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Forgive and Remember

Managing Medical Failure

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Appendix

The Field-Worker and the Surgeon

All fieldwork done by a single field-worker invites the question, Why should we believe it? It would be nice to be able to claim that I was a totally impartial observer whose characteristic ways of looking at the world allow an almost perfect mirroring of some objective reality. However, as the fieldwork experience made clear to me, I am not without my biases. I would like to pretend that this was not so for any number of reasons, but the observer role in some sense trained me to see these biases in a heightened way. As I reflect on the experience of eighteen months of participant observation in a teaching hospital, and on the dilemmas of the observer role, I feel a sense of respect for data-collecting procedures which allow the researcher to keep the sensuous world at a distance, and which thereby allow him to avoid the self-exposure, self-reflection, and self-doubt endemic to field-workers. In the field, the everyday life of his subjects overwhelms the researcher, threatens to obliterate his sense of self, and forces a reconsideration of deeply held personal and intellectual beliefs. It would be of little point, then, for me to pretend in the face of such a powerful experience that I was merely a coding machine which transcribed the events of everyday life first into field material and then into the sociological and literary order of the preceding pages. In this appendix, I would like to describe the field experience itself and the analysis of the data. This appendix should show the reader how I identified and controlled for my own biases and should allow him to control for them independently of me.

In the Field

How did I begin? The first thing I did was to approach an attending I had met at a party, explain my proposed study, and

ask for his cooperation. The attending expressed enthusiasm for the project, but refused his cooperation. He claimed that if I wanted to really be trusted, I would need the housestaff's acceptance. He expressed his fear that his sponsorship would be a "kiss of death": housestaff would view me as his spy and never talk freely with me. If I wanted my project to succeed, he advised, I needed to be seen as my own person. So rather than somehow magically start the research, he gave me the names of a number of residents and the hospital's central page number. What I learned during this interview was that there was no instant access for the field-worker. Not sure if I was receiving aid or a runaround from my initial contact, I called the first name on his list, the chief resident. We met for coffee and I explained my plans. The resident approved my being an observer on his service, but claimed he would have to check with both attendings. The chairman of my department provided a letter of introduction to the chairman of the Department of Surgery. Gaining my initial entrée was a multistaged diplomatic problem. Each interaction was a test, and access was the result of continual testing and retesting. Entrée was not something negotiated once and then over and done with. I was always entering new scenes and situations involving different combinations of people. Fortunately, of course, I could rely on what I had learned in previous encounters and the repertoire of roles that I had developed and that others developed for me. The important thing that field-workers must keep in mind is that entrée is not a single event but a continuous process.

Access—being allowed in the scene—is one thing, but approval and trust of field subjects is quite another. Just like access, cooperation cannot be ordered by fiat, but is rather earned again and again, when the field-worker shows that he or she is trustworthy and reliable. Much is made in fieldwork accounts of the "cover story" which the observer uses to explain his presence in the setting as a first and essential step in gaining trust. My cover story was very simple. I explained that I was doing a dissertation on the way surgeons learned to recognize and control error. The surgeons were, as a rule, remarkably uncurious about my research. None ever questioned the legitimacy of my research question or the nature of my methods during our initial meetings. Few even requested that I account for my presence. I was not asked for my cover story very often and, when asked for the story, I was not required to elaborate on it. In some sense, my access was secured by sponsorship of housestaff trusted by all. Once my access was established, my cover story was superfluous and served as a gloss during introductions. In the everyday course of things, my housestaff sponsor was more important to my access than any cover story I used.

Trust was gained neither during initial introductions nor by the artful manipulation of a cover story, but through my performance in roles I assumed and was assigned by housestaff and attendings. Housestaff assigned me a number of roles. Most generally, I was an "extra pair of hands," and a "gofer." During the time of my fieldwork, I became very proficient at opening packages of bandages, retrieving charts, and fetching items from the supply room. Through these tasks, I expressed some solidarity with whatever group I was observing and gave something, however inconsequential, in exchange for "observing rights." Second, I was an "emissary from the outside world." My round of life was less circumscribed than a houseofficer's: I read and watched more news, saw more movies, and participated more fully in university life outside the hospital. In some sense, I provided housestaff contact with a world they felt cut off from. During Watergate, I always brought a number of papers into the hospital. How or why this became my task I do not know. Often I purchased these papers at the hospital gift stand, a place interns and residents certainly had access to. Their general reluctance to pick such papers up is not so much a mark of their frugality as a symbolic statement about their relation to the world outside Pacific Hospital. I later learned that housestaff attach a magical property to newspapers, books, and magazines. If they bring them in to work they see this as jinxing themselves and condemning the group to an impossible busy day. It is, however, permissible for outsiders to bring such taboo items to them. My passing remarks about movies, current events, the weather—all

