

The Bordes Method?

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Up until the present the Bordes method has been widely accepted among researchers of the palaeolithic throughout the world. However, its relevance to the tasks for which it is usually applied has not been proved and, moreover, is very doubtful. Both typologically and statistically the method is pernicious. It is necessary to elaborate a new approach to the analysing of stone industries on the grounds of realization that there can be and must be more than one classification for the same material.

Methods are tools. They are developed for specific tasks . . . But methods, like tools, can be abused. The most oblivious form of abuse involves using methods not because they fit the task in hand, but because they are methods we know and can easily apply (Moore & Keene 1983:4).

. . . only when current theories and methods have been fully evaluated and their weaknesses exposed can new theories effectively take their place (Collins 1970:17).

In the forty or so years of its existence the Bordes method has, on more than one occasion, been a subject of debate among palaeolithic archaeologists. Individual aspects of it have been elaborated and added to and have sometimes been subjected to more or less severe criticism (Mason 1967:758–759, Kerrich & Clark 1968, Cahen & van Noten 1970, Kozłowski 1972:455–457, Suleimanov 1972:74–76, Matyushin 1975:44, Gladilin 1976:4–27, Praslov 1984:100–101, Minzoni-Déroche 1985, and so on). On the whole, however, this method is still predominant in palaeolithic studies, and the associated path for the analysis of archaeological material has even been referred to as the principal ‘highway’.

In this article, which consists basically of an analysis and evaluation of the Bordes method as a whole and of its component parts, we have attempted to show that in fact Bordes’ ‘highway’ leads up a blind alley, and that it is necessary to adopt another method

of analysing material, based on different principles.

We will first try to answer the apparently simple question ‘what does the Bordes method consist of?’. We will use for this the (in our view, exhaustive) formulation of the method given by D. Sonnevile-Bordes. By looking at his works, it is easy to ascertain that this formulation fully corresponds to the more extensive description of the method by Bordes himself (Bordes 1950, 1961a, 1984 and others). Thus the Bordes method requires:

- (1) the establishment of a typology which recognizes, defines and describes types through technical and morphological analysis and, possibly, classifies them in groups;
- (2) the choice of a type-list which orders these types and groups. An inventory, with percentages, which records all the stratigraphically well-defined tools, excluding raw material, which is analysed separately;
- (3) cumulative graphs which, combining the frequencies of tools by type, allow a graphic representation of the dynamic or trend of the series;
- (4) indices and characteristic groups, resembling one or several technically, typologically or culturally significant types which may be isolated and represented by block diagrams (Sonneville-Bordes 1974–75:9–10).

So, it appears, the question is clear and the essence of the method fully understood. Let us, however, turn our attention to the first

point, where it is stated that the method requires 'the establishment of a typology'. This is a key point: the success — and the very possibility — of implementing all the subsequent procedures depends on the quality of the typology. The question is, *how* should this typology be established?

Unfortunately, practically nothing is said on this score, and no instructions as to method are given, either in Bordes' works or in those of his followers. The procedure for setting up a typology is represented as being self-evident, not requiring any kind of explanation or recommendation as to method (unless, of course, Bordes' references to a 'typological eye', acquired as a result of much experience of classifying, are to be taken as such).¹ It is quite clear that without a typology (a type-list) the Bordes method is inconceivable, but, at the same time, it does not provide a method for the construction of a typology. Bordes created his own typology, but paid no attention to the question of how he did this and how, in general, to create original typologies. So our first conclusion is that the Bordes method is not a method for constructing a typology.

Nowhere in Bordes' works does he state directly that his own type-list must be applied for the analysis of different assemblages from different regions. On the contrary, the fact that formulations of the method (see above) are concerned with the creation of type-lists can be regarded as an indication of the necessity of constructing a special one for each separate case. Nevertheless, the method itself manifests a very strong tendency towards a practical change in these procedural steps — towards the use of one and only one type-list. The fact is that all the subsequent operations make sense only if material from different sites is processed according to one type-list. Only then is it possible to compare cumulative graphs and indices (and any other parameters based on percentages) and to draw conclusions with their help. The more sites that are processed according to one type-list, the more effective is the method. It is no acci-

dent that Bordes himself, as far back as the mid-1950s, began to apply his own type-list, based on Mousterian material from southwest France, not only to material from France and Western Europe, but also from the Near East, North Africa and so on.

