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Do cultures exist?

The question whether cultures exist turns as much on the concept of existence as on that of a culture, and it illustrates an important general question about the social sciences. This is, roughly, whether their theoretical entities can be taken as seriously as those of physics. This question in turn bears on the current debate about the use social sciences can properly make of the methods of the physical sciences.

We seem to know what societies are made of (*viz.* people!) before we start theorizing about them; physicists lack such conveniently *a priori* knowledge of the makeup of physical things. Their discoveries can consequently add to our ontology in a way that those of social scientists apparently cannot. It has been held on these grounds (e.g. Watkins 1957) that such 'social individuals' as cultures are mere fictions or logical constructions and so cannot be governed by autonomous laws, e.g. the laws of historical development postulated by Marxists. My object here is to investigate the supposed ontological asymmetry between physical and social sciences which gives rise to such conclusions, taking the concept of a culture as a convenient exemplar.

The term 'culture' is variously used in archaeology, anthropology and related subjects. I shall use it not of artifact assemblies (*pace* Childe 1956), but of groups of prehistoric people characterized, roughly, by what artifacts they left and where, and not otherwise directly accessible to our observation. (The argument will not be affected if archaeologists who find this use of 'culture' too heretical substitute another term.) Not all such groups are cultures in this sense, but all cultures are such groups. Now no one doubts that artifacts exist, and I take it that the existence, in the same sense, of prehistoric people is equally undoubted. The question is whether cultures also exist in this sense.

I take the concept of existence to be the same in the social as in the physical sciences. Naturally the sense of saying that electrons exist differs from the sense of saying that cultures exist. That follows from the different meanings of 'electron' and 'culture'; it need not

follow from different senses of 'exist'. Indeed I hold in general that there is only one concept of existence, applied equally to such diverse entities as God, numbers, ideas, ghosts, tables, electrons and cultures. I cannot argue this general point here (see, e.g. White 1956; Quine 1953), merely make a persuasive pass with Ockham's razor.

The temptation to invoke different concepts of existence (e.g. in Nagel 1961: pp. 145-52) results from the very different properties of the entities I have cited, and the consequently diverse methods needed to show whether they exist. Proving that there is no real square root of -1 has little in common with collecting evidence for the nonexistence of dragons. That is because numbers and animals have such different properties. Animals, for instance, have spatial and temporal boundaries: dragons, if there were any, would occupy some places and times and not others. Numbers, on the other hand, if they exist at all, exist everywhere and at all times. It makes no sense to go looking for numbers in the way one might go looking for dragons, because numbers have no properties that could let them show up in one place rather than another.

So one may be tempted to say that numbers exist in a different sense from animals. But given that the sufficiently different senses of 'number' and 'animal' make it needless to say this, it is obviously undesirable. Superfluous concepts of existence raise spurious problems about how they are related. They make plain questions of existence seem ambiguous. Take the question whether the sun has an odd number of planets. It may be taken equally as about the existence of planets or about that of their odd number. Yet these ways of taking the question plainly do not give it different senses.

No one minds talk of different 'modes' (e.g.) of existence where this means merely that things of different kinds have different properties and so are detectable in different ways. One may allow those Gods, numbers and people that exist at all to have different modes of existence in that sense. What one may not allow is that dragons, Hamlet or the real square root of -1 exist, only in different modes. We know, in each of these cases, what properties these supposed entities would have. Applying the appropriate techniques of detection we find that nothing has these properties. (I use 'has' here, and 'exists' and 'there are (is)' generally, tenselessly, in accordance with most logical usage. In tensed terms, I mean here that nothing has, had, or will have these properties.) There are no large scaly fire-breathing winged creatures with tails; no person exists whose biography could be used to 'correct' the soliloquies of Shakespeare's play; no real number has -1 as its square. These last three assertions are plainly true, and plainly unambiguous. It is not that dragons, Hamlet and real square roots of -1 have a different mode of existence, or subsist, or exist in a different sense. They simply do not exist at all; there are no such entities.

We have, of course, the concepts of these things. We know what

something would have to be like to be a dragon. (How else could we know there are none?) But the concept of a dragon is not a dragon, any more than the concept of Hamlet is a person or that of a number is itself a number. The existence of a concept does not confer any amount, kind or mode of existence on its instances (with the doubtful and much debated exception of the concept of God).

