

URBAN FIELD RESEARCH IN THE UNITED STATES:

THE LONG JOURNEY FROM THE EXOTIC

TO THE MUNDANE

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There can be little doubt that this is a time for hard and considered review and criticism of the goals and conscience of our science. Hopefully such systematic, articulate criticisms by younger anthropologists as those represented in this collection will contribute to more rapid culture change than has been typical within our discipline. Speaking from a historical perspective, the field has changed remarkably even in the past five years with a great upsurge of interest in problem or action-oriented research. Certainly this is apparent in the balance of papers presented at national meetings. Nonetheless, we are all aware that the offering of more applied courses or even of those dealing with urban anthropology or contemporary issues is still meager or even non-existent in some of our larger, more prestigious departments. There one sees still full-blown the retention of what Mitchell (1970) has termed "the Blue Lagoon Personality," the proclivity for work with the remote and the exotic. Traditionally the turf has defined the discipline for many.

Until just a few years ago urban research in our own country was looked upon with great suspicion. Urban anthropologists were seen as encroaching upon the domain of sociology or as ill-concealed social workers, either of which possibilities was a damning invective. Now we find ourselves the "hapless" victims of nationalism and fluctuating research budgets. As the traditional societies of the world tell us to go home, even if we have visas in hand, we have not the money to fly around the globe to more traditional settings. Thus nearby urban studies become more attractive to faculty and graduate students alike.

There have been a few early traditions of concern for social relevancy in student training. I personally was fortunate to have been swayed during my own graduate years by Sol Tax. As early as thirty years ago at the University of Chicago, which still remains largely a bastion of pure research, Tax began to inculcate in students a deep concern for ethical problems in general, and a desire to seek solutions for some of the social and economic

inequities of our society. Tax and a group of students working with the Mesquakie or Fox Indians of Iowa in 1948 began building a theoretical and practical model of research procedure which they called "action anthropology." The record of the integrity of the intellectual struggles of Tax and his students to develop the model and explore its consequences for the Mesquakie, for themselves as faculty, students, and scholars, and their probings for the limits of the interested compassion allowable for researchers, have made action anthropology an attractive model to me for application.

The Documentary History of the Fox Project (Gearing 1960) chronicles these struggles through a record of self-searching letters and varied other documents that resulted from over ten years of the project from 1948-1959. Essentially, Tax and his students saw "action anthropology" as an enterprise totally different from applied anthropology in terms of values and the role of the anthropologist. In the field, Tax and his students immersed themselves in the political and economic problems of the Fox Indians and then returned to the classroom to engage in heavy, introspective ethical discussions about the implications of their actions or non-actions in regard to both their informants and to our science.

By 1951 Tax had worked out the model which has characterized a great variety of his endeavors to the present. Stated Tax:

Some years ago in this journal I published a paper . . . in which I argued that since the scientist is oriented toward solving problems of theory, while the administrator or consultant is directed toward solving practical problems, the applied anthropologist must sacrifice one or the other. The scientific researcher looks to the general, while the administrator needs to know the particular; to combine the two requires the sacrifice in some degree of one or the other or both. I am now in effect reversing that position, and arguing for an activity that pursues both ends equally. I call it "action anthropology" and it requires that the anthropologist move wholly into a problem situation--say an Indian tribe or community which is in trouble--and work independently to diagnose and to treat the difficulty in all of its aspects, and to do so as a research anthropologist.

By definition, action anthropology is an activity in which an anthropologist has two coordinate goals, to neither one of which he will delegate an inferior position. He wants to help a group of people to solve a problem, and he wants to learn something in the process. He refuses ever to think or to say that the

people involved are for him a means of advancing his knowledge; and he refuses to think or to say that he is simply applying science to the solution of those people's problems.

