

Ethics in Anthropology Comments on Dilemmas and Conundrums

Lambros Comitas
Teachers College, Columbia University
Research Institute for the Study of Man

*And now, what will become of us without barbarians
Those people were a kind of solution.*

C. Cavafy

At a symposium in 1957 on *Values in Action*., Conrad Arensberg argued, as others had years before him, that values and morality cannot be scientifically determined.

I have from the beginning held that whether or not one wants to apply anthropology, and to what ends or purposes, or for what employer or institution to whom one submits one's expert knowledge and advice is in no way a scientific or public concern. It is a matter of private, moral, individual decision made by us individually in our private capacities as men, as citizens, as individuals, as believers in our own churches, religions, and ethical commitments. . . . Morals and values, *unless one is examining other people's as third party neutral observer*, are inseparable from sentiments and emotions (Arensberg 1958:25)

Logically then, as he had emphasized, it is not the function of science or scientific bodies to debate morality, to take moral stands, or to push any scientist into any kind of consensus. William Foote Whyte (1958:1), close colleague and friend, took issue with at least one aspect of this argument.. "We agree -- and yet we hold that [these issues] should be publicly *discussed*. Those two words mean quite different things to us. A debate implies an effort to convert the other fellow to one's own point of view. A discussion need not have such an objective - and, in this field - certainly should not."

More than four decades have passed since that symposium, a tumultuous period when American anthropology greatly expanded in size, scope, and focus. Growing numbers of anthropologists, many of whom first began their careers outside of the United States, began to find their way back home to encounter and pursue a plethora of new research issues, to form new research partnerships with other scientists and professionals. All of this helped to transform a discipline with a somewhat involuted and insular personality into one much more open to the scientific investigation of a wide range of issues throughout the contemporary

world. In this greatly enlarged context, a clearer understanding of the play of values, morals, and ethics between researcher, subject, and field dynamics takes on substantially greater importance now than it did a half century ago. Consequently, a heightening of such understanding is essential if the discipline is to deal effectively with the ever more complex ethical concerns generated by new interdisciplinary and cross-cultural efforts on a variety of fronts. Most anthropologists today, myself included, have become acutely aware of the ethical minefields of contemporary research and of the need, *a la* Whyte, for continuing discussion of these issues -- not to sit in judgment and not in the expectation of reaching or forcing agreement or consensus but rather as a fulcrum for enhancing sensitivity to and facilitating research in the face of ever-growing social and cultural complexity. However, for these discussions to have lasting value, several problems of contextual nature ought early to be resolved.

Defining Ethics, Morals and Values

First, the semantic hurdle. There appears to be little agreement among anthropologists as to the precise meaning of key terms, even critically important ones such as ethics, morals, and values. These terms particularly, long the subject matter of philosophical debate and controversy, often appear in anthropological writings but too often loosely and without definition. It could be argued that the academic devil is not as much in the detail as it is in the definition. If that be so, the discipline should strive for an anthropological understanding of those central terms and concepts that would be used in such discussion. Following this logic, my own understanding of several of these follow. Conventional wisdom holds that the words *ethic*, *ethics*, and *ethical* pertain to rules of conduct recognized in certain aggregations or categories of human society, rules ultimately rooted in culturally-conditioned concepts of justice. For example, it is widely held that science has its own ethic or code of conduct, one based on long recognized principles. In this regard, scientists should maintain the highest standards of work possible, protect information given in confidence, accurately report procedures and results, appropriately credit co-workers, acknowledge other writers and materials, accurately present one's qualifications, and acknowledge sources of financial support (Hobbs 1968: 161). Anthropological variants of this generic scientific code sometime include or infer aspects of the Hippocratic dictum of doing no harm. In its sociological sense, the English word *ethic* and its variations pertain to collectivities not individuals. On the other hand, *moral*, *morals*, or *morality* pertain to character or disposition, considered as good or bad, or to the distinction between right and wrong in relation to the actions, volition, or character of responsible human beings. Morals, then, pertain to and repose in the individual. Finally, value and values are the acts, customs, and institutions regarded or ranked by the individual, a group, or a people in a specific, usually favorable, way. Values, then, can either pertain to the individual or to the collectivity. Linked conceptually *ethics*,

