

THE LONDON CONFERENCES ON ARCHAEOLOGY, 1943 AND 1944

By HUGH HENCKEN

*Curator of European Archaeology, Peabody Museum, Harvard University ;
Director of the American School of Prehistoric Research*

In August, 1943, and September, 1944, there were held at the University of London Institute of Archaeology two archaeological conferences, the published reports of which¹ make very impressive reading across the Atlantic. It seemed as though the stress of four or five years of war, while halting most archaeological work, had brought into much sharper focus many questions relating to organization, method and theory that were no more than dimly envisaged before 1939.

Before discussing the Conference of 1943, which was devoted largely to questions of organization, I shall turn to the extraordinary series of papers read in 1944. Those discussions must have left a most vivid memory in the minds of all who attended them, and in them I propose to join, however belatedly. Their stimulus was largely derived from the fact that old conceptions and prejudices seem to have disappeared, and all problems were viewed anew with a fresh insight.

Professor Childe led the procession. Were there no hand-axes, he asked, in Trans-Chellasia, that vast space of the Old World where they have not yet been found? And if they were not there, was this all an uninhabited wilderness? This prompts a query about the hand-axe area itself, an enormous territory extending (with interruptions) from India across south-western Asia, over a large part of Africa and finally into the western part of Europe. Breuil has dated his Abbevillian to the First Interglacial on the Somme, but north-western France looks from the distribution like a remote peripheral extension of the hand-axe area. When and where, then, was its beginning? As for the part of the Old World where hand axes are absent, Dr. Movius² has of late drawn together various Far Eastern cultures characterized by chopping tools rather than hand axes. But there still remains the question of what lies West of the chopping tools and North of the hand axes.

Professor Childe also discussed the origin of the Solutrean in the light of Pericot's work at Parpalló. One may add to this Professor Coon's excavations at Tangier,³ which make it seem very probable that the Solutrean developed out of the Aterian of North Africa instead of spreading from eastern and central Europe.

Dr. Zeuner added to the discussion a paper on his Palaeolithic chronology. His published remarks are so brief that a reviewer has little scope for comment without in fact commenting on Dr. Zeuner's other publications. Hence I shall only say that, despite all the objections raised against it, his chronology based on

¹ University of London, Institute of Archaeology: Occasional Papers Nos. 5 and 6: No. 5 (1944).

CONFERENCE ON THE FUTURE OF ARCHAEOLOGY, held at the University of London Institute of Archaeology, August 6th to 8th, 1943. Price 3s. 6d.

No. 6 (1945). CONFERENCE ON THE PROBLEMS AND PROSPECTS OF EUROPEAN ARCHAEOLOGY, held at the same

Institute, September 16th to 17th, 1944. Price 5s.

Both published by and to be obtained at the University of London Institute of Archaeology, Inner Circle, Regent's Park, London, N.W.1.

² H. L. Movius, *Early Man and Pleistocene Stratigraphy in Southern and Eastern Asia*: Papers of the Peabody Museum, vol. XIX, No. 3, 1944 (see notice on p. 171—*Editor*).

³ Publication pending.

solar radiation is very attractive. It is in about the right order of magnitude for the duration of the Pleistocene, and it offers at least a hope of an absolute chronology based on natural phenomena. Further confirmatory work on the geological side would of course have to be done before this theory could be accepted with assurance. Ideally all Pleistocene glacial phenomena throughout the world should be shown to be in harmony with it, but this is probably asking for a good deal. Still, the evidence for the dual character of the Günz, Mindel and Riss glaciations should be capable of more extended demonstration. There is also the puzzling question of what happened to the solar radiation curve and its accompanying rhythm of glacial advance and retreat before the Pleistocene. Mr. Hawkes seemed to feel on grounds of general probability that the chronology was too long, and that students of art history would have trouble in stretching Palaeolithic art over a span of upwards of 50,000 years. But some glacial geologists who do not accept the chronology based on solar radiation would make the Pleistocene much longer than Dr. Zeuner would. In any case, human cultural development has on the whole proceeded at an ever accelerating pace. In our day the speed has become breath-taking, while in the Palaeolithic it was infinitely slow. On almost anybody's reckoning the hand-axe was a favourite tool for hundreds of thousands of years. If the development of art were not also in slow motion, it would surely be out of step with its environment.

