

by Esmée Webb (Organiser)

This two-day meeting to discuss the current state of research into all aspects of the Palaeolithic in Europe from the earliest industries to incipient plant and animal domestication was attended by about fifty people, mostly staff and students from British and continental universities, but also including independent researchers and amateurs. Belgium, France, Hungary, Israel, Italy, the Netherlands, Poland and Portugal, as well as the United Kingdom, were represented. A summary of the main themes, new ideas and conclusions is given here.

The first three papers discussed recent efforts to establish a chronological framework for the European Palaeolithic. Charles Turner (Milton Keynes) discussed the problems of attempting to correlate the available fragmentary terrestrial evidence with the deep sea oxygen isotope record, arguing that although the Holsteinian or 'great' interglacial can be correlated with both isotope stage 11 (420 - 370 ka), because that stage was the last time, prior to stage 5e, when the ice volume equated with the present interglacial, and with stage 7 (230-190 ka), on the basis of its complexity, the older correlation is preferable.

Both attributions cause problems for the interpretation of different classes of archaeological information, as Esmée Webb (Perth, WA) noted in her discussion of the chronology of late Middle and early Upper Pleistocene events suggested by recent radiometric datings. For example, the dates for the hominid fragment from Vertesszöllös (Siman pers. comm.) would suggest that it falls into stage 7, as would those from Arago (Chaline 1981), Bilzingsleben (Harmon et al. 1980), Steinheim and Swanscombe (Szabo and Collins 1975), yet they represent rather different physical types (Stringer 1981). However, all these finds have been assigned by most authors an 'Holsteinian' age on bio- or lithostratigraphic grounds. Clearly evidence for some of the interglacials recorded in the isotope record is missing on land. The problem of whether archaeological sites attributed to the Holsteinian/Hoxnian belong in stage 11 or later interglacials remains to be resolved. A consensus seems to be emerging amongst archaeologists that, while it is still legitimate to use the terms 'Würm' and 'Riss' to mean isotope stages 5d/4-2 and 6 respectively, the terms 'Mindel' and 'Günz' can only be used very loosely to mean 'sometime in the earlier Middle Pleistocene'.

The earliest securely-dated sites in Europe and the Levant remain Isernia-la-Pineta, near Rome, where simple flake tools were stratified in reversely magnetized sediments dated to >700 ka (Coltorti et al. 1982) and Ubeidiya, in the Jordan valley, also stratified in reversely magnetised sediments, attributed to the Matuyama epoch and now dated to 1.1 Ma (Goring-Morris pers. comm.). Both of these sites are located on natural routes for people, presumably *Homo erectus*, migrating out of Africa into Europe. Unfortunately, as Luís Raposo (Lisbon) reported, dated analogous evidence is not available from the Iberian peninsula, although Lower Palaeolithic material has been found which is thought on lithostratigraphic grounds to date to the early Middle Pleistocene.

Paul Callow (Cambridge) reviewed the Lower Palaeolithic of north-west Europe in the light of recent excavations, dates from which would suggest that typological criteria are useless as a guide to chronology. For example, the sophisticated Acheulean industry from Boxgrove, Sussex, excavated by Mark Roberts, now seems generally accepted as being of the order of 400 ka, and probably the oldest Palaeolithic site in Britain. However, the industry also includes undoubted Clactonian cores and flakes.

This suggests, first, that Ohel (1977, 1979) may have been right after all, even if for the wrong reason, and, second, that the archaeological evidence from Clacton, Hoxne and Swanscombe should be reviewed. John Wymer's forthcoming Hoxne report is eagerly awaited.

The transition between Lower and Middle Palaeolithic now appears as confusing as the evidence presented by the earliest industries. Conventionally, it used to be argued that the Upper Acheulean, made presumably by archaic *Homo sapiens*, disappeared at the end of the Riss glaciation, which can credibly be correlated with isotope stage 6 (160-120 ka), and was replaced by 'Würm I' times by Mousterian industries made by *H. sapiens neanderthalensis*. This scheme will no longer work. In the first place evidence from sites like Pontnewydd (Green 1984) suggests that Neanderthals were alive and well in north-west Europe by about 200-250 ka. Moreover, those radiometric dates available from recently excavated sites such as Abri Vaufrey (Rigaud 1982), Biache-Saint-Vaast (Tuffreau 1979) and la Cotte de St. Brelade (Callow and Cornford 1986) suggest on chronometric and stratigraphic grounds that industries clearly attributable on typological and technological grounds to the Middle Palaeolithic were already established by stage 6.