morals, and values are also words entangled etymologically. For example, we owe the Latinate word *moral* to Cicero, its prototype being the Greek word *ithikos*, that is, an ethical, just person. Often the actual meaning attached by anthropologists to any of these terms when describing cross-cultural or other-culture ethical eruptions can only be gleaned analytically by the reader; the words *ethic* or *ethical* being sometimes loosely or incorrectly used to describe issues or problems that are, in cultural context, cases of moral transgression or clashes of values. Without agreement on the definition of the terms, full, partial or otherwise, discussions about ethics, morals and values will have little practicable import.

Ethics in the Academy

The semantic minefield is just one symptom of a larger problem - lack of systematic preparation. Arguably, anthropology, with its holistic and cross-cultural thrusts, is the social science in most intimate contact to questions of ethics. Nevertheless, it rarely offers its students exposure to these issues in the classroom. More often than not, young professionals first become aware of ethical problems when they are alone in the field, as dilemmas and embarrassments requiring unpleasant choice, as eruptions to be weathered. These unexpected experiences are almost never analyzed or added to the disciplinary literature.¹ Generally, the only form of commemoration of these experiences is their verbal unfolding as titillating war stories to be shared in anthropological cloakrooms. In any case, these experiences, illuminating at best or pernicious at worst, cause considerable professional and communal damage. Events of this sort, especially those in cross-cultural context, are difficult to encapsulate, awkward to confront and resolve, and almost impossible to categorize and compare. Perhaps for these reasons, anthropology has had difficulty in crafting useful, acceptable codes of ethical conduct. Consider, for example, the carefully drawn statement of professional and ethical responsibilities issued by the Society for Applied Anthropology, admittedly one of the better efforts of its kind. Members of this organization are enjoined to act in ways consistent with specified responsibilities to the people being studied, to the communities ultimately affected, to social science colleagues, to students, interns, or trainees, to employers and other sponsors, and to society as a whole, irrespective of the specific circumstances of their employment. This SfAA code, in scope and focus, is similar to those utilized by other social sciences. What the SfAA presents is a non-prioritized list of responsibilities owed to several groups or categories of individuals potentially impacted by research. Focused on the ethical responsibilities of its members, it provides a logical but clearly ego centered template for dealing with that continuously fragile relationship, researcher and field. As most professional codes of conduct, it is significantly better suited for social scientists working in their own society who presumably know their own culture

¹There are exceptions of course. For just several examples see Rynkiewich and Spradley (1976; Appell (1978); Washburn (1978); DeSoto and Dudwick (2000)

than for anthropologists studying peoples and problems in foreign systems and cultures. Followed literally, the SfAA code can just as easily and just as logically lead the anthropologist to unacceptable behavior or inappropriate inaction particularly when entangled in ethically charged conflicts within and between socially differentiated elements in the study; for example, as in cases where there are conflicts with ethical ramifications within the population actually studied as well as between that population, the communities impacted by the study, and the researcher's employers or superiors. Or imagine being told by an informant of his intention to cause his neighbor bodily harm. The informant does so, and under an oath of secrecy, freely admits the deed to the researcher and reveals the weapon used. Most codes of conduct would demand that the informant's relationship to the act be kept confidential. However, morality, centered in one's culturally conditioned personal milieu, may beckon in the opposite direction. The dynamic interplay or confrontation between morality as personal judgment of right and wrong and ethics as collective standards of expected behavior are rarely dealt with by the academy. Ultimately we are left to decide the conduct of ethical behavior on moral grounds. So should morality, not ethics be the proper focus of discussion in this case?