In dealing with the Neolithic, Professor Childe was inclined to retreat somewhat from the diffusionist hypothesis. Soviet prehistorians, he said, believed that during the Mesolithic some Palaeolithic tribes took up sedentary fishing and collecting while others took to agriculture. But, since the vast spaces of the Russian forests and steppes were far away from the principal routes of the diffusionists, is not that what one would expect in Russia? Also there is evidence from physical anthropology. I cannot go the whole way with my colleague, Professor Coon, in his *Races in Europe*, but he seems to me to show that Neolithic people were newcomers different from their Upper Palaeolithic predecessors, though they often showed signs of mixture with the Upper Palaeolithic type, especially in peripheral regions. It is, however, true that diffusionists have not given much attention to numerous hints of grain and possibly domesticated animals before the Neolithic. Among others are the bones of ox and sheep found with Mesolithic material at the Portuguese site of Muge, whose middens also produced human skeletons of Neolithic type.

As for plants, in 1939 Professor Oakes Ames of Harvard stunned his archaeological colleagues, whom he had wisely not consulted beforehand, with the statement that the history of cultivated plants must have reached far back into the Pleistocene.¹

As for pottery, Professor Childe alluded also to the discovery of Boreal pottery in Denmark.² In this connexion it may be mentioned that at Tangier there was found a Neolithic stratum directly over the Upper Aterian, and this Neolithic deposit contained a pot which might almost be mistaken for an Ertebølle pot. But what connexion could there be between Morocco and the Baltic?

It should not in any case be forgotten that our system of historical chronology

¹ Oakes Ames, *Economic Annuals and Human Cultures* (Botanical Museum of Harvard University, 1939).

² Therkel Mathiassen, 'Stenalderbopladsen i Aamosen', *Nordiske Fortidsminder*, Bd. III, Nr. 3, 1943.

in the Near East goes back only to about 3000 B.C., and at that extreme range is only approximate. Before that Near Eastern dates are little better than guesswork based on dead reckoning. As for the northern dates based on geochronology, they may not be above reproach, but at least they offer the semblance of a more solid foundation. One can only wonder what changes may be made in northern chronology itself.

Personally, I should feel happier about the coming of the Neolithic if the diffusionist theory were retained, but with the proviso that it was a very slow and very gradual spreading both of new people and new ideas, that both new and old mixed together, and that the whole process may have been much longer and slower and much more complicated with cultural lags and overlaps than we, in our effort to simplify, have admitted before.

Professor Childe and Mrs. Hawkes also alluded to the vexed business of the megaliths. It is, of course, unwise to reduce any archaeological form to a rigid typology, if only because human behaviour is itself otherwise. But we must first decide whether Professor Forde was right in his views expressed some years ago¹ that the tholos was really the basic form from which the passage grave developed. Professor Childe often takes a healthy delight in undermining views which he has himself aided in establishing, and this he did when he said that all the furniture from the tholoi is against an early date. It is true that the Iberian tholoi are embarrassingly like Mycenaean ones in plan, but it would also seem to be true that their furniture is more like that of the Early Aegean world than anything of the Mycenaean Age. This does not mean that tholoi and passage graves did not survive for a long time. The segmented bead from Parc Guérin and the Wessex pendants from others are as nothing compared to Raftery's astounding discovery that one of the Lough Crew cairns had Iron Age material underneath it. This latter phenomenon only goes to show that in Ireland there is almost no limit to the possibilities of survival.