I have laboured these points especially to bring out the intimate relation between existence and truth. To deny that dragons exist is to say that there is nothing of which what we believe about dragons is true. There is no real number of which it is true that -1 is its square. There has never been a person of whom it is true to say that he respectively did and said anything like the acts and speeches Shakespeare attributes to Hamlet. In all these cases existence is a necessary condition of certain truths; and the same truths in consequence are a sufficient condition of existence. If what we believe about dragons were true of anything, that thing would be a dragon (since that, for us, is just what it is for something to be a dragon). Dragons, consequently, would then exist. Similarly, of course, for Hamlet and the square root of -1 .

We see then that the mere conception, or characterization, of a thing in no way suffices for its existence. To be convinced of that we must be convinced that the characterization is true of something.

Two questions, therefore, must be asked of a scientific theory in order to settle its existence claims. The first is: what statements does the theory assert? That is, what must we suppose to be true if we are to accept the theory? The second question is: what do these statements, if any, refer to? That is, what do they purport to be about? Then the theory may be taken to assert the existence of whatever the statements it claims to be true are about.

This may seem an excessively simple way to settle the ontology of a scientific theory. In fact neither question is as simple as it may seem. The first question is complicated on two counts. First it has been controversial whether scientific theories make statements at all, either true or false. Secondly, even supposing a theory to make statements, it is controversial whether its use involves any commitment to their truth. The second question is complicated by the difficulty of deciding just what it is that theoretical statements, if any, in science are about. I take these questions in turn.

The question has been long debated whether scientific theories make statements, that is whether they can be true or false. The alternative view is that they are to be assessed in terms of usefulness rather than in terms of truth and falsity. This view is usually called 'instrumentalism' because of its conception of scientific theory as an instrument, essentially for summarizing and predicting observations which could in principle be made and stated without it (see, e.g. Nagel 1961: chapter 6). I shall not here discuss instrumentalism further, largely because I think it false. I do not of course deny that

theories can be useful; I deny only that the concept of usefulness enables us to dispense with that of truth. On the contrary, it is generally its truth which makes a theory useful. In any case, an instrumentalist could not maintain a consistent interest in the present topic. Instrumentalism precludes any interesting ontological discussion about theoretical entities such as cultures. Their assumption in archaeological theory, like that of electrons in physical theory, would be nothing to an instrumentalist but a useful fiction. If all supposed theoretical entities in science are merely useful fictions, *a fortiori* those of archaeology are; but that tells us nothing interesting about specifically archaeological items. The present question is whether social sciences in general, and archaeology in particular, can add to our ontology in the way the physical sciences are supposed to. That is, granted a realist (i.e. non-instrumentalist) view of physical theory, if only for the sake of argument, can it be sustained for archaeological theory, and what are its consequences therein?

I shall assume therefore that theories in both physical and social sciences are advanced as making statements about their respective subject matters, whatever these may be. The second part of my first question is whether adopting such theories involves accepting the truth of these statements. I hold the increasingly heretical view that it does. Of course we know that theories cannot be conclusively verified by experience. We know also that they cannot be conclusively falsified, because of the extensive, if not limitless, possibilities of explaining away conflicting evidence with auxiliary hypotheses and modifications of the theory. All this is even more plainly true of the statistical theories that predominate in the social sciences. No single observation or series of observations will conclusively refute or establish the truth of a statistical theory. One might consequently suppose that adopting and rejecting such theories as a result of making observations is not done on grounds of truth or falsity at all.

The chief objection to that conclusion is the lack of any alternative rationale in general for adopting and rejecting theories. Suppose one said vaguely that theories are adopted because their statements provide convenient 'working hypotheses' and rejected when they cease to do so. If we try and distinguish this view from instrumentalism, we may still ask: in what does the convenience of a working hypothesis consist? Presumably in the reliability of the predictions derivable from it. Suppose such a prediction is that if experiment E is done, the upshot will be observation O (or a probability distribution over a set of possible observations O_1, O_2, \dots, O_n). But that prediction is only reliable to the extent that the corresponding hypothesis 'if E, then O' is true.

No doubt our evidence for the truth of interesting theories is usually very flimsy, and working hypotheses are entertained tentatively and modified by 'conjecture and refutation'. But the same is

true of the reliability of interesting theories. The inevitable lack of evidence for a successful theory detracts not from its truth but from the force of our claim to be certain of its truth. So we adopt theories tentatively at first and are prepared for further experience to make us modify them or give them up. That is, we assume their truth tentatively and are prepared to be shown wrong in that assumption. What is tentative, however, is our assumption, not the theory's truth.

