

THE POSITION OF OLD-WORLD PREHISTORY (I)

Part II of this survey will be published in the next issue of *Diogenes*.

This survey of Old-World prehistory may conveniently begin with the melting of the Pleistocene ice-sheets some 15,000 years ago; for the latest enlargement of our knowledge of the evolution of man and of his earliest cultures were very properly included in the 'Panorama of Anthropology' published in *Diogenes* 2. Even with that limitation of range, a survey cannot be a summary. Prehistorical archaeologists aspire to recover the history of preliterate societies all over the Old World during ten thousand years. But not even the history of England in a single century could usefully be condensed into twenty pages. Nor should our panorama be a descriptive catalogue of exciting new discoveries. Striking additions to our knowledge have of course been made during the last eight years both as a result of excavation and to reward less spectacular research in museums and libraries. (For instance, the first object, undeniably manufactured in and imported from Mycenaean Greece to be recognised in the British Isles or indeed anywhere north of the Alps was noticed in 1948 in a Cornish museum where it had been lying neglected for a hundred years since it had been dug from a barrow near Pelynt. Again the oldest metal

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helmet found north of the Alps has just been identified by searching through files of German newspapers of 1847!) Some of the finds in question no doubt have an immediate appeal as beautiful or bizarre. Many, including the scientifically most significant, can only be appreciated by specialists or after a tedious technical explanation. Yet some recent discoveries and publications have promoted, if not the final solution, at least the formulation in soluble form, of familiar problems that have pre-occupied archaeologists and intrigued historians for nearly a century. I shall therefore try here, after indicating some modern general trends in research, to present the position of those old problems in the more definite and precise form which they have now assumed. Quite a number of new discoveries can then be mentioned in an intelligible context.

I

Over most of Europe and the Near East by 1940 the sequences of cultures (i.e., the relative chronology) had been established with a moderate degree of precision in several provinces, the distribution of contemporary cultures within or between provinces had been determined by pottery or other easily variable classes of artifact. The way was open for a more intensive study of the archaeological units thus defined in space and time, with a view to grasping each 'culture' as a functioning whole, as the—inevitably fragmentary—expression of the activities of a living human society. The effort to achieve this re-animation of mute material objects has not meant, as some feared, neglect of stratigraphy or typology, of the assemblage and ever finer classification of archaeological data. On the contrary it has promoted an exacter discrimination of types, the appreciation of previously unnoticed variations and a more urgent demand for a reliable chronological framework. Nor yet need the 'sociological' approach to archaeology deprive of interest the older issues as social anthropology in Britain has diverted attention from technology. These issues too, as we shall see, have direct sociological implications. At the same time it has once more focused attention on items of equipment that, though connected with the most vital needs of any society, show little variability and are therefore ill-adapted for defining periods of time or distinguishing one culture from another. So Steensberg has written a most illuminating monography on *Ancient Harvesting Implements* (Copenhagen, 1943) and Glob another on *Ploughs* (*Ard og Plov*, Copenhagen, 1952). A revival of interest in the diet of prehistoric societies has been stimulated; it has led Helback, using Jessen's technique for the diagnosis

of grain impressions on pottery and clay plaster, to most fruitful investigations to which we shall refer again. The same impulse has inspired closer collaboration with natural sciences. In England, a committee of the South-western Group of museums has conducted a systematic petrological examination of stone implements, organised by Stone. This has demonstrated a quite unexpected volume of trade in the British Isles at a time when stone was still the normal material for implements and even weapons, has located factory sites and indicated the routes of distribution. In the same way intensive studies in the field and laboratory by Pittioni¹ and Preusschen have laid bare not merely the techniques, but something also of the structure of the prehistoric copper-mining industry in the Austrian Alps. Hand in hand with this strictly archaeological work have gone renewed endeavours to reconstruct in detail the environment in which prehistoric societies lived. Indeed a Professorship of Environmental Archaeology was established in the University of London in 1945, and rather similar departments have since been instituted in Continental Universities. Pollen-analysis, which in the temperate zone offers the most sensitive instrument for determining climate and vegetation in the past, has likewise received academic recognition. Incidentally by its aid botanists using means proper to natural science now recognise the 'cultural' changes in plant cover due to human intervention four or five thousand years ago.

