

Letters

Anthropological Theories

Sidney Morgenbesser's article, "Role and status of anthropological theories" [*Science* 128, 285 (1958)], must give all anthropologists pause for reflection. He has made accusations, most of them just, and smashed icons, most of them already cracked. But in doing so in the pages of *Science* he has written not for anthropologists and philosophers but rather for a more general audience of scientists. To me, at least, the article seems a little misleading for such an audience, since the author did not mention the chief problem of anthropology. Anthropology's difficulties and its achievements can be somewhat better evaluated if that problem is squarely set forth.

Anthropologists gather most of their data from people many of whose ideas and activities are so foreign to our own as to merit the epithet "exotic." It would be convenient, if it were possible, to examine their modes of talking and acting, and classify them into some sort of pre-existing categories about which we could theorize. Anthropologists, in fact, did that for many years; some still do. But it must be remembered that "natives," thinking and acting "exotically," do not act at random. They have ideas and linguistic terms for organizing and communicating their activities. And once an anthropologist knows a culture well enough to communicate in their own terms with the people he is studying, he realizes that to reduce their modes of understanding to a set of pre-ordained categories may be to falsify them out of reason.

Here, then, is the basis of the anthropologist's problem: not only do ideas and acts exist; they are organized some way or another by the people who communicate about and act them. Yet, that organization is seldom—one could probably say never—adequate to allow generalization or broad comparison among cultures. Therefore, something beyond this folk organization of the facts is needed, namely, an analytical organization or a "scientific" theory.

Whereas a scientific theory in the physical or life sciences has to cope only with the facts and their theoretical organization, the behavioral sciences and particularly anthropology must deal with facts, with folk organization of facts, and only then with theoretical organizations of both. I do not mean that people are not sometimes wrong in their folk organization of facts, but only that if they are, that in itself may be the key fact.

Here, then, is the basic problem. The anthropologist always discovers some

sort of folk organization in his data. But this folk organization is not in itself adequate for his purposes, so he must create an additional organization—an analytical organization—in order to compare the facts and the folk theories.

Now, both the folk and the analytical viewpoints may be perfectly sound, but they must not be confused with one another. The folk organization has as its purpose action in daily life. Overt or covert, it is adequate to the extent that it is successful from the point of view of the actors. The analytical organization, on the other hand, has analysis and theorizing as its purpose. It should be entirely overt, and it is as adequate as the theory to which it leads. The direst trap for the anthropologist is to confuse folk organization with analytical organization. The most heinous fault that a work of anthropology can have is assignment of ideas from an analytical system to people who act merely in terms of a folk organization.

To take an example: a book on the law of an African tribe, say, based on sensitive and thorough field research, can be irrevocably spoiled if the author assumes a theoretical framework such as that established by Roman law, canon law, or common law and assigns these categories to the ideas and acts of Africans without first discovering and explaining the framework of "folk theory," so to speak, on which the Africans themselves organize the facts. To take another example: in the past, anthropologists have noted quite correctly that a certain ritual increases community solidarity (whatever definition they may have given for that), then spoiled it all by saying that the natives performed the ritual for the purpose of achieving solidarity, which of course they did not.

It is mistakes of this sort that Morgenbesser quite rightly holds in disrepute when he says that many anthropologists "fill their pages with references to vague entities, such as roles, institutions, and social structures, that ostensibly influence and mold behavior." Roles, institutions, and social structures are part of analytical systems. They cannot possibly affect behavior, which is organized in terms of folk systems. Only after a people has been educated to it can analytical systems sometimes play the role of folk systems. What Morgenbesser should have scored us for—we sometimes deserve it—is inability to distinguish data from analysis.

Morgenbesser's article glides over this whole problem in its last section, where he dismisses the theorists of "systems of belief." He attacks the term "system" when he ought to have attacked the word "belief." Anthropologists use the word "system" somewhat more loosely than Morgenbesser suggests. They mean by it

something that they might better call "consistency." But "belief" is more difficult. It is an interesting fact that many non-Indo-European languages have no term which even resembles our concept of "belief." What should be studied here, instead of beliefs, is "concepts" marked by "words." Words and concepts are systematized, in some degree at least, by the nature of language. And it is these words, and their combination into concepts, which are raw material for the anthropologist—not "beliefs," which are vague, undefinable, and subject to gross and nonregular distortion in communication. The folk organization of social and cultural activity, in other words, is in terms of concepts and the word tags by means of which people identify them.

