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ABSTRACT

This paper examines researcher/respondent relationships in a fieldwork study of a hospital ward for the care of patients with acquired immune deficiency syndrome (AIDS). The nurses' work was the subject of study. Taking the position that verstehen is a precondition of research, and using Mead's argument that one can be an object to oneself, key aspects of the relationship between the researcher and the nurses are rendered problematic. The investigation illuminates institutional constraints, the ideological position of the nurses, and the social psychology of work on the ward. It is argued that substantive elements of the situation may be discovered through the analysis of personal relationship data.

[KEY WORDS· acquired immune deficiency syndrome (AIDS), fieldwork, verstehen, nurses, experiential research]

One of the first hospital wards for the care of patients with acquired immune deficiency syndrome (AIDS) provided the site for a fieldwork study of the ward's nurses and their work. This paper examines my relationship as a field researcher to the nurses and others on the ward. Relationships in the field have long been a prominent concern of researchers (Cf., Geer, 1964; Olesen and Whittaker, 1968, pp. 25–46; Wax, 1971; Shaffir, Stebbins and Turowetz, 1980), and a number of writers have probed the researcher's personal experience as a source of insight into the setting (Bosk, 1979; Reinharz, 1979; Oakley, 1981; Stanley and Wise, 1983, pp 150–175; Berg and Kenwyn, 1985). In line with Wax's position that verstehen is a precondition of research (Wax, 1971), and using Mead's argument that one can be an object to oneself (Mead, 1934),

I investigated problematic aspects of the field relationship on the AIDS ward. Several themes are prominent: the effect of institutional constraints, competing nursing ideologies, normal interaction as epistemology, and, finally, reckoning with research as everyday life on the AIDS ward. These data illuminate the pathways to analysis contained in relationships with respondents and further illustrate the intended and unintended impact of processual fieldwork in natural settings.

Studying the AIDS Nurses

A small group of nurses in the large public hospital in which the AIDS ward was located was a prime force in the establishment of the new inpatient AIDS service. Medical researchers and representatives of the gay community also lobbied actively, and, as we have described elsewhere, the nurses joined forces with these groups to found the ward in 1983 (Lessor and Jurich, 1986). The unit attracted a number of highly motivated and well-educated nurses. Initially, their reasons for wanting to work on the ward varied from actualizing a personal philosophy or building a career to "being on the cutting edge of research on a new disease." For the founding nurses, the principal reason was the opportunity to exercise influence in the hospital in a nurse-run unit that would emphasize counseling, health teaching, and preparation for self-care after discharge. This attitude increasingly characterized almost all members of the nursing staff on the ward. Such nursing efforts elsewhere in the hospital had been frustrated by cutbacks in federal revenue sharing. Their ideal was a newly-funded unit staffed exclusively by registered nurses with a high nurse/patient ratio and utilizing principles from community mental health.

Thirty six nurses volunteered for the twelve available staff positions. Seven of the twelve nurses selected were gay men, one woman was a lesbian, and the others were married heterosexual women. Two of the seven men had not discovered their gay sexual preference at the time they were hired and later "came out" while working on the ward.

Initially, the nurses consented to my studying their work in exchange for my assisting them with their own proposed studies. I am a sociologist, but I also have a Master's degree in nursing and was then associated with a school of nursing. As our relationship developed, the nurses not only relied on me for advice in writing nursing care protocols to be tested, but also for advice on approaches to hospital management and conflict resolution between the AIDS ward and other areas of the hospital. I spent varying lengths of time on the ward for several days each week over a period of nine months, talking with staff, patients, and visitors and observing the round of activity at various times of the day and night. From time to time, I was accompanied by my research assistant (who also visited the ward alone on a regular basis). Early in the field work,

we tape recorded in-depth interviews with each of the nurses (as well as with most of the physicians and volunteers), and then continued to discuss current ward events with them on a regular basis. We observed staff meetings and change of shift reports, and eventually the nurses agreed that I should sit in on their personal group therapy sessions with the ward psychiatrist. A relationship of trust and mutual assistance made it possible to discuss sensitive areas, such as our beliefs about nursing and sociological research.