Prof. Clark's masterly book, *Prehistoric Europe: the Economic Basis* (London, 1952), shows how successful archaeologists have been in conjuring up a realistic picture of many prehistoric cultures as effective instruments for the adaptation of human societies to their environments and even at times in adapting environments to their needs. He profitably appeals to local folk culture rather than to ethnographic parallels from tropical Africa or New Guinea to explain unfamiliar objects that archaeologists dig up in Sweden or Ireland. Indeed Clark succeeds in presenting each of his cultures as a working economy for the satisfaction of socially recognised needs by the exploitation of its habitat and co-operation with other societies. But his very success discloses existing deficiencies in archaeological knowledge.

Not even the economy of a society can be understood fully from estimates of the total production, the volume of trade and so forth, without reference to the distribution of the product within the society and the size of the effective market. The evaluation of the economy will

¹ 'Prehistoric Copper Mining in Austria', University of London Institute of Archaeology, *Annual Report VII*, 1951.

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depend *inter alia* on the population density it can support together with the trend of the population curve with time. Information on both these points can be inferred from purely archaeological data. Differential distribution of grave-furniture in a cemetery may disclose a contrast between rich and poor while a chieftain's 'palace' should be easily distinguishable from commoners' huts. To estimate the size of a local group you have only to count up the number of contemporary houses in the village or the number of graves in a cemetery or ideally to combine both counts. No doubt the most reliable conclusions could be drawn from a complete settlement that had been totally excavated, to which an exhaustively explored cemetery was attached. I know no example of this happy combination. But total excavations had been conducted with most satisfactory results before 1940 on a very few sites in Britain (Skara Brae and Little Woodbury), Germany (Köln-Lindenthal, Goldberg, Buchau), the Aegean (Thermi), and Iraq (Tepe Gawra). But such excavation is fantastically costly and from the standpoint of museum curators unproductive, so that since 1945 no total excavation has been published outside the U.S.S.R. Moreover, even with such data for the solution of sociological questions a reliable absolute chronology is an indispensable pre-requisite, still more so when only funerary evidence is available. We cannot begin to calculate the size of a population from the number of graves in a cemetery unless we have some idea how long the cemetery was in use. Our estimate of the social status of persons buried in the rich barrows of Wessex or Jutland will be very different if the interments are to be spread over two or six centuries and at the moment the margin of error is of just that order. Thus the new economic and sociological approaches bring the prehistorian back to the chronological questions which have preoccupied archaeologists ever since Thomson converted antiquarianism into a scientific discipline.

II

Chronology is no doubt a forbiddingly technical subject. But a reliable chronological framework within which the duration of archaeological periods can be measured and contemporary cultures compared is indispensable for any solution of the sociological questions just adumbrated or of the older questions considered below. The nature and limitations of the existing system must therefore be simply stated if the reader is to understand the archaeologists' hesitations in answering familiar queries. Stratigraphy and typology have defined successions of culture-periods

in various provinces. They tell us nothing at all about the duration of such a period and hardly anything about the temporal relations between periods in distinct provinces. In the Pleistocene period global changes in climate and sea-level provide an independent geological time-scale against which archaeological periods can be measured and compared. Nothing so comprehensive is available in the Recent. Only in the north-western corner of Eurasia does a succession of climatic phases, detected by the pollen-analysis of peat deposits, provide a coarse chronometer. Indirectly archaeological events can be fitted into one or other of the successive phases termed Pre-Boreal, Boreal, Atlantic, Sub-Boreal, and Sub-Atlantic respectively. In Scandinavia and perhaps Switzerland a simple enumeration of the layers of banded clay (varves) deposited each year by the melt-waters of glaciers should give the number of years since the melting began. With the aid of such varves de Geer constructed a 'geochronology'. Indirectly again the climatic phases should be linked up with the varve series, so that the duration of each could be measured in calendar years and its beginning fixed in terms of our era. In practice there are enormous difficulties in applying this system, and theoretical complications impugn its results so that de Geer's dates are by no means generally accepted by geologists and climatologists. In any case archaeological phenomena could at best only be dated in geochronological terms at third hand.