The clearest message to emerge from these papers was that we cannot rely on the typology of Palaeolithic artefacts to provide more than the haziest cultural chronology, that equally we cannot trust implicitly in the ages now being produced by radiometric techniques, many of which are still in their experimental stages, and that many problems still remain to be ironed out of the lithostratigraphy, both of Europe as a whole and of archaeological sites in particular. At present one can select to cite only those ages which fit one's preconceptions because there is little objective possibility of assessing the validity of the few dates available. The way to solve our very serious chronological problems is not to throw every conceivable radiometric technique at any site in a desperate attempt to 'prove' it is as old as the excavator believes, as has been done at Petralona (Wintle and Jacobs 1982), nor is it to attempt to date the fragmentary remaining deposits at key archaeological sites, particularly if there is any doubt about the stratigraphy. It is instead to carry out planned dating programmes, using as many techniques as possible, in which the samples are selected by the scientists who will process them on sites while they are in the course of excavation. Only thus can a body of dates be built up comparable to that based on radiocarbon now available for the Upper Palaeolithic and later periods, which will enable us to eliminate the anomalies.

Far too little is known, outside the Iberian peninsula itself, about the archaeology of Portugal. The survey of recent work on the Portuguese Lower and Middle Palaeolithic by Luís

Raposo was, therefore, particularly welcome. None of the sites he discussed has been dated radiometrically, however human occupation of the Atlantic coast clearly goes back into the Middle Pleistocene. Apart from some dubious 'pebble tools', about which he was rightly sceptical, the earliest artefacts are Acheulean bifaces often made on non-flint raw materials which affected both technology and final form. Several assemblages are now known which lack bifaces but use the Levallois technique. Whether they should be classed as Lower or Middle Palaeolithic is a moot point.

None of the papers dealt exclusively with the Middle Palaeolithic *sensu stricto*, which reflects, I think, current confusion over early Upper Pleistocene chronology (Webb 1988) and the models so far used to explain techno-typological variation in these industries. In Portugal, for example, there is no classic 'Mousterian' as defined in France because few sites have produced typologically classifiable retouched tools.

Catherine Farizy (Paris) argued that new interpretative approaches will be needed before assemblages attributed to the period c. 200-30 ka begin to be understood, but clearly there was some measure of cultural continuity with Acheulean industries overlapping the 'Mousterian' at its beginnings, c. 150 ka (Ronen 1982), while debate continues over the nature of the Middle to Upper Palaeolithic transition at its end (Trinkaus 1983). The paper by Patrizia Gioia (Rome), arguing that the Uluzzian represents the Italian facies of the Châtelperronian will be relevant in this regard. She emphasises that the Uluzzian is clearly stratigraphically earlier than the Aurignacian in Italy, and should probably be derived, like the Châtelperronian, from the Middle Palaeolithic. Katalin Simán (Budapest) discussed another possibly transitional industry, the Szeletian of central Europe (Allsworth-Jones 1986), which Hungarian archaeologists consider to be already an Upper Palaeolithic industry and which they now regard as comprising two components: an early facies which has techno-typological links with the central European Middle Palaeolithic with *blattspitzen*, but which disappeared without trace before 30,000 BP; and a later facies, which they link to the Gravettian, which appeared after 30,000 BP.

Our ability to identify such cultural hiatus depends partly on the discriminative quality of the artefact typologies on which our industrial attribution are based, but also on the precision of the chronological framework into which individual sites are slotted. For the Upper Pleistocene too much reliance is placed by archaeologists on the climatic sequence devised by Arlette Leroi-Gourhan and her students on the basis of their palynological studies at archaeological sites (Leroi-Gourham and Renault-Miskovsky 1977; Renault-Miskovsky 1986). However, as Charles Turner has pointed out (1985, 1988), there are serious theoretical problems with the schema they have created. Unfortunately, it has been widely-used by archaeologists to create a monolithic system of cultural phases which will be difficult to dismantle, given the frequent inertia of archaeological thinking on matters chronological. As Turner said in discussion, archaeologists must ensure they are up-to-date on the more strictly biostratigraphic literature.