Furthermore, if one accepts the view that passage graves were as a family degenerate tholoi, then to make the tholoi late would create havoc in northern chronology by bringing the Northern passage graves and their accompanying Neolithic culture down much too far with relation to the Central European Early Bronze Age. Indeed Dr. Schaeffer gives us some reason for dating this period a little earlier than accepted views would previously have allowed.

Mrs. Hawkes also touched upon the question of the social basis of megalithic diffusion. One of the oddities of this is, of course, that in western and northern Europe the architecture is imported, but the contents of the graves are mainly regional. A suggestive comparison with Latin America may be cited. In the countries south of the Rio Grande, there rise on every hand vast and gaudy baroque churches built by the *Conquistadores*. But in many places the *Conquistadores* and their descendants have disappeared. Only the Indian kneels before the shrine, and in Indian hands the consecrated wine becomes the blood of Christ. This may suggest an explanation for our shabby old megaliths with their Mediterranean architecture and their local furniture.

In another department of the Neolithic, Dr. Clark's truly astonishing account of the refinements of pollen analysis to detect the presence of a farming community

¹ C. Daryll Forde, 'Early Cultures of Atlantic Europe'. *American Anthropologist* xxxii (1930), 19 ff.

and his stress on the importance of the forest to early farmers do much to clothe the drier bones of the past with life. If there is any subject that needs to be pursued further, it is this. Chronology, megaliths and potsherds all have their place, but Dr. Clark has put his finger on the very life of the past.

The Neolithic of south-western Europe was touched upon in a most stimulating way by Mrs. Hawkes. But there is one question in her paper that causes me to doubt. It is the date of the Almerian Neolithic of El Garcel. This really seems to be based on what some have called the 'pre-predynastic' cultures of Merimde and Fayum in Egypt. Mrs. Hawkes suggests that these cultures did not last much beyond 3700 B.C., but no doubt she would agree that this date contains a large element of dead reckoning. Others would put an approximate 4000 as their latest limit. But in any case she postulates a period of a thousand years between the end of this kind of Neolithic in Egypt and its appearance in Spain. She agrees that there is really no evidence that it languished somewhere on the periphery of Egypt, though, of course, there is no evidence to the contrary either. In accepting this chronological vacuum Mrs. Hawkes is following an established view. But it seems to me that one principle of chronology is that the provincial form of a culture is at least as old as the end of the culture in its main centre. While agreeing that there is a great deal that is obscure about all this, I should suggest that, until the contrary is demonstrated, the Neolithic of El Garcel might as well be placed back in the fourth millennium B.C., if not before. It is also obvious that anyone might be richly rewarded who would look in North Africa for the links between Egypt, El Garcel and the cave culture of Morocco and Spain.

This now brings me to the papers dealing with the Bronze Age. Dr. Schaeffer has cast a great deal of light on the beginnings of the European Bronze Age, and under his treatment the famous Syrian Byblos hoard becomes merely one of many occurrences in that part of the world of ingot torques and pins of Aunjetitz type. What is more, these are not dated at 1800 as formerly, but to 2100-2000. Unfortunately space did not permit Dr. Schaeffer to explain how he arrived at this earlier date, but, if he is correct, the beginnings of the Aunjetitz-Perjamos group of cultures in central Europe could be moved back accordingly. This leads to the question of the possible eastern origin of much of the Early Bronze Age of Europe. First we know of the Early Macedonian culture arriving from Troy II about 2500. Then we have the Syrian ingot torques and accompanying pins of 2100-2000 far up the Danube. Mr. Hawkes, in his *Prehistoric Foundations* suggested Anatolian analogies for the Perjamos handled jugs. Professor Childe has also suggested an Anatolian origin for the El Argar Culture of Spain. This is a line of enquiry that might prove revealing.