As a matter of empirical experience, every culture has certain key words in its language. Almost without exception it is not possible for a person of that culture to say what they are. Those key words may be relatively few, as among some African groups or some American Indian tribes. They may be, on the other hand, so numerous that their very complexity makes it seem as if there were no keystones at all, or that this method of proceeding is not applicable in such an instance. This restatement of Ruth Benedict would seem to answer Morgenbesser's criticisms.

If an anthropologist learns the language of the people he studies—learns it really well—he should be able, in the process of translation, to pick out these terms: they are merely the terms which allow him to get his ethnography sensibly into as few folk categories as possible. I shall take examples from my own field work among the Tiv of central Nigeria. One such key word for Tiv ethnography is *tiev*, which means farm or garden. To describe the farms, the emotional attitudes to them, and work habits associated with them, is to set *tiev* into its native context. Another such word is *jir*, which means court, court case, moot. Tiv spend a great deal of time settling disputes in *jir*, and all of their ideas of justice and ethics among human beings eventually impinge on this activity. Another such term is *tsav*, a substance which grows on the hearts of "witches" and which forms the keystone of magical and religious ideas. Another is *kasoa*, which refers to what we would call a "market," but is much broader in concept than our term.

There are some half a dozen other such key words. If I write about these words, explaining them and their context fully, I can organize most of my field notes, and can communicate the major part of what I know about these people. A look into the work of any careful ethnographer will furnish just such a list of key concepts.

I do not imply that every anthropologist, given the same situation, would pick out the same key words, but I do think that the overlap would be very great. We should also remember that a few years may make a great deal of difference in the culture of a people and in its key points.

Now, in many cases, key concepts are not, by any stretch of the imagination, directly comparable from one culture to another. In many other cases, of course, they are. The next problem, in any case, is in the communication of the full meaning of these concepts to an English-speaking audience—the English ideas have become “exotic.”

This particular form of translation is the anthropologist's chief problem. He must explain alien concepts in English without assimilating them to Western concepts, which would destroy them. The task can be achieved in only one way: erection of an “analytical system”—a “theory,” if anyone prefers the term—by means of which the Anglo-American or Western notions can be compared with the exotic ones in such a way that neither of them loses its individualizing traits—its “genius,” to use the 18th-century word.

It should be noted that I have now claimed that anthropologists are interested in ideas. And so I believe to be the case. They have got involved in sociology from time to time, and they have left it with such concepts as “role” and “culture,” on both of which sociology is today more dependent than is anthropology. They sometimes get involved with psychologists and with learning theory. As Morgenbesser has shown, they

sometimes handle it badly, but they have firmly established the notion of cultural relativity in psychology. Some anthropologists try to overformalize what they are doing and they end up with the “systems of beliefs” which Morgenbesser rightly denigrates.

But, in spite of these forays, the heart of anthropology remains its concern with the ideas of people and (when we include archeology and museology) with the tangible results of these ideas, or the tools and materials by means of which they are given expression—what we sum up as “culture.”

Anthropology resembles history in its processes of gathering data. We are interested in description of events, and in the generalizations and explanations made by the actors in those events. An anthropologist can acknowledge no finer master than Marc Bloch, the great French historian, in dealing with this aspect of his problem. Bloch's *Apologie pour l'histoire* sets the anthropologist's problem as ably as it sets the historian's problem.

However, when it comes to theory formation, anthropology resembles science. The rudiments of anthropological theorizing can be set forth in a diagram (Fig. 1).

The ethnographic, or “historical” aspects of our study allow us to arrive at box N via box M. Box N, then, is to be understood on the same level as box B, the folk explanations current in our own society and, on a different level but performing a comparable function so far as the anthropologist is concerned, the theories of the social sciences of our own society. Insofar as any idea in either box B or box N explains adequately everything in boxes A, B, M, and N, it is adequate anthropological theory and can be put into box X. If it does not, then anthropological theory must be a new and original statement of sensible relationships among these four boxes. The most common anthropological errors are (i) explaining box M in terms of box B, omitting box N altogether—that is, confusion of B2 and X; (ii) explaining box N by box B without first determining that the contents of box N can in fact be derived from box M.