Thus two different interpretations of the essence of the Bordes method are equally possible. According to one of them, a typology should be created for a particular aggregate of material and then, on the basis of this, the prescribed statistical operations should be carried out; according to the other interpretation, the material should be classified in accordance with Bordes' type-list and the statistical processing should be carried out on this basis. We emphasize that the possibility of making either interpretation is inherent in Bordes' own work.

Both possibilities mentioned have been realized in archaeological practice. It is true that the Bordes method has been perceived as a sequence of procedures — the first of which is the construction of an original typology — only by specialists in the Upper Palaeolithic (Sonneville-Bordes & Perrot 1954, 1955, 1956, Tixier 1967, Abramova 1979a, 1979b), where any other interpretation of it is clearly not possible.

Moreover, Sonneville-Bordes and Perrot's original type-list came to be regarded by some as universal for the Upper Palaeolithic (Chernysh 1959, 1967). This interpretation of the Bordes method, in which the principal place is occupied by his type-list, undoubtedly predominates among palaeolithic archaeologists.

For most specialists, the application of the method under examination consists, first of all, in the division of their assemblages according to the classic type-list, which, in this way, is in practice perceived and used as a universal skeleton-key to any material. It is true that, more than 20 years ago, during the years of the triumphal march of the Bordes method through the palaeolithic assemblages of the world, some of the researchers who had enthusiastically propagandized it

wrote that a 'deeper study of flint artefacts necessitates the creation of new regional typological lists, more detailed for particular territories', but even in this case, Bordes' type-list itself was considered to be the basis of such lists, 'the framework for future processing' (Lyubin 1965:74-75).

If we turn to the publications of the past fifteen years, it is easy to see that, for researchers using the Bordes method, deviating from the classic type-list when setting up typologies is virtually impermissible. Only the occasional addition of new types to the list passes for the creative development of the method, along with the frequent changes introduced in the system of indices. There are, of course, examples where real attempts are made; on the basis of the classic type-list, to create fuller 'regional typological lists' (e.g. Kolosov 1986:16-18, Lyubin 1977), but in the majority of cases assemblages are simply assigned to the 63 categories on Bordes' list, or, more precisely, to those categories which manage to furnish a more or less suitable correspondence.

We will give some examples to illustrate what has been said. Piperno found 36 categories from Bordes' list sufficient for the Jahrom (Iran) assemblage, after which all he had left was a single *pièce esquille* (Piperno 1972). Akazawa also allotted the materials from level D of Shanidar to 36 categories from the classic type-list, adding only 'retouched rods' and declaring, moreover, that he was using 'a modified version of Bordes' type-list' (Akazawa 1975:5).

A. K. Dzhafarov, analysing material from the Talgarskaya cave (Azerbaijan), managed with 20 categories in the final table (1983:58), which swallowed up the assemblage without a trace. For the first layer of Shaitan-Koba (Crimea), 32 categories were enough (Kolosov 1972:24); for the Ketrosa site (Soviet Moldavia), 33 categories (Anisutkin 1981:48); and for Khudgi (Tadjikistan) 34 (Ranov & Amosova 1984:30-31). Similar examples could be cited *ad infinitum*.

Applying Bordes' type-list so widely and,

so to speak, automatically — using it essentially as a determinant — would be fully justified and in order only if the diversity of our materials always corresponded to the diversity of the material on which the classic type-list is constructed, and if the fixed combinations of attributes which composed Bordes' types were fixed for all complexes. It is clear, however, that in any new material other combinations of attributes may turn out to be fixed. If we then impose the classic type-list on them we will obtain a picture which does not reveal the material's characteristic structuredness or separate what is inherent in the material from what is accidental — the primary and basic task of our typology — but, on the contrary, which only conceals it.