Whether action anthropology is a variety of applied anthropology, or something quite different, depends on one's conception of applied anthropology, which is itself changing rapidly. If applied anthropology presupposes a body of scientific knowledge--compentend empirical proposition--developed by theoretical anthropologists and awaiting application to particular situations when we are asked to do so by management, government, administrator or organization, then action anthropology is far different. For one thing, the action anthropologist can have no master; he works as a member of the academic community. For another, the action anthropologist realizes that his problem is less the application of general propositions than the development and clarification of goals and the compromising of conflicting ends or values. In fact, the action anthropologist finds that the proportion of new knowledge which must be developed in the situation is very great in comparison to old knowledge which he can apply. He is and must be a theoretical anthropologist, not only in background but in practice (Gearing 1960:168).

While I was not personally involved in the Fox Project, I was an eager student in Tax's later "action" seminars, and participated in the American Indian Chicago Conference, an action project conceived and facilitated by Tax--a remarkable and unique coming together of American Indians to chart a statement of purpose for their future (Lurie 1961; Ablon 1979). Tax's unswerving concern for the socially significant, and his pervasive sense of conscience in research procedure has vitally influenced my own work and that of his other action students (for examples, see the varied papers in Henshaw 1979).

I essentially would like to address my comments in this paper to two areas of concern in urban fieldwork: substantive and methodological. What are the new subjects of study for anthropologists in contemporary urban life, and what are some of the new issues we face in studying these subjects?

Subjects for Study

Substantively, anthropologists have an incredibly broad choice of significant field research projects in our cities: issues

dealing with ethnic and sub-group relationships; sources and issues of economic disparities; health-related problems and institutions; the definition of social and political networks which may function to disenfranchise the poor, or those which offer support for the ill, the frightened and desperate. Studies in these areas may well provide empirical bases for attempts to change laws and public policy. For example, in the field of medical anthropology, I will mention a recent dissertation by a former Berkeley student--Jeanie Kayser-Jones. Kayser-Jones' dissertation research compared nursing home care for the aged in Scotland with that in the United States. She studied staff-patient interaction and the institutional milieu through participant observation informed by previous clinical training in nursing. She essentially provided an analysis of institutional barriers to quality care for the elderly in our country. The vivid examples which emerged from her participant observation activities challenge the emotions as well as the intellect. I have no doubt that publications resulting from this study (Kayser-Jones 1979, in press) will have great potential for changing policies and licensing procedures. Spradley's well-known research on alcoholic "urban nomads" in Seattle not only resulted in one of the few anthropological classics dealing with contemporary American life and culture, You Owe Yourself a Drunk (1970), but, perhaps less known to its readers, also was responsible for changing Seattle's policies in relation to the treatment of chronic public alcohol offenders. Obviously, not all of us will study outrageous or dramatic activities and settings, but we can contribute our own pieces in our own ways.

For example, I recently was asked by a high level mental health administrator if I could provide him with a study of an ethnic group relatively new to his district. Preliminary therapy sessions with individuals and families from that population indicated to therapists that in order to intervene intelligently in problematic situations they needed to know more about decision-making patterns, normative sex role prerogatives, and the role of extended kin in family life. Granted, this administrator is one of a few that we might consider a rare and enlightened breed, but he continually has expressed interest in new anthropological findings relating to his district, and no doubt will use them in the same fashion as he has used the results of previous work of my own dealing with other populations there. I am obliged to add that this administrator does not have funding for such a study; however, he would cooperate by providing valuable case records and informant families for beginning the research. No doubt other administrators could do the same in return for research findings that will assist them in delivering more effective services.

I see as a mandate that we begin to study people and institutions with power, not just the powerless, even though the latter may be more appealing to us. Anthropologists have not studied up, as advocated by Nader (1969), nor have they studied horizontally, that is, the also powerful middle classes with which I have been involved. Anthropologists in cities by and large have chosen to

study minority groups, the poor, or some variety of ghettoized populations. While it may be more congenial to anthropological tradition to study the underdog, I contend that studies of the upperdogs or even the middle dogs will provide greater research possibilities for provoking change (Ablon 1977).

