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# A RUSSIAN LESSON FOR THEORETICAL ARCHAEOLOGY: A REPLY

I am very flattered that my article from ten years ago has now generated interest and comments. I am of course grateful for the high estimate of my former scholarly activity, and for the generous words. At least some of my commentators, however, are confused (as acknowledged by Olsen and Tilley) by a situation where they are invited to judge a review written so long ago, as they have access to new information which the author could not have had. I am afraid that expressing confusion is merely a way to soften a number of considerable objections – to advance them so as not to hurt too much an author who has suffered enough as it is.

Yu. Lesman frankly admits that my review from 1990 is less interesting than my "Panorama", published in 1977, but he kindly tries to give an objective explanation: the period under review is in itself less exciting. I would suggest an even better explanation: this review (involving merely books, only from the West, and a shorter time-span) is simply not as broad in scope as the first one. This was also the author's choice, however, and it is at least partly due to his approach, although it is largely the same as in the "Panorama".

My opponents are very lenient, but I cannot accept such indulgence.

Both Olsen and Tilley insist that archaeological thinking has cardinally changed since 1980, and they consider the ideas prevailing now as new, important and fruitful. All this I strongly doubt. To me, the ideas propagated by the younger British generation of theoreticians, insofar as I have become acquainted with them, do not seem all that new, and hardly fruitful. Of course, much has changed; some trends are now more clearly developed, and the field of the 1970s is better seen from 1990–91 than from 1980–81. From today's perspective, I would write the review somewhat – but not completely – differently. Assuming, for the sake of argument, that my review of the past period of theoretical debate was mostly good, and that it proceeded from a plausible position, its main characteristics must then be not only of interest but also of an objective value even ten years later. If this is not the case, then the review was poor, and the author must decidedly revise his position – if he can.

The crux of the matter is the content of the objections and their justifiability.

First of all, Tilley says it was unfair to use the title "Theoretical archaeology etc." He thinks that, in reality, the issue was not theoretical archaeology but general, abstract archaeology, and the real themes are systematization and methods. We obviously differ fundamentally both in defining theory and in understanding the purpose of such reviews. Already in the beginning of my "Panorama" from 1977, I pointed out that I prefer in such surveys a broad notion of the subject, and in practice this is the most useful one for the readers. I essentially mean archaeological thought – much like Trigger in his latest book (1989) and Lamberg-Karlovsky in his collective volume (1989).

Tilley wonders why a considerable part of my review is devoted to methods and not theory proper. But should we not also wonder at the entrenched way the popular cliché of "theory and method" is used in so many titles of archaeological books (I am sure examples are unnecessary). Why are these terms so closely linked?

I also disagree with Tilley in the exact definition of "theory" as a concept. For him, to deal with theory is "to discuss the concept of totality, of subjectivity, of contradiction, of power, of discourse, of ideology, what it means to interpret, the consequences of modernity in relation to archaeological knowledges, the nature of ma-

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terial culture as a signifying system." From this it is clear that under the rubric of archaeological theory he understands mainly the philosophical problems of archaeological cognition. The theme in itself is interesting, but why call it "theory"? The reason for this is clearly an intention to replace proper archaeological theory by philosophical learning, which is determined not by the development of our discipline itself but by the processes of another discipline, and – through them – by the ideology of a certain political group and ultimately the social positions and interests of that group. The result is a direct application of certain areas of philosophical learning to archaeological facts.

But this has already been realized. The nearest, though by no means close, example is the New Archaeology. These archaeologists preferred one philosophy, while Tilley and his comrades give preference to another. But a strong dependence on philosophy remains. A more radical example (and perhaps closer to Tilley) is Soviet archaeology. We, Soviet archaeologists, know from experience what it means to subjugate archaeology to philosophy. Fruitful ideas? Perhaps. But the fruits were terrible (cf. Bulkin at al. 1982; Klejn 1992). Berdyaev said that it was Russia's fate to go down the wrong path so that others might benefit from the negative lesson – and see the way one must not go. It appears that young Western intellectuals, who never had this lesson, have instead read into fashionable philosophy and are not adverse to toying with strong Leftist ideas. These are dangerous games – both for scholarship and for society.