were taken as an indication of what educated people on the outside were thinking. Third, I was a "fellow-sufferer." As a graduate student not released from training, I was perceived as occupying a position analagous to the houseofficer's. My own career problems and expectations were topics that houseofficers initiated much conversation about. They constantly compared and contrasted our different experiences. During such exchanges, houseofficers constantly emphasized the indignity of their roles and often suggested that their present burdens justified their future rewards. From me, they sought to learn about the generalized indignities of the subordinate role in sociological training. I regaled them with my wildest recollections of coding data and proofreading galleys.

Fourth, I was a convenient "sounding board." I was surprised at the degree that informants sought me out to relate stories of practice that they disagreed with. Feelings that were not shared in the group, discontents, uncertainties were taken to me. I knew that observers were often sought by organizational malcontents; what surprised me was that all my informants were at one time or another malcontents. Such a label was not a stable organizational identity as much as a fleeting reaction to behavior, which for one reason or another offended the houseofficer's sensibilities. Disfiguring palliative operations, patient discomfort, and the openness of communications among the ranks were the most common complaints. As a "sounding board," I was implicitly asked to play a quasi-therapeutic role: to listen without judging and to understand. The fact that I was asked to play this role so often by so many speaks both to their understanding of what an observer does and to the deep feelings that physicians repress as a matter of course. As a rule, we, as medical sociologists, have not concentrated enough on how fragile physician defenses are, what events disturb them, and how primal the existential material they are dealing with is. Birth, life, death are not questions that one works through definitively. We need to pay more attention to the provisional nature of the resolution physicians make to the conflicts such subjects present. My own graduate students in the field now report that their informants ask them to play this quasi-

therapeutic role, also. Like me, they find it both disturbing and flattering. The fact that our subjects choose to use us in this way suggests both that we need to learn methods for containing and managing these encounters, and also that we cannot define the field-worker role totally in instrumental terms. We come to have identities for our subjects quite independent of the ones we promote for ourselves. Ironically, it is often these identities that yield the greatest amount of data.

Fifth, houseofficers viewed me as a "referee" in conflicts among themselves over patient management, quarrels over the equity of the division of labor, and disputes about whether or not patients understood what was happening. In the midst of such disagreements, one houseofficer would turn to me and ask: "Well, what do you think? Which of us is right?" These were not comfortable situations for me when I could hide behind the observer role. A judgment was demanded as the price for my continued presence. Moreover, any judgment was certain to alienate one of my informants. I developed tactics for throwing the question back to the disputants or for pointing out the merits of either side, or making a joke of the entire dispute. Over time, I tried in vain to teach my subjects that such conflict resolution was not a proper part of my role. Nevertheless, being asked to referee disputes was a recurrent and always problematic task and not one that I ever felt totally comfortable with. As I felt more accepted, I was somewhat better able to put questions off. But in the beginning, I was stiff, uncomfortable, and always mindful of my relationships with each party. As a referee, I was able to elicit good material when I was able to turn the dispute into an occasion for discussing different attitudes and beliefs toward medical practice. Unfortunately, I was not always levelheaded enough to accomplish this because I felt so put-on-the-spot by such confrontations.