Methodology

Field work today in all contexts is more complex than it was years ago. But, I think, particularly for those working in urban areas, be they students or graduate anthropologists, the pitfalls and the complexities are greater than they ever have been. I would like briefly to point up here a few of these pitfalls as I see them, particularly those which are relatively new because of the parameters and conditions of urban field work, as opposed to that in more traditional rural settings. There are no glib directives or strategies for encountering these problems in the field, but if they are talked about and their complexities explored before anthropologists go to the field, it will certainly help. When I was in graduate school, no one on any campus talked about the nitty gritty logistical realities of field work. There were no courses in field methods. It was supposed that somehow when you went "to the field"--suffered through and survived that magical rite of passage--your field abilities would emerge through some form of "immaculate conception." Anecdotes regarding the state of one's liver or particularly dangerous or humorous situations might be related at parties, to be sure, but by and large, systematic information on the realities of field life, or the successes and failures of field workers (hence some accounting of their methodologies) were not exchanged either informally, or formally in print. Today there are numerous books dealing with field methods for all stages of research, and personalized accounts of field work abound. While many departments offer methods courses, preparation for urban field work is a particularly challenging task, and many complex dimensions of urban work still are infrequently and inadequately addressed.

For instance, even the initial definition of the parameters for a study might be a very complicated conceptual problem. When one works in a village, even though the larger encompassing economic, social and political frameworks are constant realities, one still can comprehend this village as a physical unit. The basic features of "the little community" as defined ideally by Redfield (1955)--distinct geographically, small, homogeneous, and self-sufficient, sharing cultural patterns, and systematic social interactions between members--all did exist in greater or lesser degree or relationship. Thus such small communities have traditionally constituted feasible and manageable units for examination by the researcher or several researchers. One is able to hold some hope of homogeneity despite considerable variance, and hence there exists generalizability for description and analysis. In an urban area one must think hard about comprehensible and meaningful units.

Consider the choice of a neighborhood or an ethnic group or some other social "unit" for study. Such segments of urban life may be very hard to deal with analytically. While the unit or population might have some homogeneity characteristic to its members, one must be continually aware that this unit or population is only one segment of much greater and more complex social, cultural, political contexts. Weaver and White (1972) have presented a rare and highly useful discussion of such relevant issues as scope, scale, and density that must be considered in research in urban and complex societies. Examples of field units of study from my own research will illustrate some of the more obvious dilemmas that plague anthropologists who still have in mind the traditional "community" in which anthropologists have worked in the past.

In one research situation, I studied an urban junior high school with a very mixed and troubled student body and faculty--populations which were highly diverse and lived in various areas of the city. While legally forced to cohabit an ancient building seven hours a day, these two populations were at moral and cultural loggerheads with one another. At 3:00 each afternoon both faculty and students rapidly fled the structure to return to totally different social worlds. Now, analytically, within the conceptual and methodological rubrics of traditional anthropology, how does one deal with all the factors inherent in this situation? Indeed, how anthropologists tend to study more multi-level hierarchical structures such as health or educational institutions where there are "captive" or forced populations such as patients or students and often very different controlling or bureaucratic populations. Neither moieties nor other traditional forms of dual or multiple organization or social structure characterize these social systems.

Or how does one think about a Catholic parish of 5000 people dispersed throughout a common geographic area (containing an equal number of non-Catholic residents), coming together for church activities, yet working in diverse economic and sub-cultural milieus? Or how does one comprehend a self-help group for members who come together for assistance for a shared painful health condition, but who live within a hundred-mile radius and span a broad spectrum of social, educational and economic backgrounds?

What happened to "the little community"? Are the above part-time communities, or partial or multi-communities? Now of all times, I am prepared experientially to ask for Robert Redfield's wisdom and advice about what seem to me to be extremely complex conceptual issues. When I sat in his exceptional Seminar on "The Little Community" those many years ago as a totally innocent graduate student, I had never been to the field and paled at the thought of my first informant. Steeped in British social anthropology, how could I have dreamed that one day I would be studying families of California police inspectors?

