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# interview

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## The stratigraphy of a life. An archaeological dialogue with Leo Klejn *Visa Immonen*

### Abstract

Few archaeologists can claim to have written more than 300 articles in three different languages, let alone on topics as diverse as the philosophy of archaeology, the Ukrainian Eneolithic and the anatomy of the Iliad. Still fewer to have convinced Stalin, to have been convicted for homosexuality, and to have written a half-million copy bestseller about his years in prison. Only one archaeologist has. Despite all this, he is still poorly understood. At 76, Leo Klejn is still active. And still eager to have his voice heard.

### Keywords

archaeological theory; historiography; Soviet Union; Russia

In 1998 the Russian archaeologist Leo S. Klejn visited the University of Turku in Finland to give a course entitled *Theoretical archaeology*. The lectures were met with great interest, especially since attempts to acquaint oneself with Klejn's published works and thinking can be notoriously difficult. Partly due to the languages in which he has published, partly due to the differences between his perspective and the canon of Western archaeology, his texts are not always easy to approach. Indeed, it seems that Klejn is both familiar and unknown to many archaeologists. It is as usual to know that he is a theoretician who has written panoramas over theoretical developments in the archaeological literature as it is usual not to know what exactly his position and his ideas are.

Klejn, born to a Jewish family in Vitebsk, Belarus in 1927, began his academic studies at the Grodno Pedagogical Institute, but moved to Leningrad University in 1947. He studied simultaneously archaeology, under the direction of Professor Michail Artamonov, and folklore. His teacher in the latter was Vladimir Propp, whose significance to Klejn's methodological thinking is fundamental. Klejn graduated as an archaeologist in 1951 and defended his thesis 'Proishozhdenie Donetskoy katakombnoy kul'tury' ('The origin of the Donets catacomb culture') in 1968. In 1962 Klejn was appointed associate professor of archaeology at Leningrad University, then reader, but in 1981, after 20 years of teaching, he was arrested and sentenced to prison for homosexuality. Deprived of his academic grade, title and teaching position, he remained unemployed until 1994. Later the investigator of his case publicly admitted that the accusations and the trial were fabricated and politically

motivated. In the 1990s Klejn lectured in various European universities and, between 1994 and 1997, he worked as professor at the Department of Philosophical Anthropology at St Petersburg University. The 2000/2001 academic year at Washington University in Seattle, where he was a visiting professor, was Klejn's last year in regular professorship.

Klejn has published eleven monographs and more than 300 articles, and has edited a few collections of articles. In Russian, he has published major theoretical and methodological works. The most important ones, *Arkheologicheskie istochniki* (*Archaeological sources*, 1978–95) and *Arkheologicheskaya tipologiya* (*Archaeological typology*, 1991), are available only in Slavic languages. Klejn's Russian publications also include works on the Neolithic and Bronze Age and studies of the Scythians, Sarmatians, eastern Slavs and Normans. In German, Klejn has published works on ethnic and ethnogenetic questions, while his publications in English include two major panoramas of theoretical archaeology (Klejn 1977; 1990), articles on archaeological theory and the perception of Marxism in Western archaeological thought. Only one larger theoretical work by Klejn, *Meta-archaeology* (2001), is available in English, as Klejn does not want to include the poor English translation of *Archaeological typology* (1982) among his major theoretical works. In addition, Klejn has published numerous review articles in English, particularly in the pages of *Current anthropology*.

Since his extraordinary output is only one stratum of an exceptional life, the time seemed ripe to interview Leo Klejn. The lecture given in Turku evolved into a dialogue with Klejn and, after a rich e-mail conversation, we met at his home in St Petersburg in March 2003. His answers were comprehensive, invariably incisive and sharp-witted. The interview was done in English, a foreign language to both of us, but one that allowed us to discuss in some

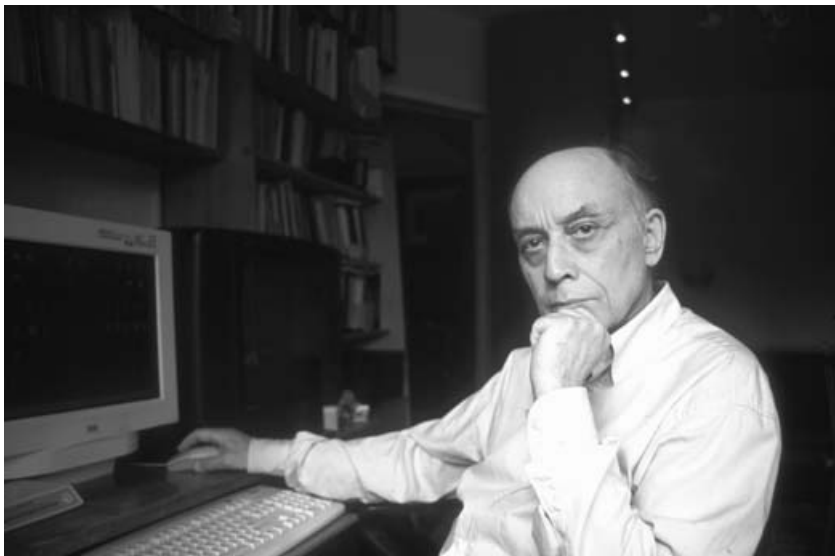


Figure 1

detail his life and work. Finally, Klejn went over the edited version of this text to ensure that it captured his thoughts.