Anthropological theory can, eventually, make predictions on the basis of manipulating the theory in box X. What it can never do is to make these predictions in terms of specific events unless it discovers a way to “feed back” the material of boxes M and N which were removed in process of arriving at the propositions in box X. Even then, accurate prognostication is extremely difficult, for the same processes may take place in many superficial, cultural forms and with many different event structures.

The difficulty in the past—and it is

one that is reflected in Morgenbesser's article—is that critics of anthropology have tried to say that it must be either history or a science. Actually, of course, it has elements of both and is perhaps not fully either. Anthropology is, like history or science, an “attitude.” That attitude consists in developing a stereoscopic view of human activities and human ideas by means of concepts that have been discovered in the processes of translating key organizing ideas from one cultural idiom into the language of another culture, usually our own, thus making these ideas in some degree available to all. Our theories, like those of the physical scientist, or indeed like the interpretations of historians, may go by the board. But our attitude, like the scientific attitude of which it is a mode, is here to stay.

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Twenty years ago I was in substantial agreement with what Morgenbesser now says, and indeed published at that time a critique of the various brands of “functionalism” (1). I thought and said that much of the anthropological theory of that time was either trivial or trite, or noncogent. Indeed, it is perhaps still true that anthropologists have a singular knack of combining these three characteristics into a single utterance. But Morgenbesser criticizes the “functionalists” as though there were no other trends and hints of new ways of thinking in the anthropology of today. The persistence of the old I cannot deny. My purpose is to indicate that there is something new and that what is new is not trivial or trite and does not lack cogency.

I will first state what I mean by the nontrivial and in this connection I will be ambitious. It seems to me that a science has dignity and value insofar as it contributes to the next more abstract corpus of insights which lie behind it. The value of anthropology, in my estimation, lies in its contribution to general biological theory, to ethics, and to epistemology. These contributions are, however, very difficult to assess, and it is easier to judge whether, by unidentified contributions to the climate of more abstract thought, anthropology is actually making contributions at a more concrete level, to those other sciences which draw upon the same abstract background.

Can we assert that anthropological theory and experience would enable any neighboring specialist to ask more searching questions in his special field? Would a biologist modify his approach to the evolution of the Ammonites or to the bacterial theory of diseases because of the influence of anthropological think-

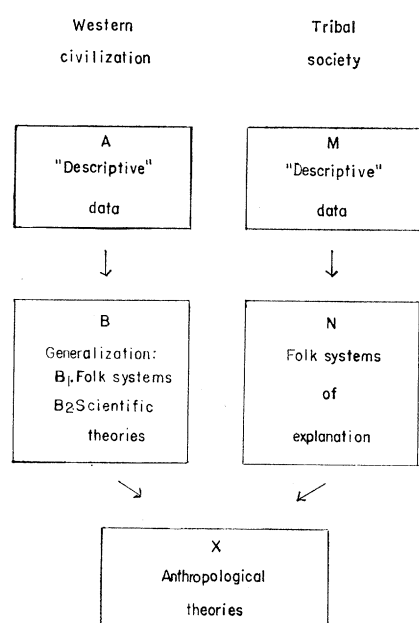


Fig. 1. Rudiments of anthropological theorizing.

ing? Has anthropology contributed anything to the understanding of the paradoxical relationship between crime and mental illness? Has it contributed anything to the philosophical argument between those who regard reality as existing only in the eye of the beholder and those who feel that reality has independent existence?

I am willing to see the discipline stand or fall according to the answers to questions such as these. If to one such question the answer be affirmative, then we as anthropologists can be proud.

