SCIENTIFIC METHOD AND THE CULTURE OF ANTHROPOLOGY[1]

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It is fairly well known, I think to all of us, that in anthropology we have had quite an impressive number of colorful debates concerning the interpretations of particular ethnographic data. For example, in 1951 Oscar Lewis published his materials on the Mexican village of Tepoztlan, which sharply contradicted many parts of an earlier description of that same community by Robert Redfield. To Lewis' critique Redfield replied with the admission that ". . . Lewis established the objective truth of certain of the unpleasant features of the Tepoztecan life . . . it is true that the two books describe what might almost seem to be two different peoples occupying the same town" (Redfield 1960:134).

More recently there appeared in the American Anthropologist a paper by Victor Goldkind which challenged Redfield's interpretations of Chan Kom, a village in the Yucatan. Goldkind states that "Instead of the classless homogeniety emphasized by Redfield, we find a heterogeneity significant for the lives of the people of the community . . . ." (Goldkind 1965:882). (Controversies of this sort are not restricted to just the ethnographic data, for it appears that similar problems of basic interpretation can be found in the domains of archaeology and physical anthropology, as was clearly demonstrated in the "Origin of Man" conference in Chicago last spring.)

However celebrated the Redfield-Lewis debate over Tepoztlan and related controversies about Chan Kom and about the folk-urban continuum, the problem of Pueblo "Apollonianism" or "logico-aesthetic integration" is perhaps even more renowned. Nowadays few texts in cultural anthropology appear which do not, at least in passing, review the pros and cons of whether the Zuni and the Hopi are harsh in initiating their kids; whether or not they have a strong aversion to alcohol; whether, or how much, they carried out warfare against their neighbors; and other questions related to their "cultural configuration" or "ethos."

The opposing points of view about the Pueblos were thoroughly reviewed by John Bennett in 1946, and he concluded that the differences in viewpoint may be explained by what "I have already suggested may be a genuine difference in value orientation and outlook in the feeling about, the reaction toward, Pueblo society and culture in the light of the values in American culture brought to the scientific situation by the anthropologist" (Bennett 1946:369).

Bennett sums up by saying that "scientific anthropology is . . . from this level of observation . . . nonobjective and 'culturally determined'" (Bennett 1946:370).

His analysis of the Pueblo controversy presented a rather serious challenge to the scientific claims of anthropology. It is therefore interesting that no one has ever seriously taken up this challenge by going to the Zuni and Hopi with a carefully designed research plan to retest the rival hypotheses. And, so far as I know, we have not been presented with a definitive restudy resolving the Tepoztlan debates.

A proposition I would like to offer here is that: One sign of the primitive state of much of our anthropological science is that our great debates usually end up in the realm of armchair and conference theorizing.

Our inability to resolve our anthropological Great Debates is a manifestation of a still larger problem. It would appear that in general, anthropological research has not produced definitive disproofs of previous theoretical positions.

There was a time a few decades ago when it was generally felt by anthropologists that removal of the entire theoretical structure of 19th century evolutionism had been a permanent and effective scientific revolution, comparable with the final, total demise of the phlogiston theory in chemistry. Yet today there exists a quite respectable school of thought according to which the evolutionists--Morgan, Tylor and the others--are largely vindicated. Many of their ideas have been slightly reinterpreted and their general theoretical constructs restored to respectability.

Major changes in theoretical orientations do occur from time to time in anthropology, but these appear to arise as changes in fashions or interests--they do not result from clear disproofs of prevailing ideas. (True, anthropologists have generally disproved the postulated existence of peoples with mouths in the middle of their stomachs, and have made a good case against the proposition that there is a race of giants at the Antipodes, but these signal advances in ethnographic knowledge do not constitute negations of propositions or hypotheses advanced by anthropologists.)

When confronted with the fact that our "great debates" seem never to be resolved, many anthropologists have taken what I would regard as a defeatist position. They have accepted the conclusion that the problems in these debates are not scientifically researchable.

To go back to Tepoztlan for a moment, Redfield himself seems to have said that we are simply stuck with two versions of life in Tepoztlan and we have to put the two together--sort of average them up--there is no way to resolve that impasse scientifically. Some people take the position that these are not scientific questions because Benedict in writing about the Pueblos was <u>not trying to "do science."</u> But surely all of these anthropologists--Benedict, Goldfrank, Redfield, Lewis--were trying to present systematic, reliable information. Otherwise why would they bother about doing the field work? None of them to my knowledge was intellectually committed to falsehood and error, and all couched their descriptions in the language of empirical observation.