Turning to very practical issues, there are many personal pitfalls working in our own society. When one is miles away from

home, he/she is able to avoid a great many personal issues of field work dealing with lifestyle, values and emotions. I have written elsewhere on this topic in more detail (Ablon 1977). We certainly do not talk enough about such issues in classes. The fact is that in our own urban areas we are very vulnerable to scrutiny. Our own personal visibility is greatly increased. People do inquire about us personally. Our informants want to know how much money we make, where we live, whom we live with, what our social life or sex life is--and they will certainly ask us. If they are middle and upper class people they are even more likely to ask more pointed questions. Furthermore, they, living close by, may utilize the same methodology of systematic observation to cross check our answers to their questions as we use to study them.

There is the issue of value conflicts with informants which may not be as burning when one is far afield because the issues here have to deal with our own contemporary political and ethnic problems. Racism and conservative values may be more apparent and repugnant to us here than when equivalent values appear in a society remote from our own personal world, especially if we have been activists in working for social reform, as have many anthropologists.

Indeed, I think such issues should be discussed to help students decide the kinds of projects they wish to undertake, and to consider the distancing which is characteristic of the anthropologist-informant relationships of alternative field work modalities. Projects more typical of survey-research modes or strictly formal interviewing in hospitals or institutions will preclude the kinds of intimate relationships that field anthropologists have traditionally maintained in small communities. For me, the richness of these latter relationships have their own unique and personal rewards, which have outweighed some of the problematic aspects inherent in ongoing urban field activities.

Aspects of "community" field work which could well be problematic are evident in issues surrounding reciprocity. In my own field research I have been very concerned with reciprocity. Particularly for those of us who work within areas of medical anthropology, our informants may share with us intimate details and feelings about very sensitive topics. These painful realities of their lives, then, constitute the principal body of data, the protoplasm, if you will, of which our dissertations, our articles, our books and ultimately our careers are made. How do we show our appreciation for such important gifts? In our traditional research contexts we have assisted informants by giving them medicines, needed supplies, transportation, or other goods or services that we had at our disposal. In ghetto areas anthropologists have helped write grants for community action programs and acted as translators or ombudsmen. As we now work with middle or upper class populations, we find there are fewer things we can do for our informants. They often do not need us or our services. Some informants contribute to our research for altruistic purposes, but I doubt that

the majority ultimately assist us for these reasons. Many become our staunch informants and collaborators in research because of their need or desire for the friendship of a person to whom they can talk candidly and who will keep their confidences. I have written of the complexities surrounding the surrogate-therapist role that many of us might fall into unless we are very careful (Ablon 1977). But beyond this I would like to point up some of the major responsibilities inherent in this very special friendship role. Foster and Kemper (1974:3) have written of the friendship dimension anthropologists have maintained with informants--certainly one of the unique features of our methodology:

From the beginning anthropologists formed close ties with the people they studied, and almost all anthropologists of that period have written affectionately about their key informants, some of whom became lifelong friends. Anthropologists quickly realized that the best and most accurate data come from persons who like and trust them. Hence, "establishing rapport" came to be an anthropologist's first assignment upon arriving in the field: to search out the most knowledgeable individuals, present oneself to them in a plausible and empathetic role, and make friends. . . . Today, in cities as in rural areas, most anthropologists retain this basic philosophy: good rapport with good friends, trust and confidence, and abundant conversation over long periods of time.

The implications of being "good friends" with informants in urban areas where we live have not been explored nor experienced by many anthropologists over a longitudinal period. I address this point from a perspective of almost twenty years of urban research in the same area. The complications can be enormous. For example, a consideration which becomes more significant with time is the accretion of numbers of informant-friends who still desire and expect us to maintain our relationships and be present for important social events of their lives--these same social events which were at one time critical for the gathering of our data.