'Theory' is a definite scholarly term. It is used in all disciplines, both in the sciences and in the humanities. Accordingly, its definition must have some recognized core. Otherwise, we must speak of different concepts, and be obliged to select different terms. I do not wish to discuss this topic extensively in this connection; I have written a special article on the subject (Klejn 1978b). Suffice it only to adduce my definition of the concept. I understand theory as a precisely elaborated programme of information processing based on a certain fundamental idea. This programme is constructed as a logical system of propositions expressing regularities (laws). It consists of concepts and definite relations between them. Theoretical concepts are ideal objects, replacing and representing real objects, and the sense of theory consists in the following: the results of operations with the ideal objects may, under several conditions, be transferred onto the real objects, ultimately permitting the explanation and prediction of phenomena. When the mechanism of processing becomes stereotyped, the theory appears as a method. For this reason theory and method are closely connected.

Such an understanding of theory is more closely linked to research practices, to the experience of the discipline, than to philosophy. I agree with Tilley's insistence that the theoreticians of archaeology must be specially educated from an early stage, as they are dealing with a special branch of knowledge. There is much that theoreticians need to know which other practitioners of the same discipline do not need. However, experience in practical archaeology is a necessary part of the education of a theoretician. How can I explain the depositional transformations of an artefact if I have not observed them thousands of times in excavations? Of what use are all my arguments on how to classify artefacts, if I cannot obtain from my own experience real problems in classifying the numbers of various pots, flints or graves? I think that the scholar, who suggested to Tilley that he prepare a solid elaboration of his own pertaining to concrete evidence, gave him good advice, and Tilley's irony is uncalled for.

In view of the above understanding of theory, it is also clear that the theoretician's sphere of interests embraces a vast range of notions, ideas and methods, for without these notions, ideas or methods theory simply cannot function in archaeology. This is why in my understanding they all constitute theoretical archaeology.

Tilley finds my review too balanced and too neutral. He cannot understand why I can note merits and demerits in almost every work under review. To me, this is the indispensable task of the survey: to point out in these works that which may be of use to the discipline and not to obscure, even in skilled works, shortcomings or mistakes, which may be dangerous if allowed to spread unnoticed. My intention was specifically to write a review article, and not a polemical one. We must recognize the laws of genre, and to me the review seems to be a useful genre. The question is whether I have followed the laws of this genre adequately, but the answer to that involves completely different criteria.

Tilley complains of never having "...really obtained any coherent idea of Klejn's own position." "What does Klejn himself advocate?"

I did not hide my own position, although I am not sure that a survey is the best context for defending it. Perhaps like other scholars, whose works are sporadically and, for various reasons, selectively published in different countries, I found that language barriers have split my image into at least three variants. In the English-speaking countries I have mainly been represented by surveys, polemical articles and comments (my monograph on typology must be excluded because of its extremely unsuccessful translation). In the German-speaking countries, I am known on the whole through my essay on Kossinna, and other articles on ethnogenesis and the genesis of culture, the problems of *ethnische Deutung*, cultural continuity and the historical integration of the so-called *Altertumswissenschaften*. It is only in the Russian-speaking countries that my purely theoretical and methodological books and articles are known. It is only in Slovenia that my main works of all kinds have been translated and published. Only there am I reunited.

In my books and articles, I primarily defend two positions, one of which has been the subject of little attention in the West, while the other is discussed in the west as well.

The first position maintains that the cognition process of the past must be clearly dissected into steps, all of which are indispensable and must follow in strict succession. Accordingly, archaeology is considered a separate discipline, essentially a study of sources, whereas palaeohistory (including prehistory, protohistory and early history) is another, distinct discipline of descriptive and explanatory synthesis. This distinction is very important for Soviet scholars, and it has been the subject of heated debate, for the borders of these disciplines have been erased and some of the necessary steps were bypassed. In organizational terms, one discipline became supplanted by the other (in varying order). Such developments can also be seen in the West as well.

