FIELDWORK OR ETHNOGRAPHY:

A CASE STUDY IN QUALITATIVE RESEARCH

Mary Anne Pitman
Department of Social, Psychological and
Philosophical Foundations of Education
University of Minnesota
Minneapolis, Minnesota

The data presented in the ethnographic literature of anthropology differs from that of the other social sciences in one significant way: the method by which it is obtained. In the tradition of Malinowski, the anthropologist researcher goes "to the field." She pitches her tent among the natives, she lives in, she consciously assumes the ambiguous role of an observer who participates.

In the past twenty-five years, a new body of literature has begun to emerge from the anthropological community which deliberately focuses on the fieldwork experience. An acute awareness of the intricate connection between information obtained and the process of obtaining it prompts some of this literature, e.g., Bowen (1954), Powdermaker (1966) and Read (1965). Others are concerned with questions of method either from a theoretical perspective, viz., Kimball (1955), Hitchcock (1970) and Middleton (1970), or from a pedagogical perspective, viz., Golde (1970), Spindler (1970), Henry and Saberwal (1969), Freilich (1970) and Wax (1960).

More recently, the discussion of fieldwork is emerging as a component in the literature of quantitative/qualitative research methodologies (cf. Hymes 1977; Rist 1977; Cook and Reichardt 1979). Hymes' contribution to that discussion is based on his experience with linguistic research. He explains that since the discovery of the phoneme in the 1920's, linguistic research has employed a qualitative methodology. More specifically, linguistic research established the legitimacy of a methodology of scientific inquiry which is both qualitative and rigorous. "There is rigor in the work," writes Hymes, "but it is qualitative and discrete mathematics, not statistics or experimental measurement" (1977:166).

However, it is possible for qualitative research, like quantitative research, to focus on methods and ignore the context in which those methods are being employed. Linguists, for example, have used qualitative data to identify the structural levels of language, viz., phonemes, morphemes, syntax and semantics. But Hymes insists that such fieldwork is not ethnography. The ethnography of speaking occurs when those language structures are used to study what Hymes calls "speech styles," that is, "a study of language that is inseparable from a study of social life" (Ibid., 169).

Hymes goes on to assert that qualitative methodology can be similarly limited in areas other than linguistic research, particularly in applied research. Thus, he advises that fieldwork be distinguished from ethnography, that they be viewed as different, non-synonymous research activities. The distinction, in his view, has two components. First, being in the field equipped only with insight and intuition is not sufficient for ethnography. The ethnographer is also equipped with substantial knowledge of human systems. Likewise, going to the field to collect information to be used as data in a pre-structured model is also not ethnography, for the insights of ethnographic research must be allowed to generate new or unsuspected configurations.

Thus, there are two components which must exist before a field enterprise is truly ethnographic. First, the researcher must have a research problem well in mind, with models or theories clearly stated to allow field data to test central hypotheses. This step guarantees that the fieldworker can sort and direct intuitive or impressionistic information. Second, the research question and its theories or models must be adapted to the particular field situation so that unexpected findings can be accommodated. This second step ensures that the field data--not the theory--dictate the findings.

The following report of my field experiences in a southern United States mountain community provides what must be a fairly unique example of Hymes' conception of ethnography. It failed to contain either of the components thought to be essential. Thus, my professional inadequacies limited the scientific value of the research in just the way Hymes has suggested.

During the first four to five months, my fieldwork was guided by intuition alone. "Neophyte" may be too generous a term to describe the extent of my professional preparedness for the fieldwork in which I participated in the fall of 1973. Equipped with a twelve year old Chevrolet pick-up, one gray cat, and little more than a layman's knowledge of anthropology, I drove from Minnesota, my home for nearly all of my thirty years, to the Blue Ridge Mountains of the southeast. There I would join a former classmate as field assistant in a study which he was about to conduct for a local social service agency.

We began our fieldwork by preparing for it. We ordered a subscription to the weekly county newspaper, collected literature from the local Chamber of Commerce, serendipitously came upon a whole sheaf of topographical maps of the entire county and spent several hours each day reading the "flood book," an account of the disastrous flood which four years previously had ravaged the homes of our immediate neighbors and claimed the lives of several of their family members. That same flood had swept away the roof of our house and the water line from the spring back in the mountain or in "the old orchard on Mars Hill" as the neighbors called it. Those same neighbors had abandoned that house and moved into a government trailer on the hill across the creek. We worked on the house putting up sheet rock, cleaning, painting, digging a trench for the water line. Slowly the rats, mice, snakes, and wasps began to move out--out of the oven, out of the drawers and cupboards, out of the walls, out of the spring and the reservoir--and we began to move in. Four young boys, inhabitants of the trailer, curious and excited about this addition to their lives, were our constant companions as we rehabilitated this house which their grandfather had built and which one of them was destined to claim as his own home one day.

