RESEARCH PROBLEMS IN THE MEDICAL SETTING:

SCIENCE VERSUS POLITICS

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Training in research methods generally considers the problems that an anthropologist encounters upon entering the field and becoming accepted within the group being studied. These problems, however, usually focus on single group units or small scale societies in which we, as anthropologists, represent a dominant group. In these situations, we routinely use our traditional methods with little, if any, questioning from the group under study. Rarely, if ever, does training consider the problems encountered when conducting research in complex settings where we have little power and, at the same time, have to deal with a variety of subgroups at one time. In these situations we are questioned about our methods and often we may even question them ourselves. Such encounters present ideological and methodological research issues that need to be examined in concrete terms as more anthropologists find themselves conducting research in complex societies.

In this paper, I will discuss some of the problems my research team encountered in a medical setting and place the discussion within the general framework of the interrelationship between social research, ethics and politics. As we shall see, I began this research with the misguided assumption that a scientific approach was the only expectation and that once my project was approved, I could utilize the guidelines within the general framework of anthropology which were written in the project proposal. As the research progressed, however, I discovered that we were faced with additional rules and expectations from outside the general anthropological (scientific) framework. After presenting these problems and expectations, I will discuss several factors that I feel should be taken into consideration when conducting research in complex societies, or as Sjoberg (1967) has said, we need to be fully aware of the social organization of scientific activity. Such awareness, of course, has serious implications for our "research styles" and research models in multigroup units in complex settings.

The Research Story

During the summer of 1975, I was contacted by the Director of Research for Rehabilitation Medicine at an urban based medical school. She wanted me to write a research proposal, using the anthropological approach, that would study the patients' perceptions

of the rehabilitation process. She wanted a draft of the proposal in two days. With apprehension, I wrote a short proposal in two days and turned it in with little expectation of funding. Obviously, anthropological methods were the basis of the research, i.e., use of participant-observation, indepth interviewing, and use of an interview schedule. I was instructed to use a broad definition of rehabilitation, and after funding, could choose a specific area on which to focus. To my surprise, the project was funded with suggested revision. I informed the Director that I did not have the time to revise the proposal and she suggested hiring a sociologist as a consultant. I agreed. So, the sociologist rewrote the proposal which expanded the structured research methods to include a set of highly sophisticated quantitative techniques.

A few weeks later, I hired two research assistants and we had our first meeting to plan the schedule for the project. The sociologist suggested that she work on the interview schedule while we spend two or three weeks doing participant-observation--a time frame she felt sufficient for this technique. It was at this point that I began to feel uneasy about the direction of the project and discussed my concern with the Director of Research, who then informed me that the sociologist had been put on the project as an investigator although I was the principal investigator. I informed her that the sociologist was afraid of being raped in the hospital where we were conducting the research. So, we worked out a procedure in which I would direct the field situation and she (the sociologist) would construct the interview schedule. (At this point I mentioned that it is very difficult to construct a schedule without having observed and talked to the patients.) Of course, this meant that I would also spend a great deal of time adjusting the structured questions to the unique setting in the hospital.

Approximately three months into the field research, I informed the Director that I needed another research assistant and planned to hire someone to observe the unit under study. She strongly recommended a specific person who had a M.A. in rehabilitational counseling and who, after my interview with her, assured me that she had training in observation techniques and interviewing. Six weeks later, without informing me, she complained to the Director of Research, the Chairperson of the Department and to the personnel at the hospital that participant-observation was an unethical technique since we were taking notes on situations that we did not have permission to observe. (Sometime before, we decided that the personnel on the unit must be included in the observation and interviewing in order to ascertain a more holistic picture of the structure and functioning of the unit which is inextricably bound to the patients' perceptions of their rehabilitations.) Handling this accusation involved building a case for participant-observation and maintaining that it is, indeed, ethical. I had long discussions with the Director, Chairperson and my assistants, and pointed out that this technique was included in the permission statement, approved by the hospital board, and that it has always been acceptable and ethical

technique in anthropological research. The issue was subsequently
dropped.