### Three Klejns

*Looking at your intellectual output, it seems as if there are three Leo Klejns – one writing in English, one in German and one in Russian. Have you consciously chosen this or that language for addressing specific topics?*

Not at all. It was dependent on the interests of journals and publishing houses and on the political situation. Of course my language capacities were also important: the foreign languages I knew best were and are English, German and some Slavic languages, especially Polish. So these countries were more accessible for my publications. I tried to publish my works first of all in Russian, but for a long time it was very difficult for me to squeeze into Soviet archaeological journals with theoretical works, because an unwritten rule dominated there: only bosses or authoritative scholars could discuss theoretical or any important problems in general, and especially express their own views.

So I tried to publish such work through German, especially East German, journals and *Current anthropology*. My German article on Gustaf Kossinna could not have been published in Soviet Russia, and it was published in Russian only recently. Then again, the article could not have been published in the GDR if it had been written by a German author either, but it was possible for me as a Soviet author! To have my book *Arkheologicheskaya tipologiya* (*Archaeological typology*, 1991) printed in Russia was equally impossible, and it was only published ten years after the unlucky Oxford edition.

Participation in *Current anthropology* opened for me the possibility of expressing my theoretical views and engaging in sharp theoretical discussions. My colleague from the GDR, Dr A. Häusler, recommended my inclusion as a participant. I received an invitation from CA and showed it to my university directors, whose permission was indispensable. They considered the problem so important that they addressed it to the Party leaders of the city in the GorCom or the City Committee of the Party. GorCom thought about it for a long time, and when finally the decision came, it was: not banned, but not recommended. I read it as ‘permitted’, but my chief read it as ‘not permitted’.

I thanked CA for inviting me but declined participation with the excuse that I could not pay the required fee. After some time, I received a reply that the fee was purely symbolical. Since I had advanced no other motive for refusal, CA held me for an associate and began to send me all papers regularly. With this reply, I went to the prorector of the University, who was in charge of international affairs. At that time, the post was held by Dr Gennadiy Shatkov, the Olympic champion in heavyweight boxing, and I asked him what to do. Shatkov reflected and said, ‘Nothing. Don’t reply.’ So for some time, I received extensive material from CA and kept silent.

Then I received a letter in which the wise editor wrote that, since there was no answer from me for such a long time, they would publish my name in the sad list of associates who had died or gone missing for no apparent

reason. Having read the letter Shatkov grew gloomy and sent the letter to the GorCom. They had made a mess, so it was up to them to sort it out. Ten months of waiting had passed when I came once more to meet Shatkov and respectfully said it was time to decide. The world-known boxer looked me in the eyes, smiled openly and chopped the air with his heavy hand: ‘Ah, damned, I’ll allow it on my own risk. Do participate.’

*A real boxer’s blow.*

A champion’s! Since that time my participation in the CA began. Soon I realized the international opportunities of being published in English and began searching for other, similar possibilities. It was very difficult to get permission to send any work abroad, even the slightest remark. Authorization from every administrative person in many instances of the department was needed, and it meant discussions, several sessions and commissions, all recorded in minutes. Once I calculated that I was forced to collect 17 signatures, with stamps, for one little note.

Soon I observed that the great number of instances makes each one somehow weaker: the first hopes to be checked by the next, and the last believes that the previous ones have already crossed out everything dangerous. When the number of my publications grew, I became bolder and began smuggling my works through several ways without censorship. For instance, I sent letters to the editor beginning with ‘Dear Sir, my opinion on the question you asked is . . .’ and continued with the text of the article in small handwriting. If the text was too long, I proceeded in following letters and, finally, I sent the bibliography in a separate letter.

*Was it not too dangerous to send such letters? They could have been read by censors.*

You see, they were purely scholarly, not political, so they looked inoffensive. To make sure, I told about my tricks to my teacher Professor Michail Artamonov, the Director of the Hermitage Museum at the time. He smiled naughtily and said, ‘You know – I do the same.’ For some years I practised this and other methods, and once I was called to the local KGB agent of the university. He said, ‘You have so many articles published abroad. Have you had all of them processed through the necessary instances?’ – ‘Of course,’ I replied. He looked at me with suspicion: ‘Show me the permissions, please.’ I was very surprised: ‘How! It is forbidden to keep any of the documents in private possession! Of course I honestly delivered everything to the officers, every time!’ Officially censorship did not exist in the Soviet Union. He hesitated: ‘Maybe you will search for the documents in the university archive?’ – ‘But you know in what disorder they are stored there,’ I replied. ‘Who will allow me to rummage there and give me enough time to do this enormous work?’ Since then, I was completely cheeky in sending my works abroad, although, of course, letting some larger works go through the multi-staged censorship. You know, some of my Siberian colleagues even asked me, ‘Lev Samuilovich, is Leo Klejn, who often publishes in American journals, your relative?’ To publish in foreign journals was, at that time, a risky and rare occurrence for a relatively young Soviet scholar.