The objection could be raised that all the arguments put forward here are of a very abstract nature, and Bordes' type-list has, moreover, apparently shown its viability in practice. Until now, the most varied assemblages from the most varied regions have been successfully processed with its help. It has been used hundreds of times. Has it, however, been verified?

Is it really possible to consider that those hundreds of occasions when material from a great many sites from different regions has been processed according to the classic type-list constitute a verification of it? This type-list is a *priori* in relation to every newly studied complex and, before applying it, it is worth asking whether, in the case in question, another typology, not provided for by Bordes' type-list, might perhaps not be more suitable for the material. To check this, it is necessary, laying aside the ready-made determinant and, as far as possible, ignoring it, to try to construct a typology stemming only from a particular aggregate of materials belonging to one complex and, of course, one culture. This approach is obviously more correct than immediately and mechanically transferring onto the new material the structure revealed in complexes which are geographically very distant and which, more

than likely, belong to different cultures,² thereby thrusting on it what is most likely an alien system of attribute-linking. As far as we know, however, none of the researchers using the Bordes method has reported any such check being conducted.

Therefore, when applying Bordes' typology *a priori*, we are probably very often just squeezing material into a framework prescribed for it and creating the illusion that the material corresponds to Bordes' typology. The character of the definition of most of the types in Bordes' list is such that it is, as a rule, easy to do this, whereas it is difficult or impossible to notice a strained interpretation from drawings of a few selected standard objects. In theory, it is possible to divide any material in accordance with the Bordes method, but it by no means follows that it is necessary to do so, or that such a division is the only possible and correct one.

In addition, work with data obtained by different authors using a single type-list (Bordes') presents quite a serious problem. The fact that two researchers have the same typological system is no guarantee that, when working with the same material, the final 'haul' in each of the cells of this system will turn out to be the same. To put it more simply, if two researchers are set to work with one set of material and one type-list they will consequently (at least in many cases) obtain different results. There are data from experiments of this kind but, unfortunately, as far as we know, they have not been published. Even so, there is fortunately some material in the literature that allows to substantiate our claims.

In 1984 Dibble published the results of his research on the Mousterian material from the Bisitun cave in Iran, which was previously studied by Skinner (1965). Both authors gave a typology in accordance with Bordes' type-list and used his definitions and indices. The differences between the total figures turned out to be appallingly great. Where the number of tools of a particular type given by Dibble exceeds that given by

Skinner this can still sometimes (though by no means always) be explained by the fact that Dibble had access to materials from level G, which were not studied by Skinner. But for 10 types the position is reversed and the discrepancies are not in ones but in tens (Nos. 6, 7, 21, 31 and so on). Finally, although Skinner discovered equivalents of only 24 types from Bordes' 62-category list in the Bisitun assemblage, Dibble used 47 items; that is, twice as many.

In order to really decide whether Bordes' typology is adequate for various materials and whether his definitions of types are satisfactory for different cases, a special check and study are necessary. Such a check is impossible without completing new classifications of material directly according to assemblage. At present it is impossible to avoid the conclusion that the tradition which has arisen of using Bordes' type-list has no factual or methodological basis and, most probably, distorts our understanding of the original material.

We now turn to the other interpretation of the Bordes method, which has found expression in several studies of the Upper Palaeolithic of Europe, North Africa and Siberia. Here, too, a whole number of problems arise, the solutions to which cannot be found in the work of Bordes or his followers.

First, as we have already noted, it is uncertain how we can establish a typology and regulate it (construct a type-list) in an altogether methodologically correct way.