Fieldwork as Method and as Problem

Objectivity is normally considered a necessary component of research methods. Techniques exist which aid in distancing the person of the researcher from the persons studied, yet field research with so-called "naturally occurring" data presents some important difficulties. The first of these is the requirement that the fieldworker be *in* the field, and secondly, that she or he interact with participants. Weber grappled with the contradiction of subject/object dualism as he tried to reconcile the requirements of science with the problems of examining the natural world; he concluded that "verstehen," the theoretical grasp of the actor's subjective state of mind, was the only reasonable stance (Weber: 1947, pp. 93–97). "Verstehen" is a precondition of research; it allows the researcher to grasp the categories and cultural objects in the subject's world in much the same way that learning a language permits communication within a foreign culture (Wax, 1967). As the researcher is "resocialized" in the setting (Wax, 1967), the meaning to the actors of their acts becomes clear. In this vein, Schutz argued that people know one another most genuinely in face-to-face interaction in the "we-relation" of the "lived-in world" (Schutz, 1964:23–27), suggesting that students of society develop methods to *account for* (rather than artificially distancing themselves from) their participation with subjects in the field. Examining oneself as a participant (Reinharz, 1979, pp. 353–368), or in Mead's terms, becoming an object to oneself (Mead, 1934, p. 140), is perhaps the only method available to researchers to approach the problems of subject/object dualism while reaping objective data from their own interaction with participants.

One way to examine ourselves in the setting is to analyze the areas in which we, as field workers, experience discomfort. Some of our methodological notes (Cf., Schatzman and Strauss, 1967, p. 101) are extensive and others brief; that we failed to write in greater detail about some personal experiences is perhaps an indicator of our dilemmas in dealing with them. Yet, when I point to areas of discomfort and call them "problems," I am also using the term in Nelson and Olesen's phenomenological sense: to render "problematic," to make the methodological point itself an object of investigation (Nelson and Olesen, 1977).

Nursing research as a constraining institution. The first problematic area was in reconciling the demands of my research proposal with the realities of the setting. Throughout my graduate school days as a trainee working with a number of talented and even luminary researchers, I had been aware that what researchers propose to do and what they end up doing, as they meet the exigencies of the setting, are sometimes different. I had experienced little difficulty with this because it seemed to me that if the demands for protection of the rights of participants were adhered to, it made little difference if in the course of discovery one's perspective on the study shifted. In the case of this study, my first as a principal investigator, my in-the-field perspective did almost immediately shift as I began to comprehend the perspective of the ward nurses. However, my sponsoring institution, a university school of nursing, and most of my colleagues therein, operated under a more positivistic paradigm. As a researcher in the school, my proposal had been awarded funds under a preliminary investigation grant. In the proposal I stated that I would examine risk management strategies of AIDS unit nurses. As a member of an occupational health group, I was urged by senior members to do so in order to increase the credibility of the study and ultimately to be funded. The advice was not unexpected in that in order to increase status and political power vis a vis other health disciplines, schools of nursing frequently have a bias in favor of verification research, preferably of a quantitative nature.

The well-socialized researcher in a school of nursing is, in fact, well aware of the prevailing positive norms. It is that objective measurement of risk management is not valuable—how many times nurses did or did not wash their hands, whether the nurses donned gowns and gloves each time they handled the patient's body, the procedures they used with needles—however, that I was more interested in how they defined their environment. For example, had I limited my observations to such activities and failed to learn the meaning to the nurses of risk management, I would have missed their unusually macrosocial perspective, their ability, in C. Wright Mills' terms, to see public issues rather than personal troubles (Mills, 1959, pp. 3–24). They saw institutional demands, rather than personal practices, as putting them at risk. Understaffing, which would force a nurse to work more rapidly and possibly expose himself to injury was seen as a "high risk behavior." Similarly, in the case of the prevention of accidental needle punctures, the nurses viewed the provision of adequate disposal units as the most important preventative factor. According to an infection control nurse, these units are costly and hospitals have been slow to provide them until it has been shown that staff do sustain needle punctures more frequently with old style units. Furthermore, all of the experienced nurses assumed that they were competent to determine whether a practice was safe or not. In this frontier situation there existed no small amount of pride in the contention

that "we are way ahead of CDC (the Centers for Disease Control, in Atlanta) in what we know."

As Stanley and Wise (1983) have pointed out, field work is not neat and tidy, with crisp questions to be derived from carefully controlled situations. Rather, what is worth knowing is usually buried in the day-to-day tangle of human interaction. It was impossible to communicate this interest in my proposal without being accused of being vague and non-specific and so I avoided that accusation, and the consequences of not being funded, by being direct and specific about studying risk management. I never felt comfortable though, not because I felt deceptive, but because I felt I was cut off from discussion and input from colleagues regarding the area that intrigued me—the way in which people were making their way in the day-to-day, lived-in world that was unlike any they had experienced before. A good many field workers undoubtedly frame their proposals as "inductivist positivism" (Stanley and Wise, 1983, p. 151), or even fewer would be funded. Yet, relatively little is known about how people live and work with AIDS, and such knowledge could contribute to social policy formulation. The results of individual risk perception scales (Cf., Rantanan, 1981) used in AIDS care may yield important data; however, they will miss important organizational features. In this case we found that the nurses' awareness of institutional support contributed greatly to an estimate of risk level that was "liveable."