Yet let us return to your question about languages. As I had to adapt to the demands of the publishers, I chose different places for different aspects of my work. Germans preferred solid works with scrupulous data on important problems, English and Americans liked brief theoretical polemics, Soviet literature was open for excavation reports, descriptive works and reviews, etc. So yes, I was forced to have many faces. But they were not masks. They were all my face seen through different holes. The only mask I wore was ‘true Marxist socialist’.

*We will come back to that later. Could you perhaps first say something about the English translation of your Archaeological typology?*

The book was written in the 1970s, and then translated into English for an Oxford edition. But the translation was extraordinarily bad. The translator knew Russian very poorly, she confused active with passive, and knew absolutely nothing of archaeology. She would for instance back-translate Childe from the Russian transcription to Chayld. My checks of the translation were interrupted by my arrest, and as the publishers had no information on my view, they decided to publish the book as it was – half of its size and without checking. The full and correct edition was issued in Russian ten years later.

A German edition was prepared at the end of the 1990s in Vienna. To this edition, I added some new sections. The manuscript was edited by Dr Falko Daim, but suddenly a new law was passed in Austria which forbade Austrian money being spent on foreign authors, and thus the publication of the German edition was stopped. The attempts of German publishers to obtain the manuscript for publication failed, because the Germans considered the Austrian expenses too high. So there exists a German translation of the new version, but it cannot be published.

### **Propp and Russian formalism**

*The expression ‘the three Klejns’ can also refer to your disciplinary fields. Apart from archaeology, you have also dealt with philological and anthropological issues. Few readers in the Anglo-American world probably know that you were trained as a philologist by Vladimir Propp. How did his form of Russian formalism influence you?*

My philological education did not begin in Petersburg but in Grodno, where my parents settled down after the Great Patriotic War or the Russian part of World War II. There I entered the Department of Literature and Language in the Pedagogical Institute. I was forced to enter the Institute as I had gone to the front without finishing high school and received my matriculation certificate from the preparatory course to the Institute allowing access only to further studies at the Institute. I spent two years in the Institute before moving to Leningrad. This ‘move’ was actually a flight. My critical speech held at the city conference of the Komsomol or the Young Communist League was immediately considered by the First Secretary of the Grodno GorCom as a subversive action. He publicly suggested at the same conference that I be expelled from the Komsomol and that the KGB study my case. As my

personal history already had an episode of being the leader of an underground youth organization investigated by the KGB, I immediately escaped from the town.

I fled to Leningrad and entered the University, first as an external student, a year later with great difficulties as a regular student. At the time, Professor Propp was known, due to his early work *The morphology of the folktale*, as a 'bourgeois formalist', i.e. a deviationist from the only true Marxist way of thinking. He was sharply criticized, but as the book had been written more than 20 years before, he was not arrested and not driven out from the university. In reality, the book was one of the first works of semiotics and of structuralism.

*What was your impression of him?*

Propp was a little old man, very shy and charming. He was grey, with beautiful almond-shaped eyes and small aquiline nose always red due to a cold. His lectures on Russian folklore, read with a sonorous baritone, were brilliant and captivating. Still being an external, I wrote a first-year paper, under his direction, entitled 'The bear in folktale, language and custom as compared with archaeological materials'. I tried hard, did my utmost to be fitting and received the desired mark from my beloved master. He estimated my work as 'excellent with an additional characteristic of being outstanding'. He recommended the work to be published (which never happened) and described me as worthy to be accepted into the number of regular students. The extraordinarily high mark, I suspect, was intended to facilitate my entrance to the University as a regular student, for simultaneously I received a letter from Propp with a detailed critique of my paper.

My Jewish origin was a great obstacle for entering the University, and this was obvious to everyone. Propp advised me to make attempts to enter the Philological Faculty, where he was teaching, or the Historical Faculty, where the Department of Archaeology was situated. He assumed that my way to the Historical Faculty might be easier since it was less anti-Semitic than the Philological one, although later the situation changed to quite the opposite. However, Propp was convinced that archaeology was very important for folklore studies, which my paper showed. At the time, he had just written a book on the historical roots of the tale. 'I wanted to deal with archaeology myself,' he said, 'but for me it is too late.' So I planned to begin my second-year studies at either this or that faculty. Although I passed all my first-year courses of both the faculties with excellent grades, I was not allowed to enter. I knocked on all doors in the long ladder of instances and got to the Minister of Education in Moscow, but everywhere a short word was written on my appeals: 'Deny'. September was coming to an end, the studies had begun long ago, but I was still outside.

When the Rector of the University returned from his holidays, I made the last attempt and went to see him. His name was Professor Voznesensky; he was an economist and the younger brother of another economist Voznesensky, the deputy of Stalin.

*Later he was executed, wasn't he?*

They both were, but in those times such a relation made our Rector very independent and haughty. I entered his study with two applications, one for the Philological, the other for the Historical Faculty. Yet being quite nervous, I handed both at once! Voznesensky looked at my applications and seeing the denying signatures of the Minister exclaimed, 'What! The Minister directs in *my* university!' He crossed out the signatures of the Minister and wrote: 'Accept. Voznesensky.' Another 'Deny'? Another 'Accept'? So I was accepted to both faculties, which was impossible in Russia. I was the only student studying at both faculties simultaneously.