Sixth, I was the group "historian." Because of the way housestaff rotate through the various services, it was not unusual for me to have been on either the Able or the Baker Service longer than any particular houseofficer. When this occurred, I was expected to know something of the history of the different patients on the service. I was expected to keep track of attendings' remarks and

verify them for absent group members. The role of group historian served me well, since it forced housestaff to depend on me for information that they needed. This created a greater sense of mutual obligation between housestaff and myself and to the degree that the information I supplied was reliable, I established my credibility. Also, I was a short-run as well as long-run historian. I would often ask housestaff about action that I could not watch but was interested in. (Much work is done individually, and on any given day I saw only a portion of possible action.) On more than one occasion, my questioning reminded houseofficers of a task that had until then slipped their minds. My unwitting reminders saved them from oversights which would have gotten them into trouble. The fact that such incidents occurred further indebted housestaff to me and heightened my legitimacy. A field-worker pays a price for this kind of legitimacy, though. The historian role itself presents some of the most common moral dilemmas that a field-worker faces. Each time I gave such a reminder to a houseofficer, I changed what would have otherwise happened without this intervention. Lab tests, consultations with other physicians, and conferences with patients and their families—all these were on occasion events that took place because I reminded houseofficers of them. By jogging the memory of houseofficers in this way, I made it impossible for myself to observe what happens when these events fail to occur. On these occasions, I did not intend to alter the natural course of events; but it did happen that I unwittingly created an occasional participant-observer effect.

Despite the fact that it was not my intention in these instances to change the action I was studying, one can see very clearly that errors of omission present the observer with a moral dilemma. If one does remind a houseofficer, one disturbs by that act the very relationships one is attempting to study. However, if one does not remind the houseofficer—and yet knows he has overlooked something—it is possible that a patient's care will be compromised. On most cases when I asked if something had been done, I did so because as a sociologist I was particularly interested in seeing or hearing a report of that specific action, and usually because I was

unaware of whether it had occurred or not—I was trying to orient myself. If the houseofficer had forgotten about the task I was asking about, if it had completely slipped his mind, then that fact told me something about the difference between a sociological perspective and a surgical one; and I learned something more about the structure of the surgeon's life-world. There was one category of event, however—conferences with patients and families—that I asked about more than others. Here I was often conscious of my participation in the scene, but thought that some patients (exactly which patients these were and why I reacted to them the way I did is a complex matter that I do not understand) deserved fuller explanations than they often got from the surgeons. A question that I cannot answer is, Did the surgeons see my role as a sociologist such that they presumed that I was interested in such group phenomena, and did they come to rely on me to remind them of their diffuse obligations to patients and their families? Is this the major role they assigned me in the group? If it was, who was responsible for making sure that this team responsibility was filled when there was no sociologist present? Whatever the answers to these questions, a rule of thumb I applied was to keep my reminders as few as possible. This was a rule I occasionally broke because of my feelings for a patient and his/her family. I must also confess to one other category of event on which I routinely broke my own rule. As a group historian, I occasionally asked questions that served as reminders to subjects that I felt were hostile and/or skeptical of my sociological enterprise to establish that I belonged in the field; that I was concerned, aware and helpful; and that I was a legitimate member of the group. The fact is I occasionally used my questions to demonstrate the ways the group needed me. (One could also argue at the same time I was proving to myself that I served some useful purpose in the group.)

If errors of omission present observers with one type of moral dilemma, errors of commission present him with another. In the case where the field-worker knows that some harm has been done to a patient through physician or nursing error, does the observer have any direct, ethical obligations to the patient and his/her

family? That is, should the field-worker either inform the patient or find some alternative means of making public the error? I chose not to do this for a variety of reasons. As a pragmatic matter, being a patient-advocate would have made the kind of fieldwork I wanted to do impossible. Moreover, I felt a responsibility to other medical sociologists who wished to undertake field projects in the future. I was aware that my conduct could either make the way more or less difficult for those who followed me. While some participant-observer effects seem acceptable to me, others, those that contravene the basic operating norms of a group, are not acceptable. These larger effects not only distort the phenomenon under study, they make it impossible for later field-workers to gain access to and legitimacy within medical settings. Most important, I felt I could discharge my ethical obligations to patients more effectively by describing the general categorization and management of error rather than tilting at windmills in one or two select cases. On the face of it, this kind of advocacy would not seem to be much of a problem; in fact, it is hard to imagine a field-worker, insistent on imposing his definitions of justice on a scene, completing his work. However, this fact is not as significant as the importance of recognizing the strong feelings that observing in a hospital evokes, and restraining the "rescuer" impulses that witnessing so much pain, suffering, and death provokes. Whatever roles houseofficers cast me in or I assumed, the major irony of the field-worker role was always apparent: on the one hand, I was intimately involved in all aspects of the everyday life of a group; and on the other hand, I was constrained by the nature of my task to exert as little social influence in that group as possible. So, my sensitivity to the group's actions and their consequences was heightened at the same time that my theoretic commitments restrained me from even raising the group's consciousness about the effects of its own actions.

