interview

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Intransigent archaeology. An interview with Evžen Neustupný on his life in archaeology *Martin Kuna**

Abstract
An interview with Evžen Neustupný opens up a range of issues regarding the theory and history of archaeology and its development in Central Europe. His view of the discipline differs in many ways from that of current global trends. His 'artefact archaeology' inverts the concept of *adaptation* and highlights the role of artefacts in the creation of the human world. The interview also shows that post-war archaeology even to the east of the Iron Curtain followed the trajectory from culture-history paradigm to processualism and onwards. It also testifies to the situation in the social sciences under the Communist regime and the ambiguous role of Marxist philosophy.

Keywords
archaeological paradigms; processual archaeology; history of archaeology; Czech Republic

Rarely does it happen that a person's name and character correspond so well that one wonders whether the name is for real or a nickname. In Czech, *neustupný* means intransigent, unyielding or pertinacious – and you will hardly find a more apt description of the character of our interviewee, Professor Evžen Neustupný. Were it not for his unyielding mentality, however, he would not have been able to take part in so many significant steps taken by post-war archaeology – despite the fact that for forty years he worked under the Communist regime and was not allowed regular contact with the outside world. His unyielding vision also eventually won over a large part of young Czech archaeologists who today approach their studies from a background in his concepts.

Evžen Neustupný was born in Prague in 1933. His father was the well-known archaeologist Jiří Neustupný (1905–81). Evžen studied prehistory and Egyptology at Prague University (1952–57). After his studies, from 1957 to 1966 he worked at the Most branch (north-western Bohemia) of the

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Figure 1 Evžen Neustupný in his home office.

Institute of Archaeology and belonged to the scientific staff of the Institute of Archaeology (a non-university research institution within the Academy of Sciences) in Prague from the end of the 1960s. From 1990 to 1993, he became its first post-revolution director and attempted, with some success, to imprint a radical new concept within its doors. Later on, in 1998, he founded the Department of Archaeology at Pilsen's University of West Bohemia, which grew significantly in the ensuing decade. He also took part in the founding of the European Association of Archaeologists in the 1990s.

Evžen Neustupný's professional development is best illustrated by his works. These gradually feature the periodization of prehistoric cultures (1956; 1959); new archaeological methods, primarily dating methods (1967a; 1968; 1969; 1970c; 1970b); the processual approach to prehistory (1971; 1976b; 1976a, 1982; 1983a; 1983b); the position of Czech archaeology in the European context (1991b; 1993b); and elements of his own paradigm (e.g. 1995, 1998b). This has been progressively elaborated, for example, in his *Czechoslovakia before the Slavs* (1961), the Czech version of which resulted in an uproar in the Czech professional world; his Marxist-inspired monograph on the rise of the patriarchate in the Eneolithic period in Central Europe (1967b); his synthesis of 'spatial archaeology' (1998c); *Archaeological method* (CUP monograph, an overview of archaeological methodology,

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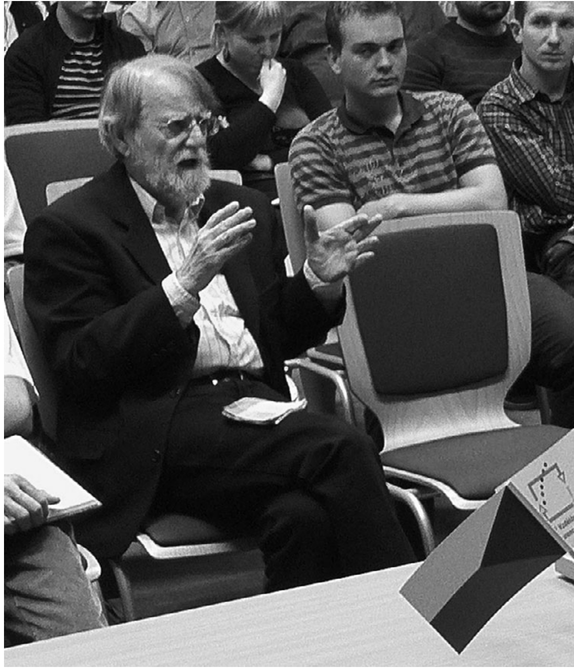

Figure 2 Evžen during a seminar at Pilsen University.

1993); and, more recently, two theoretical books (2007; 2010), still to be translated into English. Moreover, he used to develop his own computer programs at times when any commercial software was not yet available.

With his partiality for articulating pronounced and committed standpoints, Evžen, however, not only brings people together, but also has the capacity to separate them (in his words, bringing together and separation of people are two poles of one relationship, opposing indifference) on a professional, political and personal level. As he says, he about whom there are no yarns has lived in vain. This is something he needs not fear, as in this case he will perhaps be with us longer than his archaeological paradigm. Let us therefore enter his office, either at the Institute of Archaeology in Prague or the university department in Pilsen. If he were to accompany us in the corridor, let him knock on his own door and wait with him until there is no response from inside and he says, 'Well, I am not in'. These jokes go a long way . . .

Summarizing the story . . .

Evžen, you are the son of Jiří Neustupný, a well-known Czech archaeologist, a long-term head of the Prehistory Department of the National Museum in Prague, and a professor at Charles University. How did your family background influence you in your choice of career, in forming your attitude towards your professional life and life in general?

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The more time goes by since my father's death, the more intensively I realize how his view of archaeology, which I perceived since childhood, surpassed its time. My father did not articulate his own paradigm, but he clearly did operate within it.

Specifically, this meant that he never allowed himself to be enticed by migration models and chronological trajectories of archaeological types in terms of cultural historicism. He understood the importance of ecofacts and started a regional approach, which not only had local history goals, but also posed academic questions. He preferred typically synchronous topics (Neolithic fortifications, ramsons (*Allium ursinum*) in prehistory, Eneolithic sculpture, etc.). I must admit that originally I did not entirely understand this direction of his and I tried (albeit unsuccessfully) to convince him of the significance of chronology, which I was occupied with at the time. We eventually came together on the topic of theory and methodology and eventually also on the renaissance of the Indo-European question. However, it was our mutual cooperation on the outline of prehistory of Czechoslovakia (published also abroad in 1961) that brought us together the most, and which I will speak about later.

I didn't quite understand my father's efforts in the area of 'public' archaeology, mainly museum studies (which took place at least from 1945). Now I know that already then he understood the role of 'the other' (non-academic) archaeology, while most Czech archaeologists thought that this was a diversion from the 'correct' academic discipline. Today I would label myself rather as his student than as a student of anyone else; at least, I inherited from him an aversion toward migrations as universal explanation model in prehistory and an inclination toward the problems of archaeological method. I also learnt field archaeology methods from him and it was of great advantage to me to have been surrounded by his books on archaeology since childhood, books which I often went through.

I should mention that my father, who was a committed democrat, lived most of his professional life under totalitarian regimes, at first under the Nazis and then under the Soviet-type regime (until his death in 1981). As with many others who found themselves in the same inescapable situation, the following applies to him highly: if he had had the opportunity to work within less restrictive conditions, his mark on world archaeology could have been much more pronounced.

Of the deceased it is often said that they were kind, considerate and of good character. In the case of my father it was true, however. It is surprising how he could work and survive with these qualities in those times. In this sense he left behind a legacy that I can hardly match.

