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Confessional Tales

If you want to understand what a science is you should look in the first instance not at its theories or its findings, and certainly not what its apologists say about it; you should look at what the practitioners of it do.

Clifford Geertz

Chapter 3 suggested that ethnographic writing is anything but a straightforward, unproblematic descriptive or interpretive task based on an assumed Doctrine of Immaculate Perception. Rather, ethnographic writing of any kind is a complex matter, dependent on an uncountable number of strategic choices and active constructions (e.g., what details to include or omit; how to summarize and present data; what voice to select; what quotations to use). In this chapter I explore another representational form of ethnographic writing, the fieldwork confessional. It is an increasingly popular genre that contrasts sharply in a number of ways to the realist tale. The distinguishing characteristics of confessional tales are their highly personalized styles and their self-absorbed mandates.¹

The confessional tale is often a response to some of the realist conventions that have proved most embarrassing. In some instances, the confessional tale stems from the notorious sensitivity of many fieldworkers to aspersions cast on the scientific status of their undertakings. The result, then, is an attempt to explicitly demystify fieldwork or participant-observation by showing how the technique is practiced in the field.² Stories of infiltration, fables of fieldwork rapport, minimelodramas of hardships endured (and overcome), and accounts of what fieldwork did to the fieldworker are prominent features of confessions.

In other instances (perhaps more important), the confession is

a response to the growing importance and penetration of European social thought in American social science.³ In various ways, some mentioned in chapters 1 and 2, the implications of phenomenology, hermeneutics, semiotics, and other interpretive procedures are being felt in the empirical trenches. By and large, American fieldworkers have been, until fairly recently, at ease and comfortable with their seat-of-the-pants, homespun methods, and have been unreasonably proud of their outward-bound, lone-wolf, muddy-boots image. Given the lofty issues of human meanings treated in ethnographies, many a fieldworker-author fits Boon's (1982: 5) ideal type of "Icarus with dirty feet."

Such pride apparently goeth before a fall, because in the confessional form of ethnographic writing, fieldworkers now show themselves to be somewhat nervous about the looseness and open-ended nature of their work. Considerable worry is expressed about the obvious lack of a theory of description that might help legitimize an enterprise premised on the delicate good-faith assumption, the assumed self-evident value of exploring little-known social worlds, and the presumptive use of natural science notions concerning the power of observation. Such discomfort surfaces in confessions as writers try to show that ethnography is not merely old-fashioned social science in its geriatric decay. These writers attempt to demonstrate that an ethnographic report is more than a personal document; that it is something disciplined by proper fieldwork habits, including the attention an ethnographer pays to the epistemological problems characteristic of social science. Most confessionals have at their core some hope of making fieldwork, if not fully safe for science, at least respectable in terms of upholding some community standards and disciplining the undisciplined of fieldwork. As with realist writings, there are conventions at work in the confessional tale. A discussion of three such conventions follows and serves to set up an example.

Personalized Author(ity)

Author-fieldworkers are always close at hand in confessional tales. Their writings are intended to show how particular works came into being, and this demands personalized authority. No longer is the ubiquitous, disembodied voice of the culture to be heard (e.g., The police do X). In its place is a person (e.g., I saw the

police do X). There is an intimacy to be established with readers, a personal character to develop, trials to portray, and, as with realist tales, a world to be represented within which the intrepid fieldworker will roam. With this last feature, the aims of fieldwork confessionals and realist accounts may overlap, even though the textual means of supporting the resulting cultural portraits are quite different.

Confessionals do not usually replace realist accounts. They typically stand beside them, elaborating extensively on the formal snippets of method description that decorate realist tales. They occasionally appear in separate texts and provide self-explanatory and self-sealing accounts of how the author conducted a piece of research reported elsewhere. Confessions also appear, with increasing frequency, as separate articles, chapters of books devoted to fieldwork practice, or lengthy appendixes attached to realist monographs. All are distinct, however, from the ethnography itself. The confessional writings concern how the fieldworker's life was lived upriver among the natives. They are concerned primarily with how the fieldwork odyssey was accomplished by the researcher. There is then a clear break between the representation of the research work itself and the resulting ethnography (which appears elsewhere in the text or in another text altogether). Normally only the former is of concern in a confessional tale.

Much confessional work is done to convince the audience of the human qualities of the fieldworker. Often the ethnographer mentions personal biases, character flaws, or bad habits as a way of building an ironic self-portrait with which the readers can identify (See, I'm just like you, full of human foibles). The omnipotent tone of realism gives way to the modest, unassuming style of one struggling to piece together something reasonably coherent out of displays of initial disorder, doubt, and difficulty.