Dr. Schaeffer also raised the question of amber. How much of this really comes from the North Sea-Baltic area? 'Northern' amber in the Mediterranean and central Europe has long been relied upon as indicating payment for numerous southern imports. But if we remove the amber from the picture, the imports in the North are no less southern, and the connecting routes can to some extent be traced out by other objects. Hence the matter is not so vital as it might appear at first. Recently I had a surprise regarding amber. The greater part of the Mecklenburg Collection containing many hundreds of Iron Age grave groups from the eastern Alps is now in the Peabody Museum at Harvard. These graves contain

thousands of amber beads. A colleague was recently kind enough to examine them for me and reported that with one possible exception they did not come from the North at all but from Rumania. His report was the same on numerous beads from North Italian graves of Este III. This verdict is so unexpected that I hesitate to put too much reliance on it until it is independently verified, but offer it here as a sample of the shocks against which one must brace oneself.

Dr. Zeuner also warned that only East Prussia, a long way from the main centres of the Northern Area, had enough amber to support a regular trade.

A matter to which no one seems to have alluded, but which occurs to me to be one of major importance, is the question of Mycenaean influence in the Middle Bronze Age. From the works of Childe, Hawkes, Forssander and Åberg one can gather that this influence was considerable. It is represented with varying degrees of certainty by the flange-hilted sword, the socketed spear, spiral ornament on metal, the fibula, the ivy-leaf pendant, the making of vessels of hammered bronze and by glass beads. Yet no one has to my knowledge treated it fully or traced the routes by which it came.

An especially welcome addition to the conference was light on eastern Europe from Professor Sulimirski of Cracow. His remarks are a strong reminder to those of us who have not bothered to learn Slavic languages that many important aspects of ancient Europe lie beyond our ken. Of primary importance is, of course, his stress on the geographical division of the area into steppe, steppe-forest or black earth, and true East European forest. These areas supported quite different types of culture in defiance of standard chronological divisions. He shows that the painted pottery, previously confined to the Neolithic, lasts down to about the 12th Century B.C., and he wisely remarks that it is improbable that any such large ethnic group as the people that made it would be annihilated or herded off *in toto* to some hypothetical limbo by an 'invasion'.

According to Professor Sulimirski, the painted pottery was only gradually displaced by black graphite pottery which closely resembled it except for surface treatment, and this graphite pottery lasted down to the Scythian period. Surely the Lausitz pottery of Seger's Group C, similarly coated with black graphite, must be related to this.

It may be wise also to ask how far the term 'Hallstatt' may be applied, as Professor Sulimirski applies it, to eastern Europe. It often means the mixture of Tumulus and Urnfield populations in central Europe who learned the use of iron and various forms of bronzes from the Adriatic-Italic Area. It is a cultural term of admittedly rather vague application, but, when it is stretched to the Ukraine, it seems to become chronological rather than cultural. Even its chronological meaning is a matter of dispute, to which I shall return later. Would it not be wise to limit such terms to their cultural use and to try to use dates for chronology?

Professor Sulimirski also mentioned the Lusatian or Lausitz Culture and the necessity for fixing its chronology and origin. Did it really originate in the region of the former German-Polish border, or is that another 'Nordic myth'? Or was it really an offshoot of the Middle Bronze Urnfields of Hungary? If so, what did these have to do with the contemporary Urnfields mentioned by Mr. Hawkes that appear in Syria about 1400 B.C.? Also let us not fail to recognize considerable Urnfield influence in the Germanic North.

To the Lausitz Culture I shall return later. But Professor Sulimirski has drawn our attention to a most important boundary running across the middle of pre-war Poland from the Baltic to the Carpathians. It is a boundary of modern ethnographical types—and also, significantly enough, of two kinds of tadpoles. In geographical terms it separates the north-eastern forest zone from the rest of Europe. In archaeological terms it separated the Lausitz Bronze Age from various retarded forest cultures of Neolithic aspect. In linguistic terms Professor Sulimirski suggests that it divided Indo-European from Finno-Ugrian. Furthermore, beyond this boundary, flint was plentiful but copper and tin non-existent. Hence it is easy to understand why the main currents of the Bronze Age all flowed to the West of this line.