I passed all exams in both departments, which was really difficult. Gradually, I began to concentrate on archaeology, and in the last year of the five-year education I studied only archaeology. Thus I have a full archaeological education and an unfinished philological one. Nevertheless Propp suggested that I continue my education as a postgraduate in folklore, but I declined and decided to apply for archaeological postgraduate studies. Yet Propp remained one of my models, and his works had a great influence on me.

*Could you be more specific?*

The idea that there is no way of understanding a particular content except through form made my *Typology* the archaeological correlate to Propp's *Morphology*. The semiotic and structural ideas are present in the system of archaeological notions, which I have developed, with levels of content, plan, etc. His search for a mega-plot behind the particular plots of tales was the prototype for my search for the general procedure or algorithm of archaeological research as well as the search for some general skeleton of theory.

*There are other major theoreticians in archaeology, e.g. Gustaf Kossinna, Vere Gordon Childe and Michael Shanks, who have a background in philology. What was the influence of your philological background for your theoretical thinking in archaeology?*

Your list can be completed: the first head of Soviet archaeology, Professor Nikolay Ya. Marr also had a philological background. But, as I told you, my background was only partly philological as I received a full archaeological education. Yet like Kossinna, Childe and Marr, I was very interested in the problem of the origins of language families (especially Indo-Europeans and some of their branches), migrations and ethnogenesis. The works of Kossinna, Childe and Marr were inspiring and I wrote on each of them.

Marr was a linguist and this is the field where he built his revolutionary 'theory'. This 'theory' destroyed Proto-Indo-European and all other original languages of the language families and pictured the development of speech in just the reverse way – from a great many languages to a few more and more united ones and finally reaching, in the future, one single language. The methods he invented were extraordinarily simple and dim. By these methods you could prove anything. During the Revolution Marr was made the head of Soviet archaeology as well, and archaeologists had to underpin his crazy

ideas with archaeological materials. In my student years, Marr's 'theory' was considered 'the iron armoury of Marxism'.

I was enthusiastic about it for a while, and in my fourth year at university, I chose his teachings as the basis for a paper. Yet when I was let into Marr's archive and worked there, I came to the conclusion that his teaching held neither in linguistics nor in archaeology. It corresponded neither to Marxism nor to facts, and he was really mad. Artamonov recommended my paper to be read in the Institute of History of Material Culture at the Academy of Sciences before its Council. The session was a great success for me, but some incidents followed: for diverging from Marr, the Party leaders of our Faculty condemned me to be expelled from Komsomol and from the university. But before the expulsions were carried out, I sent my paper to Moscow to the Central Committee of the Party, which was the highest instance at that time. Immediately thereafter, the well-known linguistic discussion of 1950 about Marr's teaching had begun in *Pravda*, the main Party newspaper.

*Was this the same discussion in which Stalin himself said his decisive word about language problems?*

It was, and he said the final word but a little later. My paper was transferred to *Pravda*, but not printed; when I came to Moscow after my exams, I was told that all the materials were shown to Stalin, and among many dozens of papers sent from Leningrad, he picked out two as correct – including mine. I returned to Leningrad when his famous paper was printed in the *Pravda*. There he condemned Marr's theory as false. By this time it did not satisfy Stalin's new national policy. Internationalism was eventually replaced by nationalism, and the question of national origins appeared acute.

My first printed article (1955) was a review of a collection of articles devoted to the origins of the Slavs. In the Soviet Union, the autochthonous conception, i.e. that the Slavs have lived in their present areas from time immemorial, was dominating as it was considered patriotic. However, after Stalin's death Khrushchev represented a more liberal attitude to scholarship and literature. The leading Russian archaeological journal *Sovetskaya arkheologiya* accepted my review. Its editor, Professor Arcikhovskiy, was at odds with other great Slavists whom I had criticized in my article, so he was favourable.

In general, I disliked the use of archaeology for political ends, especially such ends. Why should the long, even eternal sojourn on their present territory give a people the right to possess this territory as its homeland? What if the people had come not long ago? Am I a foreigner in Russia, since my ancestors were included into the Russian Empire only three or two centuries ago? Russians did not conquer the St Petersburg region much earlier. Do they not have the right to St Petersburg? Should Russians give it back to Sweden or Finland?

*I think that Finns do not pretend they should.*

Neither do the Swedes. The right to a country as a homeland is not based on such chance facts but on a system of international agreements and political traditions, which are not very deep.



I advanced with the critique, but the old masters, who had published the autochthonist collection, felt threatened. They thought that the solid figures of the Leningrad archaeological school stood behind my words. That is why the autochthonists organized a conference in Kiev, where I was not invited. They put forth a series of heavy accusations against me and gathered a list of their serious adversaries standing, as they supposed, behind me – beginning with my teacher Professor Artamonov. Having read this list with my name first, foreign reviewers reprinted it as the list of the most venerable Russian archaeologists holding a different position as compared with the autochthonists. Probably they perceived my name as belonging to an old archaeologist, who, by chance, had dropped out of their attention. I became a ‘venerable archaeologist’ from the very beginning of my career!

My opposition to the use of archaeology for political ends pushed me to elaborate methods of archaeological argumentation and estimation of factual evidence independent of subjective drives. I had to have something essential in my hands to oppose such tendentious constructions. This was the stimulus for me to venture into theoretical research. I started to investigate theory in order to find good methods.