I had less intimate contact with attendings than with housestaff, and assumed and was assigned a narrower range of roles. Most commonly, I was seen as any other "medical student." Attendings assimilated me to the group by treating me like any

other member of the group. They had me look down proctoscopy tubes, rake abdomens feeling for a mass, and learn to hold retractors properly. Their treatment of me helped strengthen my ties to houseofficers, who saw that not only was I not in league with attendings, but that, like them, I was the occasional butt of an attending's sense of humor. By the same token, my own willingness to take part this way in group life served notice to attendings that I was willing to do what was necessary to complete my project. When attendings viewed me as a medical student, they often tried to teach me concise medical lessons. Whatever problems of identification and rapport I might have had, it is interesting to note that attendings had some of their own. Toward the end of my fieldwork, two attendings approached me, told me that I must be interested in medicine to have spent so much time at Pacific, and then informed me that if I wanted to go to medical school, they would help me in any way they could. I took their offer as an indication that perhaps I had been in the field long enough.

The incident above is related to another role attendings cast me in—their "advisee." Attendings offered two types of advice. First, there was "scientific" advice. Here attendings would address themselves to the design of my study. They wanted to know about my control groups, my measurement instruments, my hypotheses, and all similar paraphernalia from the type of research they engaged in. When I would explain that my model for research was somewhat different than theirs, they were skeptical but generally tolerant. After all, I was the sociology department's problem, and not theirs. Second, attendings offered "interpretive advice." When we were alone, they would often explain why they acted in certain situations the way they did, what they felt to be the burdens of their authority, what the major problems doing surgery in a major medical center were, what the personal strengths and weaknesses of their colleagues were, and so on. Like houseofficers (although the opportunity arose less frequently), attendings unloaded themselves on me. It is worth noting here that I was ten years younger than the youngest

attending, so the fact that they used me as a "sounding board" points to ways in which the surgeon's role remains disturbing even to those who have practiced it all of their adult lives.

In addition, attendings used me often as a "clown" to diffuse tensions in the group. When things were going poorly, attendings on occasion would question me like any other member of the group and then poke fun at my fumbling and ignorance. Sitting around the doctors' lounge, the rigors of academic life would be compared unfavorably with those of surgery; and my manly virtues would be impugned. It was not always as a clown that attendings used me to ease tensions. Just as with housestaff, I was asked to referee conflict. My study was used by them to deflect conversations from their course. So that often when faced with troublesome questions from nurses or other physicians, they would give a noncommittal response and then ask me to explain my study. They would ply me with questions until they were sure the conversation could safely resume. These three roles were not assumed with equal frequency nor were all assumed from the first day of fieldwork. From the beginning and most generally, I was assimilated as a medical student. Then I was used as a "diffuser of tensions." If I passed the test implied in this role, I became an occasional confidant of the attending. With one attending who was not greatly invested in clinical issues, I was never other than a quasi-medical student. With the others, I played all three roles, albeit with varying frequency and intensity.

So far in this description I have concentrated on the various roles I played in the field setting. The rationale for this is simple. In the analysis of our fieldwork data, we concentrate on the role relations among participants in the scene we choose to study. Yet we often pay comparatively less attention to our own role relations with the subjects who make our knowledge of the setting and of the action possible. Since in fieldwork these relationships are our major methodological tool, they require serious discussion. How we manage these relationships determines the depth, validity, and reliability of the data we collect and the inferences we draw from it. We need devices that ensure control of our like and dislike of various participants, the weighting that we give

incidents, and the ways our own everyday roles impinge on and create strains with the field-worker role. The problem of objective description and analysis is in itself formidable even if one were only observing a television program, for example. In fieldwork, the problem is made more complex because of the deep relationships and attachments one builds over time to one's subjects. As Charles Lidz (1977) has correctly pointed out, the right and privilege of being an observer is a gift presented to the researcher by his host and subjects. So the observer has, in addition to whatever the other problems that becloud his structured role-relations with his subjects, the very special problems that attend the giving and receiving of gifts. I would agree with Lidz that the recognition and proper understanding of the gift relationship serves as both a convenient theoretical framework for understanding the peculiar dilemmas of the field-worker and at the same time a formidable restraint on bias in observation and interpretation.