At this point, the interview schedule was completed, so I
directed the rehabilitation counselor only to conduct structural
interviews; she refused to take any fieldnotes. Consequently, I
gave her notice of termination for the end of the month. Also, due
to the discussion that had taken place over participant-observation,
the Director and the Chairperson of the Center were able to
understand, in a preliminary way, the findings of our research.
The Director (a female Ph.D.) was pleased with the progress of our
research while the Chairperson (a male M.D.) was positive about the
continuation of the project. However, in the site visits from
Washington, the funding representative encouraged our approach in
the rehabilitational setting, so we continued with participant-
observations, indepth and structural interviewing for eight months.
The sociologist never visited the research sites even though she
continued to be paid by the project. I pointed this out in my
monthly reports and requested that the sociologist be terminated and
another hired to help me begin the analysis phase of the research.
I was informed by the Director that the sociologist could not be
dismissed because her husband was on the review board of the Center
and the Chairperson would not approve such an action.

I began to put the pieces together and gained a broader
understanding of the situation, so I forced my recommendation to
the point of threatening to resign if I could not get help from a
sociologist who respected the anthropological approach and would
work with me in analyzing the quantitative data. At this point, the
Chairperson requested an interim report including our initial find-
ings (we had already analyzed some of the qualitative data) and a
projected schedule for the remaining time on the project. This we
did in detail and requested only $8000 and six months to properly
finish the project. Several days after the report was submitted,
the project was terminated, but the sociologist was maintained on
another project. For several weeks thereafter, I contested this
decision in an attempt to clarify in my mind what really happened.
It was then that I discovered that the Chairperson of the Research
Center was also the person responsible for the unit we were report-
ing on. This information did not appear on the flow charts nor were
the personnel we interviewed aware of his influence. So, the situa-
tion became clearer and although I was encouraged by the personnel
from RSA to submit a proposal through another sponsor, by this time
I was too disgruntled and frustrated to continue and decided to
abandon the research.

The Structure of Research

In retrospect, I now understand that although my methods were
grounded in the scientific approach to anthropology, I was "inno-
cent" of the complex setting in which I was attempting to conduct
research. After having the grant funded and obtaining permission

to enter the hospital, I assumed that the only problems I would have
would concern such traditional ones as constructing time frames,
developing sampling techniques, establishing rapport, and generally
controlling the quality of the data collected. Not so. I
encountered a multigroup unit with each level placing different
expectations on the conduct of our research. These levels and their
expectations and problems were:

1.   The Research Unit

     A majority of the problems encountered in the research unit
were expected, such as explaining our presence, establishing rapport
and becoming a part of the daily routine. The hierarchy within the
unit, "the rehabilitation team," accepted us in varying degrees. We
were more accepted and established better rapport as we descended
the levels. Next to the patients, we were better accepted by the
orderlies (lower levels of the hierarchy). We also established good
rapport with the physical therapist and occupational therapist.
However, we were either ignored or brushed aside by social workers
who at times attempted to control the research, by many nurses and
especially by the doctors, the leaders of the team. (Most of the
doctors were either female or foreign.) Obviously, this rehabilita-
tion unit was complex (Hill 1978). We had to manage our relations
with each of these categories of medical personnel somewhat differ-
ently. This took a great deal of discussion with my research staff
on how to handle varying expectations of our behavior. An example
of the problems we had involves a nurse who asked us to intervene on
her behalf to the doctor about a patient. She felt that the doctor's
orders were incorrect and was afraid to confront her. We decided
against such action, which angered the nurse and consequently
damaged our rapport with her. Perhaps the major problems we had
involved the patients' expectations of our behavior. They requested
daily that we intercede for them with the doctors and nurses. We
were being placed in conflicting roles--researcher or friend.
Although we had few problems in collecting data from them (their
perception of the rehabilitation process), we were continually faced
with expectations from them we could not fulfill. Consequently, our
"presentation of self" on the unit had to shift as we dealt with
this hierarchy of medical personnel and patients from whom we were
collecting data. However, these types of problems are generally
encountered in any fieldwork situation.

2.   The Research Team

     Another level of the structure of research in this complex
medical setting involves the research team. (E.g., as principal
investigator, I had never conducted research in such a highly
structured setting, and thus began work in a very innocent way.)
Although I followed the general guidelines accepted by anthropolo-
gists as the scientific approach (which had worked successfully in
the rural areas of the American South), I was unaware of the con-
flicting roles that would be expected of me in the research process.
Since my assistants were trained in the anthropological approach

(with the exception of the rehabilitation counselor), I naively assumed that, if all of us worked within the traditional style of anthropology, we would be accepted as "scientists" (after all, I was asked to conduct this research). In addition, I made this assumption when hiring the rehabilitation counselor as an assistant. I thought she could learn the anthropological approach as one learns to administer questionnaires. Obviously, the traditional methods of anthropology were not within her expected boundaries, and they were totally unacceptable to the sociologist since they did not produce quantitative data.