Secondly, for the construction of a typology the aggregate of materials within whose boundaries it is created is very important. Even with an intuitive division, types will take shape through some replicability of combinations of attributes, considered to be characteristic and significant. Here the degrees of replicability and fixing of different characteristics should be assessed, and, through them, the degree of similarity between artefacts. But this makes sense only within some form of limited aggregate of data, in so far as, with the broadening or nar-

rowing of its parameters (if the material is not absolutely homogeneous), other combinations of attributes may turn out to replicate themselves more in new parameters, and it may be possible to evaluate the degree of similarity between artefacts in a different way. Consequently, changes in the typology will depend on changes in the parameters of the aggregate within which it is formulated. The problem inevitably arises of assessing the criteria for choosing the aggregate to be typologized, and of defining its parameters.

Thirdly, the question arises of determining when the existing type-list can be applied to new material and when it cannot. As yet this is successful only for clear-cut cases. For the overwhelming majority it is possible to do this only after constructing a separate typology for the new group of material which interests us. If it differs to some extent from the existing typology, then which of them should be used for research? Or is it necessary to construct a general type-list for a new assemblage which includes old and new materials?

Fourthly, the problem of working with data inserted in different type-lists remains methodologically unresolved. Assemblages entered in different type-lists should be compared outside the confines of the method, that is, without applying its statistical procedures. And, in fact, in this interpretation of the Bordes' method, only the statistical part is used when original typologies are constructed — for the actual process of their construction is in no way regulated by the method.

We now turn to an examination of the statistical part of the Bordes method. We recall that it can be reduced to two basic methods:

1. The expression in percentages (indices) of the representation in complexes of certain types, groups of types or technical elements.
2. The graphic expression of relationships between complexes (cumulative graphs, histograms).

These methods are evidently extremely elementary, and they came into use in Stone Age archaeology before the Bordes method appeared. Bordes only combined them into a single research process, and suggested a number of indices not previously used.

Percentages and cumulative graphs are, however, applied incorrectly in the Bordes method. They must not be used without calculating confidence intervals, otherwise serious mistakes are possible even at the data-processing stage. Attention has been paid to this in the archaeological literature (e.g. Pislary & Pozhidaev 1982). Even more important is the fact that the interpretive possibilities for cumulative graphs are too strongly dependent on the order in which the types occur in the type-list. Kerrich & Clark have demonstrated this in a specifically archaeological context (1968:57–64). Whether cumulative curves constructed out of exactly the same data will appear similar or dissimilar depends on the sequence of types in the type-list. The same material can be put in order and entered in graphs differently, and in this way it is easy to support any intuitive method by the corresponding construction of a cumulative graph.

In this way, the 'typologically statistical' (as it is sometimes called: e.g. Vekilova 1971:120) Bordes method turns out to be unsatisfactory as regards both typology and statistics.

But, the objection might again be raised, was it not precisely the application of the Bordes method which allowed important conclusions about the technical and technological heterogeneity of the Mousterian (which many interpret in the sense of the existence of different cultural traditions) to be drawn? Does this not mean that the method works after all?

We will examine the Bordes method in its original application on French material. Several types of Mousterian were isolated: typical, denticulate, Charentian and from the Acheulian tradition. The characteristics of each of these are well known and there is

