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Role and Status of Anthropological Theories

How successful has anthropology been in producing its own theories? or even in borrowing them?

Sidney Morgenbesser

The progress of anthropology in the last hundred years has evoked, even from critical observers of the state of the social sciences, well-deserved admiration (*1*). Developments in data collection and interpretation, subtle employments of logical and mathematical techniques, a spate of monographs that tax the patience even of the most Alexandrian of librarians, all prove that the phrase "a trained anthropologist" is not a contradiction in terms.

Despite these advances, it is debatable whether the theories employed by anthropologists need win our assent. Concomitantly, it is questionable whether such terms as *culture, cultural integration, role,* and others which appear in such theories are as indispensable for a proper explanation of societal factors as many anthropologists claim.

The first point to note is that there is no universal agreement among anthropologists about the role and relevance of any of the theories currently employed. There is even room to doubt whether anthropologists have any theories at all. British anthropologists have insisted that the concept of social structure introduced by Radcliffe-Brown (*2*), developed by Evans-Pritchard, and now the subject of an interesting monograph by the late and too-little-read S. F. Nadel (*3*) provides them with a rock upon which to build. But a concept is not a theory. Many American anthropologists, moreover, find that Radcliffe-Brown's statements are either opaque or are merely developments of some trite sociological points that Durkheim made long ago (*4*). Not to be outdone in gallantry, British anthropologists find American theoretical anthropology to be either an obsession with the workings of a mystic entity labeled "culture," or applied psychology (*5*). Lest the reader think that I am recording a series of transatlantic insults, I shall quote the words of a long-respected American anthropologist, provoked by an anthology of American anthropological writings: "Some two generations ago," writes Paul Radin, "the great English legal historian (Maitland) declared that anthropology would very shortly have to choose between being history or nothing. Maitland was wrong. . . . Anthropology did not become history, nor did it ostensibly become nothing. In fact, it became everything, and seemed to have taken its etymological meaning literally" (*6*).

These dour notes serve a function. They suggest that it would be helpful to consider anthropological theories under two headings—psychological ones employed by American, and sociological ones employed by British, anthropologists. Since there are Americans in the English camp, and Britishers in the American, this division should not be taken too seriously.

The employment by anthropologists of psychological theories, especially of learning theory (*7*), might evoke surprise.

Uses of Learning Theory

A score of years ago the journals were replete with articles attempting to establish the independence of the social sciences in general, and of anthropology in particular, from psychology. Some merely insisted that cultural data cannot be accounted for on the basis of psychological principles alone. Others went further and suggested that anthropologists disregard the findings of psychology and direct their attention to the interaction of biological and cultural phenomena. Still other articles, like Kroeber's, seemed to suggest that culture is a unique superorganic entity with laws and properties of its own. Culture was the hero and psychology the villain of the drama, and not to anthropologists alone. In a famous chapter in *Experience and Nature,* John Dewey suggested that philosophers test their theories of human nature in light of the teachings of anthropology about culture and its influence. As the writings of Leslie White prove, such themes have not entirely disappeared from the literature of anthropology (*8*). But they are much rarer. Today more anthropologists couple the term *culture* with the word *learned,* or one of its cognates, or else drop the term *culture* and refer merely to "learned behavior." And since learning theory is part and parcel of the science of psychology, we have, despite earlier protestations, not merely an alliance between anthropology and psychology but a threatened domination of the former by the latter.

This turn of events is not too hard to explain. When anthropologists criticized the relevance of psychology, they were primarily refuting the thesis that infor-

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mation about mental or private phenomena will account for behavioral data, and they thus identified psychology as the science of the mental. Critiques of this version of psychology, however, are ineffective against the type of behaviorism which many defenders of current learning theory uphold.

Learning theory has some decided advantages. It deals with acquired dispositions to behave, and if its scope can be shown to encompass all cultural phenomena, then mysteries about the ontological nature of anthropological subject matter will disappear. One need no longer worry about the status of culture in contradistinction to that of the individual and need no longer pose questions about the relationships of the individual to his culture. Cultural phenomena are shared acquired dispositions, all of them dispositions of specific human beings.

Learning theory also provides a set of interlocking propositions about the mechanisms for the acquisition of dispositions and thus affords anthropologists an escape from silence. For the truth of the matter is that anthropologists, despite their persistence, have never been explicit about the conditions under which habits are acquired, and thus about the conditions for cultural transmission. Moreover, learning theory holds forth promise of being a more basic theory than any of the Freudian theories that are available. For, suggestive as Freudian doctrines are, they are primarily devoted to an explanation of the development of personality traits and structures. Such traits are acquired, hence learned, and hence are part of the subject matter of learning theory (*9*).