My professional training in English and speech communication provided me with a journalist's sensitivity to the potpourri of images and information which presented themselves in those first weeks. Thus, I gradually began to define the components of our homestead which included, on the east side of the creek, our house, three chickens housed in a converted privy, two horses and a deteriorating barn. On the west side of the creek, closer to the main highway, was the trailer housing the four boys and their young widowed mother and the large house of their immediate neighbors, an old widowed aunt, kin to the young woman's dead husband, and the aunt's servant and companion of forty-seven years.

The training in anthropology which could have nurtured and directed those early observations was not part of my repertoire. When Miss Anna, the servant, who was a regular visitor during those first weeks, would sit in our kitchen, old straw hat still tied under her chin, scarred and scabby fingers smoothing and re-smoothing the folds of her faded flour sack dress, singing out a string of words in her own rapid-fire mountain English, I would see her as an isolated individual, not as one member of the local community. I'd nod and "hmm" while I washed out the pail in which she'd brought a load of "cukes" and summer squash and "maters." By the time I'd finished arranging the flowers which invariably accompanied the vegetables, she'd be rounding up her old collie dog and saying her good-byes: "Y'all c'mon over and see us now, ya hear?" And I would have missed another opportunity to record the behaviors, dress and materials which would have been valuable in revealing that community's technological patterns and patterns of social interaction. When I finally began to comprehend her dialect, I could have learned the patterns of social intercourse which focused on the swapping of kin terms. Her kitchen visits invariably included a lengthy comment on what each one of the four boys was doing that day and was usually followed by some reference to Elenor, their mother's, activities: "I reckon Elner be takin' Cabell over ta high school fa fooball. . . ." The reference to Elenor was often followed by some reservation: "but I own know i' Jimmy [the oldest boy, given much attention and status by all three women] 'll wait dat long fa 'is supper." The reservation, I believe, was indicative of what I later discovered to be a long standing conflict between Elenor and the old lady, Miss Ellen. Miss Ellen had money and title to nearly all the hundred plus acres of that homestead. Elenor collected welfare and veteran's benefits and had title only to our house and the footage immediately surrounding it. But she had the boys, and they were the pride of the homestead.

Pattern analysis of such social intercourse emerges from a rigorous system of regular note taking, coding and categorizing. Such a system was inaccessible to me as a fieldworker untrained in ethnography. My note taking focus was not on what I observed outside myself but on what I observed inside--intuitions and insights about and responses to the field setting. This inner focus led me to experience a sense of isolation unlike anything I had ever known before. Culture, Kluckholm tells us, is a mirror for man (1949). In this instance culture was a mirror for woman, one particular woman, me. I kept running into myself at every turn, discovering things about me and my cultural background about which, until then, I had known nothing. On the one hand my privacy needs were constantly violated, and on the other my sociability needs went unheeded. But my sense of privacy, spawned in an urban, northern clime, could never have existed in this rural setting where kin ties criss-crossed the entire county and wove the generations, the social classes, the sexes, even the races into one intermingled group. A trained ethnographer would have begun to recognize that in that setting people maintained privacy by observing regular social amenities. I, however, found it disconcerting to be heartily invited to "come by and see us" at the end of every casual encounter with a total stranger.

This sense of isolation was felt most acutely when, to meet my needs, I would attempt to initiate social contact. Comments of a personal nature were responded to by Miss Anna, eyes averted, with "I own know 'bout dat, but . . ." and back into her standard kin swapping information. Elenor, on the other hand, was personal and, as I later understood, included me in the talk characteristic of female friends by discussing sex and "the coloreds." But I found her constant sexual innuendoes crude and her discussions of black people assaultive and bewildering. By contrast, my own habits of sociability included a sprinkling of Nordic-styled sarcasm. Used as social commentary, it is intended to give pleasure and thus be returned with appreciative laughter. In that mountain setting, however, it was returned with a silence which was devastating.

My frustration at not being able to engage in light and easy social intercourse was not relieved by the comfort available to the trained anthropologist fieldworker, the comfort of relating to my response as potentially useful data. In addition, for my part in this project as field research assistant, I was to concentrate on the social tasks of fieldwork, the tasks of establishing and maintaining satisfactory relationships. We assumed that those relationships would eventually elicit the really significant data. As far as I knew, I was failing. Thus, it was with a sense of relief that I left the mountains in late December, little more than four months after I had arrived, and returned to the University. Having been confronted by my own culture, because of its dissonance with the local culture, I needed for sanity's sake to find out whatever I could about the concepts which make up the science of anthropology.

