

## THE SOCIAL ANTHROPOLOGICAL FIELD EXPERIENCE\*

George M. Foster  
University of California, Berkeley

Contemporary social anthropology differs from the other social sciences in its emphasis on the field experience, a data-gathering and theory-generating technique in which, in classic form, the scientist immerses himself in a research setting in a way unparalleled in other disciplines. Over a period of months or even years, he lives a life--24 hours a day--which is quite distinct from his normal home life. Field work as a way of gathering data is, of course, by no means exclusive to anthropology. It is a technique also used by geographers, geologists, botanists, and zoologists, to name a few. But anthropological field work differs from that of these fields in the degree of personal involvement which the investigator must achieve with the local people, and in the psychological adjustments he must make if he is to be successful. In most other disciplines characterized by field trips, scientists do not necessarily need fluent control of local languages; interpreters usually can serve their needs. And while they must establish contact with local people for food and other necessities, their lives usually are rather separated from those of the indigenous population. Probably they will set up tent camps, cook their own food, and otherwise minimize contact with the human element in their field environment.

In other words, although anthropology shares the field trip technique with a number of other disciplines, the nature of field work in these subjects is quite different from that of social anthropology. It is therefore important for anthropologists to understand the special qualities of their field experience, its purposes, the methodological assumptions that underlie it, and the ways in which it influences the development of their theory.

This paper, as the title indicates, deals largely with contemporary social anthropological research, and especially with that kind of investigation we call loosely "community study," which implies a rather long period devoted to a relatively small social group. It is well to remember, however, that the field experience with the love for strange and exotic situations and the sense of adventure that accompanies it, is shared by almost all anthropologists whether concerned with primate studies, prehistory, archaeology, linguistics, or social anthropology. It is, in fact, one of the great bonds which, in a period of increasing diversity of interests, continues to hold anthropology together as a single discipline.

In discussing the anthropological field experience I am not concerned primarily with a description of what happens in a field setting, with the precise techniques for acquiring information, with training for field work, or with personal experiences. Rather I am concerned with more fundamental questions, such as the purposes of social anthropological field work, its basic characteristics, the ways in which anthropological field work differs from the data-gathering techniques of other social sciences, and, finally, the methodological assumptions that underlie our field methodology.

#### WHY FIELD WORK?

Field work is a major component of anthropological methodology, which, although anthropologists do not agree on its precise definition, in function at least is essentially similar to those of all other sciences. In a very general sense we can say that a scientific methodology is a way of marshalling resources to permit gathering and organization of data within a conceptual scheme, in order to formulate and test hypotheses and to explore problems with the ultimate end of discovering regularities which permit prediction. At first glance one might be inclined to say that anthropological field work simply represents the data-gathering aspect of its total methodology, and hence is **analogous to laboratory**

experiments, survey research, and other devices used to acquire information in other fields. But as we will see, the function of anthropological field work is much broader than mere data-gathering; it also plays an essential role in the formulation of problems and hence in the shaping of the major theoretical lines that concern us. This aspect of primary investigation is not, obviously, unique to anthropology, but in the social sciences at least the anthropological field experience seems to lend itself to this heuristic end more completely than do other more elegant research techniques.

We therefore expect our methodology, especially as manifest in the field, to operate at two levels. At the first it is a means to an end, a device to gather data which we feel to be essential in formulating models for sociocultural systems and in testing hypotheses that stem from these models. At this level the test of a good methodology is its ability to produce the pertinent data by means of an economical operation. That is, the simplest methodology that produces the desired results is the best methodology, just as the most economical hypothesis that explains a particular cluster of phenomena is the most elegant hypothesis.

At the second level our methodology is an end in itself, i.e., it serves to suggest new research leads, to point to new problems, to create new hypotheses which in turn require data not originally envisaged. And at this level a good methodology also helps the investigator to know what the right questions are to be asked of his data, to see significant relationships which might otherwise escape him. The best methodology is that which is most heuristic, with the greatest generative force in creating theory.

