

ISSUES IN THE ONTOLOGY OF CULTURE¹

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What kind of things are cultural things? Are they psychological things? Are they of an irreducible nature? What should be the relationship between a science of culture and other sciences, psychology in particular? These are the issues I want to touch on today. They have been much discussed in the past, by Durkheim, Boas, Kroeber, Radcliffe-Brown, Sapir, Leslie White, Geertz, and Sahlins among many others. We would agree, I suppose, that the arguments they used were not always as strong as the convictions they expressed.

Our present aim, it seems to me, should be to raise the level of argument and to achieve a better grasp of the issues, rather than to arrive at some final conclusion. Our understanding of cultural phenomena is far too limited to warrant a definite acceptance or rejection of psychological reductionism, or an elaborate real definition of culture.

The notion of the reduction of one *theory* to another is fairly well understood and is illustrated by famous cases such as the reduction of thermodynamics to statistical mechanics (see NAGEL 1961, Chapter 11). Loosely speaking, a theory *B* can be reduced to a theory *A* if all the generalisations of *B* can be re-formulated in the vocabulary of *A*, and if all these re-formulated generalisations of *B* can be shown to follow from the generalisations of *A*.

The notion of the reduction of one field of inquiry to another, such as the reduction of cultural anthropology to psychology, is much vaguer, and particularly so when either of the fields is not characterised by a well-established theory, or by a well-established theoretical programme. In such cases, assertions to the effect that one field can, or cannot, be reduced to

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the other are generally based on a priori convictions rather than on specific arguments. Some people believe in the Unity of Science, others believe in Emergent Evolution. I am an agnostic in these matters. I am not too concerned about the ultimate reducibility of a full-fledged culture theory to a full-fledged psychological theory. Besides, as exemplified by recent work in the philosophy of biology (DARDEN and MAULL 1977, DARDEN 1978), relationships between fields are too varied and subtle to be analysed solely in terms of reduction or non-reduction.

What I would like to know is how to go about developing a theoretical understanding of things cultural: in particular, should we bother with psychology, or ignore it? This is why I am interested in the ontological question: are cultural things psychological things? For, clearly, if cultural things are in no way psychological things, then psychology is irrelevant to their study; and if they are psychological things, then... well, then the issue is somewhat more complicated, but there is some hope that psychology might be relevant to the study of culture.

This is not to say that one could take psychological theories and somehow "apply" them to cultural data. Rather, it could be the case that at least some anthropological hypotheses have definite psychological implications, and that at least some psychological hypotheses have definite anthropological implications. If so, the need for mutual consistency would be a welcome source of constraint on theorising in both fields, and developments in each field might be suggestive of developments in the other. My estimate is that, given the present state of the arts, anthropology has more to receive and less to give than psychology, but this need not always be so.

I shall borrow from Jerry FODOR (1974) a nice way of distinguishing a more general ontological issue from the more particular issue of reductionism, or, if you prefer, a way of distinguishing two ontological questions which could be raised regarding culture: are *types* of cultural things *types* of psychological things? And here, of course, we are asking about the types that a science of culture and a psychology does, or would recognise, types about which there are, or there might be interesting generalisations. Or: are *tokens* of cultural things *tokens* of psychological things? The point of Fodor's distinction being that it is possible to have token-identity without having type-identity; we could have for instance some interesting generalisation about a class of psychological events each of which could be described as a physical event, while the class itself could not be characterised in the physical terms available to us.

I take it that if we answered "yes" to the type-identity question, it would

be but a short step, or no step at all, to psychological reductionism. However, I shall argue, we are in no position to answer the type-identity question. The reason for this is simple, even though it will take a bit of elaboration: we don't know what types of cultural things there are (and, arguably, we don't know too well either what types of psychological things there are), hence discussions of type-identity are premature.

On the other hand, a case could be made for token-identity between cultural things and psychological things. Take for instance my reading this paper in front of you: this is clearly something cultural. At the same time it is a complex of psychological and more particularly psycholinguistic events. Now, a description of it in psychological terms might be cumbersome to the point of utter irrelevance,² but this is not to say that it would be incomplete. Possibly, from a full psychological description, a proper anthropological description (whatever that might be) could be reconstructed. If we believed that such a situation generally obtained, we would be token-psychologicalists without having to be psychological reductionists.

What would being token-psychologicalists buy us? Two things: first, we could avoid having to make the assumption that cultural things belong to an independent ontological level, without having to commit ourselves to strict reductionism (and, of course, by a similar reasoning, we could be token-physicalists with respect to psychological things — see FODOR 1981, Chapters 5 and 6 — and hence consider every token cultural thing to be a token physical thing).

