interview

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In conversation with Leo S. Klejn

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Abstract

This interview discusses the comparison that Klejn draws between archaeology and forensic science. This is a comparison that has been made many times previously by many different archaeologists (and crime writers), but Klejn is unusual in that he explores the implications of their similarity. In his view it is archaeology rather than history that is most closely related to forensic science. The difference between archaeology and history is important because when this difference was ignored, in the Soviet Union, it was to the detriment of both disciplines. Hence Klejn's continued concern – that the difference between archaeology and history is still, to some extent, ignored. Other subjects discussed include Klejn's view of the role of theory in relation to practice, his criticisms of New Archaeology and postprocessual archaeology, typology, the underlying principles of archaeology, ethnogenesis and the history of archaeology. Of course, in an interview of this length not all of these subjects are covered in great depth but it is hoped that enough is said that the reader may appreciate something of the nature and originality of Klejn's views.

Keywords

forensic science; New Archaeology; postprocessual archaeology; typology; Russian archaeology

The following interview is a compilation of just some of the many questions and answers that have passed back and forth – both face-to-face and via email – between Leo Klejn and myself, between 2009 and the present day.

Klejn is fully aware that the views that he presents as an archaeological theorist are out of step with those that hold sway in the West, but he has long been accustomed to swimming against the stream, first of all against the mainstream of archaeological theory in Soviet Russia and latterly against the mainstream of Western archaeological theory.

In the West, Klejn's is a name to be respected, but his theories have not permeated the consciousness of Western theorists to any depth. However, in the other direction, it must be said that Klejn is fully abreast of Western

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archaeological thought and has been since his student days. Indeed, before the fall of the Soviet Union it was largely through Klejn's work that other Russian archaeologists were made aware of developments in archaeological theory in the West.

Of all Russian archaeologists, Klejn is probably the best known in the West, at least by name. Yet only a fraction of his work has been published in English. In this interview, I concentrate upon Klejn the archaeological theorist, but it should be said that Klejn is not only an archaeological theorist. His work has covered many different areas: he has directed excavations on the Russian steppes, and he has written on the pre-Christian religion of the Eastern Slavs, on the composition of the *Iliad*, and about the history of archaeology – about Gustav Kossinna and about attitudes to the Vikings in Russian archaeology. His criticisms of archaeological orthodoxy (in combination with his Jewish ethnicity and his popularity with archaeology students at Leningrad University) led to his incarceration in 1981–82, an experience that had a profound effect upon his view of the relation between human nature and culture – another subject about which he has written extensively. However, as mentioned, as interesting as all these subjects are, the present interview concentrates upon theoretical archaeology.

Perhaps Klejn's most alien idea – to contemporary Western theorists – is the sharp distinction that he draws between the disciplines of history and archaeology. Klejn insists that archaeology is a source-studying discipline – in Russian there is a specific word for this – whereas history is not. In this respect, Klejn argues that archaeology, much more than history, is conceptually akin to forensic science. It is upon just this contentious subject that the interview begins.

Archaeology and history

How, in your view, are archaeology and history related? Does the archaeologist process the remnants of past events so as to be able to hand over to the historian a basic account of what happened?

Something of the kind. Archaeology is not confined to any particular period of time: prehistoric, classical, medieval. The archaeologist translates the data from the language of things into the language of words. He reconstructs things and events from the evidence of traces and remnants. Without the skills of the archaeologist, any reconstruction would contain unnoticed distortions. But, although he is skilled at reconstructing artefacts and events, the archaeologist should not trespass into the historian's territory.

History is a different profession. The difference is that the archaeologist tries to answer the questions 'what?' 'where?' 'when?' and 'how?' whereas the historian tries to answer the question 'why?' In other words, archaeology concentrates mainly upon 'what happened?' whereas history concentrates upon 'why did the event(s) happen?' and 'what were the consequences of them happening?' In this respect, the archaeologist is like a detective.

But not the historian?

I would not stress that the historian is *not* comparable to a detective. You see, in practice the historian is often a combination of two different sorts of

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Figure 1 Leo Klejn (photograph by Julie Lisnyak) (Colour online).

researcher: one dealing with source studies (comparable to a detective), the other dealing with the historical process (comparable to a judge). But although this often happens, nonetheless there are two different research programmes: there is the archaeological and there is the historical.