First, what are the special features of the gift relationship? As Mauss (1967) pointed out in his classic statement, the giver and the recipient of a gift are involved in an interactional sequence that involves giving, receiving, and reciprocating. Even more important, involvement in a gift cycle creates a solidarity among participants and signifies that they have obligations toward each other that extend into the future. The fact that the field-worker is both the receiver of a gift and a guest means that he has a diffuse sense of obligation to his host-giver-subject. Field-workers have long recognized their indebtedness to their subjects. In fact, as one reads accounts of fieldwork itself one senses that this burden is truly "the magnificient obsession" of those who employ this research method. While not explicitly analyzing the observer role as a gift relationship, field-workers worry, in their writings, over fulfilling their obligations to their subjects, over balancing personal debts to individuals against universal debts to the discipline of sociology, and over discharging obligations to subjects that extend beyond the life of any particular piece of research. In addition, there is the field-worker's typical ethical dilemma: what if the data I gather are potentially harmful to my subjects? What

if the facts themselves betray those to whom I have become so attached over so many months? Others have spoken of the "tyranny of the gift" in different contexts, but it is clear that the gift of access, of witnessing social life as it is lived in someone else's environment, exercises a tyranny of its own. This tyranny has as its most distinctive features three significant elements: (1) the danger of overrapport, so thoroughly merging with the subject's point of view that one cannot achieve the critical distance necessary for analysis; (2) the danger of overindebtedness, so thoroughly feeling a sense of diffuse obligation that one can no longer assess what one does and does not properly owe his subjects; and (3) the danger of overgeneralization, so thoroughly idealizing one's subjects that one sees their behavior as overly representative of all persons in a class.

I was protected from overrapport and overindebtedness in part by the very structure of hospital life. Unlike field-workers who spend years with an unchanging population, my subjects rotated through the surgical services fairly rapidly. Some stayed for as little as a month; none stayed over three months. There were housestaff I liked very much; housestaff I detested; and others I barely got to know. Whatever the case, there was an unending parade of housestaff. The mere fact that I was observing so many people in rapid succession prevented overrapport with any one subject. There was, of course, the danger that I would identify with the structural position of being a houseofficer, even if I avoided strong attachments to specific individuals. After all, I was a twenty-four-year-old graduate student, subordinate to a dissertation committee, and struggling to achieve autonomy within my own profession. Surely there was a clear and ever-present danger that, being a subordinate myself, I would overidentify with the subordinate and his problems. Overrapport with housestaff was avoided by two features of my everyday life. First, my wife, Marjorie Waxman, was supervising child-care workers in a psychiatric hospital at the time of this research. My conversations with her made me sensitive to the problems of the superordinate, especially the difficulty of balancing the needs of patient care with the needs of subordinates to develop their own skills and judgments through their own mistakes. Second, my major field-supervisor constantly pointed out to me instances when I seemed to take subordinate complaints too much to heart and urged me to see beyond the specific perturbations in housestaff-attending relations to see what are generic problems in superordinate-subordinate relations. Of course, I also had to guard against the opposite problem, overidentification with attendings. After all, did they not have, to an exaggerated degree, the autonomy I was working so hard to obtain? Here, I was protected from overrapport by a number of factors. First, my relations with attendings were not as regular, intense, or relaxed as those with housestaff. Second, several of my own friends in medical training served as constant reminders of the subordinate's problem. Third, there is a general resistance in sociology to sympathize with the perspective of authority. Authors such as Becker (1970) constantly remind us whose side we should be on.