Again of course we often use false theories in special situations where we know their consequences to be true. This knowledge may be direct or backed up by the true theory that has superseded a false one. Thus we go on using Newtonian mechanics for convenience in almost all everyday situations. We know it to be reliable there, even though false in general, and that indeed follows from relativity theory. But to know it reliable there is to know its relevant consequences true, and that this follows from relativity is pertinent only if we in turn believe relativity to be true. Truth, in either case, is still the test of usefulness.

The tentativeness with which scientific theories are accepted tends in any case to be overrated by philosophers of science. Many theories in physics, once quite speculative, have ceased to interest philosophers because they have become as well established as the most mundane facts. The theory behind radio transmission is so reliable that its truth is no longer seriously in question. Nor, for that very reason, is the existence of its most characteristic theoretical entity, the radio wave. *How* we can know all this, on the basis of our limited sensory input, is a problem as yet unsolved in the philosophical theory of knowledge. But the difficulty of this problem arises from the very certainty of the knowledge, and equally certainly casts no doubt upon it.

The immediately relevant point, however, is that tentativeness in a theory is a sign of lack of evidence, not of lack of truth (or falsity). Evidence may be even harder to come by in the social than in the physical sciences, and archaeological theories, among others, may be correspondingly more tentative. But that affords no reason to suppose them less capable of truth and falsity than are the theories of physics.

I have dealt rather briskly with the first of the two questions I said must be answered in settling a theory's ontology. I have done so partly from conviction and partly because these topics, although somewhat controversial, are neither novel nor likely to illuminate the differences between archaeology and physical sciences. They might have done so when physical knowledge was supposed to be peculiarly reducible to incorrigible knowledge of sensations. It might then have been held, as it cannot now, that physical theory could claim truth, and thus reality for its entities, which social theories could not. I show below that lack of conclusive evidence is not to the point; but even if it were, any difference between physical and social sciences is

one of degree, not of kind. (It is debatable anyway whether archaeology is any less securely based in experience than modern cosmology or nuclear physics.) Our chief concern is with the differences in what archaeological and other theories can be taken to refer to. In what follows I take for granted that theories of both kinds can be true and hence can refer to something.

Ontological debate in the social sciences has been overshadowed by debates about method. That societies are really 'nothing but collections of people' has indeed been asserted by so-called 'methodological individualists' (see, e.g. Watkins 1953, 1957). This claim about what societies are has been less disputed by so-called 'holists' than a corresponding methodological claim (Dray 1967). The latter is, roughly, that intelligible explanation in social science must in principle be translatable into terms of motives, actions and interactions of individual people. Our present concern, however, is first with the sense and then with the truth of the ontological claim, and its corollary that societies in general (and *a fortiori* cultures in particular) are mere fictions or logical constructions out of the people who are their members.

A common analogy with physics is useful here, but needs careful handling. It is easy to conceive of a society as made up of people in the way a gas was classically supposed to be made up of discrete molecules. People and gas molecules no doubt interact differently, but that does not affect the ontological comparison. There is nothing to a classical gas but its molecules. The laws governing the gas's macroscopic behaviour we will suppose both stable and derivable completely in terms of the laws governing its molecules' microscopic behaviour. Thus everything physics takes to be true of such a gas can be translated into truths that refer only to its molecules. The truth of the kinetic theory then only requires reference to gas molecules. Reference to macroscopic samples of gas is redundant.

Now it may seem convenient to put this fact, if it is a fact, by saying that gases do not really exist, only their molecules do so. Eddington, with his notorious 'two worlds' (1927: Introduction) put a similar conclusion in this way and has been followed in this by a number of philosophers since (e.g. Sellars 1961). But this way of putting it, however convenient it may seem, must be resisted. If gases *are* collections of gas molecules (as the translation project must require), and molecules exist so collected (as the adequacy of kinetic theory requires), then *a fortiori* gases exist. If Eddington's everyday table is in reality nothing but a swarm of fundamental particles, then it still exists, as just such a swarm. Similarly, a society that is nothing but a specified collection of people must still exist while they exist and are collected in the way specified.

The question of existence must be more carefully posed, therefore, if we are to exclude trivial answers. We are not interested in

listing every kind of existent, everything that is or may be referred to in a true statement. If any archeological theory is true, cultures will certainly and trivially occur on our list, as will both gases and gas molecules. We want a list of kinds of what we might call 'basic things', of which things of all other kinds may be supposed to be made. Gases, it seems, are not basic things; are cultures? We need not enquire, of course, whether anything else is made of cultures, merely whether cultures are made of anything else. The question that concerns us is whether people and everyday physical objects between them provide enough basic things for the references of all truths about cultures. Whether truths about people and everyday objects are in turn reducible to truths about the more basic things postulated by physics is not immediately to the point.

