Is Honesty the Best Policy? Anthropology, Anthropological Education, and Methodology

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In recent years, an increasing number of anthropologists have begun to seriously consider their subject as a science. Especially in the area of methodology, strenuous efforts have been expended to make anthropology more rigorously scientific. This orientation is explicit in Pelto and Pelto (1978), a leading methodology textbook (see also Brim and Spain 1974; Johnson 1978). Although they argue that good anthropological research requires a "judicious mixing" of qualitative and quantitative approaches, Pelto and Pelto stress the need for an increased role for operationalism, falsifiability, and interpersonal replicability in anthropological methodology. Pelto and Pelto vigorously criticize the methodological laxness found in much anthropological research. They cite examples of methodological sloppiness in the work of eminent anthropologists like Holmberg, Kluckhohn, and Redfield.

Given this trend in modern anthropology, a recent book by Broad and Wade(1982), Betrayers of the Truth, should be of particular interest to the profession. Broad and Wade argue that fraud and deceit (conscious and unconscious) are continuing and significant phenomena in science. They provide examples of scientific fraud and deceit in the work of obscure careerists, contemporary heores (e.g., nobel laureates), and intellectual immortals (e.g., Ptolemy, Mendel, and Newton). They suggest that, contrary to the position presented by the typical spokesmen for the scientific community, these examples do not represent freaks or aberrations but are

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a by-product of the social organization of science.

The spokesmen for science suggest that the ideology of science (e.g., the cognitive structure of science, the verifiability of scientific claims, the peer review process) acts as a nearly impregnable defense against deceit and fraud. Broad and Wade suggest that, in fact, this ideology is often ignored and violated. For example, Broad and Wade argue that exact replicability—a keystone of the ideology of science—"...is not a regular part of the scientific process. The reason is simple: there is no credit to be gained from replicating someone else's experiment"(1982:215).

Broad and Wade conclude that:

> Science is not an abstract body of knowledge, but man's understanding of nature. It is not an idealized interrogation of nature by dedicated servants of the truth, but a human process governed by the ordinary human passions of ambition, pride, and greed, as well as by all the well-hymned virtues attributed to men of science. But the step from greed to fraud is as small in science as in other walks of life. Usually the misrepresentation amounts to no more than a sweetening or prettification of the data; less often, to outright fraud〔1982:223-224〕.

It is tempting to see Broad and Wade's indictments as endorsing or supporting the critics of scientific anthropology. However, Broad and Wade are not themselves anti-science. They consider fraud and deceit in science as alarming and persistent, but still a minor presence. They believe that over time scientific progress does occur. Nevertheless, they feel that it is necessary to recognize that a number of central characteristics of the institutional framework of science systematically tempt some people (on all levels) away from the scientific pursuit of truth. For example, they argue that the system of publication and the grant-giving and promotion processes "that encourage and reward careerism also create the incentive for fraud"(1982:220).

If anything, Broad and Wade's book should make all anthropologists,

no matter what their theoretical and methodological orientations, uneasy. For while anthropology operates within the same potentially corrupting institutional framework as the "hard sciences," the nature of most anthropological research is such that the potential for fraud and deceit is greater than in other fields.

In this paper, I shall explore some potential sources of fraud and deceit in anthropological research, and I shall suggest reforms in anthropological education that might help minimize threats to intellectual integrity.

Broad and Wade point out that the supposed safeguards against fraud and deceit in science (e.g., replication of others' findings) are not normally employed even in the "hard sciences." However, they also indicate that when suspicions have been raised on other grounds, tests for replicability have been used to successfully unmask fraud and deceit. Furthermore, although Broad and Wade show that quantitative data have been tampered with to make them more neatly fit theory, when suspicions have been raised on other grounds, statistical analyses have been used to demonstrate damning inconsistencies and to show how the data were far more perfect than could be expected mathematically.

Unfortunately, many anthropologists still do not even reach minimum standards of methodological rigor in any part of their research, and we thus often lack potentially powerful means of critically scrutinizing research results, even when we are suspicious. For example, Glascock and Kimble reported that less than three percent of the over 400 anthropological publications they recently examined presented their research designs and methodology in a way that would allow replication. Even though Thomas found that there had been an increase in quantitative material in the anthropological literature, in 1972 only 31 percent of the cultural anthropology articles published in American Anthropology had any quantified materials (Pelto and Pelto 1978:35; 125).