Mr. Hawkes and Miss Stiassna also discussed the possible connexions between the Lausitz Culture and the Slavs. It is never possible to be sure what language an ancient illiterate people spoke from the shapes of their pots, and the names they have left behind are not always helpful. How does it happen, for instance, that in ancient Europe there were three groups of people called Veneti? One group were Celts in southern Brittany; another were Illyrians in north-eastern Italy; and a third beyond the Vistula turn up later as the Slavic Wends. Linguistic arguments often hold little water. An example is the idea cited by Miss Stiassna that, because the Slavs borrowed their word for beech from German, their homeland was a place devoid of beech trees. Professor Minns remarked in that connexion that botanical names are confused by many people today. It is worse even than that. At the present time educated people in England, Australia and the United States not only apply a wide variety of names to the same kind of plant or bird, but also apply the same name to different species. For example, in America there is a bird called the robin, but it is quite different from the English robin, and an Englishman will call it a thrush. Linguists should remember that words do not always mean the same thing to everybody.

At the same time, the case for regarding the Lausitz people as the ancestors of the Slavs is at least not so weak as the German one that all Urnfield people were Illyrians. There might even be some truth in such a simple scheme of Urnfield expansion and Indo-European-speaking peoples as the following:

Eastern and south-eastern expansion = Illyrians
 Southern expansion = Italic
 Western expansion = Celts
 Northern expansion = Germans
 North-eastern expansion = Slavs.

One has, however, to remember that these expansions cut across one another. There is much Celtic La Tene influence in the Germanic area and in eastern Europe, and Mr. Charlesworth later reminded the Conference that Ptolemy records apparently Celtic names even beyond the Vistula.

The worst trouble with this, of course, is that the Greeks, the Sicels and the Gaels were Indo-European-speaking but lay outside the ambit of the Urnfields. What was the common denominator of all these groups? Was it the battle-axe-recorded ware complex? If so, Sicel and Greek seem as remote from that as from the Urnfields. Can Greek possibly hark back to the Anatolian Nasili dialect? And what about the completely Mediterranean Sicily?

To return to the Urnfields, Mr. Hawkes made most interesting observations on the fact that the Urnfield invasions of Britain had precious little standard Urnfield pottery, but used mainly the ware of the 'local substratum-population of Neolithic descent' in western Europe. This alone points up the weakness of the German idea that all Urnfielders were Illyrians. Perhaps similar studies on the periphery of the Urnfield world would reveal similar situations.

Mr. Hawkes also discussed the beginnings of the Iron Age in Central Europe and its chronological relationships, and implied a date of 700 B.C. for the beginning of Middle Hallstatt or Hallstatt B-C. I should like to give in briefest outline a purely tentative chronological scheme involving a somewhat later date. As Mr. Hawkes says, one must begin with Italy; but back of Italy one must remember the Aegean. Italian chronology is vague before 700, but the Greek colony of Cumae may have been founded as far back as the mid-8th century. The earliest Etruscan remains, such as those at Vetulonia, need not go back before 700. Lushey¹ has shown that the nearest ancestor of the Etrurian fluted bowl is only traceable in Assyria late in the 8th century.

Aberg² divided his Iron Age II into two periods, the first one native or Villanovan and the second early Etruscan. But obviously the Villanovans of central Italy did not vanish the moment the first Etruscan stepped out of his boat. An examination of the material makes the two 'periods' seem more or less contemporary. Much of the painted pottery of the allegedly earlier one could be compared with Greek wares of 750-650. Akerstrom³ shows that painted pottery assigned to both is scarcely earlier than 700 and much of it belongs to the first half of the 7th century.

Let us now look at Bologna. As Dr. Jacobsthal has recently pointed out to me, horse-shaped cheek-pieces of bridle-bits and bronze girdles very similar to those found in Benacci I appear in central Italy in clearly dated tombs of the first half of the 7th century. Benacci I need not have started until after 700. Benacci II evidently belongs to the middle part of the 7th century, and Arnoaldi begins about 625 B.C. All this is in terms of Central Italy, where imported Greek pottery and its local imitations now help to give dates. Arnoaldi runs down to about 525 and is succeeded by Certosa. The Certosa cemetery contained black and red figure vases that permit a date of 525-400 B.C., the latter date being also that of the Gaulish invasion.