Second, there would be an initial plausibility to the view that some psychological generalisations might be of relevance to the study of culture. Of course, this might turn out not to be the case at all; types of psychological things might be entirely unrelated to types of cultural things, in the same way as, I suppose most of us would want to claim, types of physical things are unrelated to types of cultural things, in spite of a possible token-identity between them.

There is, however, a difference between token-psychologicalism and token-physicalism with respect to cultural things. Token-identity of cultural things to physical things, while easy enough to accept speculatively, is hard to imagine in a direct fashion. If we tried to work out cases, say a physical description of my reading this paper to you which would refer strictly to the same event as a description of it in anthropological terms, we would have to imagine an intermediary psychological description. We

² See PUTNAM 1975, pp. 295–298 for a detailed discussion of a similar example.

would have to do so in order, for instance, to be able to select the right neurological events, those involved in my speaking and in your listening, and to leave out other neurological events, say those taking place in some of you who, instead of listening, are thinking: "if only I had not had so much Schnaps last night!" The latter neurological events, even though they might take place in this room, do not properly belong to this specific cultural event (they do belong to the wider cultural event of our Salzburg Congress, I suppose).

Token-identity of cultural things to psychological things involves no intermediary level. This suggests — though, I repeat, it does not guarantee — that there might be some degree of correspondence or even of overlap between psychological and anthropological typologies, and hence some degree of mutual relevance between the two fields.

Are we, though, in a position to maintain that token cultural things *are* token psychological things? Not even that, I am afraid. There are strong grounds to hold that environmental factors such as population density, seasons, climate, and also man-made devices from everyday artifacts to irrigation systems or telephone networks are to be taken into account in a description of cultural things, and, surely, these are neither type- nor token-identical to psychological things. However, even something weaker than strict token-psychologicalism, namely, the assumption that every token cultural thing is a complex of token psychological and environmental things — all of which are physical things — is enough for our purpose: it allows us to consider that cultural things have no ontological independence, while keeping an open mind about reductionism; it gives us reasons to hope that psychology — and ecology — might be of relevance to the study of culture.

These then are the two ontological points I want to develop here: we don't know what types of cultural things there are, and we have good grounds to believe that token cultural things are a mixture of token psychological and environmental things. In order to establish these points, I have to turn now to the analysis of anthropological concepts.

Cultural things, anthropological terms, and types of family resemblance

Do we know what types of cultural things there are? "But, of course, we do!" most of my fellow anthropologists would answer: we don't know all of the types, we don't know them too well, but we know that there are clans, and lineages, and marriages, and kinship systems, and agricultural techni-

ques, and myths, and rituals, and sacrifices, and political systems, and legal codes, and scholarly institutions, etc. Now, these cultural types are not, and do not correspond to, psychological types. There are good grounds therefore, to oppose psychological reductionism in the study of culture, rather than be agnostic about it, and good grounds to treat culture as autonomous (and we can leave it to philosophers to decide whether this is to be understood ontologically or methodologically).

This view has been most cogently developed by David Kaplan:

Anthropology has formulated concepts, theoretical entities, laws (or if one prefers, generalizations) and theories which do not form any part of the theoretical apparatus of psychology and cannot be reduced to it. This is the logical basis for treating culture as an autonomous sphere of phenomena, explainable in terms of itself. It is wholly beside the point to maintain that anthropologists cannot proceed that way, for the brute fact of the matter is that in their empirical research this is the way they do most often proceed. (KAPLAN 1965, p. 973)

Kaplan's argument rests on an evaluation of anthropology's achievements. This evaluation can be challenged in two ways, one which I shall mention but not pursue, since I believe it to be unfair and unproductive, and another one, which I have developed elsewhere (SPERBER 1982) and of which I shall try to show that it has some interesting ontological implications.

The fact is that there is very little agreement among anthropologists about anything, beyond rejecting a few old-fashioned theories, e.g. meteorological interpretations of religious symbolism; and defending the profession against external attacks: no single concept is shared by all practitioners, no theoretical entity is universally acknowledged, no theory is generally accepted. In such conditions, it could be argued, nothing can be inferred about the autonomy of culture from the state of the art. I won't pursue this argument because I am convinced that anthropologists, without arriving at any kind of theoretical consensus, have, somehow, developed a genuine common competence in the study of socio-cultural phenomena. An evaluation of anthropology's achievements which does not include an explication of this competence is incomplete, and therefore insufficient to refute Kaplan's contention.