Anthropologists historically have tended to take methodology, including the field experience, for granted. We have been uninterested in it as a topic, we have been slow to teach it formally, and we have until very recently failed to stimulate student interest in it. Training for field work, too, is a very recent addition to graduate curricula. Anthropologists of my generation had little or no instruction in how to

carry out field investigations. In the late spring of 1937, prior to embarking on my first field venture among the Yuki Indians of Round Valley, California, I asked A. L. Kroeber how I should prepare myself. He suggested I buy a notebook and pencil. At that time it was assumed that a well-prepared anthropology student who had read basic monographs and written seminar papers somehow would know what to do in the field. An unfortunate contemporary analogy to this situation, which someday we also will remedy, is the assumption that a well-trained graduate student somehow will know how to teach, with neither formal training nor advice.

As a consequence of our historic lack of concern with methodology and with training for the field, we have had to turn to our sister social sciences for materials on the formal study of methodology and technique. Participant observation, for example, is the basic anthropological field technique, but the vast majority of analyses and descriptions of what it is, how it is done, and what it leads to are found in sociological sources. Through the field experience, anthropologists sensed that they continually were generating new hypotheses and that field work itself was a major source of theoretical ideas, yet it remained for a sociologist to coin a term for this enormously important characteristic of the method. Although--fortunately, in my opinion--the expression "serendipity pattern" has not caught on widely, it has nevertheless helped us better to appreciate one of the most significant elements in our field experience (Merton 1948).

Social anthropology is in the anomalous situation of sharing with other social sciences the same basic theories, yet the route by which we have reached these theories appears to have been quite distinct from theirs. Anthropology had its origins in a desire to know about the world around us, first from the writings of travellers and missionaries and subsequently from first-hand field investigations. The gathering of data took precedence over the building of theory and, when tenable theories began to appear, they had in almost all instances been suggested by field research. Only in very recent years has the recognition emerged in anthropology that theory has priority over simple amassing of fact and that

hypotheses can be formulated at home, later to be tested in the field. Even today, I suspect, the bulk of our hypotheses are generated from personal, first hand, sensory contact with the real world. I am not sure to what extent this is also true of other social sciences, but I suspect it is much less characteristic of them.

#### BASIC CHARACTERISTICS OF ANTHROPOLOGICAL METHODOLOGY

Implicit in and basic to social anthropological research is the concept of "systems." In this the anthropologist resembles the biologist and the zoologist: regardless of what specific problems or points of theory interest him, he knows he can fully understand them only in the context of the entire system in which they are embedded and of which they are a part. The anthropologist's systems are less precisely bounded than are those of the biologist and zoologist, but we have had little difficulty in identifying them. Historically they have been based on primitive tribes and peasant communities; today the anthropologist's systems also and increasingly include units such as hospitals, medical schools, factories, business offices, and bureaucracies of many types which in turn must be understood in the context of larger systems, the societies of which they are a part.

A concern with systems analysis is of course not limited to anthropology, although it seems to be the first social science in which it appeared. In anthropology it is probably best dated from Malinowski's functional approach, beginning about 1920, and it continues to be the dominant aspect of our conceptual framework. With growing anthropological interest in dynamic processes (as contrasted to function and structure), the anthropological view more and more resembles that of the much newer "systems approach," first used in military planning and strategy in World War II and used increasingly today in corporate and business planning, in government operations, and in other fields. Jensen's recent description of systems analysis reveals this similarity: "Systems analysis permits a whole view of a problem; it takes into account means and

ends, choices and alternatives; it makes use of prediction and advanced testing to suggest objectives and course of action. A likely first consequence of systems analysis is a restructuring of the problem itself. But, most importantly, systems analysis is a four-dimensional approach: it states explicitly that the problem is future-oriented" (Jensen 1967: 1406. Emphasis added).