Pronouncements about research ethics in anthropology have long been with us set almost always in the context of the individual and a generalized society but rarely, if ever, in relativistic terms. Greek philosophers, well over two thousand years ago, were already struggling with this issue. For them as still with us, the fundamental problem of ethics and the crux of any understanding of moral conduct was "what is justice". Should righteousness be sought or should power; is it better to be good, or to be strong? Socrates' response to this ineluctable issue was "that justice is a relation among individuals, depending on social organization; and therefore, is studied better as part of the structure of a community than as a quality of personal conduct" (Durant 1926:25). For Aristotle, "the virtue of justice is a feature of a state; for justice is the arrangement of the political association, and the sense of justice decides what is just" (1962:61). Nearly three millennia later, particularly when applying the findings of their discipline, anthropologists are still grappling with the ethical dimensions of the great but unsolvable questions but now in real life situations - does a person or group have the right to exert power over others, and if so, to what extent and under what circumstances?

Science as Context

Anthropologists have always been at the interface of cultures and ethical systems - rarely as objective ethicists but much more often as active participants in or passive observers of ethical dramas of one sort or the other. The stance taken by anthropologists at such ethical junctures has depended, in large part, on their position *vis-a-vis* two key issues that have

confronted the discipline since its origins - issues elegantly discussed in the context of applied anthropology by Lisa Peattie (1958) at the *Values* symposium. The first and perhaps most critical is - what sort of science is anthropology and what should it be? Is anthropology, Peattie asks, a science that should be capable of predictions and generalizations as precise as in the natural sciences or a discipline whose center of gravity lies somewhat closer to the humanities, a science that mainly offers “illuminating insights and useful organizing concepts for experience and action.” Applied anthropologists, more often challenged by this question than “pure” anthropologists, generally concede that science, as traditionally defined, can only provide knowledge of the Is, or what exists, and that they must turn to the humanities for occasional glimpses of the Ought, or what should be. There is considerable less clarity on how this understanding should and could be operationalized to provide an ethical road map for the “science of anthropology.”

Peattie’s second question, what can knowledge of the Is tell us of the Ought, raises the question of the relevance of science on values. Many anthropologists take the position that anthropology is and has to be maintained as an intellectual discipline in its own right, that is as a science. Given this position, it follows that values and morals are essentially individual matters disconnected from scientific determination. Max Weber in his essay on ethical neutrality noted that knowledge of fact can never tell us anything as to what should be and voiced “contempt for those whose confidence in the rightness of their moral judgment is so weak that they feel the urge to support it by some authority such as the “trend of history” or its conformity with scientific doctrine in a sphere in which the powers of science are definitely limited.” (Shils 1949:). Arensberg (1958:25), representing the position of many, argued that

science has its own morality, a devotion to the evidence and to truth, which governs us in our doings as anthropologists and scientists, but which has nothing to say about the ends to which we put the truth we discover. The ends to which we apply science are our own individual business and our own free choices. By becoming anthropologists, and working as such, we do not stop being members of the cultural class, the religious, ethnic, political, etc. groups to which we have allegiance or devotion outside our efforts as scientists. I do not think we have any ends or purposes as scientists, or anthropologists, except devotion to truth and to science.

There are other anthropologists, of course, who utilize values and practical concerns to set their problem. In fact, some see value judgments as essential ingredients of their research design and operation. For many of the latter, the center of gravity of their

anthropology lies closer to that of the humanities and these individuals often profess substantial linkage between science, personal values, and ethical conduct. Certainly over the past half century value-laden terms such as "truth," "freedom," "justice," "the relativity of values," "the right of cultural self-determination," "integration," and "universal individual needs" have not only become common currency in anthropology but also the catalysts for considerable applied research. Sol Tax's Fox Project and Allan Holmberg's Vicos Project are classic examples of this general position.