My opponents held archaeology to be a part of history and used to say that historical materialism is not only the theory of history but also the only true theory of archaeology. And historical materialism was minutely elaborated in Soviet philosophical and historical institutions as an instrument to serve the political ends of the regime. With it, they could prove everything they wanted. My first intention to lead archaeology out of history stemmed from the necessity to free it from the rigid chains of historical materialism.

### **Marxism, new archaeology and hermeneutics**

*You just spoke about your Marxist mask. What is the role of Marxism in your theoretical thinking?*

I always disliked the Soviet regime and only for a short time in my youth Marxism was my sincere belief, all the rest of the Soviet time it was only a necessary mask for my three faces, especially for my Russian face. However, this situation concerns completely only the political and economic sides of Marxism. Its philosophical and methodological implications, i.e. materialism, atheism, some points of dialectic, belief in progress, were actual for me, although not all-embracing, and they still remain strong in my personal philosophy.

By the time the new archaeology emerged, I was already very critical toward Marxism and saw its many drawbacks as well as the naive mistakes of Marr and the early Soviet archaeology. So the new archaeology came across as a breath of fresh air and a useful innovation of theoretical implements of archaeology, but at the same time I observed some likeness between the new archaeology and early Soviet archaeology with all its disadvantages: the direct correlation of material culture with society, the optimistic belief that the whole of past life can be reconstructed only on the basis of archaeology without recourse to ethnography etc. Nevertheless, the new archaeology attracted me

and impressed me with its enthusiasm and capabilities and especially with its love of theory.

Naturally, I met post-processualism with the same scepticism and dislike as did Renfrew and Binford, but later, as I was acquainted personally with the charming and talented Ian Hodder, he attracted me. Still later, I discovered that in my own works (*Sources, Typology, Printsipy arkheologii (Principles of archaeology, 2001), Metaarchaeology*) there were some postmodern ideas: appreciation of history, understanding of the diffuse nature of main concepts, the importance of intuition in theory etc.

*Some of these elements sound frankly hermeneutic. What is your relation to hermeneutics?*

Hermeneutics was born from theology under the Enlightenment's strive to liberate oneself from jaundice. The first task was to unseal the subjective position of a text's author. The next step was the neo-Kantian division of disciplines into natural sciences and humanities. For Heidegger, the task was rather to understand life than text, for Gadamer rather the self than life, and for him the jaundice became the prerequisite for understanding.

You know that I lay stress on foreknowledge in building typologies, and look at my scheme of theory-making, with the individual mind in the centre, etc. These ideas can be considered hermeneutical, but they are merely particular ideas which seem fruitful to me in particular cases. I cannot consider my entire position as hermeneutical. Hermeneutics is called so by Hermes, the god of slyness and hidden thought. As to me, I prefer clear and frank thinking. I search to anatomize even the humanistic *Verstehen* itself.

*Should the building of archaeological typologies be seen as such an anatomization of archaeological contexts or Verstehen?*

Yes, the way to the anatomization of contexts lies in typology. *Rethinking archaeology* (1967) by K.C. Chang and *Invitation to archaeology* (1967) by James Deetz as well as Binford's and Clarke's works and spatial archaeology showed the path into this direction.

*You have criticized post-processualism for relying on vague metaphors such as 'material culture as text', but you have yourself used the concept of 'the language of things' to describe archaeology's task. What is the difference?*

When the former generation of archaeologists, like Gorodtsov or Zhebelev, compared artefacts with text, they simply meant that to understand artefacts is as simple as to read a book, one must only be literate. When new archaeologists spoke of 'reading off' human patterning, they implied one-to-one correlations between patterns in material culture and patterns in social behaviour. When post-processualists consider 'material culture as text', they imply their own understanding of text perception allowing the researcher to see in the text every time and for every reader a new content depending on his abilities and intentions, on his society and demands of the time; practically he reads what he wants to. When I speak of 'the language of things', I imply that information contained in the record can be perceived and elaborated by an archaeologist, who must comprehend the language of things, i.e. he must

be skilled in archaeological methods of working with such data. The scope of information obtained, of course, depends on his abilities and preparing in the frames of the social environment and time, but most of all, on the amount collected from the record and on the distortions and losses during the time past. So it is limited both by the nature of the archaeological record and by the methods and criteria used.

*Western neo-Marxism has been very significant for contemporary thought and has profoundly influenced post-processualism. How do you see neo-Marxism from a Russian point of view?*

Western Marxism is for me the same Marxism, only devoid of some of its components, but lacking new components. Among the components it lacks are those I most appreciated in Marxism. So I see nothing interesting in neo-Marxism for archaeology. Since my relation to French structuralism is sceptical its fusion with Marxism doesn't impress me either. I recollect the apt phrase of Ernest Gellner who told a story about the wedding of two young lecturers in Cambridge. At the wedding the head of the college was present and he quietly remarked to a colleague: 'I slept with both and could not recommend one to another.'

This does not mean that some of the neo-Marxists would not have sound standing. Louis Althusser, for instance, had plenty of reasonable ideas – about the independence of basis from superstructure, about science as separated from ideology. But these ideas are not Marxist in nature.