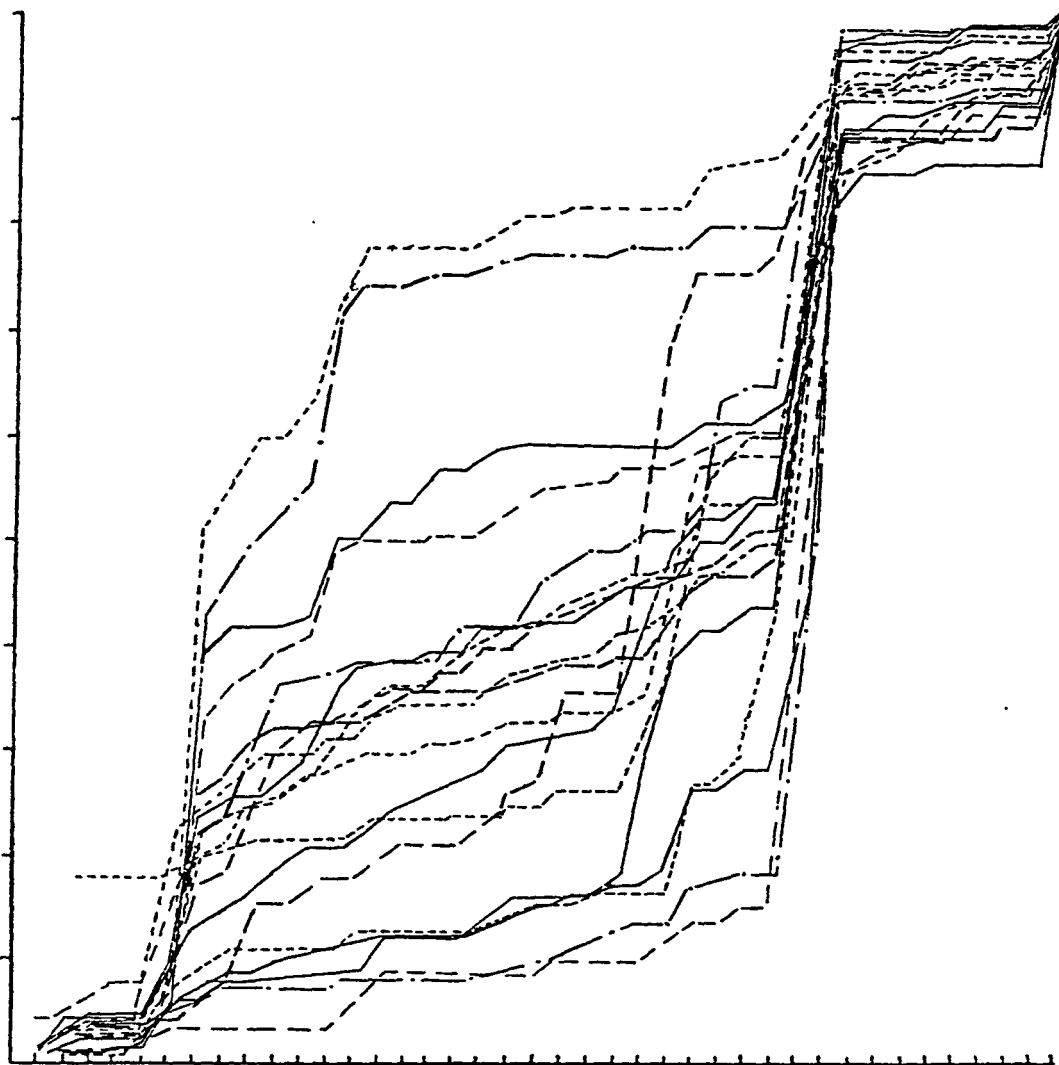


Fig. 1.

no need to repeat them here. Bordes was included to interpret these types of Mousterian as different archaeological cultures (e.g. Bordes 1960:102–103, 1961b:807, Bordes & Sonnevile-Bordes 1970:64).

We will start with the cumulative graphs. We have transferred all the cumulative curves on Mousterian sites cited by Bordes (1984) onto one graph (Fig. 1). It is apparent from this that in fact the groups of curves defined by Bordes and interpreted as charac-

teristic of different cultures hardly exist. The curves are disposed more or less evenly on the field of the graph, between the extreme values.³ The percentage indices of types for all the sites presented by Bordes change smoothly, without forming statistically significant clusters. This was also reflected in the values of the indices: they also change smoothly — a concentration can be observed only in the area of the zero values. And Bordes even used standard complexes, which

must be characteristic of Mousterian cultures. If to this is added the existence of complexes occupying an intermediate position and confidence intervals, which in this case are mandatory and which, nevertheless, have not been studied, and if Kerrich & Clark's instructions are borne in mind, then it becomes clear that statistically significant differences between the Mousterian cultures defined by Bordes are not observed or, in any event, their existence can by no means be considered proved or demonstrated.