I am not quite sure if Lesman understood my position correctly, for he places me on the same standing as agnosticists and hypersceptics, and sees only a difference of grade or level between myself and them: they are extreme, while I am moderate. In reality, I am by no means an agnosticist, nor do I take a sceptical view of the possibilities of cognition or explanation in archaeology, or on any general level. The crucial question is always *what* must be explained.

I exclude from archaeology not explanation in general but merely the explanation of cultural-historical processes: laws of development, and the causes of stagnation, revolutions, migrations etc. This is the task of history (palaeohistory), sociology, and anthropology. It has its own methodological equipment: methods of synthesizing information from various kinds of sources (methods of interdisciplinary integration), criteria of relevant analogies, and so on. Operating within archaeology are its own laws and its own valid tasks of explanation, as well as its own methods. Here, the condition and aspect of antiquities must be explained and their differences in relation to other contemporary objects. Implemented here are those laws which allow us to link the antiquities with the extinct culture and to create a more complete concept of it, to model the past events and phenomena which left behind archaeological remains.

My second basic position coincides with the general course of theoretical archaeology which began in the 1970s. This was the course towards the cognition of the mechanism of reconstruction by means of archaeological sources, and towards an understanding of the formation processes of these sources. This position is clearly linked to the first one. If archaeology is seen as a discipline studying sources, the interest of archaeological theoreticians will naturally shift from the processes of cultural history to a study of how the archaeological record was formed (Klejn 1978a).

This brings us, in turn, to my survey and my comments on the fate of the New Archaeology.

Tilley believes that the New Archaeology is "moribund" and "destroyed", and Olsen sees it as "fragmented". I agree that it came to an end, but I would prefer more exact wording. It developed into a final stage, and was transformed into other branches of learning. From today's point of view, I would claim that the New Archaeology came to an end in the mid-1970s around the same time that an independent theoretical archaeology began to emerge. Implied here is an understanding of the New Archaeology as processual, i.e. aimed at the study of cultural-historical process in a scientific way and with scientific methods.

In earlier connections, Binford (1972) and Clarke (1973) pointed to the formation processes of the archaeological record. After the mid-70s, Binford (1977, 1978, 1983) became a central figure in the study of these problems. He still considered these questions, however, as being on a lower level than the theory of cultural process, but on a higher level than theories relating to concrete problems, and advanced the term Middle Range Theory. In the mid-70s Binford's pupil Schiffer (1976) unveiled Behavioural Archaeology, with the call to study all aspects of human behaviour: how people live, how they produce their material culture, how it becomes disposed of as rubbish, and ultimately deposited.

This marked a conceptual formulation of the next, non-processual, archaeology, and the changing of theory. The formation of the archaeological record was thus recognized as the focal point of theory, at least by several successors of the New Archaeology.

Olsen believes that scientification was the main content of the New Archaeology, and therefore he does not make any distinction between the New Archaeology and Behavioural Archaeology. Although Binford himself moved towards positions which Schiffer called Behavioural Archaeology, this does not imply that these orientations were identical. Scientification was only one of the components of the New Archaeology, albeit an important one. As it is narrower in scope than the New Archaeology as a whole, its distribution in time and space is in turn much wider than the New Archaeology as such. Otherwise, we would have to class as New Archaeologists people like Spaulding, Malmer, Rudenko and others.

Tilley maintains that the New Archaeology had nothing in common with Marxism other than materialistic beliefs. To what degree – if at all – can Marxism be expressed in the principles of archaeology remains to me a major question. Did Marxist archaeology ever exist (Behrens 1984; Klejn 1992)? At any rate, Soviet archaeology claiming to be Marxist does exist, and many of its ideas, dating back to the 1920s, coincide in general with the New Archaeology. These include a stress on cultural-historical process, a strong degree of sociologization, an enthusiastic hunt for laws, a strong critique of empiricism, and many other features. I have discussed these questions in my "Panorama" from 1977.

Tilley sees Marxism in different terms than I do. He is stricken by the fact that I speak of Marxism only in terms of a Childean cultural-historical approach and philosophy (I would hasten to add: in terms of sociological analysis as well). "But what of Marxism as a critique of alienation, domination, exploitation and repression – Marxism as a politics. This is missing," exclaims Tilley. If it is really missing, I am very glad, because I can accept only a limited part of Marxism as useful for archaeology.