*Das Phänomen der sowjetischen Archäologie (1997) is a study of archaeology in the Soviet era. What is the situation of Russian archaeology and archaeological theory today?*

Since the publication of my paper of 1994 in Madrid and my last, German version of *Phenomenon* (1997) little has changed in my opinion. Archaeology gradually adapts to the situation of the discipline in a capitalist country, but capitalism in Russia is unfortunately not of the Western European type, but rather of the Latin American type. Financing for archaeology is scarce, many archaeologists are forced to change their profession; some of them escape to business, others to politics. There is an intense process of decentralization. The scholarly level of excavations and dissertations is essentially lower than in Soviet times. The libraries continue to subscribe only to the most essential books and periodicals. Salaries are very low compared with the Western ones, and thus archaeologists can go abroad only if paid from foreign sources.

At the same time, it is now much easier to have works published; that is, if the work is popular and exciting enough or if you have enough money to publish it. There are plenty of Russian publications and translated books of various quality. Nobody cares for 'ideological purity'; no works are banned or suppressed. Theory is in a state of neglect, and I have the impression that I am the only one who still works in archaeological theory here, in a state of isolation.

### Observing contemporary archaeology

*Isolation notwithstanding, you are still most famous for your two panoramas of Anglo-American archaeological theory: A panorama of theoretical archaeology (1977) and Theoretical archaeology in the making. A survey of books published in the West 1974–1979 (1990). What were your motivations for writing them?*

Only in the English-speaking world am I famous for the two *Panoramas* – they are almost unknown in Russia. They were not solely devoted to Anglo-American archaeological theory, but they embraced also German, French, Polish, Scandinavian, Czech and Russian theory. To gather them and reveal general trends was my aim. That Anglo-American theory dominated the scene was just the reality.

As I spoke several languages and had experience in theory, I thought myself to be well prepared for such a work. I wanted to transform this into a permanent activity. One person would not be sufficient for such a task, and I suggested organizing a body of observers. However, my arrest in 1981 ceased this activity of mine. Now such panoramas have become standard practice in edited volumes or specific problem surveys. It is a pleasure to think that my *Panoramas* were the beginning and that they coincided with the beginning of theoretical archaeology as a special branch of the discipline in the middle of the 1970s.

*How were they received? Tilley has described them as ‘neither-nor criticism’ (1990, 60). For you, was it a matter of presenting and commenting or engaging?*

In Russia both surveys went unobserved, but in the world they created a broad and positive response, as reflected in Trigger’s review in *Antiquity*. Of course they were criticized as well. As to Tilley’s estimation, I do not know if it can be seen as the failure of survey. I tried to see and mark in every work under my observation both its positive contribution and its defects – not in order to draw an average or balanced estimation, or to keep objectivity, but for my readers’ practical use. I didn’t see my role as a judge or a referee, but as a guide. My task was one of structuring trends and the state of problems. In this work, my own views on archaeology and the ways of solving problems were not to be expressed in detail, although I think they were reflected in my criticism ad hoc. True, critical theory requires each researcher to present his own theoretical fundamentals as they may influence his conclusions, but this is not my theory. When it comes to the future of archaeology, I expect most decisive steps from research on artificial intelligence. Adapting archaeological description and research procedures to computer intelligence leads to the elaboration of an exact language for archaeology, while the new structure of computer intelligence with the contradictive principles as the initial base will lead to a better simulation of human intelligence. This in turn will lead to an intelligence, which is almost natural but more exact, objective and free from emotions and engagement with social movements.

*Many of your statements like 'in general, progress and evolution exist' (Taylor 1993, 734) would nowadays be criticized. On what grounds do you defend such positions?*

I would defend them simply because they do exist. You will indicate many respects in which the human situation became worse, not better. I shall show you the progress of technology, knowledge, and even the growth of human population etc. You will indicate some sides of human life where progress cannot be measured at all, as in arts. I shall stipulate that even in arts progress can be measured etc. Here our debate begins to remind me of the quarrel between Ostap Bender (the hero of the famous Ilf and Petrov novels) and Catholic priests – he says, 'God does not exist'; they answer, 'How come He doesn't when all living is created by Him?' – 'No, there is no God, that is a medical fact' etc.

Even the avowed particularists acknowledge now that there are general lines of evolution and progress somewhere and sometimes in the world, and even the most extreme evolutionists now admit that there are regressive movements and many kinds and lines of evolution etc. Thus the difference is in the importance ascribed to this or that side, and this is the point of faith.

*Currently, there are quite harsh debates between social scientists and socio-biology or evolutionary psychology. Does archaeology have a position in this debate?*

My attitude to social biology and evolutionary psychology is not as sceptical as yours. For instance, in my paper 'My kroman'oncy' (We the Cromagnons, 1996) and in some other works, I consider many psychological and social traits of contemporary people as biological inheritance from the time when these traits were adaptive for mankind, from the time when they were formed, from the Palaeolithic. Look at sports, wars, defence of territory etc. When I was among criminals at a hard labour camp, it seemed to me that I entered into a prehistoric world with chiefs and castes, tattoos, initiation rites etc. I suppose that the mentioned disciplines can offer to archaeology a perspective of interpretation, but archaeology can certainly give much to both of them.