Competing nursing ideologies and reciprocal relations in the field. When people agree to give researchers their time and allow them entree into their affairs, it is usual for them to expect something in return (Schatzman and Strauss, 1973, pp. 28–32). The nurses on the ward asked me to be their research consultant. They wanted to do research on nursing protocols they were developing. They were working twelve hour days, often under harried and stressful circumstances, but in those early months several nurses were very enthusiastic about delineating the specifics of nursing *care* apart from the medical research protocols they were helping to carry out. They wanted to communicate this to their own hospital administration and also to a broader nursing audience.

One has to understand work ideologies (Berger, 1964) and the historical development of nursing ideology in the United States in order to understand the dilemma of the AIDS ward nurses. In the 1950s, nursing as a discipline was attempting to professionalize by defining its scope of practice and determining its functions that were independent of medicine. It did so by incorporating social science studies about the discipline into its emerging definition. Nurses defined their independent functions as assisting patients to cope with their illnesses and disabilities. If medicine's task was to cure, then nursing's task was to care. Nursing was, to its leaders at least, clearly the psychosocial discipline. The notion of caring emphasized "sensitivity to human feelings" and "the patient as a person." (Cf., Brown, 1961, Davis, 1966). In the years since, the rise of

technology and the fascination with objective science in academic nursing circles has somewhat eroded this long-standing ideology to the extent that, in academic nursing circles, "caring" is made an object of rationalized study. However, the older, intuitively-based caring ideology, buttressed by the human potential movement, continues to be a strong focus for many practitioners. It remains so influential that many young practitioners feel guilt when they are prevented from carrying out such activities (Kramer, 1974). The AIDS nurses almost uniformly described such experiences in their hospital work prior to coming to the AIDS unit.

The AIDS ward offers limitless possibilities for doing psychosocial nursing care, but therein also lies a dilemma. The nurses wanted to legitimize what they were doing through "research," yet they were having trouble fitting what they were doing into the objective language of the nursing literature which speaks of "care outcomes" and their "measurement" (Cf. Rogers, 1970). On the one hand, they had written a "nursing diagnosis," such as "devastation of the human spirit," and on the other hand, their discipline required them to specify a "treatment" (so many minutes a day allowing the patient to express his feelings) and an objective outcome (measurement of a decrease in certain behaviors assumed to be associated with spiritual devastation). And I found myself helping them with this language! At first this rationalizing language appears ludicrous, and in my notes I had written, "what these 'nursing diagnoses' enable them to do is not to plan objective care for the patient, but rather justify what they are already doing intuitively." The AIDS unit nurses recognized the patients' emotional needs as very great, and their work in attending to them as their most important task, yet the signals they received from other nurses, and more significantly, from nursing administrators, were that perhaps they were "doing too much." Therefore, in order to garner resources (adequate staff being the major resource) it becomes necessary to convert one's work into an objective language which carries credibility with those in power.

The nurses' methodological dilemmas paralleled my own as a qualitative sociologist working for a school of nursing. Examining my own first order constructs (the necessity for structuring research in rationalizing language) lends insights into the first order constructs of the nurses (the necessity for structuring nursing practice in rationalizing language), as the struggle for resources is played out on the ward.

"Normal" interaction as epistemology. In field research, as Charles Bosk points out, relationships are our major methodological tool (Bosk, 1979, p. 202). Our hosts give us their time and the privilege of observing them, and this in turn evokes a sense of indebtedness in us. Like Bosk, who discharged his debt to the surgical residents he studied by playing various roles (Bosk, 1979, pp. 195–202), I, too, played the role of "sounding board" and "referee" among others. As Bosk correctly observes, fieldworkers worry about the re-

straint of such roles on their observations. There are dangers in *over*rapport, *over*indebtedness and *over*generalization (Bosk, 1979, p. 204), and thus fieldwork reports are filled with discussions of attempts to strike a balance in such relations.

