Wenner-Gren Foundation Supper Conference

A second Wenner-Gren Foundation regional supper conference was held at Harvard University on November 6, 1953 (see AMERICAN ANTHROPOLOGIST 55: 353 for an account of the first such regional conference of this series). This conference took as its general theme the subject of method and theory in archeology and was based, more specifically, upon a paper by Professor C. F. C. Hawkes of Oxford University, England. Professor Hawkes, the McCurdy Lecturer in Old World Prehistory in the Peabody Museum at Harvard for the Fall of 1953, prepared his comments in advance and these were duplicated and distributed to a large group of archeologists, of both Old and New World interests, throughout the New England region. Some fifty persons, including both professionals and graduate students, attended the three-hour afternoon conference session. General discussions were opened by a group of special discussants, including Walter W. Taylor, Jr. (*in absentia*), G. P. Murdock, Anne Perkins, Frederick Johnson, H. E. D. Pollock, William Smith, E. Z. Vogt, J. O. Brew, Philip Phillips, Hugh Hencken, and H. L. Movius. G. R. Willey acted as conference chairman. Professor Hawkes participated throughout the discussions, all of which bore directly or indirectly upon points raised in his prepared paper. The present version of Hawkes' article incorporates, or in some way takes cognizance of, many of the remarks made from the floor during the meeting.—EDITOR.

Archeological Theory and Method: Some Suggestions from the Old World

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IN THIS rewriting of the theme which I prepared, in October 1953, for the conference recorded above, I have been greatly helped both by the comments made on it by the participants, and by much else that I have heard and read on theory and method in archeology during my four-month stay in the United States. The New World's interest in the subject should certainly have an Old World counterpart. And I hope that my attempts to think toward one have profited from American thinking in the New World field of study, although, naturally, they themselves belong properly to the Old World field, with which alone I can claim adequate acquaintance. At any rate my starting point will be familiar to American readers, namely, Walter W. Taylor's book *A Study of Archaeology*, which is concerned mainly with New World archeology. As an Old World archeologist, I am of course not competent to assess or criticize Taylor's detailed contentions in this book. But all readers will know the general objection that he raised in it against New World archeology, for having limited itself to what he called "mere chronicle"—an almost exclusive preoccupation with charting the connections, in space and time, of the types of archeological material obtained from sites. He insisted that such

things as these connections cannot have entered directly into the way of life of the ancient peoples concerned: they are outside it. The significance of the types inside the people's way of life will have been something immediate and local, something cultural in the functional sense of the term, which this sort of archeology was missing.

Now, if I understand Taylor aright, he did not object to "where and when" archeology in itself. Exhaustive statements of that kind of evidence are obviously necessary, if we are to obtain thorough knowledge of the geography and history of the types; the fact that the people who made and used the types need not have known their geography and history is no reason for our not getting to know them. What Taylor objected to was limitation of archeology to this "where and when" chronicle, and refusal to carry inquiry further into the way of life of the people being studied, and into the significance of this or that type to that way of life. To correct such shortcomings, he recommended a fresh approach, which he called "conjunctive." Analysis or assessment of this "conjunctive" program as a whole, from me, would be superfluous. But I should like to concentrate attention on one point, namely, Taylor's claim that if archeology limits itself to a mere external chronicling of material culture traits, it will be stopping short of its proper anthropological objective, and will be simply compiling statistics when it ought to be revealing culture. For to this point there is surely a corollary: that in order to reveal culture, the approach required will be not just a material-analytic one, but one which, whatever the details of the program, can still more rightly be called "conjunctive" because the material-analytic in it will be conjoined with other, and more deeply penetrating, lines of thought and operation.

In my submission, both Taylor's point and my corollary are in their essence just. "When and where" archeology should be a means to a further end. But the question remains, how to get to that? And here, I suggest, the answers may be found to differ, in practice if not in theory, with differing archeological fields. At the least, we cannot predicate one uniform set of answers, valid for all archeological fields, unless we have first examined several of these fields, and seen what answers appear to be valid for each one. Then, by comparing the various answers, we can see what they have in common, and so abstract the general element in them from the particular.

I will therefore now move on from Taylor and his New World program, and take the call for a "conjunctive approach" with me over to the Old World, or to such a segment as I can claim to be well acquainted with. What echoes will it arouse when relayed over there?