### Archaeology and ethics

*You have said that the responsibility of archaeologists is to follow the methods and procedures of archaeology (Kristiansen 1993, 189). However, some post-processualists state that the very claim of objectivity and value-free archaeology are a part of an ethnocentric and androcentric project, so that ethics are fundamental to archaeology. What is your view?*

I am decidedly against the distribution of the ownership of the past, no matter whether in archaeology or history. For a researcher, the past is one and archaeology is one. It is international. You cannot study someone's past while permanently separating the object from the surroundings and environment.

In general my relation to nationalism, feminism and nativism is negative. Usually they bring nothing good to the masses of people, and the basis for each of them is not only the economic situation but too often the self-interested

position of some elite. There are elites in the feminist movement, nationalist fractions and native tribes.

Of course, I am for equal rights for everyone, but one should not take it to absurdity. If I am old, I cannot pretend to compete with youth in many respects – say, in sports and sex – like they cannot compete with me in experience and erudition. If I am a man, I cannot do some things that are done by a woman. I cannot bear or breast-feed a child. If I were a woman, I would accept that a man can do some things better and I would not compete with him in these respects – the point is in which. However, when in British archaeology a quota applies for employing women, I consider this as a violation of the principle of equal rights and a neglect of the interests of archaeology. The unjust representation of women in employment should be corrected in different ways. When Anglo-American colleagues write ‘he(he)’ or ‘he/she’ or even ‘s/he’, I cannot avoid seeing it as a ridicule and childish play with words. Why not then add ‘it’ for children or sexual minorities? The last step would be to take the fusion ‘s/he’ and add ‘it’ . . . In Russia we know that the grammar was formed long ago, it has its own traditions and relative meanings, including conventional gender. Nobody demands correcting the German *das Mädchen* and convert it from neuter into feminine.

The same situation is with the conventional names ‘Negro’ and ‘Indian’. If one is to rename them, why not rename America and Australia – these terms are not native and, additionally, one of them is incorrect. The situation is very Victorian by its nature. It is not the names that should be changed but the attitude to them.

*I would argue that since the meanings of words are always beyond one person's intentions regardless of how innocent or neutral they may be, some words should not be used if they are thought offensive by the persons who they are referring to.*

If some nation dislikes its designation, it can change its name in its own language and territory, but it cannot demand other nations to make changes in their languages. In Russian the former self-name of Jews, *zbid*, eventually became odious and was replaced by *evrey*. *Zbid* began functioning only as a swear-word, but in Polish it retained its general meaning, and nobody has demanded banning it.

When natives claim Palaeolithic graves as their own, forbidding their excavation and even their salvation, I would by no means follow their rules. The duty of archaeologists would be to delicately explain the truth and act from the point of view of civilized mankind. I would say to the natives, ‘Sorry, but this is not your property and not your competence, these monuments belong to the whole of mankind. We respect your feelings and are ready to help you to acquire knowledge and techniques, which will give you an understanding that we are right. Perhaps then you could produce your own archaeologists but, as yet, you have to let specialists to do their work, even if they are from another country. This is quite similar to having to let qualified physicians do the healing instead of your sorcerers.’ This is the position of ethics in archaeology. The extreme exclamations of some post-processualists are merely populist games.

## Homer, homosexuality and anthropology

*Recently you have gone back to your original discipline: structural philology (Klejn 1994a, 1998). Was your turning to Homer influenced by dissatisfaction with archaeology?*

I haven't gone back and by no means because of dissatisfaction with archaeology. My intervention into Homeric studies was produced by some questions raised by considering archaeological materials of Ilium and some incompatibilities in their traditional interpretation. I was always dissatisfied by the incongruity of the real ruins with the description of the city in the *Iliad*, in Hittite sources, etc. Events in my personal life allowed me to realize my Homeric studies; after my detention, I was deprived of all my titles, job possibilities and ties with archaeology. I was looking for some literary activity or study in addition to chance jobs, and so I came across my old ideas about Troy and Ilium.

My structural approach to the *Iliad* was also influenced by Propp and can be distinguished from other structural approaches by its orientation to the historical roots of a source. Like in Propp, my structure led to a stratigraphy of the *Iliad*. I have approached the epic with the eyes of an experienced archaeologist and searched for opportunities usually not observed by philologists. My eye at once isolated some types or typical formulae, patterns, repetitions and correlations. Eventually, my book was a response to Hodder's *Reading the past*, something like *Digging the text*.

*Another recent publication of yours, Drugaya lyubov'. Priroda cheloveka i gomoseksual'nost' (Another love. Human nature and homosexuality, 2000), is an anthropological study on the nature of homosexuality, and your monograph titled 'Istoriya antropologicheskikh uchenij' ('The history of anthropological teachings', 2003) is currently in press. What led you to these anthropological issues?*

These books have different origins. *Another love* resulted from my arrest in 1981 and the accusation of homosexuality, which was illegal in the USSR at the time. Although the process was evidently initiated by the KGB, I had some successes in not admitting the accusation and struggling for my discharge. The investigation, process and the law itself were connected with so many iniquities and unreasonable arguments that I decided not to leave this problem after my liberation. First of all, I wrote *Perevernutyj mir (The world turned upside down, 1993)*, a book about my personal adventures: arrest, investigation, court trial, prison, hard labour camp and the participation of the KGB. The work was followed by a large discussion, and my former investigator admitted in the press that he was forced to fabricate my case. The book sold more than half a million copies and was translated into German and Slovenian. This is where I made a detailed comparison of criminal with prehistoric society.