What I want to argue, rather, is that what looks like "concepts, theoretical entities, laws and theories" of anthropology are really intellectual tools of another kind; they are interpretive tools. From their existence and usefulness it is impossible to draw ontological conclusions (or what Kaplan sees as "methodological" conclusions).

It is not just that anthropologists don't *share* theoretical concepts; it is that they don't *have* theoretical concepts of their own. What they do have

is a collection of technical terms. They are technical in the sense that they are terms of the trade rather than ordinary language terms (or they are ordinary language terms used in a non-ordinary way). They are not theoretical, though, in that their origin, development, meaning and use are largely independent of the development or content of any genuine theory.

Throughout the history of anthropology, a number of these technical terms have been critically analysed, for instance “taboo” by Franz STEINER (1956) and Mary DOUGLAS (1966), “totemism” by GOLDENWEISER (1910) and LÉVI-STRAUSS (1966), “patri-” and “matri-linearity” by LEACH (1961), “belief” by NEEDHAM (1992), and, of course, “culture” by a great many anthropologists (see KROEBER and KLUCKHOHN 1952, GAMST and NORBECK 1976). The vagueness or the arbitrariness of these terms has been repeatedly pointed out. Yet, in spite of this critical work, there are no signs that anthropologists are converging on a set of progressively better defined and better motivated notions. If anything, there is more divergence and no greater conceptual precision today than there was half a century ago.

Edmund LEACH (1961) and Rodney NEEDHAM (1971, 1972, 1975) have convincingly argued that this vagueness of anthropological terms is not accidental, that it has to do with the way these terms have been developed and with the kinds of things they are being used to refer to, so that, if we want proper theoretical terms in anthropology, we should construct altogether new ones.

Rodney Needham has further argued that anthropological technical terms are best understood as “family resemblance” or “polythetic” terms, that is as terms referring to things among which resemblances exist, but which don’t fall under a single definition. More technically, a polythetic term is characterised by a set of features such that none of them is necessary, but that any large enough subset of them is sufficient for something to fall under the term. A polythetic term need not be fully polythetic: all its referents may share one or even several features, but as long as these necessary features are not jointly sufficient, the term is still polythetic.

Actually, it is dubious that fully polythetic terms (i.e. terms without any necessary feature) are ever used. All the members of a useful polythetic class normally belong to the same domain, which determines at least one common feature. All the members of the class of “games”, to take Wittgenstein’s famous example of family resemblance, share the feature of being activities.³ Or, when Needham writes: “the members of a class of

³ An example I owe to Jerrold Katz (personal communication).

social facts may share no feature in common" (1981, p. 3), he does not mean to deny, presumably, that they share the feature of being social facts. If actual polythetic terms are only partly polythetic then their use commits one not only to the existence of a resemblance but also to the presence of at least one definite feature in the object referred to.

Now, I want to argue that anthropological terms do indeed have some kind of family resemblance organisation, but that it may be a different kind of family resemblance than the one Wittgenstein and Needham had in mind. They had in mind a resemblance between the things described by the same term. For instance, every thing described as a game resembles some other things described as games. Let us call this a "descriptive resemblance". I shall suggest, however, that anthropological technical terms are not used descriptively, but interpretively. They are not directly used to describe; they are used to translate or render native terms or notions (or notions that the anthropologist attributes to the natives). The resemblance involved is an "interpretive resemblance" between the particular notions interpreted and the notion generally conveyed by the interpretive term. As a consequence, all the notions that can be properly interpreted by means of the same term will exhibit a typical family resemblance pattern: i.e. two such notions need not directly resemble one another, but there will be at least one further notion (the notion conveyed by the interpretive term) that they both resemble.

In the case of descriptive resemblance, there is normally no resemblance between the term itself and the things it is used to describe. In the case of interpretive resemblance, a term with some notional content (more about that later) is used to interpret other notions (expressed or not by a term); it is the resemblance in content between the interpretive term and the term or notion interpreted that makes the interpretive use possible.

The view that anthropology is an interpretive science is a well-known one and has been brilliantly defended by Clifford GEERTZ (1973). This is not, however, the view of anthropology I am defending. I agree that anthropologists studying individual cultures are mainly involved in an interpretive task, i.e. in representing native representations by means of translations, paraphrases, summaries, and syntheses understandable to their readers. On the other hand, I see the task of theoretical anthropology not as an interpretive, but as a descriptive and explanatory one, i.e. as similar to the theoretical task of the natural sciences. Furthermore, I am, I believe, on my own in arguing that the technical vocabulary of anthropology is — as a matter of fact, not of necessity — itself interpretive and not descriptive or properly theoretical. It is precisely because of its ontological

implications that this claim is likely to be resisted by those who otherwise see anthropology as a fully interpretive activity.