It is, of course, not only to the analysis of the term *culture* that behavioristic learning theory is relevant. Anthropologists as well as sociologists fill their pages with references to vague entities, such as roles, institutions, and social structures, that ostensibly influence and mold behavior. Quite frequently the statements about institutions, for example, are informative, and do not reflect belief in shadowy entities that cause and direct human action. Nevertheless, many of these statements are metaphoric and not explicit enough for scientific purposes. Learning theorists, by emphasizing the importance of discussing specific human beings and their traits, behavior, and dispositions, frequently introduce much needed rigor into social science writing.

Finally, learning theory, by lending itself to experimental confirmation, indicates a continuity between the methods of the natural and social sciences, and provides ground for the hope that the latter may some day be as successful as the former. Some may think it absurd to expect that experiments on rats, chimpanzees, and college freshmen will provide conceptual tools for the analysis of complicated social structures. But, a priori, it is no more strange to expect that than to predict that rolling balls on inclined planes will illuminate the paths of the planets and the movements of the stars.

Limitations of Learning Theory

Despite these virtues, it is debatable whether the employment by anthropologists of learning theory has always been beyond reproach. Learning theory is a category term. There are many learning theories—Hull's, Guthrie's, the Gestaltists'—among which the anthropologist must choose. Frequently anthropologists have opted for Hull's "need-reduction" theory of learning, as if there were no legitimate doubts about its rigor and empirical confirmation. But on both counts Hull's theory has been found wanting, with the result that anthropological texts which depended upon Hull seem in retrospect naive and uncritical.

Even granting Hull's theory of learning and kindred conditioning theories one may still cavil at the statements which anthropologists make on the basis of such theories. For anthropologists and, it may be said, social psychologists have frequently converted the thesis that all learned dispositions may be due to the satisfaction or reduction of human needs to the proposition that all acts result from the attempt, either on a conscious or unconscious level, to reduce needs or tensions concomitant upon their unsatisfied presence. The acceptance of the latter proposition has led to much uncritical theorizing, of which the following is typical: "Suicide," writes John J. Honigman, "is a means for overcoming perplexing problems and anxieties, and it reduces a variety of tensions, but in doing so it also kills the person" (*10*).

Then again, anthropologists have frequently confused the triviality that most adult actions have a learned component with the dubious thesis that all adult behavior or action is simply learned and that a theory of learning may account for all such action. It is, of course, in light of his previous learning that a chess player decides upon a move, but it would be silly to think that all chess moves are merely learned and that we would be able to deduce a chess player's moves simply from information about his previous "reinforcements." At least some information about his perceptions and expectations would be relevant. And, it may be added, to the theory of perception and cognition, learning theory of the Hullian type has thus far made no major contribution.

There are chess players who play chess by rote and never make a creative move, or even realize when they are confronted by a challenge. If all members of primitive societies resembled this type of chess player, it would perhaps be not too misleading for anthropologists to rely so heavily on conditioning theory. But anthropologists, especially at the beginning of this century, had undermined the contrast between primitive cultures, which are ruled by custom, and complex societies, which require and demand complicated decisions. They had emphasized that challenges and creative responses mark simple as well as complex societies. About such responses, it would be naive to insist that they were only learned or conditioned, although they are caused, and previous learning and conditioning are part of their causes.

Some of these difficulties will doubtless be avoided through sophisticated uses of an improved learning theory that is integrated with other psychological theories. But even in a refurbished form, learning theory, like any theory, cannot be directly applied but requires supplementary information which it does not explain. To fix our ideas, let us examine a relatively naive use of learning theory. A psychologist, wanting to explain the proverbial Jewish love for learning, appealed to the well-known fact (sic) that all Jewish mothers put honey on books and then induce the youngsters to taste the sweetened tomes. A few applications of conditioning theory, and psychologists would have the appendix to Bialik's "HaMasmid"!

This licking theory of learning has its advantages but leaves unexplained why Jewish mothers and not Albanians engage in such nefarious practices. At the risk of circularity we cannot insist that it is part of the Jewish culture to do so, and it would be evasive to say that Jewish mothers do what they do because their mothers did so before them. At some stage we would have to appeal to some historical explanation, and it is doubtful whether psychological theories alone, much less learning theory alone, suffice for such explanations.