The historical developments which gave rise to the systems approach in anthropology are well known to anthropologists and need little elaboration. The early desire to know the world about us and to study and record the ways of life of vanishing peoples, combined with the relative lack of field workers, meant that the anthropologist lucky enough to be in the field saw his task to include the entire spectrum of culture he found: economics, religion, social organization, material culture, folklore, language, and much more. The assignment was total: find out everything there is to know about the people studied. Inevitably the anthropologist found that no institution or trait could be understood in isolation, any more than an organ of the body can be understood alone. The nature of the anthropologist's data-gathering problem, and his own limited resources, ultimately forced recognition of an organic view of society. Full comprehension of the importance of this view did not, however, spring into being overnight. American anthropologists from Boas to Kroeber and Lowie worked out a general concept of culture as an integrated whole, but it remained to Malinowski and his students fully to develop a systems concept. The explanation, I think, lies in the then new field technique of long residence in a small community combined with participant observation and control of the local language. Real appreciation of how the parts of a society fit together comes only from this experience, which the early American anthropologists never had. Much of our contemporary theory as well as our basic approach to problems--via systems--are thus seen to be a consequence of a new concept of the field experience.

Although other social sciences are by no means lacking in the concept of systems, and one--psychiatry--is based upon it, it is the

thing which most distinguishes anthropology from its closest sister disciplines, social psychology and sociology. Speaking specifically of mental health, Rapoport points up this difference:

The anthropological approach tends to be more holistic than those of its social science sister fields (i.e., it seeks to interrelate all aspects of the social and personal situation, and to relate sociocultural phenomena to linguistic, historical, and biological data as well as the narrower spheres of interest usually demarcated by sociologists).

Its approach is usually observational. It asks, "What is going on here?" rather than, "What can I demonstrate about the relationship in this situation of certain variables I have conceptually abstracted and for which I have developed precise instruments of measurement?" (Rapoport 1963:1899-1900).

Weiss contrasts this holistic approach with the "analytic" approach; he finds each suitable to special types of problems so that, taken together, they are complementary rather than competitive:

In the holistic approach the investigator sees a complex situation as containing within itself, perhaps hidden from view by the action of extraneous variables, a system of interrelated elements constituting its underlying structure, in terms of which the phenomena of the situation are to be understood. He is concerned with identifying the nature of the system rather than with focusing on particular independent-dependent variable relations. He will tend to explain particular phenomena in terms of the action of the system rather than in terms of some intersection of causal factors.... His chief interest might be phrased as "Taking it all together, how does the whole thing work?"

...In the analytic approach the investigator is prepared to see a complex situation as a tangle of related elements. He takes as his task the isolation of elements from each other, or, perhaps, the identification of a small number of linked relationships. His investigative procedure will involve the identification of independent, dependent, and intervening variables; the assessment of the direction and strength of their linkages; and, perhaps, the assessment of the possibility that the strength of a linkage may be modified by the action of elements not part of the linkage.... The analytic approach sees interrelationships among elements as meaningful quite apart from the given situation--as conceptually independent of the given situation (Weiss 1966:199).

## SOME SPECIAL CHARACTERISTICS OF ANTHROPOLOGICAL FIELDWORK

The outstanding characteristic of the field experience is that the anthropologist as a primary investigator must participate in data-gathering and be immersed in a total field setting to a much greater extent than other social scientists. Most often a single anthropologist or a married couple will be alone for a year or longer in a tribal or peasant community, speaking the language of the local people, coming to know them as friends, visiting in their homes, being present at their social and economic and ritual activities. Contact with people from their own country or with local nationals of comparable professional training or status will be minimal. This is at best a difficult psychological situation. Initially an anthropologist is by no means sure he will be accepted by the people he proposes to study. He is, in fact, at their mercy. He must gain their friendship and confidence if he is to obtain the information he needs and on which his future professional reputation depends. They thus have enormous powers over him, in that they can confer or withhold what the anthropologist has come to the field to obtain. To gain the confidence of people and to establish rapport takes much time, usually months, so that field work cannot be speeded up, and in the beginning an anthropologist cannot be sure he will obtain the data he so desperately desires. Not infrequently, after a greater or lesser period, anthropologists have had to pull up stakes and start in another community simply because they could not "crack" the one in which they had expected to work.

There is, then, a personal factor in anthropological field work largely lacking in other social science research. The field anthropologist is forced to look at himself, to examine his motivations, to be psychologically honest with himself to a degree that does not seem to characterize researchers using more precise and formal methodologies. Many anthropologists upon returning from their first long field trips feel they have undergone something akin to the psychiatric experience.