While obviously data do not exist to categorize accurately the positions of American anthropologists over the latter part of the 20th century with regard to the relationship between science, anthropology and ethics, personal experience indicates that a substantial majority seemed to have favored the notion that the discipline was a science, in fact, a "hard science" with its "own morality, a devotion to the evidence and to truth". At least in theory, this orientation made it easier for individuals to avoid responsibility for ethical disruptions, some of which they themselves precipitated. For this category, questions of ethics would not have been given a high collective priority. A smaller number of anthropologists, those who held some belief in the relevance of science to values in research, tended to favor a "soft science" orientation which, again in theory, brings to the fore and sensitizes adherents to the ever-lurking hazards of ethical contingency. Finally, as Peattie (1958:5) has pointed out, there were a few anthropologists who felt the obligation to act, to speak out in defense of victim or in opposition to oppressors, not necessarily because of any belief in the relevance of science to values, but because of special or personal knowledge possessed about a group, a people, or an issue. For these individuals, the ethical dimension of their professional work was not only critical but also omnipresent. However, over the past decade or two, I strongly believe that this rough ratio between categories has begun to shift significantly with ever growing numbers of anthropologists now falling into the "soft science" camp. In this context, therefore, while not disagreeing with those who argue that ethical issues have long been with anthropology as have informal procedures for dealing with them, I cannot agree that ethical issues have not changed in substance, perception, or volume over time, or that "informal procedures" were uniformly practiced, or with the corollary assumption that anthropology should not think it must fundamentally re-do the discipline in order to cope with supposedly unprecedented conditions. In fact, the discipline has already been re-done, in part by its denizens, in part by the impact several exogenous, unprecedented conditions.

Shifting Conditions - Altered Ethical Orders

Changes in the discipline of anthropology over the past few decades have been not just substantial but profound. For one, the growth of the discipline has been staggering. In 1950, there were slightly under 500 accredited Americans in four anthropological sub-fields.

A half-century later, in the United States the discipline has grown some 1500% admitting some 500 new doctorates yearly. In the early part of this period, before globalization became a fashionable term, American anthropology with its limited numbers was carrying out research around the globe, much of it in developing and/or colonial countries, less of it in the industrial world, particularly in the United States. Currently, the domestication of American anthropology is well underway with increasing numbers of anthropologists working here at home in applied research on public interest topics, a belated reflection of long-term shifts in the economy and research priorities of funding agencies. This, in turn, has led to a dramatic expansion of employment and research niches outside the academy -- in government, industry, labor relations, education, medicine, public health, nursing, drugs and society, community development, urban planning, migrant affairs, forensics, legal affairs, cyberspace, to name a few just in sociocultural anthropology. As a result, the employment patterns of the discipline have changed drastically. For example, 13 of the 18 listed participants in this symposium are gainfully employed as anthropologists outside of the academy. Not many years ago, this would not have been possible. The training and socialization of anthropologists is being transformed. With the American four-field approach fast eroding, sub-field specialization begins sooner, is more intense, and early identification with other disciplines or professions is facilitated. For good and bad, these and other shifts have led to a larger, internally more segmented discipline drawn more into application and public interest concern than in the past. Perhaps more significantly, these changes also contributed to a perceptible erosion of that world view or psychic unity that once characterized four-field anthropology. As a consequence of these remarkable changes in the disciplinary dynamic, the ethical issues of today, while appearing to have many of the characteristics of those faced in the past, have accelerated in quantity and rate of occurrence and are now often played out and uncertainly concluded in atmospheres highly charged and heavily ideological.