In my experience on the AIDS ward I was often aware of the difficulty of presenting a neutral and balanced response within a given interaction, and generally opted for structural solutions to the problem of balanced relationships. Howard Becker has observed that researchers frequently break the rules when we interact with the people we study, and that it goes fairly unacknowledged in the literature (Becker, 1987). It is normal interaction that mirrors the everyday world we purport to study. Without normal interaction, the picture is undoubtedly less accurate. A simple example of such inaccuracy was the nurses' reaction to another researcher who spent some time on the ward observing, but not talking, and writing furiously in his notebook. Nurses and patients referred to his behavior as "weird" or "creepy" and spoke of being careful of what they said when he was nearby. Becker says that in reality, interacting means sharing our reaction, and has found in his studies of the theater world that at times this means being drawn into the passing of information and the giving of advice (Becker, 1987). On the AIDS ward the head nurse sometimes asked me to come with him to a back room where we could talk privately. He would tell me about the latest difficulties with the administration and query me for my analysis. He saw me as an experienced former nurse who now had the ability to analyze organizations; this ability was something he needed. My notes reflect my ambivalence about studying the ward and engaging in this intense interaction, which drew me into its growing pains. The head nurse was later promoted and asked me if I would serve as a consultant to a newly created group of nurse administrators; I declined. My interaction with the nurses on the ward was open and intense, and I decided not to conduct the same level of interaction with a superordinate group. I believe that decision cut me off from data that would have contributed to a fuller understanding of the organization. Others have successfully combined formal organizational consulting with research (Cf., Kantor, 1978), but at the time it seemed important to stay close to the nurses and patients on the ward if I was to understand their experience (Becker, 1967).

The nurses walked an ambivalent line in their interactions as well. It was the line between communicating their experience in feeling terms or intellectual terms. The nurses were putting together a "community nursing model," a term which they adopted from my analysis of their work that described the complex organization of care between the hospital and other collaborating agencies. Because the course of the AIDS patient's illness required periodic hospitalizations, the hospital was serving as a hub. Each week representatives from social welfare, community nursing, hospice care and outpatient clinics met at the hospital to discuss both the current inpatients and significant problems of pa-

tients on "the loop" (another analysis term). The nurses were becoming so organizationally successful that they were called on to help others. Several nurses began to consult in other parts of the state and their advice was sought by hospital administrators and policy makers as well as other nurses. Yet, hand in hand with this rational model, the organization was drenched with feeling. A prevailing norm on the ward was that everyone should express their feelings (Lessor and Jurich, 1986). Nurses and counselors often cried over the loss of a patient, and in staff meetings physicians, too, sometimes appeared deeply touched by emotion. Gay nurses expressed anger, as well as grief. The anger was somehow easier for us to identify with than other seemingly more maudlin ways in which the staff handled feelings, such as keeping an album of patient photographs. Our notes reflect the discomfort of secular sociologists on those occasions when nurses talked of their work as akin to a religious calling. For example, when people talked in rapt tones of providing a "good death" for the patient, we wondered in our field notes whether it really mattered for anyone other than the caretakers. The nurses had asked me to help them with the rationalizing language of science, which would enable them to persuade those controlling the resources, and here they were wallowing in feeling! Yet, I knew from experience how very real those feelings were. In the middle of our study I brought my friend to the hospital, suffering from brain involvement secondary to Kaposi's Sarcoma, an acquired immune deficiency syndrome. He was moved to a second hospital, where I visited him until he died. I know the shock and disbelief I felt, and the sense of wanting to create some meaning amid the tragedy. Here were extremely bright and articulate practitioners who were executing elaborate protocols and were, at the same time, talking about high callings and "missions." When the nurses would get intensely ideological about either nursing or gay activism, I would get uncomfortable. Maybe that was my own bow to the scientific model—that somehow I wanted them to be expressing their ideas and needs in highly rational terms and speaking in that detached language which I believed would give them more of a payoff. Yet, again, I was brought back to my own first order constructs: I sometimes found it impossible to interpret my own work to others in the most "powerful" terms, in that the language of scientific rationality failed to convey life and death as it was experienced on the AIDS ward. Thus, my analysis would have to account for the nurses' two identities, encompassing both rationality and feeling.

Reckoning with research as everyday life on an AIDS ward. A final experience on the ward should lay to rest any doubts that research does become part of the round of life in the field. During the first nine months (and thereafter as well) the AIDS ward had become the elected site of numerous research studies by social scientists. Although I was on the ward first, I soon found myself jockeying for position with anthropologists, psychologists and other sociolo-

gists, and in the final months I experienced a "turf battle" with another re-searcher.