The emotional pressures under which almost all anthropologists labor in the field frequently break out in the form of culture shock. This is the psychological malfunctioning experienced by most people who are uprooted from their own societies and set down in strange societies where they have a task to accomplish, but where they do not yet recognize the local cultural cues that must determine their responses. In some degree every anthropologist, I am convinced, experiences culture shock, at least during his first field experience and not infrequently on subsequent trips as well. Not all anthropologists are temperamentally suited to the field, and some fail completely when put to the test. Fortunately, their numbers are few. In part this is due to today's better field training, and to the recognition by new field workers that depression is a common experience at some period in the field. To have a name for the malady--culture shock--and to realize that all other anthropologists know what one is undergoing are enormously helpful in reestablishing a healthy mental condition after shock has occurred.

Strange as it seems, until from 10 to 15 years ago anthropology students were not warned of the psychological changes they should expect in themselves, and when they found the going difficult, depression overpowering, and self-doubt mushrooming, they assumed they were different from and less able than other anthropologists whom they had seen return from successful field trips in a state of euphoria.

In part the infrequency of field failure among anthropologists also stems from temperament. People who are drawn to the exotic and who long for experiences outside the narrow confines of daily life are the types that tend to select anthropological careers. Most anthropologists like field work and look forward to the periods when they can get away from home and office to experience the thrill of finding new data and seeing new ideas and hypotheses and insights develop from one day to the next. For most anthropologists, field work is not just a necessary but boring preliminary to the main task of writing: it is the most exciting part of the whole research process, and the writing itself sometimes

seems to be the price one must pay for the privilege of the field experience. Most anthropologists prefer gathering new data to preparing for publication the results of earlier field trips. This temperamental difference was borne in upon me when, for several years, I was a member of the University of California's Institute of Social Sciences, which distributed small research sums to faculty members. Most social scientists asked for money to hire research assistants to go out and gather data. Almost without exception the anthropologists asked for money to go out and gather data themselves!

But is not the anthropologist's insistence on participating directly in field work, and in spending long periods away from his office, a conceit that only he can do the job? Is it not at least a confession of his failure to develop more efficient data-gathering techniques so that much of the grubby work can be left to research assistants? I think not. Anthropologists have in fact made a good deal of use of field assistants and, as quantitative methods become more important, more use is made of them. But the general employment of field research assistants as replacements for the primary researcher will not occur at any time in the foreseeable future, because the field experience is much more than a data-gathering act. It is, as earlier pointed out, the principal source of theory generation. Theory usually is not generated by paid assistants, and when it is they are scientists themselves who, like the anthropologist, take leads from their first-hand knowledge of the research setting. All anthropological field research has an exploratory quality, whatever the specific problems may be. That is, the good anthropologist continually is on the alert for new data and for new interpretations of what he already knows. This is what Merton meant when he spoke of the serendipity pattern: "The serendipity pattern refers to the fairly common experience of observing an unanticipated, anomalous and strategic datum which becomes the occasion for developing a new theory or for extending an existing theory" (Merton 1948:506). The recognition of strategic importance, of course, is not likely to come unless the researcher already has

a rather complete knowledge of the system, its elements, and how it functions, and is on the spot and sees the event himself. Time after time anthropologists have found that a chance event, a near subliminal happening, has proven to be the key to a major hypothesis or has permitted an important insight into the system being studied. Two personal experiences in Tzintzuntzan will illustrate this point.

1) One evening it dawned on me that Doña Micaela González, in whose home I have lived during field trips over ten years, when kicking the dog out of the kitchen spoke to him in the formal third person instead of the informal second person mode of address. I had probably seen her do this a thousand times, but until that moment the significance of the act did not occur to me. Grammar books tell, and most personal experience confirms, that animals, like children, are always addressed informally. My astonishment at this puzzling event started a rapid train of thought in which other speech usages flashed into mind, and within a very few minutes the theoretical ideas in "Speech forms and perception of social distance in a Spanish-speaking Mexican village" (Foster 1964) had fallen into place. The serendipity phenomenon had provided me with an hypothesis which related a great many behavior forms which previously had existed--for me--in isolation.

