course perfectly known, portraits of the dead were painted in colors over white clay, plaster, or glaze, and that was a way of preserving individual features (identity) of the deceased person.

While generally appreciating Suzanne Villeneuve's and Brian Hayden's idea (see their paper in this volume) of using proxy ethnographic evidence for justifying the existence of surplus among the Natufians, I find their reasoning to be doubtful in some respects. Thus, they refer to the amount of food used to feed dogs by the Itel'men and the Koryak of Kamchatka and the Eskimo of Alaska in the context of their traditional lifestyle. They estimate on this basis a hypothetical amount of calories which may have been required for the nutrition of Natufian dogs. However, these peoples actually used their dogs as 'cattle'. Those sled dogs worked hard (in very harsh environments) for people; therefore people had to feed them sufficiently, otherwise they would not be able to work and the economy as a whole — considerably dependent on the dog traction — would collapse. The life and use of Natufian dogs were, perhaps, different. By the way, some hunters and gatherers studied ethnographically, and people in certain Australian Aboriginal groups among them, did not feed their dogs, or at least did not feed them regularly. The dogs living in their camps or somewhere around would often hunt for themselves and eat various leftovers. Or one more issue: exotic items imported by the Natufians from afar might well have been exchanged for other valuable items possessed by the Natufians rather than for food surpluses amounting to kilocalories.

Ultimately, the term "transegalitarian society", which was widely used in the above-mentioned paper as well as in other publications of both authors in relation to the Natufians and a number of other 'complex' hunter-gatherer societies, appears to be not quite proper from the academic point of view, because egalitarianism of their ancestors cannot be taken for granted (that is, a priori).

Being fully in agreement with the assessment that the ethnographic present of hunter-gatherers, horticulturists, or herders is unable to provide us with a reliable source for drawing a direct analogy between their condition and the Neolithic transition in Southwest Asia or America or China, I am nevertheless afraid we may throw the baby out with the bathwater. Scholars should not search for direct analogies between modern and ancient cultures taken as a whole, but they may search for particular cultural traits and phenomena studied ethnographically, which could be helpful for understanding particular data provided by archeology. Thus, when I read about the ruins of the buildings excavated at WF16 (PPNA) and looked at the photos of those (*e. g.*, Finlayson *et al*. 2011), it occurred to me that an expert in the village architecture of modern African (the Konso of Ethiopia, for instance) or Polynesian farmers could provide data promising wonderful insights for archaeologists who made unique discoveries referred to above.

To sum, what archaeologists, as it appears, need to do — in order to successfully interpret their evidence on dead cultures and past life — is seek assistance from 'live' ethnographers with their ethnographic mode of thinking and ethnographic style of erudition, rather than keep 'consuming' on their own all that ethnographic data on live cultures and people's lives. Ethnographers, in turn, need to be intentionally enlightened by archaeologists, and this is the way for both to contribute to the theory of social evolution — or the theory of what could be figuratively called tectonic processes of social life as a whole — the theory that would be one and the same for archaeologists, ethnographers / anthropologists, and historians (*cf*. Finlayson, this volume).

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