In these regards, anthropologists are now faced with changing ethical orders as they enter new or transformed sociopolitical fields of interest. In the past, as often has been pointed out, anthropologists, particularly applied anthropologists, worked with populations in situations of "great disparity of power," where they have held "power over other people with different values from themselves" (Peattie:1958:5). The domestication of research, for one, has begun to change this imbalance. Nearly fifty years ago, Raymond Firth made a prescient observation about research in complex societies. "Another burden" he wrote, "may well be some sacrifice of ease of movement through the society. In the alien primitive communities that have been the classic field of the anthropologist, he has usually had remarkable social freedom. In studies in his own society he may well find that he has a tendency to become status-bound." He goes on to note the relative ease an anthropologist would have in studying a royal court in Asia or Africa but how "participant observation

might present some difficulty in Buckingham Palace” (1954:33). Four years later, Peattie (1958:5) wrote that “although I know of anthropologists who have informally put themselves at the service of the ‘underdog,’ I do not know of any clear case of the underdogs hiring themselves an anthropologist.” Over the course of a half-century, much has come to pass in American anthropology including what was anticipated by Firth and unanticipated by Peattie. Changes in the domestic research environment - a volatile scene now ranging from government agencies, cyberspace board rooms, Amerindian tribal politics, and not-for-profit “beltway bandits” to the academy - have been more than substantial. It is a new world in which the anthropologist, in terms of ethics, faces restraints more familiar to an employee than to an independent researcher.

But “the field” abroad has also been transformed as have the ethical problems facing the researcher. Anthropologists no longer work in “alien primitive communities.” Most of these have been metamorphosed into larger social units, into complex societies, into nation-states with authorities and want-to-be authorities who admit researchers, vet their proposals, judge the results, make ethical assessments, and, ultimately reward or sanction. The “great disparity of power” has been shifting. The vociferously negative reaction of the Chilean government in 1965 to Project Camelot, a U.S. Department of Defense study of “the causes of revolutions and insurgency in underdeveloped areas of the world” was a warning about the new dangers of government-sponsored team research in complex, foreign settings. Camelot brought to searing light many ethical issues, new and old, and the project itself was indeed a *cause célèbre* in American anthropology during the tumultuous decade of the sixties. Among the questions raised by Camelot, and here I paraphrase Hobbs (1968:164), was whether the intentions of a sponsoring agency should be the concern of an anthropologist even when personally allowed full freedom of inquiry? Should the anthropologist be concerned with the uses to which the results of his studies will be put? What responsibility does the anthropologist have for ensuring that the very process of inquiry does not have an adverse effect on the people being studied? Is the anthropologist obliged to preserve access to people for subsequent investigators? Do inadequacies of design or procedure, or lack of scientific merit in a study, become intrinsically ethical issues by virtue of their imposition on others.

Dilemmas and Conundrums - A Case in Point

Researchers not involved in sponsored team research were also beginning to encounter difficulties in the fast developing world. The experiences of Oscar Lewis after publishing a book on slums and poverty in Mexico is a case in point. Here we have an internationally renowned social scientist, known in Mexico for his sophisticated restudy of Tepoztlán (the site of one of Robert Redfield’s celebrated projects), for his groundbreaking

family studies, his pioneering use of oral history . Here we have an anthropologist who by any scientific standard always worked well within ethical guidelines yet who is bitterly attacked by Mexican authorities and intellectuals for ethical dereliction after publishing *The Children of Sanchez* in Spanish. In a long letter² written in 1965 to a colleague, he recounts the event with vivid illustrations of a number of the issues discussed above including the play of collective ethics and personal moralities, the status of researcher in complex sociopolitical settings, and the volatile nature of changing field conditions.

I tried to get you by phone yesterday morning to tell you that after much thought and soul searching I decided not to prepare a tape for the meetings because it seemed to me that I should be present to deal with questions which might arise concerning such an important issue. However, I thought that you should be familiar with the basic story and I plan to give it to you here in this letter.

In light of the Camelot scandal and the various questions raised both here and in Latin America about the future of research by American scholars in Latin America, it seems to me that my experience with the Mexican reactions to *The Children of Sanchez* may have some important lessons for anthropologists. Actually, the problems raised by the reactions to my book are of an entirely different order from those of the Camelot affair and the two should not be confused.