2) One morning I noted that, when a hundred school children came to Doña Micaela's house for a school-sponsored breakfast, they all ate in near silence, a striking contrast to what one might expect of a similar number of American children under the same circumstances. I commented on this, expressed my astonishment, and was told by Doña Micaela that of course they ate in silence, that they were trained to do so. Again the wheels started to spin as I groped for an explanation of this--to me--most unusual behavior, and in a short time the theoretical hypotheses in "Cultural responses to expressions of envy in Tzintzuntzan" (Foster 1965) had been blocked out and a great deal of seemingly trivial data, much of it in my notes for up to 20 years, fell into place in the context of an hypothesis about envy.

In both instances, once the initial hypothesis was formed I asked myself, "In what other areas of behavior might it be logical to expect acts that will fit this hypothesis?" As a result of the heuristic dynamic of a new hypothesis, I uncovered previously unnoted and unanticipated data, which proved to conform to the hypothesis.

#### METHODOLOGICAL ASSUMPTIONS IN ANTHROPOLOGICAL FIELD WORK

In all professions and scientific disciplines, certain basic methodological assumptions underlie the definition of goals and the mode of operation which it is believed will lead to these goals. Anthropology is no exception, and in our field work and basic methodology we see premises and assumptions, some of which we share with other disciplines and others of which are more nearly our own. Some of the more important ones follow:

1) Relationships between race and culture: Anthropologists believe cultural variation is explicable in cultural and historical terms and not on the basis of differences in average racial abilities. The reason for the rise and fall of empires, for the cultural backwardness of some peoples and the advances of others, are to be found in a complex interplay of social, cultural, geographical, environmental, and historical factors, rather than in genetic differences.

2) The case study method: Most social anthropological research constitutes case studies. Whatever the object of the anthropologist's analysis--a tribal group, a peasant village, or a factory--he assumes that, while in some ways he is dealing with a unique phenomenon, in the wider sense he is studying a representative of a generic type. Hence hypotheses and generalizations developed from a single case have heuristic value in that they provide a conceptual framework within which questions can be asked about other systems suspected of being of the same general type as that first analyzed. The hypotheses stemming from a single case can thus be tested and confirmed, modified, or rejected when applied to other similar cases, and the generality of those that survive can be established.

3) The comparative approach: This is a much misunderstood term. The so-called comparative approach, usually of individual traits or culture complexes torn from context, underlay the nineteenth-century anthropology which led to the evolutionary theory characterizing that period. After this theory went out of vogue, first under the attack of Boas, the comparative approach fell into disfavor. However, when anthropology advanced to the point of recognizing that knowledge of process (rather than of specific historical sequences) and of generic structures and functions--social types, in a word--was its primary concern, the significance of the comparative method became clear. Hence we feel today that fully to exploit the results of any field study these results must be compared with those of other studies. Contemporary comparative analysis already has told us much about the structure of societies in general, and it has contributed particularly to our knowledge of basic change processes. Just as years ago we began to recognize that individual behavior forms and institutions could be understood only in the context of the entire social system, so today we accept the fact that a single social system and its elements can be understood most fully only in the comparative context of other societies, in which similarities and differences can be noted, and in which, above all, regularities in process can be delineated.

4) The potential significance of all data: Since they believe that a culture or social system is a logical, integrated, holistic phenomenon in which the parts fit together in meaningful patterns, anthropologists assume that every bit of data and all forms of behavior in the system have meaning which ultimately can be fitted into patterns and hypotheses. This is what Rapoport means when he writes that "The anthropologist's approach is also to be distinguished from that of academic psychologists and sociologists by the anthropologist's fascination with the irrational and nonrational aspects of social life" (Rapoport 1963: 1900). Anthropologists have been criticized for their "vacuum sweeper" approach to data-gathering. This criticism, while doubtless justified in some instances, shows lack of understanding of the anthropologist's

definition of problem and of his view of social systems. Obviously no anthropologist assumes that all the data that enter his file boxes are immediately pertinent to theoretical problems; he knows in fact that much of what he records probably never will be used in a theoretical formulation. What he does believe is that the time and place may come when data previously thought insignificant will acquire great importance. Hence it is best to be as complete as possible in taking notes. I first discovered in 1940 that Mexican villagers on a trail, as soon as they are within earshot of each other, greet people with adiós. It was not until 22 years later, when Doña Micaela addressed her dog as Usted while simultaneously kicking him on the rump, that this apparent reversal of linguistic usage--"goodbye" instead of "hello"--made sense to me.