It is also very characteristic that a total of only four categories of tool (denticulate, bifaces, scrapers and backed knives) are considered important and used for distinguishing particular types of Mousterian, and only two or three for each compared pair of types. Moreover, the internal differentiation of these types on the type-list is practically insignificant and means — and this should be emphasized — that the type-list itself is of no significance. This will probably seem paradoxical, but it is the case. Cumulative graphs constructed according to type-lists do not tell us anything, and the way that they are used cannot tell us anything. In fact it turns out that it is not the Bordes method at work, but something else.

We emphasize that what we have said about the method in no way excludes the possibility that those types of Mousterian defined by Bordes exist in France. They may even have been correctly defined, but this is not thanks to the Bordes method.

We would like to comment further on the types of Mousterian defined by Bordes (possibly digressing slightly from our immediate theme). They have begun to play the same role in palaeolithic archaeology as tool-types from the classic type-list, that is, they are in fact often used as a kind of universal type-list of cultures. This was, to a significant extent, facilitated by the work of Bordes himself, who wrote that while researching European, North African and Middle Eastern complexes he had observed 'several recurrent types of Mousterian, more

or less well represented in these different regions', accompanying this claim with the famous list (Bordes 1977:37–38).

Armed with Bordes' type-lists, Soviet researchers also got down to work. 'They picked out from their materials the same variants of the Mousterian flint industry as in France' (Formozov 1977:33). In this way, mysterious 'paths' and 'lines' of development appeared, undoubtedly originating from Bordes' 'type-list of cultures'⁴ and resulting from its imposition on the original material. In our view, the correct explanation of such startlingly apparent like-mindedness of the inhabitants of the Mousterian *oikumene* was given by Praslov. He remarked that 'the processing of material according to one scheme and a single set of criteria, suggested by Bordes, always leads to the results envisaged by this system. It is for precisely this reason . . . that single 'paths' or 'lines' of development of Mousterian cultures in different territories arise' (Praslov 1984:101).

It is interesting that the present situation with 'types of Mousterian' or 'paths of development' is very reminiscent of that which pertained around thirty years ago in the archaeology of the Upper Palaeolithic, when it was in the throes of separation from the standard French scheme; true, not a culture-dividing scheme but a periodizing one — Aurignacian, Solutrean and Magdalenian. Archaeologists, most of whom have had a historical education, could, it seems, learn a lesson from the history of their own discipline, which testifies that our (unfortunately silent) material can be squeezed at will into the Procrustean bed of any scheme, however unsuitable.

We will now briefly formulate our conclusions about the essence and quality of the Bordes method:

1. There are two possible interpretations of the essence of the method and two possible approaches to it. It can be seen either as simply a succession of distinct research operations (creation of an original

typology; calculation of percentages; construction and visual comparison of graphs and histograms), or as primarily a method of working based on a universal classic type-list. The second approach to the method is the predominant one.

2. In any interpretation of the essence of the Bordes method it is (a) not a method for constructing a typology, and (b) primitive and basically unacceptable in its statistical aspects.
3. The use of the Bordes method as a universal basis for typologizing is essentially incorrect, not well founded in practice and therefore unacceptable. In fact, it violates the material, thrusting upon it a pre-ordained structure rather than revealing its own particular one.
4. The results obtained on the basis of the Bordes method need to be verified and the idea that the types of Mousterian defined by Bordes are a 'type-list of cultures' must be eliminated.

Having thus formulated our conclusions we could draw a line here and consider our task completed. However, it is precisely in connection with the above conclusions that at least two more questions arise, an examination of which — albeit a short one — seems to us unavoidable.

The first question is: if the Bordes method is so bad, what then is the secret of its durability and why is it still popular?

The second question is: if the Bordes method is no good, what should take its place?