In some regards, it is certainly easier to be a "lifelong friend" of an informant in Mexico or Asia than of one who lives a few miles away. If indeed one major aspect of reciprocity that we have to offer is our friendship and attention, how can we shuffle this off when we move on to study other groups? The fact that such new friendships are made and cultivated on our work time is no excuse to drop our informants from past projects as we move on to other ones. Yet when our own personal social hours are so limited can we afford to cube the number of our friends each several years? For example, in a recent one week period I was called by persons who represented four informant groups I had worked with, asking me to attend functions of personal importance to them. In one case the

research with that person's group had ended sixteen years ago, in another nine years ago, and in the other two cases, three years ago. These invitations were over and above the responsibilities of my ongoing field work. Refusals in these instances may be very hurtful. Granted, receiving this many invitations in one week is unusual, but the example makes the point. This kind of personal dilemma which confronts anthropologists who, perhaps perversely, have chosen to work in the traditional personalized fieldwork format, has to my knowledge not been a recognized topic of discussion or serious consideration.

Another kind of potential pitfall of urban field work is the proximity of distraction. In a remote setting, the anthropologist is thrust into a situation by himself or perhaps with one companion. He is forced to concentrate only on the cultural group he is studying and immerse himself in it. In urban American settings one is constantly diverted. I speak to this consideration with experience in both kinds of situations. During all of my research projects in this country since my dissertation work, I have been in a university position which required that at the same time I was conducting my research, I also had to be on a campus at least part of the time. Thus, my dress, my values, my intellectual concerns, and my psychological being had to be divided in very diverse, and often competing directions. It was a classic double bind, and sometimes I felt I was exhibiting the appropriate response that has been documented for this situation--near schizophrenia! One might also have to fight temptations to flee the field work scene. One can have refuges and diversions in our own cities, such as going to a movie or just saying, "The hell with it! I'm going to see some friends tonight instead of doing field work. I'm tired of the hassles." The availability of these "outs" may be a severe threat to systematic field work especially when we encounter a difficult situation.

Lastly, there is the overarching issue of how one trains real anthropologists in our own society--in our own culture. In the past, the traditional hallmark of anthropological training was the throwing of the neophyte into a culturally different situation because that essentially was the chief preparation for what he would be doing throughout his career--studying culturally different people. This raises a hard question--are we producing competent anthropologists if they have not been trained in studying another culture? Perhaps it is no longer a heresy, but rather a reality to ask, if anthropology students today plan to spend their careers working in our own society, is it necessary to train them as if they will be working in an exotic cultural situation? We, at this late date, must not allow the turf to define the discipline, as it did our elders. Our concepts and our methodologies must define our discipline.

We do have the concepts and methods to train students to be as objective and systematic in the perception and recording of mundane and familiar activities, behavior and beliefs of our own society as

we were in our documentation of very dissimilar phenomena over the world. No doubt this ability to look objectively at subjects is a greater concern when it is close to home and the subjects dress, speak and act much as the anthropologist does. Spradley (1970, 1980), for example, has provided blueprints for observing, interviewing, and recording and categorizing familiar data in our own society. Bruner (1979:18-20) has described a provocative new course he has designed which he calls "personal anthropology," in which undergraduate students are taught basic concepts of anthropology as related to their contemporary everyday life situation.

Concepts such as ritual, structure, and symbol are used in ways that enable the student to develop a more critical understanding of his/her position within society. Rather than discuss Pueblo sacred clowns, for example, the course analyzes Woody Allen and Steve Martin, those contemporary court jesters whose humor is so appealing to this generation. What anthropology has to say about rites of passage can be said just as effectively about fraternity initiations as about Ndembu ritual. Students can learn to do a structural analysis of myth by an examination of any contemporary text, be it a TV Western or Superman, without having to acquire the foreign data base necessary to understand the raw and the cooked in the South American jaguar myth. . . .

. . . We may have to learn about the Nuer and the Hopi as they were so crucial in the history of our discipline, but there is no inherent reason why these data should be imposed on our students. The same theoretical points can be made using ethnographic examples closer to home. Another factor is that anthropology in the past has been somewhat esoteric, and many of us have contributed to the popular image of the anthropologist as someone with pith helmet and camera tramping through the jungle to study some obscure aspect of kinship in a remote region of the world. We ourselves have emphasized the primitive, the romantic, and the exotic.