Before answering such questions, let me try to clear away some confusions which are present in Morgenbesser's attack. It is meaningless to suggest that psychology might dominate anthropology or vice versa. The delimitation of the disciplines psychology, sociology, economics, history, and so on, is in general now seen to have been a tragic error insofar as it hindered cooperation between them. Of course, in any wide field of inquiry there must always be specialists and there will always be a vocabulary for defining the special areas in which particular scientists elect to work. But within the field of behavioral science it is becoming evident that these various specialties have deep need of each other, and cannot go far without each other's aid. Now, curiously, the anthropologist has, especially since the days of Boas in America and Haddon in England, been the Pooh-Bah of the social sciences. Because the anthropologist has traditionally worked singlehandedly with the vast intricacies of the culture which he must study, he has had to combine—often in an amateurish fashion—the functions of all the different specialties. Anthropology cannot conceivably dominate any one of these because the specialties have their own order of expertise in their respective fields; and conversely, no one of the special fields can dominate anthropology because this subject draws upon the insights of all the others.

Indeed, it is fair to say that anthropology has contributed a very great deal to the recognition of interdependence among the specialties and to the development of what is called interdisciplinary research—a theoretical contribution of some importance.

However, this peculiar relationship between anthropology and the more special fields within behavioral science makes it a little difficult to answer some of the questions which I posed above. An incident in the evolution of learning theory provides a good example and one with which I am personally familiar. The psychological experimenters had concentrated their attention upon a particular order of learning. They had investigated the conditions under which a given organism will alter its response to a given

signal, and anthropologists had made heroic efforts to apply these findings to the analysis of native culture (2). It became evident, however, that such a limited theory of learning was inadequate for the anthropological discussion of social values. The particular order of learning which the psychologists had been able to study was *not* the order of learning with which anthropology (and psychiatry) were concerned. We wanted to know, not the answer to the question "Under what circumstances does a Balinese learn to dance?", but the answer to the next more complex question, "If the Balinese learn dancing under such and such circumstances, what conclusions can we draw from this about the character of the Balinese and their ways of interacting? Will they reduplicate in other contexts the formal patterns in which they have previously learned something?"

These considerations led to the formulation of a theory of "deutero-learning" (3), or learning to learn, for which there was already some experimental evidence within psychology (4), though the vast importance of differentiating the orders of learning had not at that time been recognized. More recently the matter has passed back into the hands of the psychologists who have provided the experimental demonstrations (5) which the anthropologist was unequipped to do.

It is appropriate to stress the fact that this shift in learning theory is in fact away from attention to the "psychology" of the learning subject, and toward a study of the *patterns of interaction* between subject and experimenter. The contexts used in earlier experimentation on learning—instrumental avoidance, instrumental reward, rote, Pavlovian conditioning, and so forth—now became elements in a vocabulary for talking about themes and patterns of interchange between organisms. And this shift was made possible precisely because anthropology is a nonspecialist subject.

Now let us look at the possible contributions of this sort of thinking to the questions which I posed above. The evolution of the Ammonites falls into place as a stochastic process to be thought about with the same sort of intellectual tools that are appropriate in problems of learning theory, and the biologist in this and similar fields will obviously do well to wonder whether there may be hierarchic orders of evolutionary change corresponding to the orders of learning mentioned above. He will also do well to wonder about the changes which he sees in his fossils, viewing these as possibly one end of an interactive process. At each step of the orthogenic sequence, did the environment or the surrounding organisms modify their characteristics so as to neutralize the advantage which the Ammonites had got from that forward

step? And did the environment thereby become such as to favor a further step in the same direction?

The question about bacterial theory points up the fact that anthropology is one of the disciplines contributing to a conceptual field of biology which we might call eco-genetics. Here the phenomena of evolutionary change, of learning, and of ecology can all be seen as closely interrelated. To any worker in this field it is no surprise that antibiotics in the long run tend to strengthen the defenses of the parasite while weakening those of the host.

The problems of reality and subjectivity are much more nebulous, but still it is possible that modern anthropological thinking is relevant even in this region. We, as nonspecialists in the whole realm of human behavior, are very much concerned with the subdivisions of this realm. What is "giving"? What is "trade"? What is "play"? We do not dismiss a piece of behavior as "mere" ritual; we ask what sort of communication is ritual. This brings us face to face with the theories of nominalism, realism, subjectivism, and so forth, not as abstract creations of the philosopher, but as categories or frames which determine and describe or classify behavior. To us, as anthropologists, the question for which men burned during the Reformation, namely, whether the bread and the wine are metaphoric or sacramental, is a meaningful question about people, and our contribution to abstract problems of this sort will arise from our study of human natural history. We shall say, not that there is one correct epistemology defining or answering these questions, but that epistemology is a body of statements about the ways in which human beings perceive and understand, and that these ways are themselves determined by—and determine—the interactive processes of learning.