THE SECRET OF THE BORDES METHOD'S SUCCESS

The appearance of the Bordes method at the start of the 1950s was undoubtedly a significant step forward in the development of palaeolithic archaeology. It replaced the 'typological muddle' (as Lyubin (1965:14) aptly expressed it) and extreme subjectivity

of the evaluation of similarity and non-similarity. In place of terminological and conceptual chaos came order (standardization of terminology). In place of the judgement of types as 'more similar' or 'less similar' came indices, graphs and histograms (filling the neophytes from the humanities with holy dread). A single typological foundation apparently paved the way for statistics, and statistics (even elementary statistics), apart from anything else, satisfied the archaeologists' desire that their discipline should become more scientific, and answered the demands of the time and of fashion. All this ensured an interested reception for the method among the majority of specialists, and its consequent victorious march through the palaeolithic assemblages of many countries of the world. The Bordes method owes its importance in palaeolithic archaeology and its great influence on the minds of researchers not, of course, to the methods in themselves, or to the statistical techniques, but, primarily, to their combination with a detailed typology which has (largely incorrectly) been taken as universal.

This combination created an illusion of the objectivity in comparisons and conclusions towards which palaeolithic archaeologists had long been striving. And when it gradually became clear that a universal typology for the study of culturally and historically specific complexes and their cultural correlation was simply not possible, it turned out that giving up the hard-won illusion was far from simple.

The method survived the criticism and is again popular among palaeolithic archaeologists.

It is popular for a number of subjective reasons.

It is popular because the nature of the types from the classic type-list allows some kind of correspondence to be selected for them in a very wide range of complexes.⁵

It is also popular because there appears to be no valid alternative, although the various attempts to create one are well known.

WHAT IS THE SOLUTION?

Specialists who find the Bordes method unsatisfactory sometimes put forward their own systems for analysing stone artefacts. In their typological aspects, however, these systems usually not only repeat the basic shortcoming of the Bordes method — that is, the claim to universality of typology — but also add to it another equally real defect: *a priori* typology. The authors of such constructions seem to think that it is sufficient to observe the logical rules for a division into ranked conceptual categories which produce an ordered hierarchy of taxa — and all the problems will be solved.

Klejn, in his seminal monograph 'Archaeological typology', demonstrated the falseness of the path leading to 'a priori universal classifications-typologies of Gorodtsov's canonical type' (Klejn 1982:esp. 266–267). Many people are evidently won over by the closeness of such classifications to Linnaean classification — considered to be the ideal — and by the possibility of dividing material according to a precise 'previously defined scheme' (Gladilin 1976:29). We repeat, however, that before thrusting some scheme on the material, it is worth asking whether it has its own inherent scheme. Or, as biologists would express it, 'the form of a system must be revealed rather than postulated' (Lyubischev 1923, see also Meien 1978:503, Chaikovsky 1986:53).

Yes, a hierarchical classificatory system based on formal logical bases can be useful, but only for a definite purpose, specifically for preliminary auxiliary ordering of material.

The current practice in archaeology is to make a single classification of one set of material. However, the view that it is permissible, or even essential, to allow the creation of different classifications for the same material, has been around for some time.⁶ This primarily concerns the division of classifications into auxiliary and research (Gryaznov 1969, Kamenetsky et al. 1975, Klejn 1979, Klejn 1982).

Auxiliary classification is intended for initial ordering and description of material; for quick and reliable searches of the necessary data for various studies; for the inclusion of extensive materials in a concise form accessible to every researcher; and so on. Such a classification must permit the immediate entry of newly discovered material and, at the same time, reflect the mass of characteristics of the objects which are necessary for the inclusion of this material in research. In order to turn to research it is usually necessary to somehow include extensive material, to get a classificatory idea of it, to select from it what is presumed to be necessary and so on. Only after this does the research itself, including the creation of research classifications, become possible. Auxiliary classification, like research classification, is intended to serve research tasks, but mainly at the preliminary stage. This is the root of their differences. Auxiliary classification must therefore make a preliminary specification of the set of attributes and their possible meanings; the attributes must be relatively easily acceptable, unambiguous and quickly definable; the structure of the classification must be created beforehand and be useful in practice; all terminology must be standardized and simple.