Perhaps Tilley is confused by my reputation as a Soviet Marxist scholar. In our country, however, theoreticians were simply obliged to be Marxists, and avowed dogmatic ones (which anyhow I was not), otherwise their activities were prohibited. All other kinds of theoreticians could express their beliefs only in a camp, and even there this was punishable. Therefore, some official Marxist declarations in our papers were simply inevitable, especially in material that was sent abroad.

Even under such conditions, I contrived to avoid the double designation of "Marxism-Leninism" as an ideological banner. I did not accept Leninism, whereas the rest of Marxist learning I accepted only as interesting philosophical elaborations on certain methodological problems and as a bunch of sociological concepts. In contemporary Russian professional literature, some of these scholarly studies are in fact very good, though this cannot be said of the classic texts. Russian philosophers and methodologists were to a great degree constricted by the bounds of official ideology. Nevertheless, some of them managed to elaborate balanced and reasonable approaches to scholarly problems. But Marxism as a sociological and economic conception contains a number of principal errors in its basis, and as a political subculture of the 20th century it is simply terrible. "Alienation, domination, exploitation and repression" are the hallmarks of Marxism as a politics – wherever it has come to power.

It is, of course, not reasonable to deny the application of Marxism to archaeology, but the possibilities of such an application are limited. Marxist interpretations and methodological insights may be useful when supplemented and corrected by ideas and conceptions from other areas.

Tilley sadly notes that "there are no politics in Klejn's discussion." Until recently in our country, an accusation of *apolitichnost* (indifference to polities) was a serious *kompromat* ("compromising matters"), and Tilley's modest voice would have sounded like a denunciation (though this, of course, is not the case). As I have said previously, I avoid politics intentionally. But let us return to Tilley, who observes: "...and because there ar no politics, he lacks a vital way of assessing the relationship between facts and values and thus achieving a critical insight in the review." There is no direct relationship between facts and values that could interest us as archaeologists. To me, seeking such a relation seems to be a wrong course. It is rather theory that has its own relations with facts, and – quite separately – with values.

I sense that Tilley is worried about the problem of naive perception by traditionalist archaeologists who consider facts as pure and objective, thus maintaining that values and ideology must simply be avoided. I raised objections against this position many years ago at the Social

Responsibilities Symposium (Klejn 1968). At the time, I was restricted by prevailing conditions, but at least some of my foreign colleagues correctly understood my suggestions (Berreman, Gjessing and Gough-Eberle in Replies to Köbben 1971).

But now I see another extreme, where facts and values enter into a direct relationship with each other, and the trend towards an objective truth is replaced by an avowedly subjective position. This is like the situation in our country in the scholarship of the Stalin and Brezhnev eras, when *partiinost* (party spirit, party principle) was the highest grade of objectivity. Implied here, of course, was that it was *our* party, and not that of the traitors or wreckers. It is all too familiar. Tilley believes that these ideas are fresh and fruitful, but I have been familiar with them from an early age, and I can demonstrate to Professor Tilley their consequent logical development in moribund reality.

Both Olsen and Tilley are kind enough to suggest that I write a review of contemporary theoretical archaeology. As they do so after such critical remarks, I feel truly impressed. I am considering such an undertaking, and even more: an extensive collective survey or annotated bibliography of the entire literature of world theoretical archaeology. Yet my (our?) survey will not be political or one-sided, though it may be sharply critical with regard to lack of validity, usefulness and originality.

With such intentions in mind, I am all the more ashamed to notice that I have overlooked a number of errors and misprints in the translation and reprinting of my review. On page 4 (middle of the first paragraph) Manfred Eggers's work on the New Archaeology is characterized as "semihistoric-semicritical or rather semicritical-semiapologetic", but the middle part of the expression is lost. On page 8, Kramer is misspelled as Cramer, and placed accordingly in the References. On page 11 (near the end of the page) Ashby is given according to the Russian transliteration as Eshbi.

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## EDITOR'S NOTE

L.S. Klejn's reply was received before the events of August 1991 in Russia.