An example: “marriage”

From now on, I shall briefly recapitulate and then pursue the argument with reference to an example. Take “marriage”. Now, here is a true technical term of anthropology, and as good a type of cultural thing as you will ever get. But how good a *type* is it? Do all marriages fall under a single definition, or do we have reasons otherwise to believe that they share some yet unanalysed common essence?

Let us look first at a couple of characterisations of marriage that have been proposed. The *Notes and Queries* (1951) suggested: “Marriage is a union between a man and a woman such that children born to the woman are recognized legitimate offspring of both partners”. Here, you don’t have to look for exotic counter-examples. In most Western societies, the distinction between legitimate and illegitimate offsprings is becoming abolished. Children born in or out of wedlock may enjoy the same rights. The only sense in which some children may still be called “illegitimate” is precisely that they are born out of wedlock. But this, of course, makes a definition of marriage in terms of the legitimacy of offspring quite circular.

Or consider Lévi-Strauss’s claim: “If there are many types of marriage to be observed in human societies ... the striking fact is that everywhere a distinction exists between marriage, i.e. *a legal, group-sanctioned bond between a man and a woman*, and the type of permanent or temporary union resulting either from violence or consent alone” (LÉVI-STRAUSS 1956, p. 268; italics added). In the very same paper, Lévi-Strauss gives a counter-example to his own characterisation. He argues that many “so-called polygamous societies ... make a strong difference between the “first” wife who is the only true one, endowed with the full rights attached to the marital status while the other ones are sometimes little more than official concubines” (ibid., p. 267). Now, there may well be a group-sanctioned bond between a man and his “official” concubine. Therefore if Lévi-Strauss wants to distinguish this bond from true marriage, then his characterisation of marriage fails.

Such failures to properly define “marriage” are not accidental. Edmund Leach has argued that “marriage is ... ‘a bundle of rights’; hence all universal definitions of marriage are vain” (1961, p. 105). The point being here that the rights bundled together vary from society to society. Leach

lists ten kinds of rights, from: "to establish the legal father of a woman's children", to: "to establish a socially significant 'relationship of affinity' between the husband and his wife's brothers." He shows that that there is not a single one of these rights which is present in all cases of marriage.

Developing Leach's argument, Needham concludes that "marriage" "is an odd-job word: very handy in all sorts of descriptive sentences, but worse than misleading in comparison and of no real use at all in analysis" (NEEDHAM 1971, p. 8). There are two ways in which "marriage" can be said to do odd jobs: it does a few different jobs for all anthropologists, and also, and I believe more importantly, it does a different job for each anthropologist in his own field.

Imagine an anthropologist who goes to study the Ebelo. She might, in principle, wonder whether the Ebelo have at all the institution of marriage, but it would be surprising if she did. It is generally taken for granted in the profession that marriage is universal. She would not however expect to find something which would fall under a well-established definition of marriage, since there is no such definition. What she expects to find is some native institution which she may call "marriage" with as much justification as other anthropologists in their use of the term.

The problem she faces is not whether the Ebelo have marriage, but, as P.G. Rivière puts it, "which of the forms of relationship between the sexes is . . . to be regarded as the marital one" (RIVIÈRE 1971, p. 65). The logic is one of a party game, really: "if one of these forms of relationship were a form of marriage, which one would it be?" It would take a very odd society, or a very uncooperative anthropologist, for the question to remain without an answer. It is not surprising then that marriage should be found in every society. This is made possible, however, precisely by the fact that "marriage", whatever it does otherwise, does not denote a precise type of cultural thing.

But how does our anthropologist go about identifying which Ebelo form of relationship is "the marital one"? Does she *look* at relationships? No, relationships are not the kind of things you can look at. What she does, mostly, is to get Ebelo people to describe in their own terms the types of relationships they entertain among themselves. She then decides which of the native notions, and, possibly which of the native terms, is best rendered by "marriage". "Marriage" in English designates simultaneously a status, a change of status, and a jural relationship. In other languages, these three notions may not come under a single term. In such cases, "marriage" would be used to render a cluster of native categories.