A considerable time-lag is indeed noticeable between central Italy and Bologna, especially at the beginning of the Iron Age. If one looks to Este in north-eastern Italy, the lag is again in evidence. Este I, the earliest Iron Age period there, is theoretically the equivalent of Benacci I at Bologna, but its remains are so scanty that it must be less a period than a point in time, perhaps about 675. When one crosses the Alps into central, northern and western Europe, the time-lag presumably existed but is more difficult to estimate.

Benacci I-Hallstatt A connexions are not numerous and scarcely imply for Hallstatt A a date as early as 700. Benacci II-Hallstatt A associations are more numerous, and serve to equate those two periods. Hallstatt B and C, assuming for the moment that they are about contemporary, do not have much Benacci II

¹ Heinz Lushey, *Die Phiale* (Bleicherode am Harz, 1939).

zeitliche Chronologie, I: *Italien* (Stockholm, 1930).

² Nils Åberg, *Bronzezeitliche und früheisen-*

³ Åke Åkerstrom, *Der geometrische Stil in Italien* (Lund, 1943).

material, but find their main Italic parallels in Arnoaldi. They probably begin in the third quarter of the 7th century and continue to the end of the 6th. Hallstatt D finds many parallels in the Certosa period, among others fibulae with the turned-up foot related to the characteristic Certosa type. Since Early La Tene started, on Dr. Jacobsthal's Italic evidence, about 450 or soon after, Hallstatt D must have¹ been a very short period in the heart of the Continental Celtic world, though it lasted longer elsewhere.

Now let us look at northern Europe. Reinecke's Late Bronze Age D equals northern Bronze Age III of Montelius. Hallstatt A overlaps northern Bronze Age IV and V. One of the most important finds of northern Bronze Age IV is the Lavindsgaard hoard from Denmark, which consisted of a number of gold dippers with horse-headed handles found in a big bronze amphora. The dippers would seem to have some relationship to the pottery dipper from the Tomb of the Trident at Vetulonia of the first half of the 7th century, and to the famous bronze cup with a bull's head on the handle from Coste del Marano, now also placed in the 7th century.² The bronze amphora itself is related to Italic bronze vessels of the 7th century. Similarly Sprockhoff's flange-hilted swords with narrow hilts resemble some short Italic bronze swords of the same time. Significantly these Italic swords sometimes have a carp's tongue point, and Sprockhoff relates his swords with narrow hilts to carp's tongue swords.

This leads us toward western Europe and Britain. It appears to me that the flange-hilted sword with the ricasso and the broad leaf-shaped blade characteristic of the British Late Bronze Age is also characteristic of Hallstatt A on the Continent and not of earlier periods. It is also apparent that the great spread of the Urnfields was an event of the same time. If the carp's tongue sword is also roughly contemporary, it would seem that the Late Bronze Age invasions of Britain were events that probably began in the 7th Century. It has been suggested that the finding of a British Late Bronze Age spear head with loops in the blade in the Huelva hoard in southern Spain with allegedly early Sicilian fibulae would give an earlier date, but these fibulae had a long life and are found even in 7th century Greece, where they are regarded as imports.³ A further link between the British and northern Bronze Ages on one hand and Italy on the other is seen in the round bronze shields, a fine example of which was buried in the first half of the 7th century in the Warrior's Tomb at Tarquinia.

There is also the vexing question of the *Grenzhorizont* in the peat bogs. This is dated in Scandinavia at 500 or 400 B.C., ultimately on Italic evidence, and Professor Jessen believes that the *Grenzhorizont* in Ireland is a contemporary with that of Denmark. What is more, in Ireland Late Bronze Age objects are found only *above* the *Grenzhorizont*. If the Late Bronze Age invasions of Britain were to be considered events of 1000 or 900 B.C., an unbridgeable gulf would be created, but this disparity becomes much less awkward if the invasions were brought down to say 650. A couple of centuries would then represent no more than the normal time-lag while the Late Bronze Age spread out of the Lowland Zone, crossed the Highland Zone and finally reached Ireland.