In the Old World, the sort of archeology most obviously comparable to that examined for the New by Taylor normally proclaims itself Prehistoric, and has been at pains to declare its independence of the written texts of history and the merely antiquarian study of historical monuments and artworks. Its pride has been to be "text-free," and not "text-aided." The proof of the geological antiquity of man, and of his Paleolithic cultures, has given a long and scientifically chartered background, at once for man's material culture and his

biological evolution, behind recorded ancient history. The adoption of Thomsen's system of classifying prehistoric archeological remains by material, into three Ages, placed those of Bronze and Iron in succession next behind the historic record, and the Stone Age behind them again, divided presently into Paleolithic and Neolithic, the former geologically Pleistocene and the latter Recent. This prehistoric archeology, which held its first International Congress in 1866, has always reckoned itself a branch of anthropology, concerned to apply scientific treatment to a natural history of man. It has applied it by way of evolutionary theory verified by stratigraphic fact, and has extended it by classification of archeological material into types, arrived at by sorting collections of specimens into groups and series wherein types could be recognized, each one uniform because expressing a consistent purpose on the part of its ancient makers, and each normally followed by another in a kind of evolution, toward an ever more efficient realization of such purpose. And by "efficient" has in general been meant efficient for the successful survival of the human group, in its physical environment at whatever period.

Now of course, in our day, the modes of formulation of this "text-free" prehistoric archeology, and of its theory of knowledge, must be admitted to have undergone great changes. Yet the underlying axioms inevitably remain; and these amount to declaring that the human activity which it can apprehend conforms to a series of norms, which can be aggregated under the name of cultures, definable in terms of time and space and recognizable each by its standard range of material products. In the standard range, however delicately the bounds of classification are adapted to the variability, the notion of types must be comported. And change, from one norm to another, is to be followed in the changing of the types, and of the standard ranges of the products whence the norms can be inferred. The notion of norms seems fundamental, since without it there could be no firm claim of comparability between the phenomena given by the material; and there must be this claim, since only by comparison can those be got to make any collective sense. It is from the comparison of archeological phenomena that one's reasoning must proceed, inductively, to the human activity that has produced them. This is what I conceive to be the process of pure archeological inference. However much scientific apparatus and intellectual refinement it employs, it has to go that way, and it has to rest on the notion of norms in man's activity, which is an anthropological generalization, based on the extreme degree of conservatism shown by primitive man in his technological traditions. Without this notion, as Movius, following Bordes, has lately been reminding us, the whole subject would crumple up.

Compared with the "text-free" mode of archeology, the other mode, which I call "text-aided," is not hard to grasp, and has been familiar longer. Its basis lies in antiquities or archeological phenomena that are known or knowable historically, from consideration of which it proceeds with the aid—direct or at least indirect—of the relevant historical texts to conclusions about the past human activity thus indicated. Some of these antiquities, to begin

with, are things that have never been forgotten, so that the activity that has produced them has always remained in some sort known, and needs only investigation by archeology to be fairly fully known. That the Romans came to Britain, and built a wall, or walls, across the north of it to keep out the barbarians, has never been forgotten in Britain. Moreover, things that have been forgotten can be rediscovered; their mere rediscovery and description may be no more than antiquarianism, but their systematic study, comparison, and classification are undeniably archeology. The object will be their correct attribution to a known historical context, using either readable inscriptions found among or on them, or the identification of their localities with localities described in a reliable historic record. From such historically guaranteed examples, this mode of archeology can then determine types, as of Greek temples or Celtic coins or Roman camps, of which it can go on to recognize further examples, previously unrecognized and no matter where occurring, by comparison. This comparison will not rest on its practitioner's own theory, as in the "text-free" archeology just now considered; it rests on textual statements guaranteeing that there were such types, standardized and varying only in detail, in the historic cultures concerned. But with such guaranteed cases as starting points, one can build these, and then other types as well, into series, the successive members of which are seen as related to each other in processes of development or degeneration. This, of course, is just like the typology of the "text-free" mode of reasoning and, like it, is based on the idea of norms in the human activity responsible—starting from cases perceived with the aid of texts, and then recognizing others by comparison and analysis. The same proceeding can be undertaken, too, with styles of ancient art; for a style is a norm, and essentially the same canons of comparison and analysis apply. But all these series must, at some of their points, be pegged to points in textually documented history. No new discovery or theory can upset the known dates of the Roman campaigns in Britain that left the camps and walls there. To that extent the "text-free" mode of archeology, with its dependence on experimental classification, is the more exacting of the two: it must supply its guarantees for itself. Where the "text-aided" mode is the more exacting is in its dependence on its documentary aids: once get too far away from these, and its logical force becomes too weak to be compulsive. And in practice, of course, we are most often making use of both modes, combining text-aided and text-free reasoning together, in varying degrees according to the circumstances.