According to Clifford (1983a), there are two conventional ways for ethnographers to orient themselves for the confessional audience. One is to cast oneself as a simple student of the observed group, an apprentice of sorts, who comes to learn of the culture much as any child or newcomer to that culture might (Van Maanen and Kolb, 1985). Learning from living in the culture is the predominant theme. The other way, possibly more fashionable these days, is to cast oneself as a translator or interpreter of

indigenous texts that are available to the ethnographer in the field (Geertz, 1973). The major problem with this tactic is convincing the audience that such texts are in fact authentic, natural, useful for analytic purposes, and more or less untainted by the fieldworker's touch. Fieldworkers, unlike literary critics, historians, or linguists, face the problem that their texts (on behavior, belief, ritual, etc.) taken from the field must first be constructed, since they do not come prepackaged. The first orientation lends itself nicely to a cognitive, rule-based and behaviorally focused ethnographic display; the second to a more reflexive, language-based, interpretive one.

The details that matter in confessional tales are those that constitute the field experience of the author. This human bundle of exposed nerve-endings stands alone in the culture supposedly perceiving and registering the various happenings around him. Emotional reactions, new ways of seeing things, new things to see, and various mundane but unexpected occurrences that spark insight are all conventional confessional materials that suggest how the fieldworker came to understand a studied scene. Moreover, confessional writings rarely portray the author as a passive, unremarkable character who simply stands around waiting for something to happen or for the arrival of the white flash of discovery. Who could trust such an unadventurous and timid soul? The narrator of the confessional is often a foxy character aware that others may be, intentionally or unintentionally, out to deceive him or withhold important information. The ethnographer as the visible actor in the confessional tale is often something of a trickster or fixer, wise to the ways of the world, appreciative of human vanity, necessarily wary, and therefore inventive at getting by and winning little victories over the hassles of life in the research setting (e.g., Berreman, 1962; Powdermaker, 1966; Gans, 1982; J. Douglas, 1976).⁴ Nor is the fieldworker who writes most confessions brimming over with correctional zeal or tied to hard-and-fast ethical principles. Indeed, some of the most unflattering portraits of ethnographic practice arise, as the label implies, in fieldwork confessions where it seems apparent that the researcher has less patience and good will than his subjects (e.g., Turnbull, 1972; Malinowski, 1967).

The Fieldworker's Point of View

As autobiographical details mount in confessional tales, it becomes apparent that the point of view being represented is that of the fieldworker. Typically, the concern for the fieldworker's perspective is told as something of a character-building conversion tale in which the fieldworker, who saw things one way at the outset of the study, comes to see them in an entirely different way by the conclusion of the study. The new way of seeing the world is normally claimed to be similar to the native's point of view. But careful attention is given to insuring that the fieldworker does not appear to be fully altered, the proverbial cultural dupe or convert. The attitude conveyed is one of tacking back and forth between an insider's passionate perspective and an outsider's dispassionate one. Perhaps no other confessional convention is as difficult for the writer as maintaining in print this paradoxical, if not schizophrenic, attitude toward the group observed. A delightful dance of words often ensues as fieldworkers present themselves as both vessels and vehicles of knowledge.

In much confessional writing, a sort of tentative "surrender" is used by the fieldworker as a temporary resolution to the daily problems of fieldwork. But, going native can hardly be presented with terminal glee. The mere presence of the confessional suggests that the fieldworker is now seriously back among his peers, ready to tell of the adventures in the field. This is perhaps why some find Carlos Castaneda, the flying nun of anthropology, such a silly character, for if he were fully committed and converted why would he bother with us?

A reader often learns of the ethnographer's shifting point of view during a period of fieldwork in a confessional. Common features of research confessions are episodes of fieldworker shock and surprise. Subjects include the blunders of fieldworkers, the social gaffes they commit or secrets they unearth in unlikely places and ways. Such accounts are frequent and indicate perhaps that despite the different theoretical languages and attitudes taken into the field by ethnographers, the significance of inserting the self into the daily affairs of others is, at least on the experiential plane, similar for everyone.⁵ The unplanned, almost random, happen-

stance is dramatically set forth in confessional tales with the universal message attached that fieldwork is as much a matter of luck and being in the right place at the right time as it is a matter of good training. Given this advice, time in the field and close, involved contact with the group studied (allowing for a greater opportunity for lightning to strike) provide the normative guidelines (the more the better).⁶

There is, however, a line to be drawn, for the fieldworker cannot stay in the field forever and still be considered a fieldworker. Conventions grow up around what is to be considered an adequate field experience, and various communities (and subcommunities) of fieldworkers adopt different standards. The more targeted or limited the ethnography is to a particular and well-defined cultural problem, the less time in the field is thought necessary in order for revelation to strike.