The story is relatively simple. The first Spanish edition of *The Children of Sanchez* was published in October 1964 (6,000 copies), was well received in the press in Mexico, and sold out in the first six weeks. The second edition (6,000 copies) appeared sometime in December and was doing very well indeed when it was attacked in early February at a meeting of the Mexican Society of Geography and Statistics.

The attack consisted of the following points:

- (1) The book was obscene beyond all limits of human decency;
- (2) The Sanchez family did not exist. I had made it up;
- (3) The book was defamatory of Mexican institutions and of the Mexican way of life;
- (4) The book was subversive and anti-revolutionary and violated Article 145 of the Mexican Constitution and was, therefore, punishable with a twenty-year jail sentence because it incited to social dissolution;
- (5) The Fondo De Cultura Economica, the author, and the book were all cited for action by the Geography and Statistics Society to The Mexican Attorney General's office; and

² This letter is the property of the Research Institute for the Study of Man in New York. During the first half of the 1960s Oscar Lewis was a Senior Research Fellow at RISM where he and his staff translated and edited most of the oral histories included in *La Vida*. I am confident that neither Lewis nor the recipient of this letter, both of whom I knew well, would have had any objection to the publication of this letter in a volume of this kind and at a time when presumably all the major actors in the event described have passed on.

- (6) Oscar Lewis was an FBI spy attempting to destroy Mexican institutions.

These charges were a grave threat to freedom of scientific investigation and freedom of press and were, therefore, taken up by leading Mexican writers and intellectuals as a threat to Mexico. Over 500 clippings from Mexico City newspapers are in the file of the Fondo De Cultura Economica and I have the most important ones.

The issues were discussed on radio and on television and in the press, in editorial columns, in cartoons, and at special meetings arranged at the University.

I believe it is fair to say that no book in this century published in Mexico received the amount of attention given to *The Children of Sanchez*. The phrase "the children of Sanchez" has become a part of Mexican popular culture. On my recent trip I asked about a dozen cab drivers if they had heard about *The Children of Sanchez* and all of them were able to recite a detailed account of the issues and a number of them had read the book. Similarly, there are references to *The Children of Sanchez* in three current plays in Mexico City. In one, in the first act, some children in a *vecindad* are running around yelling, "Here comes the gringo with his tape recorder" and this brings down the house. In another, one of the characters is named "Sanchez Lewis" and when his name is mentioned it stops the show.

But more important, the members of the Sanchez family and my friends at the University tell me that the scandal has had a very beneficial effect and that it has defeated the forces of reaction who were using my book as a first move in a campaign designed to eventually censor, if not silence, some of the contemporary young writers whose novels and short stories dealt with some of the pressing unsolved problems of the Mexican revolution.

I was also told that the government is giving much more attention to the slums of Mexico City and is spending more heavily on improvements since my book was published. If this is true, even in part, then I shall have been successful beyond my wildest dreams because I strongly identified with the slum dwellers and I hoped that bringing their way of life and their problems to the attention of readers might eventually do some good and might lead to some amelioration of these conditions. Judging from my informants in Mexico, the book has had exactly this effect.

The single grave concern raised by this scandal is the fact that a number of newspapers sent out their reporters to track down the *vecindad* of the family and *Novedades* was successful in this venture, identified and photographed the slum, and identified one of the family members, Manuel Sanchez of the book. All the other members of the family had left the *vecindad* and were scattered in different parts of the City and, indeed, in different parts of the Republic.

I had made three trips to Mexico with the specific purpose of getting Manuel out of the slum before the appearance of the Spanish edition. He was

not enthusiastic to leave and he seemed to welcome the idea of eventually being identified as one of the characters in my book. After much insistence on my part, he agreed that he would move if I could get him into some low-priced public housing. I made a number of efforts to accomplish this, particularly since the head of the Instituto de la Vivienda was a personal friend of mine of many years. However, the promises to do something never materialized so Manuel was still living in the same slum when the scandal broke.