In many Old World fields, indeed, we are so familiar with this combination that we frequently use it without reminding ourselves what it is. Yet it is a conjunction of two really different logical approaches; and when we add, as in modern practice we inevitably do, approaches from natural-scientific study of environment, techniques, and raw materials, and where possible from relevant modern folk-life too, and conjoin all these approaches as far as practicable into one, I conceive that this is in some sort a "conjunctive" approach, like that we have seen advocated for New World archeology. Perhaps, then, it is worth examining a little further.

In my Presidential Address to the Prehistoric Society, given in London in February 1951 and published in the Society's *Proceedings* for that year, I tried an installment of such examining, and an airing of some new suggestions in terminology, which should help to make the scene somewhat clearer. I pointed out that what Englishmen largely and loosely call "prehistory," namely, everything that happened in their country or in countries similarly placed on the periphery of ancient civilization before the establishment there of Imperial Roman rule, could only be rightly so called in a conversational and generalized sense. Roman Britain itself, indeed, is in good part historic; but there is much in it that is historic only dimly and embryonically, and much in the centuries after Rome's withdrawal that in historic status is more rudimentary still. The best word for these stretches of incompletely historic time, I suggested, is "Protohistory." Our cognition of them, that is to say, is based on the beginnings or rudiments, the protoplasm if you like, of textual-historical evidence, but no more. Just the same will be true, then, of the latter end of our "prehistoric" time, directly before Rome's coming: it is already "protohistoric," for there are already some texts and also some inscribed coins. And because we have protohistoric cognition of it accordingly, we can know things about it which we cannot expect to know so readily about our earlier phases. Within the resources of this protohistoric cognition, too, we can reckon the native Celtic sources, written down in Britain and Ireland indeed only considerably later, but telling sometimes of events and of institutions in both islands that take us back right through our protohistoric times.

However, this protohistory cannot very well be taken back, in its own right, before the 1st century B.C. For the few centuries before that, with British archeology illumined by comparison with a Continental Europe that was, in its own right, partly then already protohistoric or historic, I suggested in 1951 that our cognition could be called "penehistoric," because it was almost historic but not quite. I have not found this term in practice very useful, and I intend to drop it: it is not very important anyway, because the period connoted could never be more than short. What I believe much more important is that our cognition of all the better known parts of prehistoric Europe before Protohistory and History begin there, and in spite of their not having yet begun there, is not simply "prehistoric" in the sense in which our cognition of the Old Stone Age is prehistoric, but is conditioned by the fact that somewhere alongside of these barbarian regions, or at the worst somewhere a long way from them but not too far to have significance for them, there is already a history, beginning to be explicit in written texts from before 3000 B.C., in the civilized central regions of the ancient world: the Near and Middle East.

This means that wherever, between that time and the coming of our own protohistory and history, we can see cultures in Europe that have relation, in any degree, to those of the Near or Middle East, our awareness of that relation enters necessarily into our cognition of them, and conditions our archeological interpretation of them. And I distinguish two modes of cognition here, according to the distinctness and proximity of the civilized history, based on texts, to which there is relation. Where it is distinct because contacts with it

are well attested, I call the mode of cognition "parahistoric," from the Greek *para*, "alongside." From the 16th century B.C., when the New Kingdom was set up in Egypt and Late Minoan and Late Helladic or Mycenean culture in Crete and Greece, the cultures of barbarian Europe are set alongside those historic or protohistoric ones, and thus, though of course prehistoric in the loose sense of the word, are parahistoric when one speaks more strictly. Go back before that time, or to regions outside that range of relations to the historic, and you find your cognition is still to some extent conditioned by knowledge based on textual history, but the history is indistinct and a long way off. The cultures of the mature Neolithic and earliest Metal Ages in Europe are as intelligible to us as they are because they carry elements diffused to them from—ultimately—the ancient East, which was already historic or at the very least protohistoric in their day. But the diffusion was a long-distance and often tenuous affair, so that the history is remote. I therefore call our mode of cognition of these and all similar cultures "telehistoric," from the Greek *tēle*, "far off," the same word as in telephone or telepathy.