Hence for all periods since 3000 B.C. archaeologists have preferred to rely on so-called 'historical dates', for expressing in terms of our era the age of archaeological events in the life of preliterate peoples even in north-western Europe. These dates depend on the establishment of synchronisms between prehistoric culture periods and phases in the history of the literate peoples of the Ancient East, China, Greece, and Rome, that can of course themselves be dated from written records.

As a first stage it was assumed that devices created or invented by literate peoples were diffused to and adopted by illiterate barbarians. The emergence of the device in a historical context among the former then gives a *terminus post quem* for the period in which it was first adopted among the barbarians. The initial assumption, seldom clearly formulated, is plainly open to criticism, and applications of it to particular cases have been legitimately challenged in recent years as will appear below. In any case the limits it offers are far too wide. Much more reliable and accurate limits are provided by historically dated manufactures actually imported by illiterate barbarians from more civilised peoples. But such an import

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still provides only a *terminus post quem*. We can only guess at the time lag between the date of manufacture and its deposition in a barbarian's grave or rubbish pit. Strict synchronisms are only obtained by cross-dating, i.e., by finding objects manufactured by barbarians at a given culture period in historically dated contexts and civilised products of the latter date in deposits of the same prehistoric culture period. By cross-datings a historical chronology, based primarily on Egyptian records, has been extended to Palestine from 3000 B.C. and the Aegean from 2000 B.C.

Only since 1940 have equally reliable dates been established for temperate Europe and Italy. Mycenaean pottery of the styles current from just after 1400 B.C. reached Apulia, Sicily and the Lipari Islands, and recently Bernabo Brea² has found older Mycenaean pottery datable to the fifteenth century in a deeper layer on Lipari. North of the Alps the earliest manufactures to be recognised as imported from the East Mediterranean are segmented fayence beads found in graves in southern England, Brittany, Almeria, near Szeged in Hungary, and in Poland. Unfortunately while the beads from England were probably made in Egypt about 1375 B.C., the date and even the place of manufacture turn out to be less certain than was supposed ten years ago. No such ambiguity attaches to a Mycenaean dagger, dug up in a Bronze Age barrow at Pelynt in Cornwall before 1845, but only recognised as such in 1948. It affords the most satisfactory proof of some sort of trade between the British Isles and Mycenaean Greece. But it might have been made there at any time between 1350 and 1100 B.C. And it dates nothing since the barrow contained no other furniture. Luckily imports from barbaric Europe and particularly from Britain have now been identified in well-dated contexts in the Aegean and give a *terminus ante quem* for the beginning of the Middle Bronze Age. Amber, generally believed to be of Baltic origin, appears in Greece already in the Shaft Graves of Mycenae—1500 B.C. and probably earlier. A gold-bound amber disk, from a tomb near Knossos that Sir Arthur Evans dated about 1425 B.C., may well be an import from Britain since six similar disks have been found in Middle Bronze Age barrows in Wessex. Finally, in 1940 von Merhart recognised that a peculiar amber necklace found in a tholos tomb at Kakovatos in western Greece was identical with necklaces from Middle Bronze Age barrows in Alsace and Bavaria. Necklaces with the same peculiarities were, however, worn in Wessex too during the Middle Bronze Age. Almost

² 'Civiltà preistoriche delle isole eolie', *Archivo de Prehistoria Levantina*, III, Valencia, 1952.

certainly they were made in England and exported thence to Greece. Now the burial at Kakovatos took place not later than 1450 B. C. Hence the Middle Bronze Age in Britain and Central Europe must have begun before that date, say about 1500 B. C.