But surely archaeologists sometimes ask 'why?'
Admittedly there is a 'why?' element within the work of the archaeologist, but it is ultimately subordinate to the question of 'what happened?' For instance, the archaeologist might ask, 'why is this artefact fragmented in this way?' or 'why was this pot put into this pit?' yet not 'why did this culture move in this direction?' Likewise, the detective might ask, 'why did this purse appear in this pocket?' but not 'why did this man become a thief?'

Many archaeologists would see these ideas as supporting the 19th-century view that 'archaeology is the handmaiden of history'.
Yes, I have, of course, encountered this concern. Archaeologists are afraid to lose the prestige of their profession. They want to participate in solving the great problems of sociology and history directly. But we must not be misled by metaphors. Yes, archaeology serves history – but it is not dominated by it. The Ukrainian poet Taras Shevchenko called archaeology the 'mother of history'. Handmaiden or mother? The connotations are different but the sense is the same.

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Where does the archaeologist get his or her questions from?
From history, from sociology, from philosophy, and from general curiosity. Archaeologists have many varied motivations.

Are you claiming that archaeology deals with unwritten sources and history with written sources? Is this the principal difference between them?
Not at all. This is the proposed difference between history and prehistory, and as prehistory is equated with prehistoric archaeology (both in England and in Germany) the difference is misconceived as also being the principal difference between history and archaeology. A conceptual confusion has arisen, such that when I say that archaeology is the handmaiden of history it is often assumed that what I am claiming is that archaeology deals with unwritten sources and history with written. But this is not the case. I group archaeology together with numismatics, textology and toponomics. These subjects are conceptually alike in that they all prepare a set of data for the historian and the prehistorian. For, likewise, history and prehistory are also conceptually alike – they synthesize the data from various sources. The important difference is between, on the one hand, history (and prehistory) and, on the other hand, archaeology.

Then what is the conceptual difference between history and prehistory? Is it simply that history deals with written sources and in prehistory these are absent?
Not such a small difference, but even this is only an outer attribute. The conceptual difference is that history deals with man and society, and prehistory embraces the time when these phenomena were being made, i.e. the beginning of their formation. To be precise, prehistory stands between history and evolutionary biology (I should add that in Russia no distinction has ever been drawn between classical and prehistoric archaeology). But, to reiterate, archaeology is not synonymous with prehistory.

Are you unique in taking this position?
Not quite. The division between archaeology and prehistory is exemplified in the West, too, for example in the work of Graham Clark in England and Irving Rouse in the United States, who divides academic subjects into analytic disciplines (like archaeology) and synthetic disciplines (like history and prehistory). And, of course, I have some adherents in Russia, for example Dr Leonid Vishniatsky of Saint Petersburg, author of *Introduction to prehistory* [2003]. Vishniatsky is a specialist in the Palaeolithic, but he was formerly my student, especially in theory.

So, what, in your view, is there left for the historian to do?
The historian uses sound reasoning, imagination and life experience to establish causal connections. Sociology (including historical sociology) extracts laws from the facts. History uses the laws in order to understand facts and their connections. Sociology elaborates a mass of facts, and facts are interchangeable for it. From many facts, a sociologist infers one law. The historian studies the interaction of many laws upon one fact. He studies also the interaction of law and chance so as to try to reconstruct the ways in which artefacts were originally used – their original context. He is concerned

with the individuality of facts. It is this that makes history an autonomous discipline, and in part a humanistic discipline. History is necessary because, against von Ranke, the facts do not speak for themselves – chronicle is not yet history.

What is your reaction to the postmodern claim that appreciating the role played by context undermines the possibility of objective research?
I am critical of this claim – although of course as a historian I am keen to emphasize and to explore the role of context.

And the archaeologist differs from the historian in this respect?
By contrast, the archaeologist's task is to reconstruct artefacts and, at a rudimentary level, past events. Like a detective, the archaeologist reconstructs past events but he has no deep interest in why they occurred. As such, archaeology is not a humanistic discipline, but nor is it a pure (fundamental) science: it is rather an applied science – like forensic science. If an archaeologist is a detective, a historian is a judge.