I used simple kinetic theory to provide an ontological analogue of society, but it is not clear that the analogy was put the right way round. It is spatially plausible enough: molecules occur within gases much as people occur within societies. But that is not really the point. The question is: what impact, if any, does accepting an archaeological theory have on our list of basic things. In comparing social and physical theories we should presumably make their theoretical entities analogous to each other. Cultures, that is, should be made analogous to the molecules of a gas, not to the gas itself. And the analogue of the gas should then be an individual member of a culture. Or should it?

We see now a complication, which casts further doubt on the use of the physical analogue. The pertinent feature of a gas in relation to kinetic theory is that it is observable independently of the theory. The role of the theory, and of the molecules it postulates, is to explain the independently observable behaviour of the gas. The role of archaeological cultures, however, is not to explain the independently observable behaviour of their members. Prehistoric people are not observable independently of the archaeological theory; on the contrary, they are postulated by it to explain assemblies of artifacts. It is the latter which are in this respect analogous to macroscopic samples of gas.

We must take care, therefore, not to be misled by irrelevant analogies. One such arises from the prejudice that gives ontological priority to the smaller spatial parts of an object. That was the prejudice tacitly appealed to in my initial analogy. A gas sample may seem to be nothing but molecules because molecules exhaust its material spatial parts. Other items may of course be present within the gas without being part of it (e.g. electromagnetic radiation). People similarly exhaust the material spatial parts of a society, although other material objects occur within it (and may even be practically essential to its existence).

The principle I explicitly appealed to was that all truths about gases should be translatable into truths about gas molecules. This

principle coincides for a gas with the 'spatial parts' prejudice. The coincidence is a consequence of a further regulative principle (or prejudice) in favour of explaining the behaviour of objects in terms of that of their parts (see Schlesinger 1963: chapter 2). If we add the deducibility criterion of explanation, namely that what is explained must be deducible from what explains it, we arrive at the translation principle. The deducibility criterion is highly debatable in general, but it is satisfied in the gas case, if imprecision is allowed for (see Mellor 1965).

It should be clear, however, from our preliminary discussion that the translation principle is what directly settles ontological questions. It is not the sizes or spatial relations of supposed entities that matter, but the need to refer to them in making true statements. The relevance of criteria of explanation is only indirect. They may constrain what true statements about cultures archaeological theory may propose in order to explain artifact assemblies. Whether cultures are basic things will then depend on whether reference to them in such statements is eliminable (e.g. by more complex reference to their members). The fact that cultures are not spatial parts either of their members or of artifact assemblies is immaterial.

One point of difference from the gas example, however, is very material. Gases may be wholly describable in terms of gas molecules, and so eliminable in their favour from our list of basic things. Assemblies of artifacts, on the other hand, are certainly not wholly describable in terms of cultures. For one thing archaeological explanation is largely nondeductive, so that even archaeological descriptions of artifacts are not wholly translatable into terms of the cultures that produced them. Now this might conceivably (though most implausibly) be a merely transient defect in archaeological theory. Much more importantly, artifacts have many properties which archaeology does not even attempt to explain (for example, most of their physical and chemical properties). Even a deductive archaeology would still leave many truths about artifacts unstatable in its theoretical terms. Archaeology therefore cannot hope on its own to reduce artifact assemblies to the status of logical constructions out of its own theoretical basic things. It might hope to do so in alliance with a sufficiently advanced physics; but such a physics might then well be nibbling at the ontology of people which archaeology presupposes and eventually eliminate the need for reference to any purely archaeological entities.

Pending this unlikely development, archaeology plainly must have people among its basic things, because of the way its field of study is marked out. To decide that a given object was not man made or manipulated is to remove it from the scope of archaeology. Were it to turn out that no prehistoric people ever existed, the whole of archaeology would prove false. Cultures may be distinguished one from another in terms of artifacts; they are all alike supposed to be

composed of people. The supposition is not, of course, incidental; it is essential to the explanation of artifacts. Plainly the only way a culture can make a beaker is for one or more of its human members to make a beaker.

An economical ontologist contemplating archaeology might still wonder then whether it really needs culture as well as people and artifacts, given that the first is not needed as a basic kind of thing for the other two. For if talk of artifacts and their makers is not completely translatable into terms of cultures, perhaps the converse is true. Let us consider some more ontological principles.