However, in none of these answers can we claim that the thinking which anthropology contributes is in any sense the parthenogenetic offspring of anthropological theory. Our theories are themselves products of complex interactive processes, resultants of our interaction with other scientists, with philosophers, and with the multicultural world in which we live. The development and trend of anthropological thinking would certainly not be what they are without such sources as cybernetics, the theory of games, the ecological theories of Allee and others, and the thinking of such men as Russell and Whitehead.

And last, we owe a peculiar debt to the theorists of the previous generation, whom I once criticized and who now irritate Morgenbesser. The errors of Benedict, Radcliffe-Brown, and Malinowski have outlived them, of course. But the

good was not interred with their bones. What is good and vital in a contribution to scientific theory is sometimes rapidly transmuted and becomes unrecognizable. The anthropological approaches of today are still the offspring of the old controversies about "structure," "function," and so on.

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1. G. Bateson, *Naven*, 1936, 2nd Edition, Stanford University, 1958.
2. See, for example, John W. M. Whiting, *Becoming a Kwoma: Teaching and Learning in a New Guinea Tribe* (Yale University Press, New Haven, 1941).
3. G. Bateson, "Social planning and the concept of 'deutero-learning,'" in *Conference on Science, Philosophy, and Religion, Second Symposium* (Harper, New York, 1942), pp. 81-97.
4. C. Hull, *Mathematico-Deductive Theory of Rote Learning* (Yale University Press, New Haven, 1940).
5. H. F. Harlow, *Psychol. Rev.* 56, 51 (1949).

The science of anthropology is in great need of thoughtful and serious consideration by trained specialists in the discipline of philosophy. The application of traditional scientific methodology to the social sphere is fraught with difficulties, of which the lack of suitable mathematical techniques is perhaps most prominent. It was no doubt at least partially with this in mind that the Social Science Research Council of Columbia University saw fit to award a grant to Sidney Morgenbesser. Part of the result of this grant was Morgenbesser's article in *Science*.

From such an article one might have expected fresh insights of a constructive nature into the condition of current anthropological theories. Instead, the reader is presented with a number of vague generalities ("many anthropologists claim . . .") which only Morgenbesser would claim to be anthropological theories, to which he applies the most naive of criticisms: "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." Some empirical research into the subtleties of social change might make Morgenbesser a more competent judge of this position. This is not a matter of being "most likely wrong"; like all scientific statements it is subject to testing and qualification on the basis of empirical data.

Morgenbesser's charge of "banal and possibly beyond disproof" to the functionalist statement that "societal institutions are interdependent in the sense

that changes in some institutions will produce other changes in other institutions," illustrates his essentially biased position. Most accepted scientific theories are based on "banal" foundations. In fact, one indication of the soundness of a scientific theory is the simplicity of its basis.

Again, Morgenbesser says: "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." But functionalists have been asserting for years, never without opposition, that these "tautologies" are, or should be, obvious. And precisely for this reason they are attempting to establish as valid the laws under which these repeated ways of behavior operate. This procedure has been the standard stock in trade of physical scientists for centuries; why does it now come into question only with regard to the field of anthropology?

He continues: "It is not too misleading to recast these observations into teleological language and, *with the functionalists* [italics supplied], 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." To deny that functional theory is particularly exposed to the dangers of teleology would be folly on my part. But to imply that functionalists are ignorant of such difficulties or that teleology is inherent in functional analysis shows an unbelievable lack of sophistication on Morgenbesser's part. (Interestingly enough, one of his criticisms of learning theory, under the heading "Limitations of Learning Theory," seems to concern its inability to make teleological explanations!)