Why did you later give preference to prehistoric archaeology instead of Egyptology, which you actually studied?

Sometime in the 1940s I started studying the Old Slavonic language and its scripts. However, I gradually began to feel that it is a language relatively recent and I planned to go over to the study of cuneiform. This was not easy, however, as none of the university professors held any interest in a secondary-school teenager. It just so happened that an Egyptologist, professor

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Zbyněk Žába, was preparing an exhibition at the National Museum and was willing to accept me as an informal student. Therefore, before I started attending university officially, had I already mastered hieroglyphs and the classic Egyptian language (of the Middle Kingdom). It was still not possible, however, to start studying these matters directly. The first year of Egyptology opened in 1951, although there were no students keen to study it (the only student of it originally wanted to study Arabic but was pressured to take Egyptology). According to the omnipresent planning, it was not possible to open it again a year later when I entered the university. As a way out, I turned to prehistoric archaeology with classical archaeology as my second subject. Eventually I did transfer from this to Egyptology.

Soon it became clear, however, that at Charles University Egyptology was philology-based and I did not like this. I did not see any real problems here that I could resolve, or at least adopt a particular stance on. Prehistoric archaeology was different, however; there were many issues to resolve. There were also several students with whom one could discuss matters; I was not as isolated as in Egyptology. Even though I had completed Egyptology, I gradually transferred entirely to prehistoric archaeology.

Already during my studies I published an article on the periodization of Neolithic Linear Pottery culture into five phases. This article was well received, and even abroad some archaeologists openly supported it (E. Comsa, T.S. Passek, J. Kozłowski or H. Quitta), while others used it without referencing. I think that the elaboration of the earliest phase of the Central European Neolithic and its specific links to the Balkans and the Carpathian basin held major significance. And with this behind me, I left Charles University in 1957.

In the 1950s and 1960s, Czech archaeology achieved good results and was well recognized on an international scale in some areas. Specifically, for example, research of the Neolithic period, La Tène oppida, and the Slavic fortified settlements and Great Moravian centres. Do you agree with such a favourable evaluation? If so, what led to this quality – pre-war tradition or unprecedented support from the Communist regime? To what extent was Czech archaeology of the 1950s and 1960s performed on the basis of political orders?

Research into medieval sites (Slavic) was truly based on political orders by the Communist regime. It was to prove that Czechs are 'Slavs' and thus belong to the east, into the sphere of the Soviet Union. Also their culture should be – according to the official authorities – approached as being derived from the east, especially the elaborated jewellery and church architecture. In fact, these very elements cannot be derived from the east, e.g. Ukraine and Russia, but from the south-east, in that particular case from the sphere of the Byzantine empire. This approach was, of course, wrong in principle. Slavs do not form any other unit than a language one – there is no 'Slavic culture' or 'Slavic history'. Even though some in Western Europe think so even today, there is no 'Slavic' psychology either. Czechs speak a Slavic language, but their culture and history are Central European and they are entirely linked to the Latin culture of the medieval Roman Empire, a not negligible part of which

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was the Bohemian kingdom. We, the Czech-speaking population, have much more in common with Germans, Austrians, Poles and Slovaks than we do with eastern or southern Slavs.

Besides the 'Slavic research' the Communists also supported research into the Palaeolithic, with the hope (in vain, though) that this research would yield proof of the evolution of man from ape. Instead of this, significant success was achieved in the study of the stratigraphy of the last Ice Age (thanks mainly to František Prošek).

Extensive field excavations of Neolithic sites (primarily Bylany), oppida and medieval strongholds were Socialist megalomania to a large extent; excavations of a much smaller degree would have brought about the same results. It cannot be denied, though, that these excavations yielded masses of finds, which had both a positive side and a negative one: for example, it substantially held up the laboratory and analytical processing of all the finds, thus any theoretical outcome was delayed by entire decades, and by the time these excavations were published they were often outdated.

The secondary outcome of this policy of the Communist regime was the rise of a generation of young archaeologists that entered into the discipline mainly in the 1950s. Many of them worked on a theoretical level, often, however, on the basis of the long-known finds and scant information from the large excavations under way at that time. This is often forgotten, though, when it is pretended that the unquestionable success of Czech archaeology of these times was the result of the large (so-called systematic) excavations. The work of this generation was very successful, particularly in the area of classification and chronology and in the search for 'influences'. I belonged to this generation and I think that my work (the chronology of the Eneolithic, the periodization of the Linear Pottery culture, the Baden and Globular Amphora cultures, etc.) provoked imitation both locally and abroad.

The high professional level of this generation on a global scale is unquestionable, even if it took place based on the culture-historical paradigm. It stands to reason, however, that some from this generation were on the search for new approaches, such as my father, Bohumil Soudský and others.

In 1959–66 you acted as the head of the Most branch of the Institute of Archaeology of the Czechoslovak Academy of Sciences. It was here where your theory of spatial archaeology arose, as well as the organization of an institution focused on rescue archaeology. Later you often referred to your experience from this time. What do you see as your contribution relating to this and what did this institution mean for you and for Czech archaeology?

Mainly, I would like to say that I am allergic to the term 'rescue archaeology', which indicates that something is being rescued and that the heritage can be saved by excavation. In fact, any excavation is destructive and, therefore, problematic, both at sites which are not under threat and at those which are.

I spent almost ten years at this position (1957–66) and from March to mid-December I used to be out in the field almost daily. I examined sites from various periods, mainly from the Late Bronze Age and the Early Iron Age. The finds discovered were immediately processed in the lab. The intensive contact with a wide variety of finds was very beneficial for me – the majority

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of other archaeologists came into contact with their finds only sporadically, or with finds only from one period.

In Most I created an effective system for a small archaeological unit, the basis of which was the logical arrangement of steps to be taken from excavation to laboratory analysis, recording and storage. The system included also the processing, recording and storage of excavation reports, drawings and photographic documentation. Already at the end of the 1950s I introduced, for example, georeferencing of sites in coordinates. It was my aim to design an operational chain from the fieldwork up to publication of finds; I did not complete this in the end as I left the branch and lost control over these matters.

I was never named head of the Most branch, as this would have required the approval of the Communist Party cell at the institute, or maybe even from higher up. But I did actually perform the function of head, only someone else had to sign the paperwork instead of myself.

During my work at Most I conducted the first ever (in the Czech Republic) stratigraphic excavation of lake sediments (Dřínov) and quartzite mines (Tušimice). Overall, my list of field excavations from this period numbers dozens of events. Besides that, I wrote a range of theoretical papers here, a couple of books (1961; 1967b) and other things. In essence my processual standpoint was being formed at this time, too.

You published the Outline of prehistory of Czechoslovakia *with your father in 1960 – a publication that introduced new perspectives on the prehistoric period in Czechoslovakia. This work was soon translated and published also in England, the USA (1961) and Italy (as* Czechoslovakia before the Slavs*; see below), but on home ground it brought about a very stormy reaction, and mainly a lot of outrage. What were the reasons for this and how did it happen?*

Yes, the primary reaction, evoked by the Communist elite, was stormy. The elite called for the collective condemning of our work. But Moravian and Slovak archaeologists refused to take part in this and eventually Czech colleagues fell away too, so the critical review which followed in the end had only two Prague authors (B. Soudský and R. Pleiner). Its tone was that of disapproval, entirely unusual in the context of those times, as professional reviews in socialist Czechoslovakia were always formal and affirmative.