I believe this conception to be important both for the theory of our archeology, and for its method. As soon as we have telehistoric or parahistoric cognition—and still more obviously when we have protohistoric cognition—we are no longer interpreting our archeological evidence simply and solely by ideas of anthropological "process," or of ecological determination. In rural economy, burial rites, technology, sociology, or what not, there is always, somewhere or other, a point of reference within the historic order. We can interpret as we do because we are dealing with the outer parts of a diffusion-sphere, or of more than one diffusion-sphere, which we know to have history, and ultimately textual history, at its center. Our "conjunctive" approach to them therefore will always have something of the historical about it.

This has its effect both on the framework of the "prehistory" thus constructed—that is, its chronology—and also on its cultural content; I take the latter first. Let us recall Taylor's complaint that the statistical assembling of many archeological data still can leave one outside the cultural reality of the life of the peoples one is studying. A historical element among one's resources for interpretation, conjoined with those of technology and of natural history, can surely—at least sometimes—answer that complaint. The fertility symbols so prevalent in the archeological material of Neolithic and Early Metal Age Europe stand to be interpreted with the help of what is historically known about the fertility cults of the ancient East whence the diffusions to the Europe of those Ages started. The interpretation will be by reasoning in the telehistoric mode. The social organization of much European culture of the Late Neolithic, Bronze, and Iron Ages, with its little kinship groups, each headed by a father bred to bearing arms, which is displayed by the single-graves, tumuli, or barrows so frequent in these periods' sepulchral archeology, and by its settlement sites too if well enough explored, stands to be interpreted by what is historically known of the social organization of the Indo-European peoples, and reflected in their epic literature—Homer, Beowulf, the

Germanic and Celtic sagas—running from the historic and protohistoric back through the parahistoric to the telehistoric mode. Written accounts of Germanic and Celtic religion find some archeological echoes anyhow as far back as the parahistoric Late Bronze Age. Medieval laws and land-books, in certain cases, can help to interpret the ancient field systems, of that age and later, if not earlier, which air photography is revealing extensively in Britain and elsewhere in northwest Europe. Such examples will show, in general, what I mean.

But once get right away from any such historical basis in your cognition, and you will immediately find interpretation much more difficult. You will find it by no means easy to get inside your people's cultural life from their mere material remains. You will find that the "conjunctive" approach cannot always take you far enough. You can, and of course must, conjoin your archeology with natural history, in the analysis of raw materials, the investigation of techniques, and the study of environment and of your people's response to it, by every one of the natural sciences that can be brought to serve. All these things—and the study of modern folk-life too, if there is any relevant—are obligatory. But when you have done your utmost with them, how much will you have learned? Remember, you are now completely in the "text-free" mode of reasoning; you are right out of touch with history based even remotely upon texts; and if you want a name for your cognition it could well be "ante-historic," for you are in a world wholly anterior to textual-historical evidence. And from anthropology you have in the last resort only "process"—notions of a quite general sort about the social life and activities of primitive man, and the generalization about his conservatism which I mentioned some while back. Otherwise, you have got to use inductive reasoning, to take you from comparison and analysis of observed phenomena to the human activity that once produced them. How easy will you find it? I have a fourfold answer to this question.

1. To infer from the archeological phenomena to the *techniques* producing them I take to be relatively easy. The modes of research required are themselves no doubt difficult, and in detail often tedious. But the reasoning employed, I maintain, is basically simple.