We have to wait nearly a thousand years for an equally reliable date north of the Alps. By the second phase of the Hallstatt period, Iron Age 1, 2, Greek vases of the sixth and early fifth centuries begin to appear in graves and settlements, and thereafter imported Greek or eventually Roman coins and pottery provide a series of accurate dates. The intervening period is still obscure. Safety-pins, spearheads, swords and knives of types appropriate to the end of the Middle Bronze Age in Italy or the beginning of the Late Bronze Age north of the Alps do turn up in Mycenaean Greece between 1350 and 1150. But with this latter date began a Dark Age throughout the East Mediterranean that lasted for four centuries and is reflected in a divergence of the same order in the dates assigned to prehistoric European periods. Dates for the Late Bronze Age and Early Iron Age 1 used to be based largely on types of beaten metal which were common to Italy and Central Europe. Their contexts in Italy are at first purely prehistoric, but the later types should be dated by imported Greek vases found with them. But the earlier Greek vase styles themselves can be dated only by their occurrence on the sites of Greek colonies in Italy, the foundation dates of which are recorded by classical authors; so the argument is almost circular. By according to Italy a priority in the production of the relevant types and by taking minimal dates for the Greek imports, Åberg, Åkerström, and Sundwall reduced the beginning of the Hallstatt period to 625 B. C. That left five or six centuries to be filled up with the Late Bronze Age if the Mycenaean parallel be taken to date its beginning. Alternatively the Middle Bronze Age was equally prolonged by putting the beginning of the Late Bronze Age at 850 B. C. Against this von Merhart³ in 1950 adduced good evidence for the view that much of the bronze ware from Central Europe that had been accepted as of Italian manufacture was really made in Hungary, so that Italian parallels gave *termini ante quos* and not *post quos*, upper and not lower limits, for the periods in which they occur in Central Europe. A year earlier Kossack⁴ had pointed out inconsistencies in the short chronology

³ Published as 'Studien über einige Gattungen von Bronzegefässen' in *Festschrift des Römisch-Germanischen Zentralmuseums in Mainz*, ii, 1952.

⁴ 'Problemi cronologici della prima età di ferro in Italia e nell' Europa Centrale', *Atti del 10. Congresso Internaz. de Preistoria e Prgt. Postoria Mediterranea*, Firenze, 1952.

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for Italian prehistory and suggested a new spacing that would allow the Hallstatt period to be pushed back towards the traditional date of 750 B. C. and a consequent redistribution of the divisions of the Central and North European Bronze Age. So we are still left with a discrepancy of four centuries with regard to the end of the Middle Bronze Age in Denmark, though the higher dates look increasingly likely.

So even in such favourably situated regions as the British Isles and Central Europe 'historical chronology' has not taken us back beyond 1500 B. C. In upper Eurasia or Black Africa the position is incomparably worse. And nowhere does recorded history reach beyond 3000 B. C.; indeed till 2500 even Egyptian and Mesopotamian dates are subject to a substantial margin of error. Dates before 3000, even if based on the depth of deposits beneath the historic horizon in Mesopotamian tells, are just guesses.

Since 1945, however, another natural science, atomic physics, has offered a new prospect of resolving the ambiguities of prehistoric chronology and penetrating beyond the earliest recorded dates. I refer to the radio-carbon method, worked out by W. F. Libby⁵ of Chicago. Depending as it does on the amount of the radio-active carbon isotope, C 14, left in a piece of organic matter that forms an integral part of the prehistoric deposit or indeed of the actual artifact to be dated, this method is immensely superior to the indirect methods of geologists and palaeobotanists. On the other hand even an archaeologist can see that the determination of the minute quantities involved must encounter stupendous technical difficulties and hears whispers of theoretical uncertainties. All the results published up to April 1953 are given with 'tolerances' of two to four hundred years plus or minus implying a margin of error of four to eight centuries and would therefore be useless for settling the finer points of Bronze Age chronology. The radio-carbon date for pharaoh Zoser is in glaring conflict with the historical record and with other similar dates for Egyptian samples, and a couple of Danish datings seem mutually contradictory. C 14 dates must therefore still be regarded as provisional though most that can be thus checked agree satisfactorily with the figures based on written records or on geochronology. In the appropriate context we shall quote with due reserve some mean dates, reminding the reader only here that he is free to add or subtract two to four centuries from the figures.

⁵ *Radiocarbon Dating*, Chicago, 1952. Some figures used here are taken from a list issued in April 1953.