Anthropologists in general and functionalists in particular were no doubt amused to read Morgenbesser's announcement that "anthropology is threatened with extinction as a logically independent discipline." I suspect that anthropologists are less concerned with creating a "logically independent discipline" than they are with doing their utmost to reveal the wonders of social phenomena. If this involves borrowing theories or adapting to the developments of other disciplines, they will do so, without apology. If this involves following many leads and embracing a multiplicity of theoretical schemes for another hundred years, they will do that too. "The heteronomy of anthropology," which Morgenbesser deprecates, will be its greatest source of strength until sufficient knowledge is obtained to standardize its concepts and methodology. Compare the

situation today with that of the not too distant past, when anthropology labored under the burdens of social evolution theory (a burden now carried by religious historians and philosophers).

To top everything, the tired old proposal that anthropology confine itself to historical statements is presented in a pretentious tone calculated to lend it an aspect of novelty which it lost before Malinowski ("I spit on history"). For an earlier discussion of what Morgenbesser's conclusion merely rehashes, see C. W. M. Hart's charming article, "Cultural anthropology and sociology" [in *Modern Sociological Theory*, Becker and Boskoff, Eds. (Dryden Press, New York, 1957)].

Since *Science* is catering to an audience capable of digesting Ingraham and Green's "Role of magnesium in enzyme-catalyzed syntheses involving adenosine triphosphate," or White's "'Root pressure' in gymnosperms," it has no excuse for publishing an article with a trivial message merely because it concerns social science. It is impossible for a serious discussion of anthropological theory to totally ignore the work of Levi-Strauss (as well as that of many others) as Morgenbesser did, except for the abbreviated versions he found in R. Firth's "Function" in *Current Anthropology* [W. L. Thomas, Jr., Ed. (University of Chicago Press, Chicago, 1956)]. This was an insult to conscientious anthropological theorists. The first 55 pages of *The Structure of Society*, by Marion J. Levy, Jr. (Princeton University Press, Princeton, N.J., 1952) assays most of what Morgenbesser has to say, in detail and thoroughness that surpasses the chemical reports that fill the pages of *Science*. Any introductory philosophy text duplicates whatever Levy ignored.

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My paper was not devoted to the impossible task of reviewing all aspects of anthropology, but to that of assessing some theories currently employed by anthropologists. Bohannan, the most temperate of my critics, has therefore rediscovered my aim when he notes that my treatment was selective, and did not fully consider some important anthropological problems and attitudes. Although Bohannan's sharp distinction between concepts and beliefs seems to me to be debatable, much that he says about the goals of his discipline is instructive. But he does not discuss any theories that can be employed to reach the goals he specifies. Hence Bohannan's paper may be taken as an addition to, rather than as a criticism of, my own.

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Similar remarks are apposite to Bateson's provocative and revealing paper. He, too, complains that I have failed to fully quarry the anthropological mine, and sets out to unearth some riches that I neglected. What he discovers are some anthropological—or, shall we say, Batesonian—exports to biology, psychology and epistemology. Such exports fail to impress me, but that is comparatively minor. The difficulty with Bateson's letter is that it can be taken by an unfriendly critic as evidence for the thesis that the status of anthropology does not differ from that of sailing or mining. For it is well known that problems of, and discoveries by, miners and sailors have resulted in advances of physics. Whether Bateson would take this as denigration of anthropology or elevation of mining is unknown to me. I do know that his letter was motivated by the conviction that anthropologists have discovered many interesting and important truths about men and their cultural activities. I share Bateson's convictions. Nevertheless, I doubt whether anthropological findings have thus far led to comprehensive theories. And my doubts on that score were not allayed by Bateson's otherwise instructive paper.

Hansen impresses me as a zealous knight who is prepared to break many a lance for the discipline to which he has sworn fealty. Unfortunately, he either has never taken, or has forgotten, his vows of chivalry, and thus, alone among my critics, Hansen finds nothing in my paper that he can bless. Perhaps this judgment is too strong. Except for the unfounded suggestion that I criticize learning theory for failing to supply us with teleological explanations, he neglects to comment upon my views about recent anthropological employment of learning theory. His silence on the score may indicate consent. In point of fact, his reticence is puzzling, but not as much as some of his explicit inferences.