In our case there were two main reasons for this kind of review. The first was the offended self-conceit of 'leading personalities' in Czech archaeology, who apparently felt that they were the ones who should have written a book like this. The second reason related to the content of the book, which very much deviated from the culture-historical archaeology that the initiators of the review regarded as a given fact. The contents did not concern migrations and cultural influences much, but focused on the continuity of prehistoric cultures, and economic and social issues. Some theories clashed with common notions; we postulated, for example, the local origin of the Corded Ware and Funnel Beaker cultures in Central Europe, and the rise of the Lusatian, the Knovíz and the La Tène B cultures was described as the continuation of the preceding Bronze Age and Hallstatt periods in this area. Our book went on to become one of the roots of the Czech version of processualism.

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In spite of your reputation as one of the renowned personalities of Czech archaeology, your position at the Institute of Archaeology was never really certain until 1989. With the exception of the end of the 1960s, you were not allowed to travel; teaching or climbing up the career ladder was out of the question. Even quoting Western literature was a risky business for you, as well as for most other archaeologists. What was it like to work at the Czechoslovak Academy of Sciences at the time of 'normalization' in the 1970s and 1980s, i.e. at a time when one's life wasn't at stake as such anymore, but 'merely' one's professional existence or academic career?

Based mainly on my radiocarbon work I had several very attractive offers from American universities and I would definitely also have got by very well in Europe. I did not want to emigrate, however, as I thought that someone should remain behind in Prague. It wasn't like emigrating from a democratic country, where one could come back anytime, visit family and friends and remain in close contact with them. For the Communists emigration was an unforgivable criminal act. There wasn't an option of returning from exile (except straight to jail), one's name was socially wiped out and erased from any professional literature, and relatives were subjected to repression. Also I saw no sense in public protest against the Communist regime, which would lock me up and then condemn me to spend the rest of my life in manual labour. With the exception of a few Western journalists, no one else would have known about this.

My brother emigrated after the Russian invasion of Czechoslovakia, and never returned home. He was an outstanding general linguist and an expert in Japanese. He worked for the Monash University in Melbourne and later at several universities in Japan. The Communists did not allow him back, not even for our father's funeral. As a result of his emigration I encountered many problems, for example they wanted to throw me out of the institute; one Communist stirrer called on the committee deciding on my 'guilt' to 'let his brother support him'. Also my wife had problems, even though she did not even know my brother until the mid-1980s. And this all happened while my brother wasn't even politically active – all he did was decide to follow his wife (a foreigner) and live in another country.

And so I tried just to continue with my work at the Institute of Archaeology. Theoretical matters, however, again became dangerous terrain (after the short-lived liberalization in the late 1960s) and so I specialized in working in the natural sciences, mathematics and computer programming. This did not bother Communist ideologists that much and it did, after all, correspond with my processual interests. It later came about that it was also a good starting point with which to grasp archaeology to a deeper extent.

And I must add that I did not believe that the Communist regime, secured by Russian troops in Czechoslovakia then, would ever come to an end during my lifetime. Already in 1968 the Western superpowers clearly demonstrated that they would observe the Yalta Agreement of 1945, in which they handed Czechoslovakia to the Russians. To me the fall of the Communist regime in 1989 was something unexpected as the Soviet Union still had a sufficient amount of atomic weapons by which it could maintain its powers.

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Who from the ranks of archaeology, anthropology or philosophy inspired you the most in your theoretical work? You often mention Marx, de Saussure, Binford . . .

In creating my theories I was extensively inspired by the ideas of previous generations and my contemporaries. Some of them helped in showing me which way not to take; this is also an important form of support.

I'd also like to add at least G.W.F. Hegel to the list of people who inspired me. It probably was Karl Marx who inspired me the most, though. I managed at a very early stage to separate his political doctrines from the well-reasoned economic theories and I gradually managed to overcome the intellectual illness that drove many people to socialism – sympathy with those who suffer, in circumstances where there was no solution other than an unsatisfactory one and where any revolt would be fruitless. Class struggle is a communist theory, not Marx's economic principle. I never focused on class struggle – as opposed to many archaeologists who accept similar issues in the form of the 'theory of power'.

And while we're on the subject of Marxism – supporting it wasn't easy even under Communism, and it isn't easy even today, albeit for different reasons. But I still regard your Patriarchy *from 1967 as an exemplary model of Marxist interpretation in archaeology, as well as about the only really theoretical work on prehistoric archaeology in the Czech post-war period. If the book were to be published in English in the West, it would most probably have become renowned – I judge this from the fact that practically identical ideas appeared thirty years on (only), for example among some of the Dutch prehistorians. How do you see this work today and how do you see the potential of Marxism as a theoretical point of departure for contemporary archaeology?*

Yes, it was risky being supportive of Marx, even in Communist times. Communist archaeologists did not understand him and jealously monitored what support of Marx (even if just in the form of quotation) by a party non-member would mean – it might be a potential attack on their position of power.

In my work *The beginnings of patriarchy* I discussed important theoretical topics and in the 1960s this was unimaginable without an abundant use of quotations by Karl Marx. I quoted Engels less, and Lenin not at all. I only added one quotation of his after the intervention of a Communist ideologist, who reviewed my work based on the wishes of my director, Jan Filip. Filip was one of those who did not understand it at all and was not willing to take responsibility for my potential deviations from the 'true Marxism'.

Anyway, using Marx was not that costly for me. The given work is not, however, truly Marxist as I did not deal with class struggle at all; it was written from the position of materialistic economism. I think its arguments are still tenable, although I would elaborate on this topic in another manner today.

On the question of the potential of Marxism today, I would say there is none. Both the theory of class struggle and Marxist economism are

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compatible, at the most, with processualism and this no longer presents a living theory of human society.

With regard to the acceptance of Marx's ideas, I would like to say that people in former non-Communist countries sometimes take the incorrect stance. For them a 'Communist' is one who quoted Marx, and not the one who, based on his membership of the Communist Party, enabled and executed Communist policies and suppressed his colleagues and fellow citizens. For them, even cooperating with the Communist secret police seems forgivable. They do not understand that active cooperation with the Communist Party in the totalitarian regime is something completely different to membership in a political party in a democratic country.

After the renewal of democracy in November of 1989 it was clear to all that you were the primary candidate for the first director of the Institute of Archaeology in Prague – the most distinguished position in Czech archaeology. And within several months you became just this. Did you see this as a form of satisfaction, did you expect this? What were your priorities?

As I have already said, I did not count on any such possibility, but based on my efforts to reform the Institute of Archaeology in 1968 (during the Prague Spring) I had many ideas already formed in my mind. I did not feel it as any form of satisfaction whatsoever: my thoughts were directed towards the future, not the past.

I had two basic priorities – first, to get the employees of the Institute of Archaeology though the turbulence following the 'capitalist revolution' in a way that would not destruct capacities in Czech archaeology (this I managed); and second, to close down the Institute of Archaeology or to drastically reduce it (this I did not manage).