To my knowledge, the true names of the family appeared in only one edition of Novadades and after that, due to some unknown cause, the newspapers seemed to withhold this information almost as if they sensed the indecency of what they had done and certainly the violation of my intent and pledge to the family to do everything possible to maintain their anonymity.

Of course, the family was aware, beginning with the early stages of the study back in 1955, that I was writing a book about them and they signed releases allowing me to publish all the data they had given me individually with the understanding that their names and those of their friends and relatives would be changed. All the members of the family except Manuel have tried to keep their part of the bargain and have denied consistently their identity as characters in my book. Manuel, however, for a variety of motives - one of which was his belief that he was helping me disprove the charge that I had made everything up, identified himself. Moreover, he made a tape recorded statement available to a Mexican professor of sociology and this tape was played at a very crucial meeting of students at the National University which was attended by a great overflow crowd and featured a leading anthropologist, a writer, a sociologist, and one of the men from the Geography and Statistics Society who had originally attacked the book - Sr. Morlet.

I have a tape recording of this meeting and a number of clippings from the newspapers. It is clear that the students gave Sr. Morlet a very rough time. Ricardo Pozas, a Mexican anthropologist who had written a very critical review of my earlier work *Five Families* when it appeared in Spanish, publicly defended *The Children of Sanchez* and attacked the Geography Society for its stand.

In April the Attorney General's office issued a statement clearing the book, the author, and the publisher of all charges and reaffirming the freedom of investigation and freedom of press as a basic Mexican value. However, despite this ruling, the publisher was unable to get his Board of Directors to approve the third edition so that during the entire period of the scandal from February through April, and for that matter until now, there wasn't a single Spanish edition available. The book was selling for a few hundred pesos on the black market during this period and the original price was only 44 pesos. It is estimated that we might have sold 100,000 copies were the book available. The failure of the Fondo De Cultura Economica to publish a third edition is a serious blow to this institution and has made its Director terribly unhappy.

I have the detailed story of the politics of this decision but I do not feel at liberty to publish these details. As you can see, this issue was carefully considered by people who were very high up in the government. The members

of the Board of Directors of the Fondo De Cultura Economica who failed to approve a third edition included the novelist and the present Minister of Education, [A.B.] Augustine Yanez; [C.D.], present Minister of Foreign Affairs; and [X.Y.] Ortiz Mena, the present Minister of Hacienda. The first two men were all in favor of a third edition but apparently they did not consider it politic to oppose [X.Y.] Ortiz Mena who controls the purse strings.

Fortunately, a nongovernment controlled publisher, Joaquin Mortiz has just published a third edition of 12,000 copies. The official statement of the Mexican Attorney General is included as an appendix so that, in effect, the book now has government approval.

Throughout the entire affair the Sanchez family has continued their friendship and sense of loyalty and identity with me. For this I am most grateful and I am deeply moved. Nevertheless, this case does raise the more general question about the possibility of doing this kind of intimate research on the lives of people. Is it really an invasion of privacy when one has the permission of the subjects as I had in this case?

Fortunately in this case the government ruled in my favor. Had they found me guilty on the charge of social dissolution, I suspect that further tape recording research anywhere in Mexico, particularly by American anthropologists, would have been an impossibility. Even as is, some of my colleagues are afraid that the Sanchez affair may adversely affect their own research plans in Mexico.

The study of slums and poverty by Americans in Latin American countries - particularly in a country like Mexico with its strong anti-gringo tradition - is fraught with dangers of a kind that archeologists, for example, working in Mexico would not be troubled by. The American archaeologist who can show that corn is 2,000 or 3,000 years older in Mexico than has heretofore been known becomes a hero. The social anthropologist who describes the misery of people who live in slums automatically steps on many toes of men in high places.