Forensic science relies upon the idea that a focused accumulation of data will provide us with reliable and useful knowledge. Is this also the case for archaeology?
Why not?

Can we reach certainty in archaeological investigations?
We may never reach absolutely certain knowledge but we may progress along the road towards it. Good scholarship will guard against the influence of ideology and politics and so on.

Is there any discipline that is wholly humanistic?
Perhaps the appreciation of art or philosophy (if the latter is not a special kind of knowledge – neither scientific nor humanistic).

Is it your view that archaeology is the methodology of history?
Not at all. What a strange idea – rather like claiming that physics is the methodology of chemistry. Not at all. To reiterate, the difference is simply that the fundamental task of archaeology is to answer the question 'what?' whereas the fundamental task of history is to answer the question 'why?'

So in your work analysing and distinguishing the various components of the Iliad *– your philological work – you worked, in effect, as an archaeologist?*
In essence, that may be correct.

Is the distinction between history and archaeology the central motif of your work?
No. I have had many tasks and interests: the struggle for objectivity in history, for a better social and political situation for scholars, particular studies of ancient cultures ... But it was, and is, one of the main motifs.

Why is it so important to determine the precise relationship between archaeology and history?
Establishing the relationship between archaeology and history has a practical repercussion with respect to the organization of university departments. But

also, without this sharp distinction, the best elements of both the study of archaeology and the study of history will deteriorate.

This happened in the Soviet Union, where history was considered the main vehicle of Marxism. History and archaeology amalgamated, with history predominating, and for many years publications of reconstructed history appeared instead of publications of materials [see Klejn 2012b]. This is a danger that we must continue to guard against. In the West, prehistory and prehistoric archaeology also amalgamated – for different reasons (to bestow archaeology with high historical aims) – with the consequence that both historical aims and source study are neglected. The source of the danger is different but the danger itself is the same. This should not be seen as a 'purely academic' debate.

But you yourself are an archaeologist and a historian?
That is true – and an anthropologist and a philologist, etc. This objection is often raised against me. I have written works on archaeology, philology, history and anthropology. And I have written poetry, played music and made drawings. But that it is in principle (and in practice) possible for one person to embody several different professions does not transform these activities into one. It does not imply any contradiction in my argument: I maintain that archaeology and history are separate disciplines.

Archaeological theory

What is the task of archaeological theory?
That is a good question. It should be asked more often, especially among archaeological theorists. The task of archaeological theory is to analyse the methods employed in what are commonly agreed to be the best examples of archaeological practice and to make these explicit. In this way, archaeological theory describes a programme for extracting information from artefacts (using some explanatory idea). In fact, in all sciences, theory is a programme of information-processing based on some explanatory idea. Theory, thus conceived as stereotyped operation, appears to be method [Klejn 1993–94].

Most other archaeological theorists prefer to concentrate upon the historian's questions of 'why did these events happen?' or 'how did this process occur?' or 'what laws are the moving forces of these processes?' but this is not archaeological theory. I prefer to concentrate upon information-processing. By reflecting upon the best methods of information-processing, archaeological theory can itself play a part in the processing of information. We should not think of archaeological theory as interpretation in the abstract, nor is it another word for speculation, generalization or the formulation of laws; it is rather the means, basis and logic of work.

My view on the role of theory in archaeology may be made clearer with the help of a diagram [figure 2] taken from my *Meta-archaeology* [Klejn 2001a, 70]. This diagram should simply be read as expressing the idea that theory has the same relation to metatheory as object has to theory, and conversely theory has the same relation to object as metatheory has to theory, and so on.

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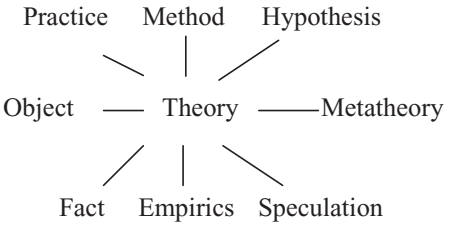

Figure 2 Theory in oppositions.