Research classifications aim not just at ordering and description of material, but at the revelation of some kind of essential links and characteristics in it. They and their results are better termed 'typology'.

The most obvious and usual aim of archaeological research is the uncovering of the cultural specifics of a complex and its correlation with other complexes. When constructing such a typology it is possible to proceed solely from the cultural context of the given complex, or of several complexes if they are known to belong to the same culture. Each complex then begins to generate its own type-list, but the limits of the universality of a type-list are the framework of one archaeological culture.

Of course, even with this approach, a multitude of problems arise and demand to be solved — first of all, the problem of comparing the different authors' data. As it is not possible to examine this in detail here, we will only note that the traditional approach also — as shown by the example of Skinner and Dibble's typologies for Bisitun — in no way rescues us from the necessity of taking account of and overcoming the subjectivity of our typological values.

Bordes' typology fulfils both the auxiliary and the research functions (as does Sonneville-Bordes and Perrot's typology), although in fact it was originally intended chiefly for the research function and for fully defined material. The failure to distinguish between these classificatory systems has had far-reaching negative consequences. Description, reporting and publication of finds are conducted according to Bordes' type-list, and all research is also carried out on exactly the same basis, as recommended by Bordes himself. Bordes' typology, however, is not suitable *per se* for studying new material — as indicated in the main part of our article. It is also poorly suited to the role of auxiliary classification. In Bordes' works there is neither a system of attributes for description, nor the means of establishing them for new materials.

The fact that work based on Bordes' typology began to acquire automatically the status of scientific research for this reason alone, has long played an extremely negative role; the application of Bordes', or any other, typology in research does not in any way ensure that it is scientific. The use of his typology in description and publication of material does not in itself ensure the quality and scientific nature of this work. The quicker we rid ourselves and the literature of the illusion that Bordes' types alone are scientific, the less we will have to alter and the sooner we will obtain new scientific information.

The question of the necessity of carrying out both auxiliary and research classifi-

cations and of the impossibility, as a rule, of combining these two functions in a single classification has ceased to be a purely theoretical question in palaeolithic archaeology. It is now a practical question: wide experience of work with many kinds of material has already shown that we cannot get by without creating an auxiliary classification, separate from the research classification, if we do not want our discipline to stand still. This means that the Bordes method must be replaced not by a new typology and some kind of method based on it, but by an auxiliary classification intended especially for it, and by separate research classifications together with methods for constructing them and working with them.

NOTES

- 1 'One has to see a great number of implements, classify them, see them again several times, before one acquires a "typological eye"' (Bordes 1972:141).
- 2 Bordes' type-list is itself quite eclectic from this point of view, being only a guide to 'the definition and illustration of types', combined with a single list based on the choices of a single researcher. As Parkinson, in our view quite correctly, noted (although not specifically in relation to Bordes' type-list), 'many of the implement and cultural type names were coined in the nineteenth or early twentieth centuries when they proved useful reference points in a discipline still groping in the dark. However, there was a danger that these reference points would become pigeon-holes into which further data would be pushed or at times forced' (Parkington 1972:11, quoted in Klejn 1982:91).
- 3 'Given the same pair of curves some observers might find them to be quite similar and others might find them quite dissimilar'. This is one of the particular weaknesses of the method, as noted by Kerrich & Clark (1958:68–69; see further Minzoni-Déroche 1985).

- 4 For example, in the Caucasus, Lyubin entered all the Mousterian cultures he defined into three lines of development: typical, denticulate and Charentian (Lyubin 1977, 1984:69–70).
- 5 Such a possibility, latent in the type-list, is the sort of trap which it is possible to escape from only once one has understood its construction, which is far from obvious.
- 6 A similar necessity is also felt in many other disciplines, in particular, in biology where 'since Linnaeus' time a debate has been going on about the fact that at least two systems are necessary — an artificial one, useful for locating a species, and a natural one, expressing, as Linnaeus said, the essence of the thing (bearing in mind that naturalness is itself a relative concept)' (Chaikovsky 1986:48).

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