Events like this, I think, explain the anthropologist's wide-ranging and insatiable curiosity about almost any form of behavior, which other social scientists often find difficult to understand. Such curiosity does not reflect lack of mental discipline, a purely antiquarian bent; rather it is basic to the anthropological method, essential to the best field work.

5) The use of multiple data-gathering techniques: No single data-gathering technique is adequate to the anthropologist's task of acquiring as many and as varied data as he can. The open-ended or unstructured interview with an informant continues to be his single most important technique. The assumption underlying this method is that a few people telling the anthropologist a great deal will produce kinds of data not obtainable by a great many people telling him relatively little. The open-ended interview functions in much the same fashion as the psychiatric interview: the informant, like the patient (and not the anthropologist or the psychiatrist), defines the problem areas, although it is unlikely that he is aware of what he is doing. It is the task of the anthropologist, like the psychiatrist, to recognize the significant clues which, on the basis of his training and experience, permit formulation of hypotheses that ultimately should lead to success.

The "key" informant is an invaluable ally; the best field work cannot be done without him. However much one is part of a community, however fluent in the language he may be, and however free his access to community activities, the time comes when the anthropologist must sit down with a knowledgeable and willing person who in systematic fashion will answer his questions.

But the key informant technique, if overdone, produces a culture of informants rather than a culture of society. In addition to living in a community and looking, listening, asking casual questions, and working systematically with a few people, the anthropologist will use a great many other data-gathering techniques which provide information not otherwise available. These may include the taking of a census, the use of projective tests, the recording of life histories, and the search for local statistical and historical materials. Increasingly anthropologists use questionnaires to get at special problems. These, however, they tend to see as adjuncts to other methods, as devices to quantify special problem areas, rather than as the primary data-gathering technique itself. The relative superficiality of survey research techniques makes them unsuitable in most anthropological settings, except for certain limited goals and usually after the basic culture is rather well known.

6) The use of native languages: Anthropologists assume they must speak and understand the language used by the people they study if their research results are to meet today's exacting requirements. To live, probably as the only representative of his society, in an exotic community for a year or more means the anthropologist, for sheer survival and communication, must learn the local language. Although much early field work was done through interpreters, today's social anthropological field research cannot be done other than on the basis of good linguistic control. In addition to being able to ask questions and understand answers an anthropologist must, so to speak, eavesdrop: he must be able to understand what people are saying when they forget his presence, when they talk among themselves oblivious to his reaction. When on one occasion

Don Melesio, Micaela's husband, asked her, "When did Christ walk on this earth?" and when, after much discussion they decided it was a little before the appearance in Mexico of the Virgin of Guadalupe--which they promptly and correctly informed me was Saturday, December 9, 1531--I learned something about their concept of time depth that could have come in no other way.

But beyond these obvious uses anthropologists increasingly see language as an indispensable probe which permits them to penetrate more deeply into the culture and psychology of a people. When, as in Tzintzuntzan, people continually speak of life as una lucha, a battle, a contest, they are telling much about their cognitive orientation. When they use euphemisms for what to me are not sensitive areas of culture, they show that these are points they find psychologically disturbing. When, again as in Tzintzuntzan, there are three times as many adjectives that describe character adversely as those that describe it favorably, the anthropologist learns much about a guarded view of human nature and personality. Language, then, is an indispensable prerequisite for the anthropological field experience. Without it the researcher must confine himself to very special problems.