Yes, I remember: three years on, as a result of the break-up of Czechoslovakia, new competitions took place for the posts of directors of the Academy of Sciences institutes. You did not manage to defend your post, most probably because your concept of the institute was too assertive and involved major changes in the institute's structure, the range of its activities and personnel structure. Do you see this as a mistake today, and if so, was it a mistake in terms of concept or tactic? Would you do the same today?

When the Communists founded the Czechoslovak Academy of Sciences in 1953 based exactly on the Soviet model, their main goal was to establish control over this important part of society. One of the most active individuals in this process was the archaeologist Jaroslav Böhm, who became the vice-president of the entire Academy and the head of its Institute of Archaeology. In the area of archaeology he met the expectations of the Communists to the last detail. He centralized almost the entire discipline and took it under his control. The Institute of Archaeology amassed a large part of Czech and Moravian archaeologists (in the 1980s there were up to a hundred research specialists, plus auxiliary personnel, working at the Prague part of the Institute of Archaeology alone). Their primary task was purely theoretical research. Basically no one was taking care of excavations on sites under threat (with the exception of one or two workplaces, such as the Most branch and a

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few isolated small museums), but sites that were not under threat were being investigated on a large scale for many years, even for decades. All decisions regarding archaeology were made at the institute, by Böhm and his successors.

This system was very ineffective and created an unjustified preference for archaeologists in the academy who, on the grounds of their employment, had all the right work conditions that socialism could provide, as opposed to the sparse levels of archaeologists working in museums and at universities. These few archaeologists, for example, could only conduct field excavations with the approval of the Academy of Sciences and effectively publish only in their journals. Naturally, the majority of the specialists of the Academy of Sciences did not do proper theoretical work at all or did it to a negligible degree, while other segments of archaeology remained undersized.

And even when the institute started investigating sites under threat for economic reasons from the 1970s onwards, the described system continued on and created a very undemocratic atmosphere in Czech archaeology. This was the main reason I wanted to close down the institute and transfer its specialists to other sections of the profession. To a large extent I succeeded, but only in detaching some archaeologists, namely those who moved over to the heritage sector. I didn't manage to transfer the research specialists to universities, where they would mostly belong in democratic countries.

It seems to me that in 1993 personal interests prevailed among archaeologists in the institute. No one wanted the Communist Party to be involved any more, but they did want to maintain the majority of that which this party introduced. But it was no longer possible to bring back the range of relations prevalent in the Socialist era. It is difficult to say how I would proceed today, considering I know the entire ensuing development. Definitely, though, I would not change my priorities.

I understand your argument regarding the former concept of the institute. But aren't your doubts a bit outdated today? The Institute of Archaeology is no longer merely a venue for theoretical research. In many of its roles today it follows up on its former, pre-war tradition and its former mission in, for example, the coordination of rescue excavations, administration of central archives, etc. It appears to me that the merging of these tasks with theoretical views is desirable, but universities logically cannot cope with such tasks properly.

It is difficult to follow up on the first half of the 20th century, when the structure of archaeology was not set yet. In democratic countries excavations of sites under threat are conducted by heritage-care institutions and excavation units, whereas theoretical work is done by universities (and I'm not talking about the tasks of museums here). There are, of course, some research institutions that do not teach, but their activities are directed mainly abroad. The main task of archaeologists in the Czech Academy of Sciences is still theoretical work. In this they don't differ from universities, except in the fact that university departments are still small and undersized, so their personnel must mainly teach. An unequal situation arises compared with the specialists of the academy, who have scientific research as their main work activity (and sometimes they conduct rescue excavations). The

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administration of a central archive is definitely an important issue, but it should be done by a different and separate institution. I am distrustful of the matter of coordination if it is to be done by the largest body; in the past this kind of coordination always carried with it hidden and unequal relations.

I think, though, that the Institute of Archaeology will still go on for a long time to come as it is backed by the same logic that stands behind the other institutes of the Academy of Sciences. It is possible that it will transform gradually, but the requirement to conduct 'primary' theoretical research is still hard and fast. It is also obvious that in the case of change it would be necessary to reorganize archaeology as a whole, not only the institutions of the academy. We have, however, missed a revolutionary opportunity to do so.

In 1998 you founded the Department of Archaeology at the university in Pilsen. At that point in time it was the third archaeology department in the Czech Republic, after the traditional departments in Prague and Brno. You gradually gave the Pilsen department an unprecedented and distinct theoretical direction and you are still an active professor there. How do you see the results of the department today and how do you judge the fact that other departments of archaeology were founded at other universities, too? Are eight university departments of archaeology not too many for ten million people?

I founded the Pilsen department not only because students didn't have a place to get educated, but also, mainly, for them to have a chance to learn a different kind of archaeology than they have had to date. I managed this for the most part. My students accept new theoretical ideas, they try to apply them and enhance them, and they use the language of new archaeological methods. We had, and we have, some very good students – such who appear only after dozens of the average ones.

I acquired a range of excellent archaeologists for lectures, mainly from the Institute of Archaeology in Prague and from the university in Brno, and their standards were very high, especially in the beginning phases. However, none of them were able to work full-time and for a relatively long term for the department in Pilsen.

The problems that I mentioned now are basically a set issue for every minority community of researchers. There are not enough good experts to cover all the needs in the fields of the discipline. That is why I am a bit reserved when it comes to founding new university departments in the Czech Republic. I know from experience how difficult it is to find really experienced archaeologists that students would esteem. I've noticed that some new departments to a large extent engage recent university graduates, persons who are not desired elsewhere or, at the best, middle-aged archaeologists who do not have any striking scientific profile. These people will get themselves a habilitation qualification somewhere or even the title of professor and there is the danger that as a result of this they will have a disproportionate influence in the field. There are only an isolated few of them that I'd like to see in Pilsen.

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. . . and archaeology itself

Since the 1960s you have been developing topics that are, in the world, mainly linked with processual archaeology. At random we can mention the deductive cognitive model, multivariate methods in mathematical analysis, the question of paradigms in archaeology, etc, let alone casting doubt on migration and the parallel coexistence of various archaeological cultures – the favoured models of cultural-history archaeology. The coincidence in time and topic with the pioneers of processualism is sometimes amazing. For example, in 1981 L. Binford publishes his Bones. Ancient men and modern myths, *while your article on the preservation of bones at prehistoric settlements is published in the same year. The idea that elements of processual archaeology were formulated independently at the same time in two different corners of the world fascinates me, and it is extremely interesting in terms of the history of science. How independent were your discoveries really? Was there information available in Czechoslovakia then on the parallel development of the field abroad?*

My path towards the Czech variety of processualism began in the 1950s, when I was studying literature on cybernetics, logic, the philosophy of science and mathematics. At that time I was also intensively reading up on economy and philosophy. I learned how to program computers (my first usable program was developed in 1968). In a processual spirit I studied ecofacts (although I wasn't aware of the term 'ecofact' as such), i.e. pollen spectra, magnetism of the Earth, the basics of geophysics, demography, later on human nourishment, the destruction of bones, etc. On the contrary, no form of history intrigued me.

Sometime in the 1960s I got hold of an issue of *American antiquity* and so I was able to see that there was something going on abroad. However, it was not yet processualism as such. I did not know of the existence of Binford until 1969, when my main points of view were already formed. A while later I managed to get access also to literature on the British processualism. I was thrilled with the 'New Archaeology' as in it I saw the independent confirmation of my theoretical efforts.