Societies as Integrated Units

Reduction of his theories to those of psychology does not prevent the anthropologist from devoting himself to other tasks, of which the paramount one has been the depiction of the interrelationships between elements of distinct cultures. Frequently such descriptions have resulted in monographs that, like the recent one by Francis Huxley on the Urubu Indians of Brazil (*11*) are interesting but devoid of theoretical concern. In the case of such thinkers as Malinowski, Benedict, Opler, and others, concentration upon distinct cultures as integrated units has fathered a school of thought about such cultures and about primitive societies.

The importance of these scholars in general, and of Malinowski in particular, must be acknowledged. They were sensitive observers and effective writers, setting high standards for both field work and publication. Their criticisms of naive evolutionary theories, of uncritical studies of cultural diffusion, and of dubious attempts to compare cultures, were instructive and emphasized the importance of concentrating upon the life of a people as a whole. Finally, by presenting theses not about individuals but about groups, societies, and other collectivities, they offered examples of theories which, prima facie, are independent of psychology and thus indicated another limitation to the learning theory approach to anthropology.

The significance of this school may easily be overestimated. Some of the criticisms by its members, especially of evolutionary views according to which societies have uniformly evolved from the simple to the complex, were not original, similar criticisms having been made earlier by Boas and others. Other aspects of their criticisms were unclear, for the criticisms rested upon misapplication of the postulate that the same institution in different cultures means different things. It was in light of this postulate that they criticized not merely naive evolutionary views and uncritical applications of the comparative method but any attempt to find laws about institutions which hold true in all or most cultures. But a law about marriage, for example, is not a statement about every one of its aspects but simply a proposition about its relationship to at least one other cultural or biological factor. To admit that every A is associated with B does not entail the hypothesis that all A's are identical, nor does it deny the thesis that differences among the A's might be accounted for by reference to the contexts in which the A's appear. Hence, no anthropologist need be suspended, like Buridan's ass, between the hay of admitting with Malinowski that no two manifestations of the same cultural unit are identical and the hay of searching with the Yale school for traits that are associated in all or in a statistically significant number of cultures.

The major limitation of this school, however, is that it is not a school. Though Malinowski, Benedict, *et al.* used common language and collectively engaged in bootless rhetoric against vague theories to the effect that cultures are heaps, or sums, or bundles, of traits, they did not offer common theories. They used terms like *integration* and *whole* differently, and they offered hypotheses that differed in range and significance.

Benedict's thesis is perhaps the easiest to understand and the one least vulnerable to criticism. She did not claim that all cultures were integrated, but only that some simpler ones were. No general laws about the integration of cultures, at any rate, were stocked in her theoretical arsenal. Except for occasional slips, she did not attempt to explain, but merely to describe, the cultures she found integrated, maintaining that a certain emotion or attitude pervaded all, or the major, activities of an entire people. It is therefore no criticism but simply evidence of a realization of the limitations of her position to point out that although she documented the hypothesis that the Zunis were moderate, she explained neither why they were moderate nor why they did the specific moderate things that they did.

Such limitations seem to be absent from the work of Malinowski. Not only did he describe individual cultures such as that of the Trobriand Islanders brilliantly, but he also offered general principles in light of which these and other cultures can be understood. As already stated, he insisted that cultures were wholes and that they must be investigated as units. He did not simply mean that a certain mood or attitude pervaded an entire culture. What, then, did he mean? No simple answer can be given, for he seems to have meant many things, all of which have been subsumed under the general rubric of a functionalistic analysis of culture and society. This analysis, though partially disclaimed by Malinowski himself, has had great influence and is therefore deserving of at least a limited review.

Limitations of Functionalism

Functionalists (*12*), among whom are to be counted not only anthropologists influenced by Malinowski but also sociologists instructed by Parsons, are triply distinguished. They emphasize the interdependence of cultural units. They compare cultures to purposive agents or mechanisms. They fail to illuminate both the interdependence of cultural units and the analogy between societies and teleological entities.

Functionalists are, of course, correct if they mean that societal institutions are interdependent in the sense that changes in some institutions will produce other changes in other institutions. This position is both banal and possibly beyond disproof. Functionalists are most likely wrong if they mean that every change in every societal institution will produce at least some change in every other societal institution. There were, for example, many Indian tribes (though, to be sure, not all) who incorporated the horse into their way of life without altering many of their habits, customs, and ceremonies. The functionalists present merely prefaces to programs when they inform us that societies are integrated units and do not bother to specify the criteria for integration.

The statement that unless certain requirements for group survival are met no group can continue is a tautology. It is equally trivial that in any on-going society, certain institutions or repeated ways of behavior are either necessary or sufficient for meeting these requirements. It is not too misleading to recast these observations into teleological language and, with the functionalists, to consider societies as aiming at the solution of certain problems, and to consider institutions as functioning effectively or ineffectively with respect to such solutions.