2. To infer to the *subsistence-economics* of the human groups concerned is fairly easy. Operationally, of course, it is laborious. Not only must their material remains be closely studied, and the economic purposes implied by them carefully nosed out; their physical environment also must be investigated and its potentialities assessed: this will mean bringing in natural scientists and weighing the human significance of their expert testimonies. Yet, in the end, the reasoning one must use is not so hard. The impressive book lately published by my Cambridge colleague, Grahame Clark, *Prehistoric Europe: The Economic Basis* (Cambridge, 1953), is a fine example of this sort of work, and a wonderful compendium of knowledge. But its logic is simple, and need never be anything but straightforward.

3. To infer to the *social/political institutions* of the groups, however, is considerably harder. If you excavate a settlement in which one hut is bigger

than all the others, is it a chief's hut, so that you can infer chieftainship, or is it really a medicine lodge or a meeting hut for initiates, or a temple? Richly furnished graves may help you, but what if the graves are all poorly furnished? Or if the more richly furnished graves are women's, does that mean female social predominance, or male predominance using the adornment of its subjected womenfolk for its own advertisement? How much could the archeologist of the future infer, from his archeology alone, of the Melanesian institutions studied by Malinowski? No, reasoning of this kind surely cannot all be easy.

4. To infer to the *religious institutions and spiritual life* may seem superficially, perhaps, to be easier, and for the first few steps it may sometimes be so. Paleolithic art clearly has much to do with institutions of hunting-magic and, in the case of the so-called "Venuses," with expressions of desire for human fertility. Grave goods, again, indicate a belief that the dead need material supplies or equipment, as though still alive. But how much further can one go than that? Besides the animal and human portrayals in Stone Age art, are there not very many abstract signs whose meaning most often is just unknowable? What part were the dead, furnished with grave goods, supposed to play in the life of the community still living? You can use ethnological data obtained from modern primitives to stimulate your imagination by suggesting the sort of religious institutions and spiritual life your prehistoric people may or could have had, but you cannot this way demonstrate what they did have, and you know you cannot even hope to unless you can show some real connection between *this* modern and *that* prehistoric. I have heard the thing attempted, indeed, from the side of the modern South African Bushmen and the significance of their paintings, back to prehistoric African, and then maybe European, Stone Age paintings and their significance. But it is a very long shot, and even the possibility of it, in the Old World, is something very rare. In general, I believe, unaided inference from material remains to spiritual life is the hardest inference of all.

And now there is worse to come. If material techniques are easy to infer to, subsistence-economics fairly easy, communal organization harder, and spiritual life hardest of all, you have there a climax of four degrees of difficulty in reasoning. What is this climax? It is a climax leading up from the more generically animal in man to the more specifically human. Human techniques, logically speaking, differ from animal only in the use of extracorporeal limbs, namely tools, instead of corporeal ones only; human subsistence-economics differ from animal more obviously, but only (again logically speaking) in the amount and degree of forethought which they involve; human communal institutions next transcend the animal level very considerably; and human spiritual life transcends it altogether. So the result appears to be that the more specifically human are men's activities, the harder they are to infer by this sort of archeology. What it seems to offer us is positively an anticlimax: the more human, the less intelligible.

And the critical factor, standing between fair intelligibility and stark un-

intelligibility, is surely ecology, the study of the physical environment. So long as you can depend on that, as you can for the material aspects of man's life, his technology and his economic existence, your exercise of this sort of archeology is rewarding. How rewarding, a book like Clark's well shows. But now transcend that, and your returns diminish sharply. There is nothing in North American ecology, by itself, to compel either Iroquois institutions, say, or the Constitution of the United States. I do not say that you are left in the end with nothing that you can apprehend. But I do say that there cannot be much, and that we should ask ourselves just what there can be.