¹ Paul Jacobsthal, *Early Celtic Art* (Oxford, 1944).

² Åkerström, *op. cit.*, 151.

³ C. Blinkenberg, *Les fibules grecques et orientales* (Copenhagen, 1926), 23, 203.

Of course, the weakness of the scheme outlined above is the fact that, before the later 8th century, Italian and Aegean chronology is too scanty to be a sound foundation. Perhaps our colleagues in the Aegean and the Near East can add to our knowledge here. If they can, they will illumine not only their own areas but all Europe from Italy to Scandinavia and the British Isles.

Mr. Burkitt made a plea after one of the papers that students should remember the grammar of their subject. I am in heartiest agreement with the viewpoint of scholars like Dr. Clark who aim at breathing life into dry bones. Chronology is at the same time an indispensable framework without which cultural currents flow in the wrong direction and the whole picture is confused and misunderstood. For example, if my grammar is reasonably correct, the scheme outlined above indicates that the Late Bronze Age of Europe was very long. The Aegean inspiration that had been a strong influence in the Middle Bronze Age was cut off with the collapse of Mycenaean civilization, and the cultural level remained on the whole static for some 500 years. But contact with higher centres of civilization again became possible when the Greeks and Etruscans settled in Italy in the late 8th and early 7th centuries. By the 7th century Europe beyond the Alps blossomed with Mediterranean ideas, and these included painted pottery and iron. This impulse was the natural sequel to the Mediterranean influences that had distinguished the Neolithic and the Early and Middle Bronze Ages, and was the natural prelude to the spread of the Roman Empire and the dominant position of the medieval Church. We know much about the social and economic consequences of Mediterranean contacts in historic times, but we have yet to discover much about the pre-historic ones.

Let us now go back to the events of August, 1943. This conference dealt not with archaeological theory but with organization. Uppermost in the minds of those who attended it was the question of state-supported archaeology, with the problem of how to muster public opinion behind such a scheme. But any lengthy comment from across the Atlantic on this programme would be superfluous, since it is purely an internal British affair. I shall limit myself to one point. Before the war England was always considered a country where archaeology was in private hands. In 1934 a German archaeologist, in advising the government of another state to adopt a national archaeological programme, remarked that such schemes were part of the machinery of government in all civilized countries—except, of course, Great Britain. No wonder that some looked with misgiving upon the idea of putting themselves and their researches at the mercy of a hierarchy of archaeological bureaucrats. It must have startled them, and perhaps comforted them, to realize that a legion of such bureaucrats ruled over British archaeology in the '30's' already, and they were none other than familiar figures such as Messrs. Bushe-Fox, Clapham, Crawford, Kendrick, Hawkes and Mortimer Wheeler.

Professor Childe spoke of the unity of archaeology, and blamed the universities for departmentalizing a unitary subject. The trouble seems to me to be that, while general principles are applicable over wide areas, the archaeologists themselves are not so much interested in general principles as in results. To achieve results still other divergent methods have to be employed: classical texts, Maya codices, tree-ring chronology, or Pleistocene geology. The farther one goes the greater the

divergence. Certainly at Harvard efforts to establish a general archaeological discussion-group even of the most informal character have come to naught because it has proved impossible to persuade people interested in Greek vases, Maya temples, Pueblo pottery and Palaeolithic implements to listen to one another. But from the American viewpoint a tenuous bond with the Old World may be emerging, with the idea that our oldest stone tools can be traced to an Asiatic origin and that the earliest pottery of our northern Atlantic coast is one end of a vast continuum that stretches across Eurasia until it reaches the Atlantic again as British Neolithic B.