My resolution to the problem of overindebtedness was somewhat different than the resolution to overrapport, and unfolded over time in two quite separate phases. A moderately sensitive observer of life in the surgery wards of a hospital will be flooded with feelings of helplessness. These feelings themselves have two distinct components. First, witnessing so much pain and suffering, the field-worker wants to roll up his sleeves and do something, anything. At the same time, seeing death as an everyday event makes one guilty and overly aware of one's own good fortune. As a field-worker, I was often made uncomfortable by what I saw. I felt I had stumbled into incredibly intimate and significant slices of patients' and doctors' lives. Much like any person who sees more than he would like of a friend's life, I felt guilty about some of the knowledge I had gained, worried over what the boundary between privileged information and data was, and wondered about how I repaid my obligation to my subjects. In the short run, the housestaff resolved the problem of helplessness and indebtedness by the roles they cast for me. When housestaff demanded that I help out by wheeling the chart rack, opening dressings, acting as a group memory, they provided me a means to cope with my own helplessness and assuage my guilt at

the same time that they incorporated me into the group. While I was in the field, my involvement in the group resolved for me the problems I experienced as an indebted guest.

These problems reemerged when I left the field and began writing up the report. I saw much that was wrong in surgery, but what I saw emerged against the background of dedicated people working tirelessly at very difficult and complex tasks. What if what I reported was harmful to those that made the account possible? I had the problems of balancing my universal obligations to sociological analysis to my particular obligations to my research subjects. Unfortunately for me, I could not expect anyone to point the way by the everyday roles they cast me in. One thing I did was not begin writing immediately on leaving the field. Before drafting this report, I let the freshness of the experience recede somewhat so that I would not be overwhelmed by the memory of my relations with particular individuals. Second, when recording field notes, I made every attempt to keep my description of events as behavioral as possible, and my recording of conversations as verbatim as possible. At all times, I tried to keep "in situ" analysis separate from my field descriptions. I kept two different categories of field notes: (1) a log of happenings, conversations, and conferences; and (2) a separate running analysis. In this way I was later able to identify for and correct problems that resulted from overraport or overindebtedness. By this procedure, I would see where the data confirmed, or failed to support, my analyses. There is in fieldwork always the problem of selective data collecting and analysis that might harm one's subjects. Any definitive resolution to this problem awaits more sophisticated, but at the same time unobtrusive, techniques of gathering field data. At the present, the length of time we spend in the field and our own intellectual integrity is our only protection to this problem. Third, I shared my report with the surgeons upon its completion. We discussed areas of disagreement between our interpretations. In particular, they objected to the rhetoric of sociology; they saw my framing the problem with a deviance and social-control vocabulary as unnecessarily pejorative, but they accepted (even if they did not

agree with or fully understand) my rationale. They agreed that I had most of the phenomenological description right, if not always the interpretations. However, where there were interpretive disagreements, the surgeons attributed them to my being a sociologist, and accepted my analysis as valid from my frame of reference. They suggested ways that I could better protect the anonymity and confidentiality of individuals. For example, at their request, I changed the pseudonyms I originally chose and excised all dates from my field materials. I resolved part of my debt by allowing the surgeons to observe me as a sociologist at work.

Overgeneralization is also a recurrent problem for field-workers at two levels. First, there is the danger that one particular event will become etched in the field-worker's memory as emblematic of the way action is organized in an environment. That is to say, field-workers may overgeneralize incidents and see them as representative of categories of action. Second, field-workers may overgeneralize from their particular sites to all other types of similar settings. In the first case, I avoided overgeneralization by making sure I had at least two independently generated examples of the same phenomenon before I began to make inferences. My operating rule here was, as far as I can see, not fundamentally different than those that survey researchers use to ensure reliability in their studies. Also, I was very careful to follow particular incidents through many levels of social organization. For example, I was able to test my inferences about normative error in the promotion meeting, where I observed the criteria attending surgeons use to judge the fitness of housestaff for surgical careers. Throughout my fieldwork, I was very careful to test observations in one context against those of another. On the other hand, there are observations I made that did not find their way into the fieldwork because I felt my inferential base was too thin. On one occasion I watched a series of unexpected deaths and complications, which occurred in quick succession, temporarily destroy the morale of Able Service. These occurred during the end of a rotation, while a chief resident was on vacation. I developed an explanation which related the occurrence of failure and group

panic. However, during the rest of my fieldwork, I did not have the opportunity to observe another rash of failures. As a result, such speculations did not find their way into the manuscript. As an aid to the reader, I have tried to indicate throughout the text where inferences are based on slim observation.