Are there other theorists interested in the formation of the archaeological record?
There are not many. An exception is Michael Schiffer, but he has of late turned away from archaeology. Schiffer was Binford's pupil. He quite rightly focused attention not on cultural processes (the historian's and anthropologist's field) but upon the formation of the archaeological record. This was the main idea of his *Behavioral archaeology* [1976]. I did not agree with everything in *Behavioral archaeology* but I approved of its focus. Both Binford and, to a lesser extent, David Clarke were moving in this direction. This change of direction, whether it was admitted at the time or not, marked the end of New (processual) Archaeology.

The formation of the archaeological record was the main theme of my *Archaeological sources* [Klejn 1978]. Yet my inspiration was not Schiffer but the German archaeologist Hans-Jürgen Eggers, in particular his works of 1950 and 1959 on the 'inner' criticism of archaeological records. This idea had previously only been applied to written sources.

You draw a distinction between archaeological theory and theoretical archaeology. What is the difference?
Archaeological theory is ultimately part of archaeological methodology – it describes a programme for extracting information from artefacts. This means, of course, that there are many different archaeological theories. Theoretical archaeology is that branch of archaeology that embraces them all. I invented special terms for branches of theoretical archaeology. It can be divided between endo-archaeology, meta-archaeology and para-archaeology. Endo-archaeology concerns those theories that deal with archaeological objects – theories of classification, typology, migration recovery, etc. Meta-archaeology concerns the nature of the discipline of archaeology (as an object) itself. Para-archaeology concerns theories developed outside archaeology but used within it, for example theories about culture and ethnicity.

New Archaeology and postprocessual archaeology

Do you feel closer to New Archaeology or to postprocessual archaeology?
If I had to choose I would say I feel closer to New Archaeology.

Did you ever call yourself a New Archaeologist?
No, I never called myself a New Archaeologist: it was for me always a school that was distinct from my own position. It was a school

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towards which I felt some sympathy but I had criticisms of both New Archaeology and postprocessualism. Whereas New Archaeology orientated archaeology towards anthropology and sociology, postprocessualists blurred the distinction between archaeology and history: they were less interested in material remains than in semiotics and the meanings of symbols. They were also less interested in formal arguments than in intuition. Now this trend is in decline and a new variety of evolutionism has arisen.

What is your opinion of evolutionism?
My *History of archaeological thought* [Klejn 2011] has several chapters devoted to evolutionism. I have numbered the different versions. The first evolutionism was manifested in the work of de Mortillet and Pitt Rivers, and their efforts to follow the teachings of Tylor and Morgan. The second evolutionism, neo-evolutionism, consisted of the work of Gordon Childe, Leslie White, Julian Steward and their pupils, and Braidwood. The third evolutionism – the present trend – consists of the work of Robert Dunnell, Steve Shennan and others. It is important, when we talk of evolutionism, not to confuse these three varieties. I think the present evolutionism offers great hope for the future of archaeology.

Has it fulfilled anything of its promise in terms of books produced?
I believe it is beginning to fulfil that promise – in the work of, for example, Steve Shennan.

What were your main criticisms of New Archaeology?
New Archaeology failed to pay enough attention to the formation of the archaeological record. Archaeologists have to deal with a twofold break. By this I mean that as archaeologists we are twice removed from the objects of our study – twice removed from being able to give a basic account of what happened. First, we are removed from them as from mute material objects while our inferences must be expressed in thoughts and words. Second, we are removed from them as from dead remnants in the contemporary world while they must be viewed as parts of the cultural life of the past.

Consequently we have to first assess what evidence there is and then interpret how this evidence has been distorted with the passing of time. For example, not all materials are equally durable. Actually, the archaeological record is always incomplete. This fact alone, if unattended, will create a distortion in our reconstruction. It was only by ignoring this that Binford and Clarke were able to transform archaeology into anthropology. New Archaeologists made the mistake of acting as if archaeologists study living cultures. They saw any apparent change in the state of a culture as having occurred due to some impact from the environment or other cultures whereas it might simply be a result of post-depositional processes.

They used, and discovered, processual laws – good – but they ignored other laws dealing with how past phenomena left a trace in the archaeological record. For example, ceramics will accumulate more quickly in the archaeological record than metal, because metal is more likely to be reused. On this point, I agree with criticism made by postprocessualists. I also felt that New Archaeology had a very impoverished conception of history.

If we are twice removed from being able to give a basic account of what happened, we cannot know the belief systems of past cultures to the same extent as we can know the belief systems of present-day cultures?
That is correct. This belongs to the same problem of the double break, a problem that distinguishes archaeology from other disciplines.