As a result of these problems with the research team, the research process continued to progress unevenly and can generally be characterized as tension ridden, especially with the power levels of the research structure (to be discussed next). Throughout this time I remained somewhat confused, extremely frustrated and, at times, made unwise decisions concerning my relations with the research team and the dissemination of our preliminary findings. Thus, I found that the traditional role expectations involving behavior in the field did not "fit" the field situation in a multigroup structure even when planning and relating to the research team who represented different disciplinary models. The traditional methods, however, were producing good data although I felt that collecting every piece of data was a struggle. And, at that time, I was so immersed in the situation that I could not determine the variables that were causing our problems. Only later was I able to put most of the pieces of the puzzle together.

3.   The Research Center

A major source of the problems came from the research center itself and my lack of understanding of the politics involved in the many aspects of research. Expectations were placed on me from the Director of Research in terms of the general administering of the funds and hiring of personnel. I was unaware of the politics within the Center and the priorities placed on different types of research. (Later I discovered that social science had the lowest priority and that my project had been funded, and indeed solicited, because the Center was under pressure from Washington to fund a social science project.)

The hierarchy of the personnel in the Center did not initially appear to be a significant factor in my study; after all, the project was funded for two years and I had the time frame for data collection and analysis worked out over this period. However, when problems arose as the project proceeded, I began to discover that I lacked the backing of the Center necessary to properly conduct the research. It was at this point that I began to research the roles and statuses of the personnel of the Center and found that kinship ties, conflicts between administrations, and conflict of interest were major factors that ultimately led to an early termination of the project.

I discovered that the Director of Research and the Chairperson of the Department were in continual conflict with one another and that I indeed had the backing of the Director, who had nonetheless been overruled on the decision not to support my project by the Chairperson. Furthermore, the husband of the sociologist worked closely with the Chairperson, a fact I did not know when she was hired on this project. The Chairperson of the Department was also responsible for the unit on which I chose to write my interim report that contained the data on the patients' perception of the rehabilitation of the unit, together with my analysis of the problems related to the function of the unit. He was obviously uncomfortable with the report, substantiating Nader's observation that "Ethnography is uncomfortably revealing at times when studying American society since the anthropologist is often studying the health problem from the point of view of the professional" (1976:177-178).

By now, it should be obvious that I was working within a framework I did not completely understand—but the important point here is that I did not feel it necessary to find out the politics of the research center which granted my project. However, the political situation and the closed system of medicine obviously had ramifications throughout the entire structure of research.

4. The Anthropological Community

In addition to expectations from the research setting, there are also specific expectations from our discipline. First, we are expected to follow the general methods and code of ethics of our profession. Secondly, we are expected to publish the findings of our data. These are sometimes conflicting expectations. That is, in this project we were faced with ethical dilemmas about publishing the results or even showing the information to our superiors and, at the same time, protecting the right to privacy of our informants. We assured our informants that the information they gave us would be regarded as confidential. However, as scientists, we are required to collect detailed documentation if we are to make generalizations about behavior. This involved collecting information on concrete human interaction and human perceptions of the research unit. We were thus faced with the following questions: (1) Who should have access to this information? (2) What should I publish? (Colvard 1967; Harrell-Bond 1976; Hansen 1976; Chrisman 1976).

Toward an Expanded Research Strategy

It has become clear to me that the politics of research in a multigroup structure become just as important in executing a research project as the research model itself. That is, we can have the most thorough, logically consistent research plan but, as a result of our inexperience with the conflicts and alliances within the research structure, not be allowed to conduct the study according to "our" rules of scientific investigation. Therefore, I suggest that our traditional research paradigm be expanded to include the

politics of research in our training. We should stress the impor-
tance of becoming aware of the structure and functioning of the
complex structure within which our data are collected and not just
the most immediate levels. Based on my experience, which can be
taken as a case study for social inquiry (Stake 1978), I recommend
that the following topics be included in this expanded research
strategy:

1. Delineate the structure of research from the top levels
(funding agencies) to the objects of research and map the varying
units and the relationship among the levels. Pay attention to
conflicts and alliances among these levels and units and the
informal and hidden rules.