François Djindjian (Paris) discussed the beginnings of the Aurignacian in Europe. This technocomplex is now generally

recognised as the earliest 'true' Upper Palaeolithic manifestation in western Europe, where it is clearly intrusive (Djindjian 1986), but its origins remain as obscure as ever. This was one of many problems which the Russians, had they been able to attend, might have helped to clarify. Those radiocarbon dates now available suggest that Aurignacian assemblages developed some 10,000 years earlier in eastern and central than in western Europe, indeed a clear chronocline can be charted from east to west, which is sometimes linked to the invasion of Europe by *H. sapiens sapiens* moving from the Levant northwards (Mellars and Stringer forthcoming). However, although industries called Aurignacian exist in the Middle East, in the opinion of Levantine specialists (Bergman pers. comm.) they are not analagous to the European Aurignacian and cannot be interpreted as its possible precursor.

In Italy, it might have been expected that glacial environmental conditions would have been ameliorated by the Mediterranean and that people could have lived there more easily than farther north, however the evidence available suggests that human occupation was not continuous. Margherita Mussi (Rome) argued that the peninsula was abandoned for several millennia between its occupation by early Aurignacian people before 30 ka and its recolonisation in late Gravettian times after 25 ka. It is possible that the extensive continental shelf which would have been exposed by eustatically lowered sea levels in the Adriatic region was occupied, particularly during the glacial maximum, however this area is little known archaeologically. Had Ivana Radovanovic (Belgrade) been able to attend the meeting she might have been able to clarify this point. No satisfactory explanation for the observed hiatus has yet been proposed, however Mussi did emphasise the cultural isolation of Italy during the early Upper Pleistocene. Regular outside contact was not apparently maintained until late Gravettian times, as demonstrated by mobile art, discussed by Daniela Zampetti. In Portugal, by contrast, João Zilhão (Lisbon) demonstrated a complete cultural sequence from earliest Aurignacian to final Magdalenian. Presumably the greater land area available in the Iberian peninsula permitted continuous occupation by human groups who would by no means have been protected from climatic oscillations resulting from movements of the Polar Front in the North Atlantic (Ruddiman 1988).

Marcel Otte (Liège) considers that the Gravettian shows the influence of earlier traditions, suggesting that it can be locally derived (Otte 1981; Kozłowski 1986). Similarly, he suggested that the high-cost bifacially pressure-flaked Solutrean 'leaf-shaped' points might have been a response by people using a basically Gravettian technology to enforced social crowding during the last glacial maximum, when environmental conditions confined them in south-west Europe and increased the need for inter-group differentiation. He argued for the existence of two main areas of influence during the late glacial, one in the Paris Basin and the Low Countries, where the classic Magdalenian is found, the other in north Germany and Britain, where tanged points of the Hamburgian/Cresswellian tradition are common. These two technocomplexes appear to have overlapped both in space and time, particularly in the Low Countries, although Charles Turner warned against his basic assumption that the late glacial climatic phases (Bølling, Dryas, Allerød, etc.) could be presumed to represent penecontemporaneous blocks of time. Almost certainly

such vegetational changes were diachronic and not distinctively present everywhere in Europe.

These papers clearly demonstrated that for much of the last glaciation many parts of Europe were probably only marginally suitable for human occupation, for example Poland, discussed by Janusz Kozłowski (Krawkow), north Germany, and Scandinavia. Certainly the archaeological record is patchy over most of central and north-west Europe during the Upper Palaeolithic. Given the amount of fieldwork that has now been carried out in these countries (Kozłowski 1983; Otte 1982, 1987), these gaps in the cultural record are likely to represent real human absences. If the evolutionary biologists are correct in suggesting that speciation occurs primarily in small, isolated, outlying populations (Dawkins 1986), by analogy we might logically expect to see greater cultural differentiation and economic diversity in these marginal regions. Such an argument was put forward by both Zvelebil and Goring-Morris (see below) to explain facets of late glacial archaeological diversity.