What is your opinion of Hawkes's ladder?
Hawkes was right.

What are your main criticisms of postprocessual archaeology?
In Lampeter, postprocessualists put it to me that it must be acknowledged that people in the past had some influence over their own environment and their will as individuals played a part in their actions. This I willingly acknowledged. And yet, I replied, you must also acknowledge that there remain laws. Yes, I concede that we, all of us, choose our own path and that there is, to some degree, agency, and that this is true for every individual, but the statistical laws remain.

Postprocessualists, influenced by Marxist critical theory, sought to persuade me that bias is inevitable and self-criticism should make us aware of this fact. I objected that my self-criticism would also be biased. What is the solution – to emulate Münchhausen and try to escape from the swamp by pulling our own hair? No. I proposed rather my own idea that scientific methodology must be constructed in such a way as to eliminate bias – if only we have the will to apply it.

Typology

Klejn claims that whereas history is interested in the individual nature of facts, the archaeologist is more interested in what is typical (the latter claim brings him closer to New Archaeology than to postprocessualism). It thus comes as no surprise that, as an archaeological theorist, Klejn has paid particular attention to problems of classification and typology.

As a historian of archaeology, Klejn has written (2010) about the pioneering work of the Swedish archaeologist Mats P. Malmer in this field, seeing him a predecessor of New Archaeology. But Klejn has also produced his own original work in this area, most notably in *Archaeological typology* (1982). However, at the time, as Klejn was imprisoned in the last major crackdown upon the intelligentsia in the Soviet Union, there was no opportunity to check the translation before it went to press and consequently the book was published in a poor translation.

What are the principal ideas of your book Archaeological typology?
As to *Archaeological typology*, I don't believe that anyone in the anglophone world will be able to fully understand it from a damaged and incomplete text. The Russian and German versions of the book are far superior to the English (there is, incidentally, a German version of the book that brings it fully up to date, but that book exists only in manuscript as there is still some dispute about its fate among its potential publishers).

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Archaeological typology has two principal concerns. The first is to distinguish between classification and typology. Archaeologists seem to use either term indiscriminately. My aim is to reform this state of chaos. I argue that classification is what Aristotle described as such, the rigid distinction of artefacts into non-overlapping categories – like the distribution of finds by the boxes in the cabinet. Typology, on the other hand, is the description of ideal objects to which real objects, in various degrees, conform – they are more or less 'typical'. Classification makes use of sharply defined borders, whereas typology makes use only of conventional borders. These are two alternative ways of ordering artefacts.

The second concern is with the way in which archaeologists arrange their evidence. The usual practice is to cut reality into the smallest possible particles ('attributes'), then to group them by correlation into higher taxa ('types'), and finally to group these into still larger communities ('cultures'). Yet in this way you cannot reveal the cultural functions of your taxa; you cannot distinguish cultural types among empirical types. Therefore I inferred that we should proceed in the reverse direction: that we should first have some hypothesis concerning the culture, and then work downwards from the types to the attributes.

But how to detect cultures if not through types?
The difficulty is, of course, how to have prior knowledge of cultures – but this is not an insurmountable difficulty. We have some evident types, we have some evident cultures, and so on.

Now let us pass to the evaluation of results.
As we know from the study of various practices, reality inevitably suggests different ways of grouping the same material. We must ask ourselves, which classification (or which typology) is better? Which works better?

And what does it mean to work better?
That which works better correlates with the other parameters of material. If our types, for example, are distributed in clusters at different strata and in different places on the map, we conclude that they are real, and conversely if they cover the map chaotically, most probably they are fictitious and the typology is a fiasco.

So which groupings are better?
The observation of practice shows that groupings tend to be better – that is to say, they work better – when they are built upon some anticipation of the results.

You take a Kantian approach to typology?
Neo-Kantian. There is some influence from Rickert and Windelband in my work.

The principles of archaeology

The position elaborated in The principles of archaeology *(2001b) and* Meta-archaeology *(2001a) is that at the foundation of archaeology there are two basic sets of principles. These principles can be grouped in two*

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rows with each element directly facing its opposite (determinism versus indeterminacy, universalism versus particularism, etc.). No one element within these oppositional pairs predominates in all circumstances over its opposite. Rather, in the face of every new problem, it is the archaeologist who decides which set of basic principles will predominate.
That is correct.