I must now come back to the other, or text-aided, mode of archeological reasoning. I mentioned above its helpfulness to the making of a framework for prehistory—the framework of measured time. Time is an essential category of all archeological thought. It is the time dimension, which archeology as an extension of history alone can give, that entitles it to a unique and indispensable place among the anthropological disciplines. The studies in cultural or social anthropology today which are limited to the "social present" (of the three generations normally alive at once) are depriving themselves, for purely practical reasons, of something that anthropology should never wish to be without. The practical reasons are of course perfectly sensible on a short-term reckoning of advantages. They include a very proper refusal to fill the place of a history not immediately knowable by an imaginary one fabricated by the anthropologist himself. But for social anthropologists to forget that they are practical reasons only, and to slip into the habit of ignoring the time dimension altogether, is surely exceedingly unwise. Indeed, I should call it scientifically indefensible. To restrict anthropological field work to peoples whose real history does appear at present more or less unknown is a convenient, and sometimes popular, way of running away from the time dimension. But were it to become a universal habit, anthropology would be quickly ruined. It is a vital function of archeology to stand with history in reminding anthropologists that time really does exist, and cannot ultimately be run away from if truth is to be served. All the more, then, does it behoove archeology to be very careful about its own treatment of the time dimension, and to see that it is intelligently managed.

There is to my mind no inherent difficulty in this, provided that one does not let oneself get muddled. Unfortunately, people in the past, sometimes very influential, have in fact been muddled in their chronological thinking. It was due to the 19th-century climate of thought in which they grew up. When prehistory was supposed to be a single, simple, tale of human progress, through Pleistocene into geologically Recent times, it was given a simple division into periods on a geological basis. The Neolithic was Recent, the Paleolithic was Pleistocene and divided into Lower, Middle, and Upper because it should lie stratigraphically that way. But for a long while no need was felt to distinguish, terminologically, between periods and cultures; and presently, when later ages were likewise subdivided, the same confusion was re-enacted. Montelius established four periods for the Neolithic of northern Europe, and

six for its Bronze Age, and all the material remains assigned to any one period were labeled by that period's serial number. Reinecke did something similar for the Bronze and Iron Ages of south Germany, only using serial letters instead of numbers. All that was then needful was to give each period its absolute date in years B.C. And nobody saw that there was a confusion of thinking here at all, until it began to appear that different groups of people within the same period had left different kinds of material remains—in other words, that several cultures could exist within the same broad region simultaneously. Now that that has been realized, things have become apt to look very complicated, because the descriptive terminology will not fit what it is supposed to be describing. Thus, the notion of periods of time that are automatically also units of culture history has proved to be a serious nuisance in European prehistory and we have to rid ourselves of the muddles it has caused. The way to do so, of course, is to distinguish fundamentally between periods and cultures, and construct the former as a fixed framework of time units, into which the cultures are then stuffed, in their sequences and juxtapositions according to the evidence, and given names of their own, distinct from the names or numbers of the periods.

The absolute dating of the periods, of course, will still have to be found. This, where any historical system of dates is available, as are those of the Near East for Europe from about 3000 B.C. as above explained, has to be done by establishing synchronisms between points in the scheme of periods and points in the system of dates. Such synchronisms can be given by proving a reciprocal exchange of material products, or adopting or copying of some specific type or design or symbol or style of art or ornament—a "horizon style"—or borrowing of some technique, from the region already dated into the region requiring to be dated. Some of these synchronisms of ours are good and tight, others are more dubious and may wobble over a margin of error which has to be allowed for. When there is doubt of that kind, some chronological weighting may be brought in from the quantitative bulk, and the degree of typological uniformity, of the material representing the cultures which the period or periods must contain. Thus in one way and another, and with many varying degrees of precision, can be constructed a chronological framework for the 3000 years B.C. in prehistoric Europe: most precise where our cognition is protohistoric, less so where it is parahistoric, and least so where it is telehistoric.

Natural science, however, can also furnish us with dates. Records of eclipses, comets, etc., astronomically datable exactly, have a distinct part to play in framing the historical date-systems of the Near East, as of course have the dated recurrences of the heliacal rising of Sirius for Egypt. But natural scientific dating can be taken right outside the historical sphere by the now well known carbon 14 method; there is of course also dendrochronology, and the probably less reliable varve-analysis of sedimentary geology; and finally, transcending all these in the huge lengths of time which it seeks to embrace, the astronomical chronology of Milankovic for the climate phases of the Pleis-

tocene, based on the periodicity of solar radiation. The trouble about all these methods, however, is that either the time-lengths they give are so large that many human events which we should like to have chronologically separated can have occurred within any one of them, or else, as with carbon 14, the number of determinations required to establish cultural duration, as opposed to pin-pointing single local episodes, is rather large. Thus the theoretical possibility that by all or any of these means the ante-historic stretches of prehistory may come to be as closely charted chronologically as the para- or tele-historic, is probably still rather far from being realized.