There is a second type of overgeneralization—generalization from the specific case, Pacific Hospital, to hospitals that are not included in Pacific's class. Pacific is a member of the medical elite. There are perhaps twenty hospitals in this country with the same reputation for excellence that Pacific has. I am confident that the description of controls at Pacific is one that fits virtually all members of this class. I am also confident that I have described and analyzed a professional "ideal type," an environment where the major preoccupations have to do with the aesthetics and elegance of surgery, uncontaminated by such mundane matters as fees, social networks to generate referrals, and market pressures. How the system of social control I described is modified in more modal settings is a question that deserves further research, as is the question of how comparable it is to the systems of social control in other professions. These are questions that I am beginning to work on now. There is certainly no intrinsic reason that fieldwork cannot be as cumulative as any other area of sociology. The benefit of using a site like Pacific as a starting point is that physicians there are quite self-conscious about their place in the medical world, and make explicit reference to why they deserve an esteemed place in the profession. Moreover, being so self-conscious, they are eager to inculcate into their young recruits the values in which they believe so strongly. Attending surgeons see their trainees as extensions of themselves in many ways; one of these is that they expect the conduct of those whom they train to reflect honor and glory back on Pacific.

Out of the Field

One peculiarity of field research is that one discovers what one learns in the field often only after one has left the field. So, strangely, the most creative and fruitful periods of field research

are those where the researcher steps back from his immersion in an alien world, takes stock, and decides where to go next. By alternating periods of total immersion with periods of analysis, the field-worker can avoid phenomenological fatigue, that is, the sense of "I've seen it all before," and can continually refine and sharpen the questions asked of a particular research. For this study, I normally spent two or three months in the field, full-time, recording my observations in as straightforward a manner as possible, left the field for two weeks to a month to analyze my data, and then returned with a greater sense of what I now knew and what I still had to learn about the conduct of surgeons. For example, after retreating from the field the first time, I discovered in my notes that surgeons treated some mistakes as normal occurrences, while other events were treated as quite extraordinary and unacceptable performances. But at that time, I did not know why one set of events was categorized by actors in one way and another was so differently treated. It was clear very early in the study that the seriousness of an error was not determined by a set of precedent variables such as the patient's age or social status, nor was it determined by such antecedent variables as what happens to the patient. An error's seriousness was related only incidentally to the patient and his condition. On the other hand, seriousness was related in a very direct fashion to the attending's reaction. Discovering this, I felt reassured and at the same time I knew nothing, since I did not know what determined the attending's reaction.

My first immersion in the field in some sense determined the direction of most of my subsequent observation, as I tried to unravel what the bases of attending evaluation were, how clear these were to housestaff, and how widely they were shared among all members of the team. A dialectic of immersion and reflection that began the first day I arrived on the surgery wards at Pacific—and which I am sure is not completed—allowed me a continuous sense of discovery. It is worth noting, for instance, that I did not discover quasi-normative errors until after I left the field entirely. I had not seen while in the field so clearly how the lines of cleavage among the ranks were structured. In turn, my new

understanding that there were two distinct types of normative error forced me to revise my conclusions about the social controls in surgical training by allowing me to see some of the ways its ethical content is undermined. I also gained a new respect for what it means "to let your field data speak to you." There are any number of things about the field that one discovers only by not being there. I discovered the "charisma" of surgery, not in the hospital but at parties and other social events. Being a sociologist does not normally make one the center of attention; however, being a sociologist who studies surgeons does. As my research progressed, I was struck by the almost primal awe my friends and acquaintances had for surgeons. Normally sophisticated urban dwellers with Simmel's (1970) blasé attitude would literally beg for details about what surgeons were really like, about what went on in operating rooms, about what their doctors were really like. It occurred to me that I had, through my close association with them, borrowed part of the surgeons' charisma. Seeing that the surgeons' charisma was not just some dramaturgic creation in organized social settings helped me understand how the surgeons' autonomy was as great and unchecked as it was. It also made me see that this was not simply a result of surgeons' behavior but was also nurtured by patients' needs and desires. Cocktail party curiosity also alerted me to how little people know about the medical care they receive, how few people have ever been in an operating room as an observer, and how powerful and coexistent are the contradictory impulses both to glorify and to degrade the surgeon. (Cocktail parties also taught me of the need for circumspection and tact, which I shall comment on below.)