Much of the confessional genre is familiar to readers of method texts where the various pros and cons of intense involvement or participation in the culture of interest are discussed. Within confessional ethnography, however, the writers seem less sanguine about the presumed wide range of role options available to fieldworkers. There is, in fact, something of a they-made-me-do-it character to many confessionals in which certain non-negotiable demands are made by the natives, the refusal of which would mean instant exile. These demands may be tied to biographical particulars (e.g., young women must behave appropriately) or to situational particulars (e.g., "don't do that now"), but such demands are represented as being made on the fieldworker in no uncertain terms. In confessional tales, then, cultural knowledge may rest securely on the testimony of personal experience and can be presented to readers in the form of explicit behavioral norms or interpretive standards the ethnographer learned to follow in the field in order to stay in the field.

Naturalness

The last convention of the confessional tale I want to exhibit is also the broadest and perhaps the most inconsistently treated one. It concerns the way fieldworkers argue that their materials are reasonably uncontaminated and pure despite all the bothersome problems exposed in the confession. Fieldwork confessions nearly

always end up supporting whatever realist writing the author may have done and displayed elsewhere (in or out of the text in which the confessional tale appears). The linguistic footwork required is considerable, but it often boils down to the simple assertion that even though there are flaws and problems in one's work, when all is said and done it still remains adequate. Though confessional writers are forthcoming with accounts of errors, misgivings, limiting research roles, and even misperceptions, they are unlikely to come to the conclusion that they have been misled dramatically, that they got it wrong, or that they have otherwise presented falsehoods to their trusting audience. The implied story line of many a confessional tale is that of a fieldworker and a culture finding each other and, despite some initial spats and misunderstandings, in the end, making a match.⁷

No doubt part of this is due to the screening policies of the professional communities at which fieldwork accounts are aimed, as well as the self-screening work of the authors, so that the only ethnographies in print are the more-or-less successful ones about which the author (and at least some reviewers) are fairly confident that the work is up to snuff. We rarely read of unsuccessful field projects where the research was presumably so personally disastrous to the fieldworker that the study was dropped or failed ever to find its way to publication. While there may be some nervous indications that things are not so certain as they appear in print or that future voyagers into similar research worlds may see things in different ways, confessional tales usually end on an upbeat, positive, if not fully self-congratulatory, note.

Stoddart (1985) provides a happy list of conventional practices of confessionalists by which some intractable fieldwork dilemmas can be said to be overcome (for all practical purposes). One practice, readily apparent, is the way authors normalize their presence coming on the scene, in the scene, and leaving the scene. Adequate ethnographic practice in the confessional requires fieldworkers to tidy up their roles and tell how they think they were received and viewed by others in the field. The good guy presentation is one familiar role, as is the just-like-anyone-else role, where the fieldworker claims to more-or-less melt into the research setting by virtue of being ever present and hence, disattended to by all.

Sometimes member tests for fieldworkers are represented as ways of displaying the acceptance and competence of the ethnographer. The confessional becomes, in part, a special kind of etiquette book in which fieldworkers show how they learned to comport themselves according to the proper standards of behavior in the culture of interest. The writer becomes a Miss Manners of fieldwork, a Dear Abby of the studied scene. Typically lessons are said to be learned through breaches of local propriety. Thus the experiences of the bumbling, awkward fieldworker, painfully figuring things out, provide a good deal of the substance of the confessional tale. The result is a guide to how to get along and live with grace and honor among fierce warriors of the Gitchi-Gumi, shy hunters of the frozen north, or laid-back winos of Peachtree Plaza.

Another way of showing that one has the right stuff to get to the heart of a culture is through displays of empathy and involvement. Under most conditions, fieldworkers are expected by readers, if their accounts are to be trusted, to like and respect those they study (and vice versa).⁸ They are also expected not to withdraw from the passing cultural scene but to become as involved and fully engrossed in the daily affairs of the people studies as possible. Empathy and involvement are, however, tricky matters. Writers of confessionals are therefore quick to point out that they liked some people more than others, and that there were certain periods during the study that were dull, uncomfortable, and perhaps distasteful. Moderation becomes the key which normalizes the setting and conveys to readers the sense that fieldwork is not very different from other kinds of work. The exotic is downplayed, the theatrical is understated, intense feelings are left out, and few of the absurdities of minding other people's business are allowed into the confessional tale.

Finally, consider how natives, as informants of the fieldworker, are handled in confessional writings. An often-stated platitude (however infrequently it is treated as such) notes that fieldworkers are only as good as their informants. Fieldwork novices are sternly reminded of such things in confessional accounts in which ethnographers must reveal (or claim to reveal) how they came to know what they know. In Back's (1956) words, the "well-informed informant" is one answer to this problem, and fieldworkers are

often under some obligation to trot out these legendary figures when daring to bare all. Such figures must be said to know the culture well. They are represented therefore as "experienced," "veteran," "revered," "respected," "senior," and "central" informants. The question here is how much knowledge the fieldworkers should attribute to their having squatted at the feet of their informants during their field trips.