In any case, it is on the whole clear enough that archeology's claim to a distinctive place among the anthropological disciplines depends to a great extent on the accuracy of the chronology for events and cultures that it can offer. And this is particularly clear when we come to consider the phenomena of diffusion.

That elements of aggregations of culture have been diffused, by one means or another, from certain centers over wide areas of the inhabited world, has been repeatedly demonstrated, most often with the help of archeology. But the demonstration, to be fully acceptable, ought to prove not only the fact, and the correct setting in chronology, but the means whereby it has been accomplished. It seems to me that these means may be broadly divided into two. Actual folk movements or migrations of peoples, or human groups of whatever size or character, give what I call a *primary diffusion*. Influences, transmitted from one group or people to another without actual group migration, give what I call a *secondary diffusion*. In assessing the question whether a newly claimed diffusion is really that or not (the alternatives being independent invention, or convergent development, of the things advanced in evidence), it is important to have clear ideas on which of the comparable known diffusions in one's field of study are primary and which secondary. For, in the newly claimed case, the one might be possible, the other not; and in any event, the loose and vague use of words such as influence, transmission of elements, etc., ought wherever feasible to be replaced by more precise expressions, if only for the sake of tidy thinking.

It is also important not to forget the diffusions, whether primary or secondary, that are known historically. For these include movements that on archeological evidence alone would be got quite wrong. The Hellenization of the Orient, in the three centuries before the Roman Empire, for example, would never archeologically be supposed the sequel to a single military expedition headed by the Macedonian Alexander. It is more often the secondary diffusions resulting from such primary movements that loom large in archeology, and the archeological elusiveness of primary diffusive movements is a thing that prehistorians should carefully remember. For a large part of their business is the inferring of diffusions from archeological evidence alone, and the determination of their true character. And this is a delicate and exacting task, for which they should neglect no comparative resources that may be helpful.

There is here one specific point, to which I cannot forbear to call attention. Where natural life-forms, or rock or mineral products, have been moved about the world by human agency, there is a great deal to be learned by correctly attributing them to their proper original habitat or location. That the first domesticated cattle and sheep, wheat and barley, attested in prehistoric Europe were not native there, but introduced from the southeast by Neolithic man, is a fact of the highest importance contributed to human by natural history. A much higher degree of precision can be obtained by similar work on transported stone and minerals, as by the British Committee for the Petrological Identification of Stone Axes which has been working for a number of years and issues its periodic reports in the *Proceedings of the Prehistoric Society*. Other examples could easily be given, and many will be found in the book by Clark already cited, drawn from all over Europe.

But it remains for me to point out that far less such work has yet been done on transported mineral products other than stone, and above all on metal ones. The metals or metal alloys of which prehistoric artifacts were made can frequently, like the stone of the stone axes but of course in a more complex manner, be assigned by analysis to their original mineral deposits. It is easy to see the importance for Bronze Age archeology of an application of this method on a wide scale. Yet what has been done hitherto has been restricted by the techniques available. An apparatus has lately been devised and constructed in the Clarendon Laboratory at Oxford, with the encouragement of Lord Cherwell, whereby quantitative analyses of archeological metal objects can be obtained, without damaging them in any way, by X-ray spectrometry. It is now being employed experimentally with a view to the further development of the method. There seems no doubt that if laboratory research of this kind were employed systematically to localize the sources of the metals, and also to discern the metallurgical techniques employed in products transported over such large areas of distribution in the ancient world, the extension of knowledge would be very great, and questions of diffusion and long-range commerce, and of comparative technology, at present often handled with too little recourse to factual evidence, could receive methodically documented answers.

We need, then, both precise and sensibly expressed chronology, and accurate determinations of our raw materials. According as we are well or less well equipped with these, and competent or less competent in our techniques, we shall be able to undertake the task of getting from our people's external archeology to the internal content of their cultures, and to the interrelation of these in the pattern created by diffusions. I have explained that in the conjunctive approach that is necessary to this end, I believe a historical, text-aided element to be required for any full success.