For four weeks, I immersed myself in the literature and discussed it with my mentor, Marion Dobbert, an educational anthropologist. I read widely in the history of theory in anthropology while I simultaneously read one ethnographic case study after another. I then arranged to continue my reading on an independent basis. Forthwith, I shipped two cases of books, one extensive bibliography and myself back to the Blue Ridge Mountains. Because of that introduction to the literature of anthropology, my definition of my task had changed from "I am here to interact with the people and see how they live" to "I am here to identify the technological, ideological and social patterns of a specific local community." But in my case a little learning was indeed a dangerous thing because it helped steer me into the second and more common pitfall of fieldwork, the one Hymes describes as "pre-coded content" (1977:170). "The student armed with qualitative methodology," he writes, "can be just as a priori in assumption, just as prone to overlook disquieting empirical facts . . . as can the quantitative researcher" (Ibid., 167). This was true of me primarily because I chose to disregard the intuitive, impressionistic discoveries of my earlier fieldwork. I believe, unlike Hymes, that such an approach to ethnography is an important ingredient in the initial stages of data collection. The ethnographer needs to hang out, to get a feel for the community, to just be there with her intuition for awhile. But equipped with my new and highly limited knowledge, I chose to abandon the immediate world of Miss Anna, Miss Ellen, Elenor and the boys and to concentrate, instead, on an adjacent community which was racially distinct and geographically isolated and, therefore, amenable to hypothesis testing.

My colleague and I decided to validate the hypothesis which claims that a loose-knit social network correlates positively with close conjugal relationships and, alternatively, that a tight-knit social network correlates positively with role-defined conjugal relationships (Bott 1970). The procedures we used included weekly pre-observation strategy meetings and immediate post-observation note and discussion sessions, structured interviews and life histories designed to provide the kind of information needed in a social network study, and the tabulation of that information from local marriage, property and census records which would help substantiate our hypothesis. That is, we were trying to overcome the problem of the intuitive maelstrom by adopting a more structured and problem-focused research approach. But since the entire community ended up being clumped at one end of the continuum, the information could not be said to fit our pre-defined social network hypothesis. Indeed, our data could only have been explained from the perspective which could have been provided by conducting a true ethnography, one in which the research question and its theories are adapted to the particular field situation. But we were out of time. We had spent four months familiarizing ourselves with the county and identifying a segment of it for the study. The next four months were spent gathering data for the social network hypothesis. During the final four months, we analyzed our data and wrote the report.

This personal history of my field research illustrates the two dangers, the Scylla and Charybdis between which true ethnography steers (Hymes 1977:170). In my work I was drawn into both at different times. By initially relying only on insight and intuition, I was overcome by the experience of social dissonance. Such dissonance is inevitable whether one is trained or not, but one's ability to cope with that dissonance exists in direct proportion to one's ability to use it scientifically as a source of data. That ability is found only among those who understand that anthropology is a demanding discipline, a rigorous science in which amateur impressions and skills will not suffice (cf. Middleton 1970). However, the true test of an anthropologist's skill surely lies in avoiding the second danger, i.e., in being able to encounter each field setting on its own terms. The research questions which were formulated before I immersed myself in the racially and geographically segregated community became a barrier to a true understanding of that community. What I did learn is that anthropology is an inductive science, and neophyte practitioners must not succumb to the fear that they will find nothing and thus decide ahead of time what to look for.

REFERENCES CITED

Bott, Elizabeth M.
1970 Family and Social Networks. London: Tavistock.

Bowen, Elenor Smith
1954 Return to Laughter. New York: Harper Brothers.

Cook, Thomas D. and Charles Reichardt, eds.
1979 Qualitative and Quantitative Methods in Evaluation Research. Beverly Hills: Sage.

Freilich, Morris, ed.
1970 Marginal Natives: Anthropologists at Work. New York: Harper and Row.

Golde, Peggy, ed.
1970 Women in the Field. Chicago: Aldine.

Henry, Frances and Satish Saberwol, eds.
1969 Stress and Response in Fieldwork. New York: Holt, Rinehart and Winston.

Hitchcock, John T.
1970 Fieldwork in Gurka County. In Being an Anthropologist. George Spindler, ed. New York: Holt, Rinehart and Winston.

Hymes, Dell
1977 Qualitative/Quantitative Research Methodologies in Education: A Linguistic Perspective. Anthropology and Education Quarterly 8:165-176.

Kimball, Solon T.
1955 Problems of Studying American Culture. American Anthropologist 57:1131-1142.

Kluckholm, Clyde T.
1949 Mirror for Man. New York: Whittlesey House.

Middleton, John
1970 The Study of the Lugbara: Expectation and Paradox in Anthropological Fieldwork. New York: Holt, Rinehart and Winston.

Powdermaker, Hortense
1966 Stranger and Friend. New York: W. W. Norton.

Read, Kenneth E.
1965 The High Valley. New York: Charles Scribner's Sons.

Rist, Ray C.
1977 On the Relations Among Educational Research Paradigms: From Disdain to Detente. Anthropology and Education Quarterly 8:42-49.

Spindler, George, ed.
1970 Being an Anthropologist. New York: Holt, Rinehart and Winston.

Wax, Rosalie H.
1960 Twelve Years Later: An Analysis of Field Experience. In Human Organization Research. Richard Adams and Jack J. Preiss, eds. Homewood, Illinois: Dorsey.