More basically, it is because most anthropological research is ultimately based on field work that both our opportunities for fraud are great, and

the chances of our being caught are low. Broad and Wade point out that one important way in which suspicions of fraud in the sciences have been raised initially is when colleagues observed unusual behavior and practices in the laboratory(1982:14). The anthropologist, unlike many other scientists, does his research where he cannot normally or consistently be observed by his peers. Furthermore, the ethics of anthropological research, which require that we protect the privacy of our informants (e.g., through the use of pseudonyms) can also protect the unscrupulous researcher. The extended and immense efforts that are usually required to build up rapport with anthropological informants, also, make it unlikely that one anthropologist could undertake to reinterview a peer's informants merely in order to check up on him.

In fact, the close relationships and sense of identification that the anthropological field worker often develops with informants have other potentially dangerous effects on the anthropologist's attitudes towards his data. The way anthropologists often talk about the communities they have studied—"my village," "my tribe"—is symptomatic of the possessiveness anthropologists often feel about their research data. It is not surprising that the results of anthropological research are seldom presented in such a way that other anthropologists could successfully interpret the data for themselves. How could anyone else possibly understand my village better than I do?

Before pursuing our subject any further, it is imperative to emphasize that I am not suggesting that anthropology is full of scoundrels and rogues, cheats and liars. The problem is far more complex than that. Broad and Wade suggest that in science *self*-deception is a pervasive problem. However, in the opinion of Broad and Wade, self-deception and outright fraud, though they differ in motivation, should probably be viewed.

> ...as two extremes.of a spectrum, the center of which is occupied by a range of actions in which the experimenter's motives are ambiguous,

even to himself. Many measurements that scientists take in the laboratory admit judgment factors to enter in. An experimenter may delay a little in pressing a stopwatch, perhaps to compensate for some extraneous factor. He can tell himself he is rejecting for technical reasons a result that gives the "wrong" answer; after a number of such rejections, the proportion of "right" answers in the acceptable experiments may acquire a statistical significance that previously was lacking. Naturally it is only the "acceptable" experiments that get published. In effect, the experimenter has selected his data to prove his point, in a way that is in part a deliberate manipulation but which also falls short of conscious fraud (1982:108).

The scope for these unconscious or semi-conscious forms of deceit is particularly great in anthropology. For example, often notes can only be written up hours or days after the observed events. Yet, D'Andrade found that there are at least two kinds of biases in long-term recall (i.e., 10-15 minutes, or more, after an event), even in a trained observer. There is a bias toward orderliness: "behavior that was disorderly is selectively remembered as orderly." There is, also, a bias toward a *particular kind of order*: "'contradictory' behavior is selectively remembered as conforming to the observer's prior expectations" (Johnson 1978:4-5). Murphy (1971:39-40) has, in fact, suggested that the culture shock the anthropologist often initially experiences upon entering the field results from the fact that he has not yet acquired that society's own biases. Without the "native's" own distorting models, the anthropologist is initially seeing the truly contradictory and disorderly nature of much of social life.

Ironically, it may be the long-term nature of field work and the holistic character of our data collection—we accumulate so many observations, interviews, anecdotes about so many aspects of life—that give vast scope for selective biases to operate. Murphy has observed that:

...most anthropologists find that their most productive analyses require

an absence from the field of at least two years. It is not so much a matter of analyzing all that data as forgetting all those people and happenings (1971:40).

It is not necessary to continue cataloguing the potential opportunities for fraud and deceit in anthropological research. Any experienced field worker can, once he has begun to seriously consider the problem, recall from his own experiences countless temptations. The important issue is: How can we train our students and run our professional activities in such a way that these potential problems can be minimized?

This issue is particularly crucial because it seems to have been ignored in practice. Despite anthropology's unique problems, the curricula and teaching approaches of anthropology departments typically differ little from those of other departments in a university. Althogh field work and analysis of field data are at the core of anthropology, those activities have had an astonishingly small role in typical anthropological education. As a result, I would suggest, anthropological education often does little to decrease the problems discussed above, and, if anything, it probably increases them. Johnson points out that:

Most scientific disciplines regard training in professional standards of measurement to be an integral part of the educational experience. Students of chemistry, for example, required from their first introductory course to measure weights and volumes following precisely formulated procedures: specimens must be properly dried, balances must be dust-free, and so on (1978:15).