I should mention how all this is possible. Literature from the 'West', particularly from the USA, was a very rare occurrence in Prague libraries and even journals were incomplete. A trip to Western Europe or to the United States was unthinkable. Written correspondence with colleagues was made unpleasant by the fact that at the end of the year I had to submit a report on everybody I'd written to abroad and about what. In principle, it was all controllable, as I had to have the letter envelopes open when I took them to the post office and the post office worker checked whether the return address was the same as the address on my ID card. And I am not talking about the content of foreign literature – in this there was always a danger (particularly in the 1950s) that someone would declare some quoted 'Western' work as a means of ideological diversion and that that would cost the author his job (without any possibility of returning). These practices used by the Communists led to a very thorough isolation of people, including archaeologists.

The parallel and independent development of archaeology in the same direction by several researchers in various places does not seem all that

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mystical to me. A change of paradigm usually 'hangs in mid-air' in many parts of the world, where the discipline is sufficiently developed. Those studying similar topics can very likely come to the same, or similar, conclusions. If, that is, they are able to avoid eclecticism and keep their thoughts in logical contexts.

Even though I did reach a certain variety of processualism on my own, I naturally wanted to learn from others, too. For example, I learned about multivariate methods from L. Binford, even though I think that I understood them well and developed them as I programmed them into the computer myself. Already from the onset of my interest in methodology I talked about the testing of models that must be designed, while Binford for long talked about the testing of hypotheses that could be obtained in whatever way, for example by hallucination.

Many archaeologists (and not only archaeologists) see development in research (mainly within a minority community) as a series of influences that stem from progressive intellectual centres, usually the mainstream ones. It is a precise analogy of cultural-historical diffusionism and it is interesting that this appears without relating to the research paradigms of those archaeologists. While such influences do come about every now and then, the usual efforts at determining a new concept come from an inadequate state of archaeology at home and from ensuing attempts to change it. As I said, if there is a similar point of departure in various countries and a similar intellectual environment, then the results may be similar. A change that occurs in a large mainstream community can result in a change of paradigm (processualism in the USA or the UK); transformation in a small minority community can lead only to partial changes, however comprehensively they may characterize some individuals (the case of Czech processualism).

Anyway, all that is merely a historic view, as I no longer regard myself as a processualist.

Towards the end of the 1960s you focused on the issues of absolute chronology. You were the first in the world to point out the need to calibrate radiocarbon dates and you tested this procedure on concrete data. You also participated in a Nobel Foundation conference, met with W.L. Libby and published in the journal Nature. *When and how was this promising research interrupted?*

Relative chronology sufficed for cultural historians; in their eyes absolute data was suited more for reasons of popularization. My processual interests, however, led me to absolute dating. Vladimir Milojčić, a well-known opponent of radiocarbon methods, wrote me long letters in order to deflect me from radiocarbon. He believed that I had become a victim of the influences of foreign archaeologists. And by the way, not even he believed that I would be able to think up the calibration of radiocarbon data on my own.

My father and I accepted radiocarbon dating as far back as in our book *Czechoslovakia before the Slavs* (1961; 1960 in Czech). Later, my 'ecofact' interests led me to study changes in the Earth's magnetic field on the basis of archaeological samples (together with geophysicist Václav Bucha). I came to the conclusion in 1967 that changes in the intensity of the Earth's magnetic

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field, which were based on Bucha's measurements, must have had an impact on age measured by radiocarbon methods. This was confirmed by the first measurements of tree rings from American sequoias (the results from Rocky Mountain bristlecone pine weren't available yet). On this basis I devised the first calibration of samples from the Neolithic period of Central and South-Eastern Europe. The results were shocking: Neolithic and Eneolithic periods were almost two thousand years older than Vladimir Milojčić had believed them to be (myself included, in my chronology of the Neolithic from 1956).

Back then my work drew a significant response from geophysicists and I was therefore invited, as the only European archaeologist, to the Nobel symposium held in Uppsala, Sweden, in 1969. That happened soon after the Russian invasion of Czechoslovakia, when the Communists hadn't yet fully renewed their control over the people, so I was allowed to go. I had many specific plans as how to go about my chronology studies, but the trip to Sweden was to be my last contact with the outside world for a long time to come because the Communists managed to regain power. In the coming years I was happy that I managed to stay in archaeology, but any dreams about international cooperation were out of the question.

I remember that in the 1980s you did not agree with the acceptance of postprocessual ideas. But you have accepted some of them since, haven't you? How do you see the contribution of the postprocessual school today?

What I've always appreciated (since the 1980s) about postprocessualism is its interest in symbolism. It is of immense significance for archaeology, but processualism vastly underestimated it. The interest in the role of the individual is likewise a realistic problem, but postprocessualism did not introduce any objective methods on how to study it. Its approaches (the reading of archaeological sources as text) seem to me to be a complex of subjectivism. What I also don't like about postprocessualism is its leftist orientation, because I am more than aware of what the leftist orientation of the Czech intelligentsia led to in the 1920s and 1930s (to the substantial advantage of the later Communist regime). I think that postprocessualism was a fundamental chapter of archaeological thinking, but today it is losing ground.

In the 1980s you proposed a relatively influential concept – the theory of community area. How did it arise and what forms its core?

What led me to the theory of community area was my theoretical considerations and my field experience in Most. Both theoretical and empirical research clearly demonstrate that prehistoric settlement indicated by archaeology cannot be expressed by dots in space, but that it forms larger and continuous areas. For me, a settlement area of a prehistoric community consisted of a range of components (areas of various activities) such as residential area, storage area, burial area, grazing land for domestic animals, etc. I also anticipated other types of area (i.e. fields) that must have existed in the agricultural prehistory, but we do not have any empirical evidence of them in the Czech lands. If we don't realize this, though, we'll never find them. Activity areas (components) need not mutually rule one another out (for

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example, they could mutually overlap one another, such as residential areas with manufacturing and storage areas), and in this very fact lies important information. For me, the prehistoric landscape stopped being merely discrete points on a map and began to be rather a structured territory which we can hypothesize on, based on archaeological finds. Later I supplemented this theory with the idea of *social landscape*.

I had considerable success with this theory in Czechoslovakia; today my terminology is widely used, even by archaeologists who don't at all realize that I introduced it only quite recently, in the 1970s.

In your latest work, Theory of archaeology, *you present a new paradigm – artefact archaeology. What would you say lies at the core of this paradigm and in what does it surpass preceding ones?*

I'd mainly like to point out that a new paradigm doesn't come about by someone sitting down and thinking it up. Furthermore it is unlikely today that it would appear outside any mainstream community – that is, a community of research specialists in large and developed countries – as mainstream archaeologists are not much interested in theories developed in other (i.e. minority) communities. Therefore the most I can do is to comment on the tendencies that I observe in current archaeology, or on what I think a new paradigm should include, in my opinion.

A new paradigm should mainly return to artefacts as a basis of archaeology. No man's harmony with past nature, no individuals and their ways of thinking, no people masked behind artefacts who must be discovered are primarily addressed in this paradigm. On the contrary, this paradigm is (or should be) deeply anchored in the concept of the artefact and in the idea that all progress of knowledge in archaeology can be achieved only through scrutinizing the inseparable *artefact–man* pair. Artefacts are means that create and maintain human society.