Thus, I do not know how Hansen concluded that I wanted anthropologists to restrict themselves to historical statements. It is well known that explanations in history are given without the aid of any special class of historical theories, and my hint was that similar conditions may some day obtain in anthropology. Incidentally, Hansen more or less agrees with this point, when he notes that anthropologists are "doing their utmost to reveal the wonders of social phenomena," and that they will attempt to do so even if it means borrowing theories from other disciplines. But this means the autonomy of anthropology is not a necessary condition for its success. That, among other things, was the point of the analogy between anthropology and history, an analogy that aroused the ire of my critic and led him to forget parts of my paper. Had I wanted to give an air of preten-



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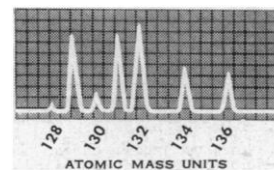
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tious novelty to my remarks, I would never have quoted Maitland.

Hansen insists that functionalists are well aware of my criticisms, and therefore seems to be disputing not the truth, but the novelty of my remarks. I will bypass the problem of originality, and simply take Hansen's letter as a datum against his own view. For he fails to understand, much less agree with, my critical observations. Thus, I was not concerned with the danger of teleology, but with the dispensability of certain locutions, and with the conditions that must be satisfied by any explanation, teleological or otherwise. It was the failure of functionalists to produce bonafide explanations, not their employment of a terminology, that perturbed me.

I was equally disturbed about the empirical status of some statements spon-

sored by anthropologists. I reported my findings, and to controvert them, Hansen administers a methodical spanking. To answer my charge that some functionalistic propositions are banal, he observes that most scientific theories are based on banal foundations. "In fact," he continues, "one indication of the soundness of a scientific theory is the simplicity of its basis." I surmise that even Hansen, in his more charitable moments, will find a difference between banality and simplicity, and will discover little that is banal about the foundations of, let us say, quantum electrodynamics. At that time, I am sure he will also find it difficult to specify a sense for the phrase "foundation of a theory" and to find a method for measuring simplicity.

To meet my claim that some statements are false, and others tautologous,

Hansen points to the self-corrective nature of scientific inquiry. False statements can be replaced by more warranted ones, and tautologies, when incorporated within theories, some of whose premises are true, but not logically true, can lead to fruitful empirical results. But the possibility of self-correction is no proof that the correction called for is not in order. Or perhaps Hansen merely set out to show that the presence of faulty theories is no proof of the demise of a discipline. But I can assure him that I never set out to bury anthropology. I only questioned some currently employed theories, and for that Hansen and others seem ready to banish me as a nonscientific infidel.

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Box (give number)
Science
1515 Massachusetts Ave., NW
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POSITIONS WANTED

Bacteriologist; Ph.D., desires academic position commencing June or September. Any location. Box 20, SCIENCE. 2/6

Biochemist, Ph.D., 30; 5 years of medical and graduate teaching experience. Interests: enzymology and protein metabolism. Fifteen full-length publications. Relocation desired. Box 15, SCIENCE. 2/6

Ph.D., biochemistry; 5 years, analytical chemist, process control section, large industrial company, East. Medical Bureau, Burneice Larson, Director, 900 North Michigan, Chicago. X

Ph.D. desires supervisory position, clinical laboratory, with part-time research or teaching. Experienced in general clinical laboratory work; strong experience in clinical chemistry and bacteriology. Past 5 years in administrative capacity. Prefer California. Box 26, SCIENCE. X

POSITIONS OPEN

Biologist to head scientific staff of growing, established biological supply house. Field collecting experience, plus knowledge of preserving and other biological techniques necessary. Ph.D. degree desired. Administrative ability and teaching experience required. Paid vacations, holidays, benefits, salary opportunities. Box 8, SCIENCE. 1/23, 30; 2/6

Chemist. Organic preferred. Doctorate with teaching experience. Salary and rank dependent on qualifications. Submit personnel data to President Ralph Prator, San Fernando Valley State College, 18111 Nordhoff, Northridge, California. 2/13

POSITIONS OPEN

University of Kansas, Graduate Teaching and Research Assistantships in Anatomy. Applications are invited from well-qualified students who wish to work toward the M.A. or Ph.D. degrees for positions as assistants in teaching and research. Stipends, depending on experience and training, will average \$200 per month for either a 9- or 12-months' appointment. Studies are offered in the following fields: morphology (human and comparative); neurology (experimental and comparative); anatomical and physiological basis of behavior; reproductive physiology; experimental and chemical embryology; cytochemistry and histochemistry. For further information write to Paul G. Roofe, Chairman, Department of Anatomy, University of Kansas, Lawrence, Kansas. 2/13, 20