Thereafter, knowing how my co-prisoners suffered there, I wrote *Another love* on the nature of homosexuality in the frame of nature of man. Since two editions were sold in one year, the publishers demanded and received the next volume consisting of biographies of twenty famous Russian homosexuals



**Figure 2**

(Klejn 2002). Currently I am writing the third volume devoted to foreign biographies. These books can have a burning significance today, since the restitution of the law banning homosexual relations is being discussed in public and in the Duma, our parliament.

*What is the main argument of Another love?*

The main idea is that such love is in the nature of man, for whom it is usual to develop artificial unproductive varieties, like in gastronomy, clothing, architecture etc., as natural expressions of life. Most interesting are bisexuality and multiple trips of heterosexuals into homosexual behaviour. The roots of homosexuality are partly genetic, partly hormonal and partly social. The best solution for heterosexuals is not to take note of homosexuality and for homosexuals not to irritate heterosexuals. I don't know to which extent it is correct to call this work anthropological. Probably it is.

My *History of anthropological teachings* is of another origin. In Soviet times, cultural anthropology was banned like a number of other disciplines, so when in the new Russia it was allowed, there was a shortage of experienced teachers. As I had been participating in *Current anthropology* and was well read in this discipline, and theoretical archaeology and folklore studies are near to cultural anthropology, I was invited to give courses in this discipline at the University of St Petersburg and the European University in St Petersburg.



After that, I was invited to the universities in Ljubljana, Cişineu and Seattle to present the Russian view on the problems. Hence the book.

### **Influence and future**

*Some Russian archaeologists identify themselves as your pupils, and there is said to exist a school of Klejn. Do you think that there is such a school?*

The clear distribution of archaeologists in schools and trends is a specificity of Russian archaeology probably maintained by the custom not to change the place of studies or student's personal mentor during all five years of education, or the place of work during one's life. Everyone knows who are his personal teacher and pupils, what is his scholarly pedigree. The word 'school' as applied to scholars has two meanings: the list of pupils of one person and the community of like-minded confederates. In the first meaning my school is rather small; for a long time, I could not have pupils at all as I had no high titles or rights to direct theses. But in the second meaning my school is bigger but more diffuse. Who has many ideas has many pupils. By lecturing and tutoring students in expeditions and continuing friendship with many of them after their student years, I practically directed – or rather advised them on – many of their theses. Especially important in this respect was my long-standing Problem Seminar at Leningrad University. The members of that seminar built a solidary community held close together by common beliefs, common tasks and common dangers.

In the Russian edition of my *Typology* several chapters were written by my pupils (they are indicated in the list of contents). Even my occasional students were engaged in my work. For instance two listeners of my course in Copenhagen came to me thereafter to St Petersburg and helped me to transform this course into a book (Soeren Sindbaeck in content, Ian Simpson in language), and I am very sorry that the editor of *Metaarchaeology* omitted my Introduction where my gratitude to them was expressed.

*What do you regard as your greatest accomplishment in archaeology?*

It is difficult to make a sound self-estimation even if one is at the end of one's life.

The historian of Russian archaeology, A.A. Formozov, has placed me in a small handful of Russian archaeologists who in the post-Stalin period dared to challenge the Soviet regime and stimulated the development of Russian archaeology into a democratic and international future. I think this is reasonable. My defense of migrations and especially one of them – defense of Normanism – had a special importance in this respect. In general, I tried to break the borders and broaden the horizon of Russian archaeology to global size, and to acquaint the Western world with Russian archaeology.

Secondly, I was participating in the making of theoretical archaeology as a special branch of the discipline and as a world community. Renfrew marked me as one of the few European archaeologists who could respond to the challenge of the Americans.

In theoretical studies, I have systematized various conceptual attitudes and introduced them into the general frames of theoretical notions of science.

I have discovered the cardinal difference between classification and typology as well as the importance of foreknowledge for the building of workable typology. Studying the system of the main principles of archaeology, I have found the need to split it into two controversial systems. This may also appear important for other disciplines. I hope that my idea of communication theory as applied to cultural evolution will find its development in anthropology and archaeology.

Some friends said that my discoveries in Homeric studies are comparable with my accomplishments in archaeology. The highest estimation came from the most authoritative Russian historian, the late Igor Dyakonov. It convinced me that there are some reasons behind these friendly compliments. He wrote that my discoveries are considerable and irrefutable, and that since them Homeric studies must turn to a different way. One of the books has been translated into English but a publisher has not yet been found for it; the other exists only in Russian.

*What are you currently working on?*

At my age, it is risky to make great plans, but I nevertheless do. There are two manuscripts on my table: *Time in archaeology* and *Rebirth of Perun* – a history of East-Slavic pagan religion. They are ‘almost ready’, but still need much work. I have also written half a book on *The time of Centaurs*. It deals with the restored ancestors of both Greeks and Aryans as expressed in their mythology and art compared with Ukrainian Eneolithic. I also have a dream to write a history of the world’s archaeological thinking. Although the necessary materials are already gathered, I need one more life, you see. I really crave for having it.

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