While I believe we should train advanced students through the use of intensive reading exposure to classical cross-cultural research, they should be able to complete their field rite of passage in more familiar settings if they so choose. If indeed our discipline has substance and currency, we should be able to apply our concepts and methods to the gamut of social situations--pure or applied, in contemporary urban United States or bush Australia.

On a practical level, we must intelligently and thoughtfully develop training and a new self-image in keeping with new job descriptions that we have to fill. For instance, it might be much more important to suffer "computer shock" than to experience "culture shock" if that which is tied to the computer experience is what is needed in order to carry out effective, sophisticated applied research!

A major rethinking of our methods courses is in order. No doubt this will require much more of the faculty than students. Updating oneself for field work in a world one could not have envisioned twenty years ago is a formidable task, but a responsibility incumbent upon us who would retain the respect of our students and do justice to our discipline. Mead (1978) astutely analyzed what has been simplistically called the "generation gap" as instead a major gulf between very differing cultures created by the life experiences of those generations enculturated before and after the cataclysmic advents of nuclear power and almost instant worldwide communication. The immense differences she pointed up both in the pre- and post-1950s world and the kinds of people who were raised in them--for our purposes, often faculty on the one hand and students on the other--provide significant bases for understanding the premises and responsibilities for a major updating of methods, goals and essential purposes of our field.

Tax and his students demonstrated many years ago that action-oriented research often feeds back findings to the store of science which would never have been gleaned through traditional methods of data gathering. Ironically for those purists of our field, it is becoming apparent that in our complex contemporary world the insights gained through problem-oriented research may be necessary to provide the balance crucial for not only a diversified but a theoretically rich discipline.

REFERENCES CITED

Ablon, Joan

1977 Field Method in Working with Middle Class Americans: New Issues of Values, Personality, and Reciprocity. *Human Organization* 36(1):69-72.

1979 The American Indian Chicago Conference: A Unique Experiment in Action Anthropology and Political Organization. In *Currents In Anthropology: Essays in Honor of Sol Tax*. R. Henshaw, Ed. The Hague: Mouton.

Bruner, Edward M.

1979 Personal Anthropology. *Anthropology Newsletter* 20(8):18-20.

Foster, George M. and Robert V. Kemper, Eds.

1974 Anthropologists in Cities. Boston: Little, Brown and Company.

Gearing, Fred, Robert McC. Netting and Lisa R. Peattie, Eds.

1960 Documentary History of the Fox Project 1948-1959: A Program In Action Anthropology. Chicago: University of Chicago Press.

Henshaw, Robert, Ed.

1979 Currents in Anthropology: Essays in Honor of Sol Tax. The Hague: Mouton.

Kayser-Jones, Jeannie

1979 Care of the Institutionalized Aged in Scotland and the United States: A Comparative Study. Western Journal of Nursing Research 1(3):190-200.

In Press Care of the Institutionalized Aged in Scotland and the United States: A Comparative Study. Berkeley: University of California Press.

Lurie, Nancy

1961 American Indian Chicago Conference. Current Anthropology 2(5):478-500.

Mead, Margaret

1978 Culture and Commitment: The New Relationships Between the Generations in the 1970's. Garden City, N.Y.: Anchor Press/Doubleday.

Mitchell, J. Clyde

1970 Urban Anthropology: Its Perspectives and Problems. Unpublished paper. University of Wisconsin at Milwaukee Conference on Anthropological Research in Cities.

Nader, Laura

1969 Up the Anthropologist: Perspectives Gained from Studying Up. In Reinventing Anthropology. Dell Hymes, Ed. New York: Random House.

Redfield, Robert

1955 The Little Community: Viewpoints for the Study of a Human Whole. Chicago: University of Chicago Press.

Spradley, James P.

1970 You Owe Yourself a Drunk: An Ethnography of Urban Nomads.
Boston: Little, Brown and Company.

1980 Participant Observation. New York: Holt, Rinehart and
Winston.

Weaver, Thomas and Douglas White, Eds.

1972 The Anthropology of Urban Environments. Society for
Applied Anthropology Monograph Number II.