But if teleological language sometimes can be substituted for standard scientific terminology about necessary and sufficient conditions, the converse always can be done. Even if we grant that societies are functioning organisms or, more guardedly, if we consider it of heuristic value to compare them to teleological entities, societies need no more be discussed in the language of purpose and function than Plato need be discussed in Greek. But the main problem is not the indispensability of a terminology but its employment to construct general theories. Judged by the latter criterion, the success of the functionalistic analyses of society is questionable.

It is, for example, doubtful whether societies are teleological entities in one clear and crucial respect. Ordinarily when we assert that men, animals, or machines are functioning purposively for the accomplishment of given ends or goals, we do not simply mean that some of their ways are sufficient for the achievement of the goals in question. We also mean that if they are precluded from manifesting these patterns, they will exhibit others which once again are directed toward the accomplishment of their aims. A certain plasticity of behavior is characteristic of teleological mechanisms and is not always characteristic of societies. The latter have standard ways of assuring their survival and of meeting other social requirements. And although some societies occasionally exhibit intelligent redirection, others are marked with a rigidity that expedites their extinction.

Moreover, since Radcliffe-Brown's influential work in the middle '30's, it has been a functionalistic commonplace that although many institutions are socially effective and do contribute to group survival and integration, others do not, and are, in the terminology introduced by Merton, dysfunctional. A general law about the social functioning of institutions is therefore ruled out by this admission, and we are left with the noncharismatic truth that some institutions contribute to group survival and some do not.

To criticize functionalism as a general theory is not to cast any aspersions upon specific analyses it has inspired. When, for example, we read Evans-Pritchard's account of the lineage system of the Nuer and discover that it contributes to social integration, we are enlightened (*13*). But our instruction consists in learning a surprising condition for social unification, not in discovering a fact which dovetails into a general theory of the functioning of societies as wholes.

This is especially true of Malinowski's work. He has informed us not only about the consequences of group actions but also about the interdependence of cultural units. But the interdependence is not a causal relationship, and it is not one that exists between institutions or ways of behavior. Malinowski demonstrated that the beliefs of primitives or members of simple societies cannot be investigated singly—that primitive beliefs form an interconnected set.

To speak of interlaced beliefs does not mean that we commit ourselves to a geometric treatment of these beliefs. All of us, for example, have diverse opinions about our friends that impinge upon, and are impinged upon by, our beliefs about politics, love, and baseball. But it would be silly to think that these beliefs form a system with a few major postulates from which every opinion is deducible. That primitive beliefs form a system in this sense has been, at least in part, the motivation of the work of men like Opler, and for such an assumption I see no evidence.

Nor is there any basis for the thesis that once we know the beliefs of a group, we will be able to completely understand or to predict its behavior. There are no mechanical rules for the translation of belief into action, and not all primitive peoples act according to their beliefs. Malinowski himself noted many discrepancies between the beliefs or ideals of the people he investigated, and their actions. This discrepancy is not accounted for by any of the functionalistic theories of the types we have reviewed.

Comparison with the Study of History

If a discipline is considered logically independent only when it has its own laws and theories, then anthropology is threatened with extinction as a logically independent discipline. The application of learning theory is an explicit attempt to reduce anthropological theories to those of psychology. Functionalistic analyses of culture have become incorporated into general sociological theories. Neither learning theory nor functionalism exhausts the theories employed by anthropologists, but they are the most important and the most typical. The heteronomy of anthropology is also revealed in its theoretical studies of primitive languages or social evolution.

The lack of distinct theories cannot be taken as a sign of the richness of anthropology. Nor can this lack be excused on the grounds that anthropology is a young and struggling discipline. It is old enough to have some character. On the other hand, this lack does not mean that anthropology is not a distinct discipline, with its own problems, techniques, and subject matter. It means only that anthropology may have to borrow its theories from other domains.

Anthropology has frequently been compared, and even coupled, with psychology, sociology, and other generalizing social sciences. It may be more fruitful, with Maitland, to liken anthropology to history.

The main task of the historian is to describe significant periods and to explain crucial events, both of which aims he accomplishes with theories borrowed from all of the social sciences. For that we are in his debt. Historians who present general laws or philosophies of history are not useless. They mesmerize and provide interesting examples of fallacies even for the nonpositivistic philosophers of science. But they are on the periphery of the camp of the working historian.

It took much methodological ink in the 19th century to establish these views about history. Their counterparts about anthropology have not as yet been widely accepted (*14*).

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14. Work on this article was done while I was on a grant from the Social Science Research Council of Columbia University.