Our anthropologist comes to the conclusion that "marriage" corre-

sponds to the Ebelo term *kwiss*. She then goes on to explain what she understands the Ebelo to believe, namely that marriage, i.e. *kwiss*, is a bond between a man and a woman blessed by ancestral spirits. Note in passing that “bond”, “blessed”, “ancestral spirit” are also used interpretively in this characterisation of the meaning of “marriage”/*kwiss*, i.e. they are not used to describe things, but to render further Ebelo notions.

Now a new case of marriage, the Ebelo case, has been added to the anthropological stock. It has been added on the basis of a resemblance. So had all previous cases, even the first one. “Marriage” became a technical term of anthropology when an anthropologist — or was it an historian? — decided that some exotic notion was best rendered by the ordinary language word “marriage”. From then on, “marriage” began to swell and lose its contours as more and more different notions were interpreted by means of it. The notional content of “marriage”, in anthropological writings, became a loose synthesis or compound of the sundry particular notions the term served to interpret. The point to stress is that, for a new notion to be rendered by “marriage”, it need not fall under the general notion conveyed by the term, it need merely resemble it. That is why, also, the fuzziness of anthropological terms is no obstacle to their use: fuzziness is no hindrance — if anything it is a help — to the establishment of resemblances. Other terms such as “taboo” or “totem”, became technical when an anthropologist decided to borrow a native word rather than translate it, and the family of interpretive resemblances was then built around this first exotic notion.

That the anthropological notion of marriage should be a family resemblance notion is thus no accident. It is a result of the very way in which it has been and is being developed. Resemblance — and not the possession of definite features — determines where “marriage” is to be applied. There is no reason to expect the development of anthropology to reverse this state of affairs. Actually, the better anthropologists come to know a greater variety of cases, the looser becomes the resemblance between instances of “marriage”.

Is, however, the resemblance involved in determining the applicability of “marriage” one among the things called marriage, or one between the notion interpreted and the notion (or notions) generally conveyed by the term used to interpret it? Is it, in other words, a descriptive or an interpretive resemblance? If the account I have sketched of how anthropologists go about identifying new cases of marriage is correct, then, clearly, the resemblance involved is an interpretive one.

Ontological implications

The two types of family resemblance, the descriptive and the interpretive one, have different ontological implications. If you take “marriage” to be based on descriptive resemblance, you should envisage that the term is only partly polythetic. Surely, all marriages are relationships; plausibly, they are all jural relationships. So, when you describe something as a marriage, this may well commit you to the existence of jural relationships as a fundamental *type* of cultural things. Now, unless you are prepared to argue that jural relationships are also a proper type of psychological things, using the polythetic notion of marriage so understood does not allow to keep an open mind about psychological reductionism: it should set you against it.

Not so with interpretive resemblance. Imagine that our anthropologist reports that Peter and Mary, two Ebelo individuals, are married. Is she, in so doing, *stating* that there is a bond between Peter and Mary that has been blessed by ancestral spirits? Presumably not, if only because it would commit her to the existence of ancestral spirits. She is reporting, rather (in the free indirect style — see SPERBER 1982), what the Ebelo people involved believe about Peter and Mary. She is interpreting Ebelo ideas. What does such an interpretation commit her to, ontologically speaking? It commits her to the existence of certain Ebelo people, and to the existence of certain representations in the minds of these people. Does it commit her to the existence of a thing, or a state of affairs (which could be called a marriage), and which would be distinct from the fact that certain Ebelo people hold the view that Peter and Mary are *kwissed*? I don’t see how. Our anthropologist might *want* to further commit herself in that way, but her report would give us no ground to follow her in such a commitment.

If “marriage” is an interpretive term, used to render a variety of native notions (or notions attributed to the natives, or notions synthesised from several native notions), then every anthropological account of a case of “marriage” is, when properly analysed, an account of a set of psychological facts. More specifically, to say that two people are married is to say that representations to the effect that these people are *kwissed* (or whatever native term is rendered by “marriage”) are properly distributed in the population. What constitutes a proper distribution is determined by the native notion of *kwiss* itself. For instance, it may be part of the native notion of a *kwiss* that once a priest and the spouses hold it that the latter are *kwissed*, then they are. To say that two people are undergoing a marriage ceremony is to say that some physical interaction is taking place

between people such that will cause the proper representation to be properly distributed.