In relation to nature, it is important to realize that the historical task of man is to *create* (create artefacts, society and symbols), not to fight with the environment (nature, other societies) for success or even for life itself. Archaeology falls among positive, optimistic disciplines that, by the way, rule out the theory of class struggle. The creation of artefacts has a practical function, providing social meaning and symbolic significance, and none of this can be left out.

As to human consciousness, a specific entity oriented towards the future, one can stress the task of symbols and signs, the 'companions' of fully developed artefacts. Archaeology deals with symbols far more than it does with the practical life of humans.

Artefacts (these same artefacts) are both structures (regularities) and events (unrepeatable singularities). These can become the point of departure for examining the role of individuals. However, methodologies for the study of events are not well elaborated – most likely because such work requires a new paradigm (Neustupný 2007, 2010). The merging of the study of structures and events into one paradigm would restore the view of these two aspects of the human world within one framework. The 'artefact paradigm' must also encompass certain methodological principles.

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A new paradigm is usually not a set of better answers to old questions, but a response to *other* questions. And so that is another view of our discipline. The proposed paradigm is mainly optimistic (it highlights *creation* against *fight*) and removes the one-sidedness that accompanied all previous paradigms (*structure* being stressed by processualism, while *events* were being stressed by the culture-historical and postprocessual paradigms). It accepts both structure and event and is interested in the function, significance, meaning and expression of artefacts at the same time. In a certain sense it therefore allows for a dialectical view of the past.

One of the main highlights of your paradigm is the new interpretation of adaptation. You see it not as the adaptation of man to nature, but the other way round, as the adaptation of nature to man. At the same time, however, you do not rule out that adaptation is a reaction of society to a situation (crisis) in the outside world. Isn't that just quibbling, then? For example, if the rise of arable agriculture is a reaction to the depletion of certain sources at the end of the Neolithic, then doesn't it matter whether we see the plough as the adaptation of society to the outside world or the adaptation of the outside world to society? Because it came about, after all, due to an external impulse and it involved a two-way process …

There is a simple example that shows that it is not the same. It is assumed that adaptation, in interpretations other than mine, is often the result of the development of nature – for example, climate. Today we have reliable evidence that the Earth's climate has changed only slightly in the last approximately ten to twelve millennia. But human culture at that time has changed in an incredible manner. Changes in the human world therefore cannot be the result of adapting to nature.

It's the same with the rise of the Central European cultural landscape in the Eneolithic: villages (i.e. houses and their surrounding farm buildings) close to water sources, surrounded by fields, a graveyard further away (later also a church) and the remains of small clumps of forests, with distances of a few kilometres between settlement areas. This system endured in agricultural landscapes until the first half of the 20th century, regardless of major social changes that occurred then. The structure of village areas changed radically in the second half of the 20th century, without changing nature.

In some cases the direction of adaptation may seem to be the other way round, but the determining of the cause and consequences is still not a mere pedantry.

Along with Marshall Sahlins you understand prehistoric communities as an affluent society where life was not a constant fight to stay alive, but a relatively facile matter. If that is so, then why did man leave this stage? By chance? Or was it due to unexpected consequences of the permanent efforts to reach abundance, as you write in your Theory of archaeology*, where artefacts serve mainly for 'conspicuous consumption'? Is social development an unexpected result of the production and overproduction of artefacts?*

I interpret the leaving of a society of abundance and its replacement with a society with worse nutrition (1983b) and the need to constantly invest more

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work (cf. the standpoint of E. Boserup of 1965) through the term *density of social relations*, which I regard as the basic quantitative parameter of human society (2010). The density of social relations has an effect on how easily people connect with one another whilst performing economic activities, in searching for partners, in ritual warfare, during significant religious rituals, etc. Although the density of social relations is not identical with population density, an increase in population density does usually lead to better social relations. Agriculture increased population density and thus also the density of social relations. That is why people preferred it wherever it was possible and farmers moved in colonizing waves which hunters living in a society of abundance could not resist. Worse nutrition and the need to perform more work was the price to be paid for improved social relations.

One would rather have thought that an increase in population density would lead to conflict. Also anthropologists claim something similar, when they compare hunter-gatherer societies with farming societies. And what about today – isn't the overly large population density in some parts of the world cause for alarm?

Until now I have not come across any realistic conflict theories, but it could just be due to my unawareness of the proper literature. In terms of prehistoric demography I came to the conclusion that people (both prehistoric and modern) intentionally increase population density when conditions allow, or when they can draw benefits from it – both in immediate form (family benefits paid out by the state) and in long-term perspective (children taking care of parents or, in general, of previous generations). I'd furthermore add today that numbers of children could also be a matter of prestige (a symbol of success). More generally it is the increase of density in social relations.

Achieving demographic growth is easy, all you have to do is hold off or restrict methods used against the natural growth of the population, the use of which we must anticipate for a large part of the past – otherwise the Earth would have been overpopulated long ago in prehistoric times. Even 'primitive' society can introduce (or eliminate) simple rules limiting growth and thus give rise to a stationary population.

The current demographic development in the world is a fact, I don't know whether I have the right to judge it. If I am correct and it is due to an increase in the density of social relations, then methods such as, for example, using contraceptives must be entirely ineffective.

Another significant element in your paradigm is, at first glance, surprising: the concept of event. You see events as 'subjects of structure', specific manifestations of universal relations. Your approach, however, sees the individuality of events merely as a result of outer conditions and of the simple fact that 'things just cannot be always made the same'. Isn't this interpretation of event too shallow, aren't we losing the perspective of previous development (history), specific interests of individuals or social groups which need not overlap with the interests of the whole? Even war (in prehistoric times) seems more like a hockey match in your interpretation, after all, being an activity

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beneficial for both sides. It seems to me that your portrayal of prehistory has started lacking any form of dialectics ...

In my opinion artefacts can be seen from two perspectives. One is through their regularity and structure, and the other is through their singularity (that is what creates event). You can also say that events are subjects of structure, and can be studied by formalized methods of analysis and synthesis. From this perspective each artefact is an event (more simply, event is the creation of artefact, the demise of artefact, and other points along its time trajectory) as well as structure. While, from the point of view of previous paradigms, events (war, the death of rulers, etc.) were distributed relatively sparsely, events as artefacts occur practically continuously. Besides that, artefacts are singularities that have the capacity to become a point of departure for the study of individuality.

The concept of artefacts as events is therefore of major significance for the study of the human world. For example, war as the purpose of artefacts (weapons, enclosures, etc.) naturally has both a structural aspect and an event aspect (a specific war is an event). War as a human activity in the *world of otherness* (the world outside one's own community area) also has positive effects alongside negative effects, i.e. it brings together communities which would otherwise have no relation. Here we cannot view prehistoric warfare one-sidedly from a philanthropic or pacifist perspective.

I do not consider the influence of previous development and interests of the individual or groups as factors of events as they produce regularity. They should be seen from the perspective of structure. This is a demonstration of how postmodern archaeologists are unable to shake off structural issues. I always draw attention to the fact that events have, for the time being, not yet been adequately studied and many of their aspects have not yet been sufficiently clarified.