Much was said also about museums. Mr. Hawkes and Dr. Wallis made a strong plea for living museums rather than lifeless repositories. Professor Beazley remarked on the other hand that the Oxford and Cambridge museums did a service both to the student and to the public by exhibiting almost everything that they had, while that at other places concessions had been made to 'vulgar showmanship'. Surely some middle road exists between these divergent views. Showmanship can degenerate into vulgarity if it is used to misplace emphasis, but lighting, colour and proper distribution of the objects exhibited can also be used to place the emphasis where it belongs. Two things are quite certain. If museums are to survive, they must be much more than mere repositories for discarded objects of the past. They must bring to the man in the street the meaning of the past. But at the same time they should regard themselves as research institutes, where scholars will have the fullest access to all the material whether exhibited or stored. Otherwise our science will degenerate into cheap journalism.

Another means of bringing the past and its meaning to the public is, as Mr. Kendrick rightly stressed, the guidebook. Give us in future guidebooks without verbiage, but with a clear and brief statement of fact and equipped with good maps so that the traveller can find his way readily to his destination. 'Recent antiquities', such as early railway stations, should of course be included, for they connect the remoter past with the present. There is an astonishing Neo-Gothic one at Salem, Massachusetts, which is worth at least two Baedeker stars.

The Conference of 1943 also dealt with some of the broader aspects of archaeological method. Sir Cyril Fox offered a plan for a card-index of archaeological material, not a haphazard amateur effort, but a properly supported professional undertaking that would not only be made but kept up to date. This recalls the Index of Christian Art at Princeton University. That was in many ways a more difficult undertaking, but it has not proved overwhelming, and is of enormous assistance to scholars.

Much stress was also rightly placed on environment by Dr. Zeuner and Mr. Hawkes. Recognition was given to the fact that chronology is only a frame, but the frame must contain a picture, and this picture can only be obtained by a study of the environment of ancient peoples. Mr. Hawkes proposed a National Committee for Quaternary Research, to be something like an enlargement of the Fenland Research Committee that meets at Cambridge. This would extend the excellent work of that organization in England, and link the country more completely with the work carried out in the past around the shores of the Baltic and more recently in Ireland.

Mr. Hawkes and Mr. Richmond also made a plea for total as opposed to selective excavation. Total excavation is, of course, an essential means of putting

a living picture into the chronological frame. I have tried my hand at total excavation in special circumstances where the selective method for local reasons would not work, and it has proved very rewarding indeed. I also derived some amusement from the difficulties of a classical archaeologist who was called upon to review one of my reports. He complained that my publication was not archaeology at all but sociology!

In this same regard I should like to enter a plea against lumping the small objects from Roman sites in the category of things too numerous to mention. The standard set by such writers as Curle and Jacobi in reporting Newstead and the Saalburg have not always been maintained. Small objects have three kinds of importance. One is sociological. Another is that small objects travelled into areas peripheral to the Empire, and the third is that many survived the Empire in the cultures of the Dark Ages, thus forming a link between the two periods.

Dr. Clark and Mr. Varley made outstanding contributions to the philosophical aspect of the discussion. Mr. Varley spoke of the menace of a world overrun by scientists versed in the atom and other such matters, but with minds completely closed to historical understanding. He added also some words which deserve repetition. ' . . . What man must make, if he is to survive, is a sense of community with all other men in the world of space; and he will be better able to achieve that, if he understands the historical explanation of the diversity of culture which has acted as such a barrier to international understanding. Archaeology is only in its infancy . . . but it belongs to the group of humanistic sciences which, together, can steadily dissipate ignorance and bunkum, inspired or otherwise, about the evolution of human societies. . . . In popularizing the subject, there is a risk that scientific exposition will sink to the level of journalism . . . but much worse will befall if poor deluded humanity is to attempt to survive with no sort of historical sense; and my personal experience is that archaeology seems to be one of the keys which will unlock the door, behind which lurks the desire to know more about human beings.'