A preference for the spatial parts of things is not the only basis of our inclination to give people ontological priority over groups of them. Another basis is epistemological. Recall that the existence of something purportedly referred to by a statement is only needed for, and provided by, its truth. We can only be certain of the existence of things about which we can make some certainly true statements. Now the statements we are concerned to make in the sciences, in physics as much as in archaeology, are not made certainly true by being necessarily true. They may be true or they may not, and in the last resort we have to decide which on the basis of some sensory experience that we take to be relatively indubitable. It is the object of an experimental scientist in any field to relate his theories to some such experience. He will try to devise experiments and observations whose unmistakable results are as sensitive as possible to the truth or falsity of the theory being tested, and as impervious as possible to any extraneous influence. When we say that people and gases ^{are artifacts} are more readily and directly observable than either molecules or cultures, we refer to the relative ease of attaining this object in the former case. By and large ^{artifacts} observation can settle the truth of statements about people and gases, much more readily, and subject to less questionable assumptions, than it can settle the truth of ~~statements about molecules and cultures.~~ Yet we think we know the truth of some statements about molecules and cultures. (If we did not, I have argued, they would set us no ontological problems.) How is this more theoretical knowledge possible?

The early, and overoptimistic, answer of the logical positivists was to suppose the translation process I have cited reversed. They supposed observation to be the only source of empirical (as opposed to logical and mathematical) knowledge, and scientific (as opposed to metaphysical) theories they supposed to be both empirical and knowable. Appearances to the contrary, therefore, such theories could not really refer to unobservable things like molecules and cultures. These apparent references were regarded as compendious ways of referring to the multiplicity of observable things whose properties they explain. In terms of these observables, then, all theoretical entities must be definable.

Logical positivism, thus crudely characterized, had variants,

distinguished by the kinds of things admitted as adequately observable. The most stringent admitted only immediate sensory experience of such items as patches of colour in one's visual field. Only these were sufficiently indubitable to be acceptable as 'sense-data' and hence as basic things for the whole ontology of knowable natural science (see, e.g. Russell 1917; Ayer 1936). Scientists of positivist leanings were by and large less (and less consistently) sceptical, and prepared to take ordinary macroscopic objects as their basic observables. So, under positivism's 'operationalist' guise in physics, the content of all physical theory was supposed translatable into statements about the results of manipulating balances, clocks, meter rules, and the like (see e.g. Bridgman 1927). These, consequently, by our translation principle, were supposed to provide a basic ontology adequate for all physical theory. Far from gases being constructions out of gas molecules, it was the latter that were regarded as dispensable constructions out of the former. In psychology, the 'behaviourist' version of positivism gave its doctrines an explicitly ontological form (e.g. Watson 1925, Ryle 1949). Minds, and mental processes, it said did not exist, precisely for the reason that nothing irreducible to statements about behaviour could be shown to be true by observation. Whatever we can rightly claim to know of mental activity must therefore, for a behaviourist, be so reducible, and minds consequently must be mere fictions, logical constructions out of patterns of behaviour.

All this is now past history, because it has become clear that (i) no informative statements of the results of observation are absolutely indubitable, (ii) even if they were, statements about ordinary macroscopic objects (which are even more dubitable) are not reducible to them, (iii) statements about the theoretical entities of physical science are not reducible to statements about ordinary macroscopic objects. The demand must be given up that scientific knowledge be indubitably grounded in sensory experience. We must therefore equally give up the claim that either sense-data or ordinary macroscopic objects provide adequate reference for all scientific knowledge.

It nevertheless remains true that observation provides an inimitable check on claims to scientific knowledge. The more directly observable we suppose a kind of item to be, the less problematic are claims to knowledge of truths about it and hence to knowledge of its existence. Now on any currently conceivable scientific theories of human perception, we can generally observe people more directly than we can observe groups of them, just as we can observe gases more directly than we can observe their molecules. We are rightly convinced by this that people and gases are more certain to remain in our ontology than either molecules or cultures.

This need not mean that gases must either be or remain in the *basis* of our ontology, among the things of which all others may be

supposed made. Successive physical theories may assert gases to be composed of different kinds of particles or other entities. Our more secure knowledge of gases tells us only that there must be some such composition, and that providing it is a *sine qua non* of any acceptable physical theory. It is still most likely that no such theory will ever list gases themselves among its basic things; that it will always provide translation into its own terms for everything we more certainly know about gases.