On the other hand there are anthropologists who would insist that all these problems <u>are</u> scientifically researchable, and the solution lies in quantification. All these problems can be solved by running over to the computer center, learning Fortran or some other system of programming, and converting all those masses of raw data into numbers for analysis in terms of mathematical models.

Yet another school of methodology would insist that our salvation lies in use of a meticulous structural analysis, modelled after linguistics.

I would argue that questions such as those about the Pueblos and Tepoztlan (and more recent problems of the same sort) are in principle quite researchable and can be made scientific; and that the research strategy needed to cope with them is available to us. What is needed is systematic application of some generally accepted and elementary principles of scientific method. These rules of scientific method are not the special property of any one discipline, but rather are procedures expected in any attempts to establish new knowledge or to test hypotheses. Without going into detail, the following rules of research are essential to systematic accumulation of knowledge:

I. PROPOSITIONS OR AIMS (THE HYPOTHESES) OR PARTICULAR PIECES OF RESEARCH MUST BE CLEARLY STATED, IN A LOGICAL FORM THAT IDENTIFIES WHAT THE RESEARCH IS REALLY ABOUT. Many of our controversies in anthropology involve debate in which nobody is quite sure what is in fact being claimed--the disputants are often not even in the same field of discourse.

II. The essential elements, or terms, of the specific problem or hypothesis must be defined. (At this point many of us stray onto a slightly **distracting and unnecessary pathway** by insisting **that we must** first adopt clear, unambiguous, and universally acceptable anthropological terminology. While a common terminology is a highly commendable goal, we don't need to wait for it.) The main requirement for our definitions of terms is that we clearly state the operations--the criteria of observation--by means of which research involving our key terms will be carried out. I believe that, in principle, operational definitions of "cultural homogeneity" can be devised by means of which Chan Kom can be restudied. The same goes for "degree of integratedness," or even "degree of Apollonianness" for study of the Pueblos. I'm not saying that the research itself will be easy. I'm only claiming that those terms can be operationally defined.

This problem of operational definition of terms is, I feel, the key to methodological problem in anthropology. It is to me quite amazing that anthropologists--who claim to know the most about the arbitrariness of words, of the relativity of concepts, of the general semantic problems in definitions--go on using terms like "patrilineal," "homogeneous," "integrated," "solidarity," "acculturation," "identification" and the like without stating the rules of research observation by means of which these terms are given informational content.

III. The actual procedures of observation employed in the research must be so described that another anthropologist (or other scientist) reading the research plan or the research report can evaluate the adequacy of the observations and interpretation and can clearly understand the basic steps that would be necessary to replicate the research.

IV. Finally, all of these basic methodological elements must be so constituted that it is possible for other researchers to see what data would constitute a negation of the results described by the researcher. That is, the work must be falsifiable.

If these elements of scientific method are well-known in practically all realms of science, why is it that we do not find them systematically employed in anthropological work? The answer, I feel, is to be found in some deeply entrenched cultural traditions of anthropology.

1. Perhaps the single most significant cultural element (or trait) influencing the present state of anthropological work is the tradition that strongly militates against studying the same community that another anthropologist has studied. It's not really considered polite to go to that other man's community; it is less polite to study the same things that he studied; and one <u>never</u> tries for a real replication of another's research. I say never, because as far as I know there is not a single instance of a true replication study in all of cultural anthropology. There have been re-studies, yes (for example, Tepoztlan; repeated study of the Hopi and Zuni; and return trips to Truk, Samoa, the Trobriands and many other places). But these were not replications in the scientific sense, in that the same methods of research were not employed to test exactly the same propositions advanced by the original investigator.

2. Another significant culture trait complex is the set of assumptions about the "integration of culture" and (related to it) the importance of so-called "holistic study." At its best, "holistic study" refers simply to the anthropologist's willingness to consider a wide array of variables or causal factors in seeking explanation of particular phenomena. In examining cultural change, for example, he does not restrict himself only to economic factors, only to psychological factors, or to some narrowly defined technical or "practical" elements.

In actual anthropological practice there is another, less defensible aspect of "holism" operating, however. This culture trait is expressed in the fact that the anthropological field worker is almost always ready to collect unlimited amounts of data in all directions--seldom in practice limiting himself to a carefully planned set of observations directed to the test of a specific hypothesis. So the field worker goes to a particular community or set of communities, with perhaps a well-stated hypothesis, but with no prior commitment concerning the definitions of the terms in his hypothesis. Hence the range of data he might collect is practically limitless. When he comes back from the field he often reshapes his hypothesis to fit the data that he collected. And part of this holistic tradition means that no matter what specific hypothesis a field worker went out to test, we will expect him to be able to give us lots of observations on child rearing, types of house construction, uses of kinship terminology, and everything else in the local culture. We will literally force the field worker to look at everything in that society; thus, specialized study of one carefully delineated aspect of culture is seriously hampered.