One day, the conjunction of all our other resources together with natural-scientific chronology may enable us to transcend the limitations under which we now labor when we have left such aid behind. But I think that day is still distant, and there is much for us to do meanwhile: above all, I would suggest,

in the fields like my European one where an element of such historical aid is present for some thousands of years in varying degrees, amenable to what I have called protohistoric, parahistoric, or telehistoric cognition.

I have often been embarrassed by the formal necessity of beginning the prehistoric narrative at its beginning, where we know least, and proceeding from that forward. I should feel so much happier if instead of proceeding from the unknown toward the known, one could proceed toward the unknown from the known. And now I really wonder whether, by taking my three orders of cognition in their reverse order of time with the protohistoric first and the telehistoric last, one could not in fact attempt to do that.

To take what one knows from history and protohistory first would be like using what in the New World would be called the direct historical method, combined where possible with the use of recorded ethnological data (which of course are tantamount to history, in their own way). By this one could establish that in historic or protohistoric time there had been certain specific movements or diffusions or developments, which account for certain distributions and interrelations of culture within one's field. One could plot these in time and space as historic or protohistoric *diffusion-spheres*. That would be the first step. Then one would take those as data, preconditional to the next step in the investigation. That next step would be the plotting of parahistoric diffusion-spheres, to account for as much as possible of one's evidence that the historic and protohistoric ones would not account for. Next, one would take those in their turn as data, preconditional to the next step again, which, of course, would be to do the same thing with telehistoric diffusion-spheres. When one had exhausted all the possibilities offered by this series of steps, there would be left a residue, which none of them would have accounted for. For this there would remain only the possibility of ante-historic diffusion-spheres, such as those created by the spread of Paleolithic or Mesolithic culture about the world—for example, out of Asia into America. So one would take one's courage in both hands and try what one could do with those.

And then finally, when the whole gamut of the music of these diffusion-spheres had been played through, one would at last be in a position to try assessing the ultimate postulate of the comparative method in anthropology, which of course was the postulate with which the old style researchers began, assuming its validity a priori. That postulate is that the culture of all mankind rests in the last resort on things common to all men as a species, inherent in their culture-capacity from the start. If there are such things, it seems to me, they can in good logic be apprehended only by the abstraction of all those whose comparability is due to subsequent diffusions or developments. Some might argue that when all those had been abstracted one would find the remnant to consist only of truisms, too general to give insight into specific cultures. Or one might find nothing specifically human left at all, because one would be down to the level of the prehuman primates. At all events, I do not see any other way of enabling ourselves to judge, except the method I have suggested which is essentially a historical method. It begins with the historic,

and works backward through from that to the ante-historic. It works as one peels onions; and so it reaches the final question, has the onion in fact got a central nucleus at all or is it just all peel?

That, I suggest, is the way in which archeology can create what anthropology cannot escape the intellectual need of, namely, a sound critique of the comparative method in its reasoning. It was the fashion once to contrast the historical, as something merely particular and episodic, with the scientific, as something alone capable of formulation in general laws of what is now called "process." What these laws or "regularities" may be we all want to find out; and I believe that my suggested way of doing this, just because it is essentially historical, is essentially scientific. For logic, which is wisdom, should be justified of all her children.

It will follow that whereas the old style conception of prehistoric archeology regarded it as most fully anthropological where the cultures examined by it were most primitive—that is, in the Paleolithic, as one has it argued in a book like W. J. Sollas's *Ancient Hunters and their Modern Representatives*—we shall regard it as most fully anthropological at precisely its opposite end, the protohistoric and historic one. The making more fully anthropological its other end, if that is feasible, we shall take not as our a priori starting point, but as our goal, or ultimate objective. What we should do, therefore, is to divide up our Old World research program by historically or protohistorically determined regions, like those in Europe carrying Greek or Italic or Illyric or Slavic or Teutonic or Celtic culture in historic or protohistoric times, and institute a policy of regional research to work through their prehistory backwards. One would then be doing far more than finding out about the Greeks or Celts; one would be contributing directly to the progress of the whole science or study of mankind.

Archeological method must not merely be technically excellent; it must express good archeological theory. Good archeological theory demands a conjunction of methods, conjoined on a rational basis of good logic. History and Science have not to be segregated, but identified together. And that should be archeology's service to anthropology as a whole.