The model of prehistoric warfare as a way of mutually beneficial contact between communities can be appreciated because it apparently corresponds with the known sources better than models derived from contemporary life. But this kind of contact is not conflict in the true sense of the word. Does that mean that prehistoric society did not know any conflicts? When did that change, then, and why – if the increasing density of social relations should, according to you, lead to their still improved quality?

I'd say that this stance towards warfare is related to the denial of the proposition that human life is one permanent fight. Prehistoric warfare was ritual, more a type of sport, even though people died in it every now and then (in this sense it was also a cruel ritual). I allowed myself to be influenced a little by the Russian ethnographer Shnirelman. I think that conflict did exist, but primarily on an individual level. It is not clear when this idyll ended and when wars began in order to plunder, gain territory or kill (the destroying of enemy military units or also civilians). I don't know medieval history sufficiently, but it seems to me that many elements of ritual warfare were still present then.

Taking the mass production of goods in today's Western consumerist society as an example, is this also motivated by society-wide success, i.e. by the

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creation of human relations and the availability of artefacts (as you write in your Theory of archaeology *of 2010), and not by the interests of producers?*

Among a part of intellectuals, contempt for consumption has been spreading in the past few decades. This also pays for the creation and utilization of artefacts – especially complicated artefacts which cannot be acquired very easily (for example, passenger cars). For these people a supermarket is a place of horror, where humanity is disfigured. Common people see it, however, otherwise. For them it is a place where they can obtain artefacts (which was undreamed of for them before) easily and for a reasonable price. People, as opposed to ascetic intellectuals, like artefacts and enjoy surrounding themselves with them, even if they do not need them at any given point in time. This stems from the idea that common people *feel* the meaning of artefacts as factors generating their humanity. This is why 'conspicuous consumption' has been going on since prehistoric times (for example, Neolithic longhouses).

I believe also that mass production for unlimited consumption allows relations to be realized between persons through ownership and utilization of artefacts (by the way, food sold in supermarkets is also an artefact). I have no doubt that manufacturers are interested in the widespread consumption of their goods; from the way you formulated it, it sounds a bit as though it is something bad, because you place it in opposition to 'society-wide benefit'. I, however, would not use this term and such an opposition at all – it reminds me of the phraseology of past times.

How do you see some key cultural changes in European prehistory, such as the Neolithic 'revolution', the spread of the Bell Beaker culture or the 'first Slavs' (Prague-type culture)? How should these issues be approached?

The rise of archaeological cultures was a significant theme in the culture-historical archaeology because it was assumed that culture means ethnic group and that the appearance of a new culture means migration. Particularly the second of these propositions always disturbed me and I put a lot of effort into finding an alternative solution for specific cases (mainly for the cultures of the Central European Neolithic and Eneolithic periods).

Not long ago I published a more universal solution. I realized that certain cycles exist in the prehistory of Central Europe. A cycle like this usually begins with a strict (fundamentalist) culture well defined by its widespread and distinct symbolic systems, and ends with a culture more at ease, divided into local groups, as a whole sometimes hardly definable. Each of these cycles lasts from 400 to 1,000 years.

Strict cultures were always regarded as proof of vast migration, but I'd give preference to their explanation by means of the rapid transformation of symbolic cultural systems. This theory is still in the works, though.

Could you give us an example?

Tentatively this involves the following strict cultures in Central Europe: Linear Pottery, Lengyel, Funnel Beaker, Corded Ware and Bell Beaker, Middle Bronze Age Tumulus, La Tène (LT B), the Roman Period and Prague-type cultures. Some cultures in this list are indeed the result of migration (Linear

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Pottery culture, for example), but for the majority of them we need to find another solution.

A typical example of a strict culture is that of the Corded Ware culture, spread across the entirety of Central Europe, a large part of Scandinavia and some parts of Eastern Europe. This culture arises after a wide range of local culture groups of the Middle Eneolithic period, for example the Řivnáč culture, which does not occupy even the whole of Bohemia. Basically everywhere the Corded Ware culture shows a strict symbolism, such as the formally distinct burials of males and females, the avoidance of vertical dimensions (except the digging of grave pits and the raising of mounds) and the 'absence' of settlements (a related phenomenon). Part of the pottery, decoration and stone tools (mainly battle axes) is almost the same everywhere in its first phases (i.e. in the pan-European horizon).

Concerning its origin, I rejected the idea of invasion from Eastern Europe (Sulimirski, Childe, Gimbutas) as early as the middle of the 20th century; everyone who knows something about archaeology in Eastern Europe would agree that manifestations of Corded Ware Pottery culture are just as 'foreign' there as they are in Central Europe. At first I looked for the origin of Corded Ware culture in the Globular Amphora culture, but later I realized that it could only be a partial solution. There must have been a very rapid change in fundamental symbolic systems across the majority of the territory of the Corded Ware culture. It is this rate of change that apparently forms an obstacle for us in trying to understand the details of the rise of this culture, even though many features of Corded Ware culture point to Central Europe.

Maybe even more types of cyclic phenomena could be found in prehistory and archaeologists are aware of them; you are, however, one of the first not to be scared to give them deeper meaning. What could a 'change in symbolic system' concretely mean? As far as I know, such a model has been elaborated only for the rise of Neolithic cultures, when it can be linked to the spread of agriculture.

I don't know the theories on changes in symbolic systems during the spread of agriculture. On the other hand, however, I understand the spread of agriculture in Central and Northern Europe (and obviously also the Balkans) as one of the few demonstrable migrations of a population. This involves perfect cultural continuity with the Neolithic groups of the Balkans, and there is no evidence whatsoever that the previous population (Mesolithic) played any major role in this spread.

The unexpected appearance of a 'strict' culture at the beginning of a cycle never means an absolute discontinuity, but still we cannot talk of a continuity such as is displayed, for example, by the transition from the Linear Pottery culture to Stroked Pottery culture. In the case of strict cultures we are usually unable to find any 'typological' transition forms of artefacts – even after long-term research. Ties to previous situations are usually rather unspecific in nature. For example, the surface of Corded Ware pottery is sometimes finished using straw-striation like pottery of the Middle Eneolithic period; the same basic shapes also continue, and stone artefacts belong to the same types as in the previous cultures, etc.

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Using standard methods, we are able in Central Europe to determine typological differences in prehistoric finds in the order of 80–100 years. Strict cultures appear 'suddenly', i.e. they arise relatively rapidly and this rapid onset cannot be captured using regular typological methods. For this reason the rise of strict (fundamentalist) cultures is not easy to capture. We have to come to terms with the fact that it cannot be clearly proven that these cultures would arise in another territory and would spread by some kind of migration.

You have one indisputable gift – you always manage to surprise, even after the many years that we have known one another. For example recently, when you acknowledged Gustaf Kossinna's interpretation of ethnicity. Because it was your opinions that taught our generation how to depart from culture-historical archaeology, I feel that your statement was just a bon mot. But still, can you comment on it? What is ethnicity actually?

No one can suspect me of being sympathetic towards chauvinism. But I do think that we can separate Kossinna the archaeologist from Kossinna the chauvinist and find a principle that can be used. Natural language is one symbolic (sign) system that expresses group identity, and symbols in artefacts another. I think that it is unreasonable to assume that these two systems do not correlate just because such correlation was once (or more times) misused. I do not believe, however, that the relationship between natural language and archaeological symbolism was unambiguous.