You mention in Meta-archaeology *that this idea parallels Niels Bohr's principle of complementarity. Did he inspire you?*
I can't now remember whether I took the idea from Niels Bohr or whether I arrived at the idea independently and then noticed a parallel in the work of Niels Bohr.

It strikes me as an idea of philosophical interest.
Maybe. But I tend to be only interested in the ideas of philosophy to the extent that they can be put to use in archaeology and anthropology. I am not greatly interested in philosophy for its own sake. Philosophy is not a positive science. It is like religion in that its hypotheses cannot be materially proven.

The idea that archaeology is founded upon a series of antinomies is itself of great interest, but in The principles of archaeology *you go on to connect this idea with another extremely interesting and controversial idea, namely that artificial intelligence might enact the process of archaeological interpretation. Can you explain this? I would have thought that artificial intelligence would be of limited use to any discipline founded upon intrinsically opposed principles.*
Artificial intelligence could certainly not do this in the same manner as, for example, providing a medical diagnosis on being fed the data of certain symptoms. But that is not to say that the task is completely impossible. In the most general terms, it might be envisaged that one virtual computer system would be based upon one set of principles, and another on the second set, with a third system arbitrating between the other two. This idea reflects the workings of our own brain with two opposing hemispheres and some balance between them realized in the medulla oblongata.

Ethnogenesis

Klejn has written a long article (2000) on Kossinna in which, whilst making a full record of Kossinna's mistakes and excoriating the use to which his ideas were put, he yet claims that there was a grain of sense in Kossinna's work. However, despite this startling claim, it turns out that Klejn's ideas in this area are for once actually broadly in line with those of most other contemporary archaeological theorists, in that he believes that the binding idea of an ethnos is the idea of a common origin – all else is built around this.

What is the grain of sense in Kossinna?
I think his greatest contribution to archaeology lies not in his 'solutions' but in having posed the problem of ethnicity. Before him archaeology was tribeless. After Kossinna, archaeologists began to draw ethnic maps of the past. These may have been wrong but the problem of ethnicity remains. Furthermore,

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he directed the attention of archaeologists toward the question of migrations from central Europe. This remains a promising focus of attention with respect to the origin of Indo-Europeans.

My concern is that in some discussions of ethnicity, perhaps in reaction to Kossinna, the criteria by which to substantiate migrations were such that it would seem impossible to give persuasive evidence that any migration had ever occurred. The problem was compounded in the Soviet Union by a politically motivated mistrust of anything but local origins. In particular, Nicholas Marr was influential as an advocate of autochthonous development. Questions about ethnicity were in fact, for Marr, quite unimportant (I have described how this mistrust of anything but local origins manifested itself in the attitude to the Norsemen in Russia in *The Varangian controversy*) [Klejn 2009; see also Klejn 1994]. This 'localism' was at its height in the Soviet Union in the 1930s, 1940s and early 1950s. It was in order to counter this trend that I introduced the concept of 'sequentions', which simply means sequences of cultures (as traced in the archaeological record). The main idea behind this concept is that although the material is initially given to us in *column sequentions* where cultures follow each other on the spot, we can nonetheless trace *track sequentions* where cultures follow each other independently of developments within a particular location. Migrations are implied.

My interest in this question, which goes back to my earliest days as an undergraduate, comes in part from Michael Artamonov, the director of the Hermitage Museum and my teacher in archaeology. He was an unusual figure in Soviet archaeology. He was interested in the study of ethnic migration at a time when Soviet ideology considered this to be an unacceptable subject.

Another teacher was, I believe, Vladimir Propp?

Yes, another teacher was Professor Vladimir Propp, a folklorist and a pioneer in structuralism and semiotics. He is known in the West. He was my teacher in anthropology, and in general my first teacher at university. I was his only student in archaeology. He became interested in archaeology but when I was his student it was too late for him to begin studying archaeology himself. Nonetheless, he recommended that I should study it. Propp's influence can be seen in my work on the Iliad, on the pre-Christian religion of the eastern Slavs and on archaeological typology, and in general in my wish to provide multifaceted definitions of the terms of a debate. Thus my early academic background is partly archaeological and partly philological.