(I neglect, in the above, the considerable ability of physical theories to correct as well as to explain longer standing knowledge of physical laws. The point has, if anything, been overstressed in recent literature: given due weight, it does not affect the present argument. See e.g. Mellor 1965, 1969; Popper 1957a.)

What follows from all this about cultures? Cultures are not as directly observable as gases, but we see from that example that this need not affect their standing as basic things. It means only that particular hypotheses about cultures must be held more tentatively than the gas laws and are more subject to revision and rejection. But while such a hypothesis is held, however tentatively, those who hold it must take any culture it refers to to exist (i.e. have existed, see p. 60).

It is clear, moreover, that none but the most naive hypotheses about cultures could be reducible to the statements about artifacts which provide the evidence for them. In the first place, the serious postulation of a culture is bound to have consequences beyond what can be tested archaeologically. No one supposes that the members of a culture laid down all their artifacts, or confined themselves to laying down artifacts to the exclusion of breathing, eating, sleeping, breeding and other archaeologically undetectable activities. And of the artifacts a culture did lay down, no one will suppose that all must have survived, let alone been discovered and correctly identified.

Postulating a culture therefore entails many truths untestable by archaeology, as well as many possible archaeological discoveries which will in fact never be made. In the same way, postulating a table entails many truths untestable by viewing it (e.g. about its weight) as well as many possible views of it which are in fact never seen. These latter facts have been taken to be fatal to positivist attempts to reduce talk of tables to talk of views of tables. The former are equally fatal to an analogous reduction of culture talk to artifact talk. For one thing, the reduction in each case requires adding admittedly possible but in fact nonexistent items to the inadequate number of actual artifacts and views available. The objection to that is simply that merely possible artifacts are not available for this purpose (or any other), precisely because they do not exist.

One must beware of possibilities in ontology. It is all right to say that more things of some kind might have existed than do exist. It is

dangerous to say that there is a possibility of more of them and quite wrong to say that they exist as possibilities. The last version is either unintelligible or false. If intelligible, it either involves self-contradiction (saying that the things both do and do not exist), or different senses or modes of existence (see p.60 above). Otherwise it postulates entities at once obscure and useless for the purpose. A possible but not actual artifact, for example, if one can conceive such a thing existing, is no substitute for a real one; certainly no known cultures ever produced any. Dragons, similarly, might exist even though they do not, but that does not mean that there are somewhere in the world such things as 'possible dragons'. There are no more possible dragons in the world than there are real dragons, viz. none. And there are no more possible artifacts in the world than there are real artifacts. In particular there are none available to replace cultures as the referents of archaeological truth.

Another reason why cultures are not reducible to artifacts is that the relation between the two is essentially statistical. That is, a given culture is credited only with a statistical distribution of artifacts. Even allowing for artifacts unmade, destroyed and undiscovered, no postulation of a culture would prescribe *exactly* the observed distribution. It will be compatible with an unlimited number of vaguely specified 'similar' distributions. This is not just to cover our experimental error in locating artifacts. The identity of no man or culture can be supposed to turn on such accidental behaviour as the precise laying down of an axe. So a culture can no more be completely defined by its actual traces than the bias of a coin can be completely defined by the results of its actual tosses.

It is obvious then that cultures are not definable by or reducible to their archaeological traces. It is also true, though perhaps less obvious, that they are not definable in terms of their members. We have observed that it is essential to archaeological explanation that cultures are composed of people. But that is not what distinguishes one culture from another, or cultures from other human groups. Nor is the content of archaeological theories provided by independently specified differences in their members' characteristics. There is no analogue of the kinetic theory's postulates governing their behaviour, from which that of their culture is derivable. I have already remarked the commonplace that we have no access to such specific knowledge of prehistoric peoples independent of the archaeological theories that postulate them. Nor has the development of social psychology and anthropology yet provided adequate grounds for postulating such knowledge on the independent basis of knowledge of modern societies. Moreover in modern societies we do have access to the individual members, and yet our knowledge of them does not enable us to deduce what is known of the societies they form. And if present societies must thus be admitted as ineliminable basic things, how much more must prehistoric ones be.

It may be argued that this conclusion merely reflects the limitations of our present knowledge. As we discover more about people, past and present, are we not bound to suppose a stage when all our knowledge of groups is derivable from our knowledge of their members? Not unless we beg the question by assuming *a priori* that group facts are just facts about their members. It is just as possible that knowledge of groups may perennially outstrip what can be inferred from knowledge of their members. That is speculation. When asked now what kinds of basic things there are, we can only answer in terms of what we know now. And it would seem that those who know archaeology must put cultures on that list.

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