Unfortunately, I did not learn as much from leaving the field as I might have, because I did not keep a careful record of why I chose to leave the field at the times I did. I assume that had I been as clear as I might about my comings and goings, about what was going on in the field that would make me willing or eager to leave, then I might have gained some greater understanding than I have about the surgeon's life space. I know there were days when I had to force myself to go to the hospital, and other days when I

grabbed at any straw as an excuse for a breather. But when and why these feelings intensified at the times they did, I do not know. Such knowledge is of more than private psychodynamic interest (though it is of that also), for it helps make clear what strains field-workers are subject to in medical settings, how they can better prepare themselves, and how they can be better supervised. The end result from such understanding would be more valid and reliable monographs, less likely to be subject to any "observer effect."

Data, Confidentiality, and the Field-Worker

I indicated above that I learned some lessons at cocktail parties about circumspection. In a literal sense, this is not true; but social situations presented me with a sticky problem. When I was in the middle of the field, disguising the place and principals of my study was not as easy as it is in this report. I was always aware when I spoke that others knew those I spoke of, and that a too-loose tongue could hurt me and them in many untold ways. Since I promised my subjects confidentiality and anonymity, the "cover story" I devised to manage social situations was as consequential as the one I devised to manage field introductions. Only by assuring confidentiality and anonymity could I satisfy my subjects that my study would be within the bounds of current medical ethics. Both promises present some dilemmas, however.

First, I could not ever be sure that some enterprising person would not be able to figure out my place and principals. Essentially, confidentiality and anonymity were the promises I made but I had little control over their fulfillment. There have been recent debates about whether field-workers should go to the bother of making general "covering names" for their sites, and whether they should disguise their subjects. It seems to me that such fictitious names do more than provide confidentiality and anonymity: they highlight the generalized features of our descriptions and minimize the particularized aspects. To my mind, this

aspect of naming is even more important in some ways than confidentiality and anonymity in that it creates a fieldwork literature rather than a description of specific places; for example, Bellevue, Long Island Jewish Hospital, Johns Hopkins.

Others have advocated that to make fieldwork more rigorous and to display our methodology more openly, we should open our notebooks to the curious. Such procedure would allow others to see how we manipulate our data and fit the canons of science in general. Such a proposal troubles me because, as a sociologist, I gathered litigable material from subjects who trusted me. As a sociologist, I have no legal right to claim a privileged or confidential relationship with my subjects; my notes are subpoenable. If I opened my notebooks in the manner necessary to make clear the operations I performed on my data, I risk having those notebooks put to uses other than those I approve of by people whose motives I may distrust for reasons I think are less than just. Involved here is a difficult problem: how to afford my subjects and myself enough protection so that we feel comfortable doing the study, at the same time displaying my data in a way that assures others of the validity and reliability of my research. I have indicated for the reader what I have done to satisfy myself of this report's accuracy. For the moment, I suggest that this—along with giving and receiving adequate supervision—is the best I can do.

Conclusion

As a research method, fieldwork yields results that often are phenomenologically rich, theoretically provocative, and practically useful. The major liability with this research method is that there are no procedures internal to the techniques of field research itself that control validity and reliability. The major data-gathering technique that the researcher utilizes is his relationship with his subjects. For better or worse the rules that govern relationships are less precise, harder to articulate, and more complexly interwoven with other normative systems than the rules that govern, for example, item construction on a questionnaire. By the same token, the field-worker's sampling procedures and

the manipulations he performs on his data are often left unexplained. Clearly it is not that field-workers do not gather data by rules, sample from everyday life in a complex fashion, or manipulate their data. Rather, it is the case that to state what the rules are requires statements of such generality that they are of little use in any particular setting. The reason for this is not hard to understand: our subjects are never simply subjects. They also occupy a variety of other roles and the rules that govern relationships, for example, with physicians are different than those that govern relationships with heroin addicts. This inherent variation imposes on the field-worker his special obligation. The field-worker must describe the role relations that he had with his subjects as clearly and honestly as he can. The field-worker must describe how he avoided overinvolvement or on what occasions he succumbed to it, how he avoided overgeneralizing, and how he avoided overindebtedness to his subjects. A clear statement of the social matrix out of which the field materials emerged allows the reader to judge validity and reliability for himself. At the same time, it has the added benefit of providing comparable accounts of the fieldwork experience which allows us to see what is general to a researcher's relations to his subject and what is particularly his own.