The other researcher's intent was also to study the nurses on the unit. He had, however, gained entree through physicians in the internal medicine depart-ment, not through the nurses themselves. He proposed to re-interview the nurses and observe them in their meetings and at work. When we talked it seemed clearly understood that it would be unfair to impose yet another battery of interviews and observations on the nurses, and, would further jeopardize both field studies. It seemed clearly understood that he would do something else. We then discussed my experience and I excitedly talked about gaining entree, key people, the building network of social relations and so on, thinking that I was talking about data with a colleague, not a competitor. A week later, I regretted this conversation when he informed me that he was going ahead with his study. I was incredulous. How could this interloper take over "my respon-dents," I raged in my field notes. As Arlene Daniels describes, I was infatuated with my respondents (Daniels, 1983) and I couldn't believe that another suitor was trying to steal them away. I, however, calmly tried to dissuade him on the obvious logical and ethical grounds, even pointing out that he did not yet have human subjects approval from the University Institutional Review Board. He was undaunted and maintained that just because a study was difficult to do was no reason to be deterred from doing something he was interested in. He then went onto the ward and informed the head nurse that he had talked to the physicians about being there and had also talked to me, leaving the nurse with the impression that his research was part of mine. When I arrived the next day the head nurse asked me, "Don't you university types ever talk to each other?"

I was embarrassed about the way that we university types do sometimes handle ourselves, and I called the other researcher's senior faculty mentor (we were both new post-doctoral researchers) to discuss the problem. When that appeal also failed, I played the only card that I had, which was to tell the nurses on the ward about what had gone on.

Two days later, I met with the entire nursing staff, and we spent thirty minutes talking about the role of social scientists on the ward. They let me have my say and then they had theirs. Some expressed justifiable exasperation over the conflict. One of the nurses made an appeal that mine was the preferred project on the ward because "even though these others (other disciplines) want to study the nursing work, they don't even have a nurse on their team." More nurses agreed. The consensus was that I would be allowed to stay and my erstwhile colleague was going to be put off for a few months. The nursing had linked me to the nurses and enabled me to better understand their experience, and now it was saving my study.

The saga of the turf battle makes a sociological point, in addition to its

lesson to perhaps not take ourselves so seriously. Our ties to the setting are less based on formal affiliations than on presumed shared identities. Thus, it is necessary to identify the nature of those identities and interactions if the intent of fieldwork is to capture the "normal" and everyday nature of life in the setting.

I developed a high regard for the nurses I studied; we liked each other, a not uncommon occurrence in field work (Geer, 1964; Hughes, 1971). In this case, one is sometimes hard put to write about one's contradictions, or, as was the case with me, to write about the intensely ideological side right along with the rational or leadership side. Making the self of the researcher as much an object of study as that of the respondents enables one, however, to keep from erring on the side of glorifying the respondents on the one hand or vilifying them on the other. Judith Stacy has recently written about her disillusionment with field work. She worries that if she is intellectually honest about what she saw and heard she will be betraying people (Stacy, 1987). That may be the case if researchers hold themselves up to be detached observers who only examine "subjects." We are on firmer ground, however, when we examine ourselves, as participants, with our contradictions and the ideological biases we clutch to our breasts.

We were often struck by the intensity of feelings that nurses and others expressed; in searching for sociological explanations for this, it was useful to examine our own experiences even sooner. For instance, my friend's death was highly significant for me. I arranged his funeral. The staff on the ward often attended patients' funerals. We need to write about these events in ways that analyze our own experience and understanding. Another example was my research assistant's initial fear about going on the ward. I had no idea until later that she was petrified for the first three months. In looking carefully at how she became comfortable on the ward, and she did become comfortable, we learned more about how AIDS anxiety is replaced by saner approaches.

Verstehen, which prompts us to examine our own first order constructs, reminds us of the discomforts of lived experience in real settings.

So what are we to do? Do we confine our investigations to circumscribed areas in which we can convince ourselves that we are sufficiently distanced to be objective? Or should we, on the other hand, simply take our subjectivity for granted? As Joel Brodsky has put it very well, the epistemological problem inherent in subject/object dualism is no more by a frank subjectivity than it is by a ruthless positivist objectivity (Brodsky, 1987). I would suggest that the answer is to take Mead's advice and become an object to ourselves, acknowledging that in fieldwork, researchers are participants to be accounted for and that their experience and relationships are important sources of data.

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