But what about “marriage” in theoretical or comparative work? Doesn’t it, there, correspond to a general concept? Well, if you believe it does, say *which* concept. I am not claiming that it would be impossible to define a general concept which could reasonably be expressed by “marriage”. I am suggesting that there is no obvious reason why you would want to define a concept meeting this particular condition, and that anthropologists, notwithstanding the appearances, have never truly bothered. They have found it useful to abstract from interpretive ethnographic reports in order to arrive at general interpretive models. These models are not *true* of anything; what they do is help the reader get a synthetic view of ethnographic knowledge. They also serve as sketches of possible interpretations for further ethnographic work. So, “marriage” in these general anthropological writings is both a loose topic-indicating word, and an interpretive term considered not in relation to any one of its particular uses, but in relation to several of its actual or potential uses.

What is true of “marriage” is true of the technical vocabulary of anthropology in general. “Tribe”, “caste”, “clan”, “slavery”, “chiefship”, “state”, “war”, “ritual”, “religion”, “magic”, “witchcraft”, “possession”, “myth”, “tales”, etc. are interpretive terms⁴. There is a family resemblance, but an interpretive one, between all the notions each of these terms serves to render. When these terms are used to report specific instances of events or states of affairs, they help the reader get an idea of the way in which the people concerned perceive the situation (“seeing things from the native’s point of view”, as the phrase goes). What do these interpretive reports tell us about the *nature* of whatever is taking place? Well, they tell us that there are some psychological and some physical things going on, and that’s it.

A few general terms used in anthropology are not interpretive in that sense, but nor do they suggest the existence of a distinct ontological level of culture. Some are straightforwardly psychological, such as “color classification”. Others are straightforwardly ecological, such as “dam”. What differentiates these psychological or ecological terms used in anthropology from the proprietary vocabulary of the field, is that they apply quite independently of the “point of view” of the subjects concerned. People can

⁴ See DETIENNE 1981 for a relevant discussion of “myth” and SPERBER 1982, Chapter 1, for one of “sacrifice”.

have a color classification without being aware of the existence of such things as classifications, and we can agree that beavers build dams without attributing to them any cultural point of view. On the other hand, a marriage cannot take place without some people entertaining the idea that a marriage (or a *kwiss*, or something of the sort) is taking place. Moreover, it is unclear what other necessary conditions there are for something to be a marriage beyond its being represented as such in the appropriate minds.

Conclusion

I have tried to make three points:

(1) The technical vocabulary of anthropology is neither observational nor theoretical, it is an interpretive vocabulary. Moreover, each anthropological term serves to interpret a great variety of native notions which share among themselves a mere family resemblance. Terms the use of which is determined by interpretive resemblance carry neither ontological nor typological implications.

(2) We just don't know, therefore, what *types* of cultural things a science of culture would recognise. We don't know whether these types would be reducible to psychological types. There is no a priori reason, either, to assume that these types would correspond, even approximately, to the technical terms of current anthropology, since these terms are not even aimed at identifying such types.

(3) Interpretive accounts of particular cultural phenomena, an Ebelo marriage, or the reading of a paper at a philosophical congress for instance, allow one kind of description: culture phenomena are mental representations being distributed, over time and space, in a human population as a result of physical interactions and cognitive processes.

I want to suggest, in conclusion, that this token-identity of cultural events to distributions of ideas is what we have to start from if we are interested in a scientific understanding of culture. What does that imply?

A distribution of ideas would not be likely to fall under any type recognised in the cognitive psychology of the individual organism, nor is there any reason why it should. The study of the distribution of ideas stands to the study of individual cognitive phenomena the way epidemiology stands to the study of individual pathology. Epidemiology and individual pathology use different data, concepts and method; epidemiology takes into account environmental variables of various ontological status; but, whether it is approached from the point of view of individual pathology or

from that of epidemiology, the ontology of diseases is basically the same, and the two fields are highly mutually relevant. Epidemiology does not “reduce” to individual pathology, but they both work — individual pathology exclusively, and epidemiology essentially — within the same ontological level of biological facts.

Similarly, I am not arguing for a reduction of cultural anthropology to individual psychology. I am suggesting rather that the scientific study of culture might take the form of an epidemiology of ideas (a notion that has been toyed with by a few anthropologists, social psychologists and biologists at various times, but never been properly developed). Like the ontology of disease epidemiology, the ontology of an epidemiology of ideas would be somewhat messy; it would take into account a variety of environmental variables; but its basic subject-matter would be of course psychological. Hence a relationship of mutual relevance with individual organism psychology, and in particular with cognitive psychology, could be expected. I am aware, though, that there is no direct path going from the type of ontological clarification we have been concerned with, to what is truly essential and will ultimately decide the issue, namely the development of a scientific understanding of culture.

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