The remaining five presented papers surveyed aspects of the human adaptive response to changing environmental conditions during the late glacial. In the Italian Alps, Michele Lanziger (Ferrara) discussed Epi-Gravettian evidence for seasonal transhumance between lowland winter camps and high altitude summer camps. Similar research has been carried out by Berit Eriksen (Aarhus) for south-west Germany. Zofia Sulgostowska (Warsaw) charted changing settlement patterns in the Dnestr-Vistula basin during the same period (12 - 10 ka), suggesting that the area was more densely occupied during the Younger Dryas than the Allerød period, the reverse of expectation. Human groups were by this time highly mobile, able to exploit a wide range of ecological conditions from temperate forest to tundra and capable of maintaining long-distance trading contacts, up to 600 km, if the distribution of raw materials is any guide. Clive Bonsall (Edinburgh) argued on the basis of his excavations in the Inner Hebrides that people colonised Scotland with a Late Palaeolithic technology before the Loch Lomond readvance, rather than in the Mesolithic after it, as is usually assumed.

Marek Zvelebil (Sheffield) presented a highly speculative paper suggesting that, whereas Late Palaeolithic hunter-gatherers probably were forced by the harshness of living conditions at the time to maintain egalitarian social groups, in the Mesolithic people, far from being 'noble savages', were forced by deteriorating post-glacial environmental conditions into increasingly differentiated and hierarchical social structures. His thesis is difficult to substantiate, thinking of the Sungir' burials for example, and generated much lively discussion. It nonetheless merits consideration.

Finally, Nigel Goring-Morris (Jerusalem) argued that in the Levant the origins of sedentary human communities subsisting on domesticates lies in the preceding Epi-Palaeolithic period, when fluctuating climatic conditions in this region caused hunter-gatherers living in marginal environments, into which they had expanded under optimal conditions, to retreat from the periphery during periods of ecological stress to optimal areas where they concentrated on collecting plant resources. The prevalent faunal resource, gazelle, was not amenable to domestication. These shifts in a growing population sufficiently disturbed the local

ecological balance that, by 10,000 BP, Levantine hunter-gatherers were forced to diversify their economic base and cultivate their food supply.

The underlying themes to emerge from these papers were:

1. Artefact typology, once an end in itself, is now used merely as a means to the end of explaining past human behaviour. The determination of the processes by which stone tools were made, the extraction and distribution of the raw material on which they were made, and the use to which they were put are now of primary consideration. Typology alone has been demonstrated to be inherently unable to solve any of the chronological or socio-economic questions with which Palaeolithic archaeology is now concerned.

2. Environmental data, both floral and faunal, have always played a larger part in stone age studies than in the later archaeological periods, however the information they offer is now used with increasing sophistication in the creation of consciously structured models aimed at the interpretation of past economic systems; not just to provide a chronological framework. Archaeology is slowly coming to grips with the complexities of the Pleistocene seen from a foraminiferal perspective and beginning to be able to answer the complex questions concerning past socio-economic systems which it has always attempted to ask.

Unfortunately, although it had been my explicit aim to invite as many speakers from Eastern Europe as possible, and funds had been successfully sought sufficient to finance the stay in London of up to a dozen East Europeans, who were duly invited, in the event most of these scientists were unable to complete the necessary exit procedures in time to attend. Only the speakers from Hungary and Poland were able to present their papers in person. However, it is expected that most of those unable to attend will submit their papers for publication in the conference proceedings. It was particularly unfortunate that the Russians, from the Archaeological Institute in Leningrad, were unable to attend, since their abstracts suggest that their approach to archaeological problems is rather different from those with which we have become familiar in the west. M.V. Anikovich, for example, argues for what we would see as a return to a technology-based chronological scheme of Bone, Stone and Ceramic ages, when we are now more concerned with socio-economic questions.

The paper by P.M. Dolukhanov was full of good ideas discussing the inadequacies of the concept of 'culture' as an explanatory paradigm in archaeology and argued that two spheres of influence existed during the Palaeolithic, the Mediterranean area and the north-central European Plain. I was a little startled, however, to find 'Basque-Caucasian' suddenly proposed as the language of the former, and 'Uralian' as that of the latter, while Indo-European became the *lingua franca* of early agriculturalists! Papers by L.V. Golovanova on the Acheulian of the Caucasus, A.A. Sinitsyn on *pièces esquillées* and S.A. Vasil'ev on the archaeology of north-eastern Asia, were more concerned with factual matters and concentrated more on analysis and description than explanation. Clearly, had they been able to attend, there would have been many interesting discussions on the Russian approach to archaeology. I particularly regret not having

been able to meet Vasil'ev, given my interest in the PalaeoIndian colonisation of North America to which his work in Siberia is directly relevant.