Training in the anthropological equivalents of those procedures—primary data gathering through field work—is, by contrast, minimal. There is not as yet a consensus on proper anthropological research procedures (Johnson 1978:15). But standardization and, more importantly, increased sophistication are unlikely to develop when students do not learn through extensive first-hand experience to think seriously about methodological and technical

issues as much as they do about "the raw and the cooked" or "the core and the periphery."

Having little experience with actual research, students often seem unable and/or unwilling to critically examine anthropological studies on methodological grounds. Instead, they often become prematurely devoted to abstract theories that happen to appeal to the current ideology of their peers. The "true believer" nature of many anthropology students is probably encouraged by an education that rarely forces them to go through the process of relating "raw data" to theory. Instead, they concentrate on highly abstract (and quasi-theological) theoretical discussions. In sum, by keeping students naive about crucial research issues, we systematically train anthropology students to be ready victims of self-deceit and/or deceit by others.

When our students are finally asked to do some extended field work, either to complete their Masters or their Ph.D., they are in a painfully vulnerable position. Inadequately trained and inexperienced, they are asked to perform their first serious research in a situation in which the stakes and anxiety level are high. In some ways, this is the equivalent of giving aspiring musicians years of training in musical theory but no practice in playing their instruments, and then making their failure or success depend upon performance in a concert.

Agar has suggested that, although he has not done a study, it is likely that:

> ...if you plotted a graph of the proportion of anthropology departments offering a "field methods" course over time, there would in fact be an upward turn in the curve, although quite recently (1980:3).

I would argue that giving a field methods course or sending students out for a week of field work each year, although definitely moves in the right direction, are only first steps. There is a need for a far more fundamental restructuring of anthropological education, if our students are to become serious researchers and, as importantly, not be easy marks for

deception by self or others.

Anthropology departments must become full-time field schools. Students and faculty must be constantly involved together in field research and the critical analysis of field data. There are numerous subjects for anthropological research on or near any university campus. Undoubtedly, some professors and students will complain that the topics that can be studied near the university are not interesting to them or are outside their specializations. However, musicians do not usually enjoy playing scales that are necessary to build the skills required to play the pieces they do love.

Many have criticized 19th century anthropologists for being "armchair theorists"—depending almost exclusively upon the observations of others. Yet, modern anthropology students and anthropologists are frequently another type of armchair theorist. Often letting months or years go by between periods of extended field work, their observational skills deteriorate. As old data are worked and reworked, selective memory becomes, over time, even more conveniently selective.

Constant practice in first-hand research should be the basis of anthropological education. However, there are many other reforms that can be instituted that would help us mold students who are unlikely to be "betrayers of the truth."

For example, one way to train students in the habit of intellectual honesty can be adapted from practices I observed in seminars at the University of Helsinki. Whenever a student presents a paper, there should also be a respondent who is responsible for critically and scrupulously examining it. The respondent checks to make sure that quotations are not taken out of context, authors' positions are stated accurately, and quantitative data are consistent and statistically believable. Further, whenever a paper is based on field work, the author and the respondent should be required to analyze the data from several different and competing theoretical orientations or perspectives. Agar(1980:49) and Pelto and Pelto (1978:285) advocate the examination of alternative hypotheses as a crucial

element in anthropological methodology. However, unless that approach is consistently incorporated in all phases of a student's education, it is unlikely that he will later use it in a Masters thesis, Ph.D. dissertation, or professional publication.

Even though most of our undergraduate majors will never become professional anthropologists, I believe that my suggestions are still relevant to their education. The active practice of anthropology is at least as intellectually stimulating and at least as important as the playing of elaborate games with other peoples' theories. Furthermore, it can promote a sensitivity to the needs and problems, tragedies and joys of a broad variety of people. It can develop skills in analysis and communication that are important in any liberal education and walk of life. Above all, a rigorous education in anthropological practice can foster the priceless virtues of intellectual honesty and integrity.

Although I have based my comments on my experiences teaching anthropology in universities in Finland and Korea, as well as in the United States, clearly many of my observations and suggestions reflect an American intellectual orientation. Universities in different societies have their own unique cultures and social structures, and I have purposely done little to adjust my suggestions to fit the realities of Korean universities. If the basic issues I have raised seem legitimate to Korean anthropologists, it is better that they make their own culturally and socially appropriate responses and modifications to my proposals.

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