Botanist, Ph.D. Liberal arts college. \$5400-\$6500. Annual raises. Union College, Barbourville, Kentucky. 2/6

Literature Searchers and Writers with experience in preparing commercial literature in organic chemistry field. Permanent New York City positions with technical publisher. Non-contributory profit sharing pension plan. Box 22, SCIENCE. 2/6, 13

Microbiologists. M.D. or Ph.D.; minimum of 3 years research experience. Significant publications essential. Openings available in the following fields: clinical virology, epidemiology of infectious disease, biochemistry, cytology and cellular metabolism and biophysics. Research is conducted on an individual and/or group basis. Positions offer cultural and scientific opportunity for continued education and development. Forward replies to M. H. Edelman, Personnel Department, Chas. Pfizer & Co., Inc., Terre Haute, Indiana. 2/13, 20

NEW WORLD-WIDE SUMMER PLACEMENT DIRECTORY, over 12,000 fascinating opportunities in all states, many foreign countries. Complete verified information including salaries. Send \$3.00 now. CRUSADE, Sci., Box 99, Station G, Brooklyn 22, N. Y. eow

Postdoctoral Traineeship in Neuropharmacology Available for the Ph.D. in chemistry, biochemistry or pharmacology or the M.D. who would like to broaden his training and enter research in neuropharmacology. Stipends from \$4500 per year and up plus dependent allowance. Postdoctoral fellowships in other areas, such as biochemical pharmacology, antibiotics, energy and drug enzymology, are also available. Write Chairman, Department of Pharmacology, Washington University School of Medicine, St. Louis 10, Missouri. 1/9, 16, 23, 30, 2/6

Pharmaceutical Chemist. Opportunity for creative, experienced man to head section in research division of a rapidly expanding, well-known, upstate New York ethical pharmaceutical company. Emphasis on analytical methods and new product development desired. Send full information to Box 23, SCIENCE. 2/13

POSITIONS OPEN

MEDICAL WRITER

Opportunity available for young physician in Professional Service Department of Medical Division. Should have ability and interest in medical writing. Clinical or laboratory experience desirable. Please send complete résumé to:

Technical Employment Coordinator
THE UPJOHN COMPANY
Kalamazoo, Michigan

Postdoctoral Traineeships in Lipid Chemistry are available for 1959 at the University of Tennessee Medical Units, Memphis. The program offers persons holding Ph.D. and M.D. degrees an opportunity to learn modern analytical techniques in lipid chemistry, including the use of radioactive isotopes. Applications may be sent to Dr. D. B. Zilversmit, Department of Physiology. 2/6, 13

SCIENCE TEACHERS, LIBRARIANS, ADMINISTRATORS urgently needed for positions in many states and foreign lands. Monthly non-fee placement journal since 1952 gives complete job data, salaries. Members' qualifications and vacancies listed free. 1 issue, \$1.00. Yearly (12 issues) membership, \$5.00. CRUSADE, SCI., Box 99, Station G, Brooklyn 22, N. Y. ew

Senior Analytical Chemist for development of new methods of analysis of pharmaceutical products. M.S. or Ph.D. in organic analytical chemistry or B.S. in pharmacy or chemistry, plus 5 years of pertinent experience. Experience in pharmaceutical analysis highly desirable. Send résumé and salary requirement to Dr. James A. Garrison, Professional Employment Manager, Mead Johnson & Company, Evansville, Indiana. X

(a) Two Young Internists for openings in domestic or international divisions, one of leading companies; would be engaged as assistant or associate medical directors; duties include evaluation of new products; preferably physicians who have completed residency or who have had 2 years' practice; scientific training or clinical experience in clinical research helpful; East. (b) **Director of Organic Research,** Ph.D.; new department, pharmaceutical company; medical center, South. (c) **Child Clinical Psychologist;** new position; Ph.D. required; community guidance center, large city, Southwest; \$7500. S2-1 Medical Bureau, Burneice Larson, Director, 900 North Michigan, Chicago. X