All the papers given at the meeting will be published together with additional papers solicited both from those invited speakers who failed to attend and from additional researchers from both eastern and western Europe, either known to me personally or recommended by those speakers who did attend. If any reader feels that he or she has research on hand which falls within the conference topic, a detailed abstract should be sent to me at the address below as soon as possible. The resultant volume will provide a comprehensive, up-to-date guide to current thinking by native researchers on problems in Palaeolithic archaeology throughout Europe, and should prove very useful to all those interested in this topic. A publisher is actively being sought for what is likely to prove a sizeable tome, since it will comprise about forty papers, each up to twenty pages long. If anyone has any constructive suggestions, do please let me know. So far, I have approached Plenum and the Presses of the Universities of Chicago, Edinburgh and Sheffield, since they all have well-established reputations for speedy, low-cost but high quality publication in the archaeological field. It is hoped that the book will appear early in 1989.

Altogether, despite the absence of so many East European speakers, about ensuring whose attendance at future meetings I have learned a lot, the conference was, according to those who attended, a resounding success. I am relieved that my efforts culminated so happily for all concerned, particularly since I am leaving England at the beginning of July for a new career at the University of Western Australia in Perth, where I shall continue my interest in the analysis of human responses to environmental change by conducting research into Late Pleistocene Aboriginal settlement patterns.

Acknowledgements

This meeting was made possible by grants of supporting funds from the following organisations, whose assistance is gratefully acknowledged: the British Academy, the British Council, the Great Britain-East Europe Centre, the Great Britain-USSR Association, and the Royal Society.

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FLINT AND STONE IN NEOLITHIC BRITAIN, CARDIFF 25 - 27 March 1988

by Frances Healy

Nearly sixty participants, including two from the Netherlands and two from the United States, gathered for the first joint meeting of the Neolithic Studies Group and the Lithic Studies Society. The weekend was admirably organised by Tim Darvill, Stephen Green, Alan Saville and Alasdair Whittle, supported by Elizabeth Walker of the National Museum of Wales.

An opportunity to handle material from Hazeldine Warren's excavations at Graig Llwyd elucidated the debitage techniques employed at the site, while Stephen Green suggested that the evidence recorded in Warren's original report may indicate an organization of work comparable to that now reconstructed for the Langdale quarries.

Alan Saville's opening address charted the past progress and remaining shortcomings of Neolithic studies and emphasised the continuing need for higher and more consistent standards of recovery, recording and curation of lithic material, without which its value for the overall understanding of the period cannot be realised.

The subsequent papers, abstracts of which are reproduced below, left participants competing for discussion time and raised several salient points:

1. The extra-functional significance of at least the finer stone and flint axes, seen in the costs of extraction and manufacture, in their final mode of deposition (for example in hoards or in contexts from which they could scarcely be retrieved, such as the bed of the Thames or the East Anglian Fens), and in the presence of exotic axes in areas which had more than adequate local supplies (for example Group VI axes in Ireland).
2. The operation at a single source of different working methods at differing levels of efficiency, although at both Great Langdale and the Sussex flint mines it remains to be seen whether these variations are of chronological or socio-economic significance. It remains also to relate them to variations in scales and patterns of distribution over time.
3. The value of seeing lithic procurement and use in the context of local geography and settlement. Richard Bradley painted a vivid picture of the working of Great Langdale tuff as an aspect of the use of upland summer pasture by communities cultivating the Cumbrian plain and of their communal monuments as centres from which the axes passed into wider circulation. On a more modest scale, John Schofield's division of the Meon valley (Hants) into zones of extraction and settlement and my own use of East Anglian settlement assemblages and fieldwalked collections as a key to the use and dispersal of Grime's Graves flint adopted a similar approach.
4. Our still lamentably poor understanding of flint mine sites. This is at least in part due to the nature of the material extracted from them. Flint, at least in Britain, is seldom macroscopically attributable to a particular source and, more