A view ahead

In your work you relatively often advocate engaging standpoints. Do you really not like romantics and intellectuals who overrate the significance of the individual in history? Nor environmentalists and ecologists who fight to preserve the natural landscape and do not understand the creation of artefacts as the essence of what makes man man? The idea of 'return to nature' is certainly naive, but don't you mind, for example, that a large part of the landscape around towns is becoming inundated with artefacts in the form of supermarkets that could be built elsewhere and with less greenery being destroyed in the process? Don't you mind the unnecessary artefacts, senseless waste and needless polluting of the environment?

I don't like extremes whatever and I respect things that have prevailed on the basis of reasonable need. Supermarkets are being built based on people's interest in tangible artefacts – interest that has created man and accompanied him throughout all generations. At the least, man has been significantly changing the 'natural landscape' since the Neolithic; today's activists would say that man devastates it. Where will the romantics turn to? To the Palaeolithic period? But then they would have to accept the Palaeolithic population density. I must point out that I do not question the issue of limiting future population growth, but the issue of limiting absolute numbers of people already living on this planet. By the way, hecatombs resulting from some kind of 'solution' would substantially disturb the natural landscape. Perhaps some intellectuals do have a simple method of how to achieve the 'natural state' of man without artefacts, supermarkets and without the demographic reduction

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of the population (and as a result its complete deprivation). Such kinds of solution were always thought up by social engineering, in the last case by the Communists, and I am happy that not many people believe in these solutions today.

I have nothing against a specific supermarket being built in another, more suitable, place, but that is not what it is about for many intellectuals: they don't want supermarkets at all. That is a Quixote-type standpoint with no chance of success. Man has been changing nature ('destroying' it) at least since the Neolithic. The degree of devastation of the primaeval forest by the slash-and-burn agriculture must have been extremely vast in some regions (i.e. in Central Bohemia) then. Since the Eneolithic period the landscape has started to take on today's appearance, but I have collected evidence that the level of deforestation (thus the 'destroying' of nature) must have been high already in the La Tène period (1998c). Therefore not even the prehistoric age was a period of romantic equilibrium between man and nature, as some politicians would like to have it.

How do you see Czech archaeology's chances on the European playing field today? Is it catching up the losses suffered in the Communist times by isolation from the Western world, or still lagging behind? How do you see the generation of young Czech archaeologists that graduated after 1989? Are they on average more educated than your generation or mine or less so, due to life and education becoming commercialized in the past twenty years?

I think that Czech archaeology has nothing to catch up: this does not mean, however, that absolutely everything is in order here. Because we have a lower number of archaeologists than larger communities, we just have less 'pure' theorists. But that can be seen also as a certain advantage.

I'd say that today's generation of young archaeologists, at least those who studied in Pilsen, have been educated *in a different manner* to my generation. We definitely read more literature on specific finds and their culture-historical evaluation – and not only in this country, but also in neighbouring countries. Besides that, though, we did not learn much more at university – except the history of the Soviet Communist Party, and some general theories, if we managed to extract these from the ideological texts or from the passages of Marx, Engels, Lenin or Stalin which we had to read – but few really tried all that much. Most archaeologists in my generation studied ethnography as a 'second subject', but I did not notice that anyone went on to introduce something positive for archaeology from this field. We learnt almost nothing about fieldwork, about processing finds or about ecofacts.

Our students today most probably have a worse understanding of the taxonomy of artefacts and the literature describing it, but they know a lot more about fieldwork, methodology in general and the theory of archaeology. Most of them are very computer-literate and have solid foundations for using basic formalized methods. I do not wish to compare both systems of education and to decide which part of knowledge can more easily be obtained by self-study; students today do not have the time to embrace both approaches at the same time. So I don't expect that the current system of education is ideal, but its improvement is a matter of a longer period of time.

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I don't know what you mean by life and education being commercialized in the past twenty years. Students mainly live off their parents' money or bursaries, as it was in the past, and university education is paid mainly by the state, as it was in the past. The only difference is in the fact that older students now often earn something on the side (usually through archaeology), but this is voluntary and we would also have done this if it had been possible on a larger scale back then.

What kind of relationship do you personally have with other archaeological communities?

For most of my life, I met foreign archaeologists mainly at conferences organized in Czechoslovakia or in other socialist countries. Maybe due to my father's influence I had a very positive relationship with them. I travelled within the Soviet Bloc whenever possible (but even here there weren't many opportunities). I studied foreign languages. German and Russian were mandatory at school (at various points in time), but despite this I don't have a bad stance toward these languages. At a later stage I added other languages. I taught myself English and with this I listened to the BBC, which I didn't understand for a long time. Despite this I was greatly drawn to English, and with it to my English and American colleagues. I've already mentioned how enchanted I was by the 'new' Anglo-Saxon archaeologists. I've always had very friendly ties with Polish archaeologists, maybe because of the language similarities.

Only gradually did I realize that sympathy and personal friendship do not suffice to overcome the difference between archaeologists from different communities, even where there is no language barrier. As I've already mentioned, scientific communities (not only archaeological) are divided into mainstream communities and minority communities, and Czech archaeology clearly falls into the latter (1998a). Mainstream archaeologists simply do not need us and show it, mostly unawares, for example in the area of quoting or, in general, in using foreign opinions. Also other minority archaeologists have come to this conclusion, as have specialists in other scientific disciplines. It is a complex issue that is based on objective relations and cannot be resolved using moral principles; it cannot be approached on an emotional basis. Maybe that's why I've been publishing mainly in Czech recently, even though writing something in English poses no problems for me.

You are now surrounded mainly by young people, the majority of whom speak the scientific language which you yourself created for archaeology. And some of them might even understand it. How do you cope with the position of being a classic?

I think (and verify it in examinations) that good students understand my terminology. I am convinced that with processualism archaeology entered into a scientific phase where propositions cannot be formulated using 'natural' language alone; at least some concepts must be explicitly defined, i.e. by limiting their meaning in common language. It is an understandable tendency which I tried to support conceptually and terminologically.

Archaeological paradigms, before the onset of processualism, usually resolved the problems of other sciences, particularly history, and conveyed

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these mainly with the help of a few specific concepts (typological development, archaeological culture, etc.). Today one cannot borrow scientific terminology from other disciplines to apply in archaeology, for the simple reason that none of these deal with the central concept of archaeology – the artefact. Therefore archaeology has its own methodology and its theory, and I tried to explain this in a series of publications (1993a, 2007, 2010, etc.). It is clear to me also that entirely new issues will come along, which I never solved, and some will achieve different solutions to those that I've put forward.

I don't wish to be a classic who is evaluated in books on the history of archaeology. My aim is to positively influence archaeology (and through this also society) with ideas that I regard as being correct. I'd be satisfied with young people (as you call them) reacting to my ideas, whether positively or negatively, i.e. showing that I did not go by unnoticed.

And I would also be happy if I could still manage to work for some time, because I still have many ideas that I consider interesting. But I guess that is something that you can't arrange.

Nonetheless, I hope that it works out – for you and for the rest of us.

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