#### THE FUTURE

A field methodology is not static. During the past century, field methods and the assumptions that have underlain them have evolved through several stages, and this evolution obviously will continue. Anthropological field research came of age with Malinowski, with the pattern of participant observation over a period of one or more years, in contrast to the relatively short trips that previously had characterized field work. This permitted an analysis in depth absolutely impossible with lesser methods. Curiously, nearly a generation elapsed before the significance of Malinowski's discovery was fully appreciated in the United States, and only since the end of World War II has the year-long social anthropological study become the standard for American Ph.D. candidates. By British standards this is still too brief a period.



What are the next steps? How can our contemporary social anthropological field methods be still further improved? An important next step, I think, was first worked out by the late Clyde Kluckhohn: a series of field trips, many of relatively short duration, but extending over decades, to a single people (in his case the Navaho). This system permits gathering of data of a richness impossible even with a three or four year trip. More important, it permits time between field sessions for the anthropologist to digest his data, to develop new hypotheses, to note questions he must ask on his next trip. As his theoretical concerns develop, and with increasing maturity, he will find that he has a ready-made laboratory in which the ground work has been laid, in which he can test hypotheses in economical yet comprehensive fashion. The time and effort he has invested in a single community becomes a source of wealth too great to be abandoned after an initial major study. Malinowski's discovery of the extended field trip was the first great revolution in anthropological field methodology. A second is just getting under way: the recognition that many anthropology students in their first long studies have acquired lifetime responsibilities. They will expect, and their colleagues will expect them, to keep their communities under more or less continuous observation as long as they do field work, in order fully to exploit the multiple opportunities for theory building which now exist in our field.

Long-term responsibility does not mean anthropologists will concentrate only on that community to the exclusion of others. Many will certainly want to make other studies, some of which may be fairly complex in themselves, simply for perspective. But it does mean that some anthropologists will want to identify themselves with a community which in fact becomes a field laboratory. Some of these laboratory communities, and the files of data that go with them, will be handed down to succeeding generations of anthropologists so that in time we will have what is denied us today: a century and more of detailed, continuous observation on the life of a single group. Think what we would know about process if

Tepoztlán had first been studied in 1870 and continuous records had been kept to the present!

A second major step in social anthropology which will modify greatly the field experience will be increasing interest in systems within systems, in bureaucracies, social movements, factories, offices, retirement communities, and the like. Much of this work will be viewed as applied or semi-applied, in that it will provide more cues for social and economic action than does most contemporary anthropological field work. It is quite likely that ultimately more anthropologists will engage in this kind of research than in analysis of peasant and tribal communities. Basic methodological assumptions will, I think, continue to be similar to those of today, but obviously much of what we view as unique to the field experience will be lacking. Anthropologists, like today's sociologists, may continue to live at home in an academic environment while doing research. They will be little bothered by culture shock, control of foreign languages will be less essential than today, and amoebic dysentery and other field hazards will be less a topic for after hours conversation. It will be a different kind of field experience, but it will be good anthropology, and it will reveal things about societies and cultures and their dynamic processes that we are not learning with today's field experiences.

#### NOTES

\* This paper is based on notes used in a lecture, "The Anthropological Field Experience," given at a regular meeting of the Departmental Seminar of the Anthropology Department of the University of California (Berkeley) on October 30, 1967.

## REFERENCES

- Foster, George M.  
1964 Speech forms and perception of social distance in a Spanish-speaking Mexican village. *Southwestern Journal of Anthropology* 20:107-122.  
1965 Cultural responses to expressions of envy in Tzintzuntzan. *Southwestern Journal of Anthropology* 21:24-35.
- Jensen, Neal F.  
1967 Agrobiology: specialization or systems analysis? *Science* 157:1405-1409.
- Merton, Robert K.  
1948 The bearing of empirical research upon the development of social theory. *American Sociological Review* 13:505-515.
- Rapoport, Robert N.  
1963 Social anthropology and mental health. In *The Encyclopedia of Mental Health*. New York: Ranklin Watts, Inc., pp. 1896-1906.
- Weiss, Robert S.  
1966 Alternative approaches in the study of complex situations. *Human Organization* 25:198-206.