2. Explain the research design (methods) and the purpose of
the research to all levels of the research structure, with special
emphasis on the reasoning behind participant-observation and inten-
sive interviewing and our ethics in utilizing these methods (Sjoberg
1967; Rynkiewich and Spradley 1976; McCall and Simmons 1969).
Perhaps this effort will be a first step in revising the status of
participant-observation in the social sciences. Medical settings in
particular are more familiar with experimental research where more
control can be exercised over the data.

3. Be perfectly clear, at the onset, who controls the project
and who makes the administrative decisions involving salary and
personnel. Alliances based on kinship or friendship tend to become
important in research centers, especially at times when jobs are
scarce and competition exists for positions on funded projects.

4. Determine, in writing, who controls the report, the proce-
dure surrounding the confidentiality of the report, especially who
approves it and who can release the information. Who is account-
able? (Spradley 1976; Colfer 1976; Jacobs 1978; Clinton 1978). What
do we do with our findings if they are in conflict with expectations
of the research structure?

5. Develop a "research style" that will aid in establishing
rapport within the multigroup structure. This involves impression
management (Berreman 1962) at each level which may fluctuate from
group to group depending on their expectations and the "rules of
their game." We have multiple identities as we move through the
research structure and our research training has resulting in
teaching, in Nader's (1972) terms, to "relate down rather than up."
I suggest that we learn to relate up, down and sideways if we are to
succeed in conducting research in complex institutions. Perhaps I
am suggesting another set of ethics--ways to adapt to the politics
of research in complex settings.

Of course, every fieldwork situation is unique in some ways;
however, given the unavoidable personal involvement (Watson-Franke
and Watson 1975 and Honigmann 1976) we encounter in using tradition-
al methods and the similarities of the structure and functioning of

bureaucracies, these general guidelines are applicable to the study of American institutions. Furthermore, I do not mean to imply that these problems are not encountered during fieldwork in less complex societies, nor that two different kinds of methodologies are necessary in anthropological inquiry. We have discovered that the politics of research is important in any research setting in the world today. In addition, I am not implying that our traditional methods are not applicable to research in complex societies. Indeed, I have argued just the opposite (Hill 1973; 1974). However, I am suggesting that research in a powerful multigroup structure, where the anthropologist has neither the prestige nor power, requires a different "research style"; one in which we have to conform to the expectations of the entire research structure. Some anthropologists may find this difficult since we are probably conforming to a set of rules that we do not agree with or perhaps disdain. We are American anthropologists studying our own institutions and, for some reason, feel that some of the rules we follow in conducting traditional research do not apply in American institutions. As a consequence, we appear innocent and naive in the research setting, a situation that is detrimental to research in our own society.

Implications for Research Design

So far, my discussion and suggestions involve the practical aspects of expanding our research strategy in order to adapt to a range of expectations in bureaucratic institutions. Now, very briefly, I will make several remarks dealing with the more abstract implications of expanding our strategies. In recent years, we have experienced a growth in applied research projects. The problems attendant in conducting research to solve theoretical problems and those related to conducting research toward solving human problems become clearly defined as we move into research settings in complex institutions. In expanding our paradigm, we will have to take into consideration the expected goals, investigative techniques and analysis of data of the research structure in which we conduct our research. After all, a paradigm is an agreed upon set of rules to follow if a practitioner is to have his/her work accepted by a specific scientific community (Kuhn 1970). The rules within the research structure change depending on policies, priorities, and personnel. This does not render our research on contemporary issues more biased and less objective than studying more distant and by-gone societies (Nader 1976), but does mean that we must expand our research strategies.

Therefore, I suggest that we follow two sets of rules in con-ducting research in multigroup units: (1) the rules agreed upon in the research structure, and (2) the rules agreed upon in the scien-tific community. This may leave some researchers in a state of "paradigmic dissonance." We were trained in the rules of pure research and are now discovering that we are faced with additional rules and expectations from outside our discipline. The old rules no longer define the "playable game." However, we should keep in

mind that our methodologies and theory building are inextricably bound, and I predict that, as these two sets of rules interface in one paradigm or research design, we will have an accessible and exciting laboratory in which to build our theory and define our methods. Perhaps we can develop what Warren (1977) calls a diagnostic paradigm, i.e., strategies for dealing with social problems which include the pure research model and the applied research model. These are issues that we have to face and, interestingly enough, that have, to a great extent, been forced upon us by pressures outside our scientific paradigm. Hopefully, we can learn from our mistakes and, in doing so, develop more refined research strategies that allow us to expand our methods and theories as well as our acceptance in American institutions.

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