This tradition of holistic study, if applied to human physiology, for example, would result in no one doing really effective study of the kidneys since the individual researcher would be pressured into doing quite a bit of work on heart, liver, lungs, and brain during the research process.

(In passing it should be mentioned that the assumptions on which anthropologists base their "holistic studies" and their statements about cultural integration have nowhere been seriously tested, and are generally accepted rather on the basis of anecdotes and some sort of rule of unanimous consent.)

3. A third important culture trait that seriously disturbs our possibilities of effective research methodology is what is often called the "field work mystique." Anthropologists have been generally unwilling to describe publicly just how particular kinds of data are gotten in the field. In fact, this vital information is often kept from our own graduate students. The mysteriousness of some aspects of field work procedure is even converted into an important virtue, labelled "insight."

4. There is the frequently mentioned cultural tradition according to which ethnographic data are mainly qualitative--they cannot be quantified (hence operational definitions and statistical analysis are supposedly not relevant). Yet many of the descriptions of these supposedly qualitative data include such statements as "the average person in the village" or "they often travel many miles a day" or "most of the people believe that" or "they often tell their children"--all of which statements imply numerical analysis, even if we are quite sure that the field worker made no such counts.

5. Many of the culture traits of anthropology appear to be closely linked to a fundamental postulate according to which the normal condition within a community is thought to be a general homogeneity or uniformity of cultural beliefs and practices, analogous to the shared uniformities of language patterning. (Cf. Kluckhohn 1941:109-130.)

Anthony F. C. Wallace has forcefully stated the case for cultural pluralism as opposed to this assumed uniformity of patterning, but little has been done to test the idea empirically.

6. Probably because of the pervasive influence of the postulate of cultural uniformity or homogeniety, it is characteristic of anthropologists that much description and debate centers on a particular type or trait rather than on a continuum of variation. Thus anthropologists most frequently discuss "patrilineal societies," "shifting cultivators," or "matrifocal families" rather than considering societies in terms of "degree of patrilineality," "shiftingness of cultivation," or "degree of matrifocality."

While our anthropological traditions appear to have hampered acceptance of certain elements of scientific method, it should be noted that our culture is changing rapidly. Acculturation is proceeding at an increasing pace, in part because of contacts with powerful neighbors in the other sciences. For many of the points I have raised, examples of new anthropological research can be cited as already measuring up to rigorous standards of methodology. From the evidence of changes that have already occurred, I would venture to predict some further important developments:

1. We anthropologists increasingly will go to our field laboratories (even to previously unstudied societies) to carefully test specific propositions, with a willingness to come home ignorant of the aspects of cultural behavior which were not in the focus of research.

2. We will go more often to each other's research communities without embarrassment, frequently with the intent to test or retest the other fellow's hypotheses.

3. We will, after careful census work in our research communities, follow some sort of careful sampling procedure for establishing something of the range of variation found in particular behavioral patterns. In so doing, we will be consciously testing the usefulness of assumptions concerning cultural uniformities.

4. We will describe, in much more detail than heretofore, the procedures by means of which particular data are obtained, seeking for more and more of those methods that can be directly replicated by the next researcher working on the same problem.

5. We will not, of course, give up usual practices of participant observation and interviewing, for with appropriate refinement, these are essential tools of research.

As I mentioned, there are already good examples of anthropological research that carefully specify the research hypothesis, give operational definitions of terms, describe the research operations clearly, count the numbers of cases for and against (if such counting seems necessary), and give a statistical analysis of the results. Since a principal objective of more systematic methodology is to make the research procedures publicly available to criticism, the weak spots in such research are the more glaringly evident. We have already found that it is easier to criticize the specific conclusions of this kind of research report than it has been to pick apart traditional anthropological reporting, in which methodological weaknesses could often be covered up by a colorful literary style. As we experience the joys of exposing the frailties of this newer kind of research, it is well to keep in mind Sir Francis Bacon's dictum that: "Truth emerges more readily from error than from confusion." (Quoted by T. S. Kuhn 1962:18.)

ENDNOTE

[1]Paper presented at the 64th Annual Meeting of the American Anthropological Association in Denver, November 1965. This paper will also appear in the bulletin of the ANTROPOFORENINGEN of the University of Stockholm (Antropolog-Nytt 1966).

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