Another problem raised by my book was related to the traditional image of what anthropology is. Anthropology in Mexico has been almost exclusively identified with the study of Indians and rural communities. It came as a surprise to some Mexicans to find that a study of urban slums was being done by an anthropologist. Indeed, one of the points raised by the minority who attacked the book was that this was not traditional anthropology and they couldn't quite place it. Moreover, the novelty of the approach, namely, intensive family studies, was also a bit disorienting to some of the Mexicans. They wondered whether family studies were as scientific as community studies and it was difficult for them to grant that a book with literary quality could also be scientific. Nevertheless, the whole affair was really a great victory for anthropology in Mexico. I believe that some of the factors responsible for the defense of the book were:

1) I had invited some leading Mexican intellectuals to one or two of the slums during the study; and

2) I had played the tapes in my home for Mexican writers, one or two anthropologists, and for a few people in the movie industry.

In short, I had taken the Mexicans into my confidence as the work progressed so that they knew that a book of this kind was forthcoming. I also played the tape for some of the leading economists and historians and for the head of the publishing house which later published the book in Spanish. These people were able to refute the absurd charge that I'd written a book of fiction.

Well, this has turned out to be a long letter. It would have to be much longer if I were to analyze the content of the Mexican press on this issue.

Again, please forgive me for not attending the meetings; but, as you have already agreed, it is quite urgent for me to go to Puerto Rico to try to prepare the way for my forthcoming book and try to reduce, if not entirely eliminate, the possibility of a scandal.

Postscript

Oscar Lewis was exonerated by Mexican authorities of all ethical and ideological charges which did not, as one might expect, immediately nor fully restore his scientific reputation in that country. That aside, the affair did cause substantial damage to the image of American and other foreign anthropological research in Latin America, some still lingering. It was a forecast of the mercurial and precarious field situations that we later experienced and in which we now habitually labor not only abroad but at home. Ignored by social historians, this uproar over the publication of a book is an event that should have been of considerable importance to anthropologists, since it provides an invaluable view of the growing interdigitation of ethical confrontation. In fact, Project Camelot and the Lewis incident should have prompted calls for the systematic scrutiny of ethics in anthropological activity - that is, on its multifaceted nature and on the paths to be traveled in order to clarify, defuse, and possibly resolve dilemmas and conundrums. In retrospect, they did not. No longer is there time for debating but only for ongoing deliberations and discussions of these vital issues.

References

- Appell, George N.
1978 *Ethical dilemmas in anthropological inquiry : a case book*. Waltham, Massachusetts: Crossroads Press.
- Arensberg, Conrad
1958 "Values in Action: A Comment," *Human Organization*, Vol.17(1):25.
- Aristotle
1962 *The Politics*. Penguin Books.
- DeSoto, Hermine G. and Nora Dudwick, eds.
2000 *Fieldwork Dilemmas: anthropologists in post socialist societies*. Madison, University of Wisconsin Press.
- Durant, Will
1926 *The Story of Philosophy: The Lives and Opinions of the Greater Philosophers..* Garden City, NY, Garden City Publishing Co.
- Hobbs, Nicholas
1968 "Ethics – Ethical Issues in the Social Sciences," in *International Encyclopedia of the Social Sciences*, Vol. 5, David L. Sills, ed., The Macmillan Company & The Free Press.
- Firth, Raymond
1964 "Social Organization and Social Change," in *Essays on Social Organization and Values*, Raymond Firth, London School of Economics, Monographs on Social Anthropology, No. 28, London, The Athlone Press.
- Peattie, Lisa R.
1958 "Interventionism and Applied Science in Anthropology," *Human Organization*, Vol.17(1):5.
- Rynkiewich, Michael A. and James P. Spradley
1976 *Ethics and anthropology : dilemmas in fieldwork..* New York : Wiley.
- Shils, Edward A.
1949 "Foreword" in *The Methodology of the Social Science*, Max Weber. New York, The Free Press.
- Whyte, William Foote
1958 "Editorials," *Human Organization*, Vol. 17(1):1.