The history of archaeology

A revised edition of Klejn's *The phenomenon of Soviet archaeology* (1993; Spanish edition 1993; German edition 1997) was published by Oxford University Press in 2012 as *Soviet archaeology. Schools, trends, and history*. Readers who do not know Russian have hitherto had very little information on this subject. They have had Bruce Trigger's account in his *A history of archaeological thought* (1989) and recently the English abstract of Nadezhda Platonova's *A history of archaeological thought in Russia* (2010), and A.L. Mongait's *Archaeology in the U.S.S.R.* (1959), but little else.

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What is your opinion of these works?
What a mixed group!

If I am asked whom I feel closest to among all Western archaeologists, I reply that I feel closest to Bruce Trigger, but of course when it came to the subject of archaeology in Russia Bruce had the disadvantage of not knowing Russian (in general, it should be said that native English-speaking archaeologists are disadvantaged by their often poor knowledge of other languages. In my opinion it is essential for all archaeologists to have some basic knowledge of foreign languages. Bruce Trigger had knowledge of other languages but not of Russian). Although we met only once, Bruce and I were in continual correspondence from when I first read his then newly published *Beyond history. The methods of prehistory* [Trigger 1968]. He was an inspiration behind both my *History of archaeological thought* and my *The phenomenon of Soviet archaeology*. Yet, for all my great admiration for Bruce Trigger's work [see also Klejn 2008], I have nevertheless written my own two-volume *History of archaeological thought*.

The same title! You believe it will supersede his work?
I hope it will at least be useful. It is, of course, influenced by Trigger's *A history of archaeological thought*, but although both Trigger and I are materialists, in my work as a historian I generally pay more attention than Trigger to the human characters of the main protagonists.

If and when this is published in translation I believe the English-speaking world will have no choice but to take notice of you. But I have interrupted you, excuse me. What do you have to say about the other authors?
Platonova's book, though not without faults, is a very thoughtful and clever work, but only the abstract is in English. As to Mongait, he grew ashamed of his apologies for Soviet archaeology – to the extent that he asked me never to refer to his earlier works. In his later years, he became increasingly critical. He died long ago – in the Soviet era. Yet it must be said that the general atmosphere of Soviet archaeology in the 1950s is accurately reflected in this 'parade-ground' book.

It is impossible for anyone to say how they will themselves be seen by future historians but a common role that you seem to have fulfilled is that of the independent outsider – the little boy who dares to say that the emperor has no clothes.
I think your comparison applies to Formozov rather than me. Two of my colleagues compared me in print with a 'progressor', a fictional figure of a man from a progressive society who appeared in a backward world. He possesses fruitful ideas but society is not ready to listen. Of course I hope that society will listen.

I am curious to know: when did you feel less isolated – and perhaps less of an outsider?
Formerly, in Soviet times, because I was surrounded by young supporters, but now most of them are dead, and there are few to take their place. But, you must understand, I am not someone who feels nostalgia for the Soviet era. Nor

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should my citations of Marx and Lenin be misunderstood. In the Soviet era it was accepted for all scholars to garnish their work with citations of Marx and Lenin. But both Marx and Lenin wrote so much that it was possible to select quotations so as to cite their support for almost any argument. In Russia we were very skilled at this. But to use Marxist ideas as a substitute for research is one thing; to use those ideas as an aid to understanding other societies is quite another. In the latter role – as a tool, rather than as dogma – Marxist ideas still have some worth.

Your pupils died early?
You must understand that in Russia, people – especially men – die on average twenty years younger than in the West, due to bad medicine, alcohol and smoking. Russians are some of the biggest consumers of cigarettes and alcohol in the world. And then there is stress . . . I myself, although I did not abuse my health, am surprised to have lived so long. I am seen as a relic from another age.

But you are a very unusual relic – vivacious and productive: in recent years you have written a great number of books – books that I believe sell very well?
I have said that I am seen as a relic but I haven't said that I feel myself a relic. I feel I am just at the beginning of the way: so much remains unknown, so much to do, so much to learn. I am not afraid to die, but it hurts me greatly that I will not see the new discoveries made after me and without me.

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