Confessional ethnographies are ordinarily vague on such matters, for being precise may raise anxious questions for the reader about who is doing all the ethnographic work, anyway? Too little reliance on entitled informants may suggest that too many imaginative liberties are being taken in the realist claims of the ethnographer. Too much reliance on informants also raises anxious questions about the representativeness of the fieldwork materials and may lead readers to worry about the identity of the real author of the realist tale. Either over- or underappreciating informants provokes concern in readers.

Producing Confessional Tales

These three conventions provide a short guide to how confessional tales are constructed. The genre is now a fairly large one. While the quality of confessionals varies tremendously in terms of both the self-reflection of an author and the sophistication with which an author faces the epistemological issues involved in fieldwork, the necessity of providing a confessional to supplement substantive (realist) reports of fieldwork is now more or less institutionalized in both anthropology and sociology. It is pro forma these days to append a confessional to a fieldwork dissertation or to include one in a separate chapter of the thesis under the "methods" label. Most confessions, like most dissertations, never see publication. Those that are published, however, normally issue from authors who have first published notable, attention-getting tales in the realist tradition. The confessional is apparently interesting only insofar as there is something of note to confess as well as something of note to situate the confession.⁹ It is apparently more difficult to achieve the latter than the former. Authors of unknown studies, while they surely have much to confess, will rarely find an audience who cares to read their confessions.

Collections of autobiographical reflections on past projects represent the most common outlet for confessional tales of the field. In anthropology, Casagrande (1960) is a standard setter, focusing on work with informants. Other, more general-purpose collections include Epstein, 1967; Kimball and Watson, 1972; Freilich, 1970; Spindler, 1970; Ben-David and Clark, 1977; Naroll and Cohen, 1970; and, in a reorienting mission, Hymes, 1972. In sociology, Emerson's (1983) recent collection includes a good number of confessionals. Others include Shaffir et al., 1980; Bell and Newby, 1977; J. Douglas, 1972; Habenstein, 1970; Filstead, 1970; and Vidich et al., 1964. Also, since the confessional tale is ordinarily tied to giving the craft norms, a reader can find confessions—although they may be abstracted as missteps to be avoided—in fieldwork method primers, where authors in search of examples (extraordinary or dull) reach back to their own field experiences for guidelines for the novice. Examples include Agar, 1980; Burgess, 1983; Douglas, 1985, 1976; Loflanda, 1971; Schatzman and Strauss, 1973; Pelto and Pelto, 1973; Powdermaker, 1966; R. Wax, 1971; Glaser and Strauss, 1967; and Schwartz and Jacobs, 1979.

Let me now provide a reasonably elaborate example of the confessional tale. Again, it is my own work that serves as the exhibit. The excerpt is called "Johnny gets his gun." The materials were originally published under the more somber and serious title, "Notes on the production of ethnographic data in an American police agency," in 1981. The piece is drawn from a collection of confessionals written by fieldworkers interested in the sociology and anthropology of law. Unlike the previous example of a realist tale, which was reasonably self-contained, this illustration is only a small part of a fairly lengthy, normal-form confessional.¹⁰ It is edited here in rather herky-jerky fashion to explicitly highlight a few of the more rampant and obvious conventions of the genre.

Johnny Gets His Gun

In 1969, I wrote in my thesis proposal: "The police are quite possibly the most vital of our human service agencies. Certainly they are the most visible and active institution of social control, representing the technological and organizational answer to the Hobbes-

ian question of social order, the *deus ex machina*. Through their exclusive mandate to intervene directly in the lives of the citizenry, the police are crucial actors in both our everyday and ceremonial affairs, and, as such, deserve intensive and continual study for their role and function in society is far too important to be taken for granted or, worse, ignored."

Such high sounding sentiments provide, I am sure, the sort of doctrinal or ideological canopy which covers virtually all police studies. Yet, speaking sociologically, such statements are inadequate explanations for why such studies are undertaken in at least two ways. First, questions about the place of police study within the social sciences are glossed over neatly when a researcher points only to the "peculiar and significant" aspects of a specific research location. Second, research, especially research conducted in the fieldwork tradition, is both a social and a personal act, and, as such, is subject to the same sorts of biographically and situationally specific understandings through which any individual act can be understood.

Social scientists generally adhere to something of a hierarchy of professional values in which personal motives rank low and scientific motives high. At the apex of such a hierarchy are usually the formal theoretical concerns—what is it that is to be explained by the research? In my case, I was interested in questions surrounding adult socialization and the formation of occupational identities. As such, I searched about for a work world that might compel new entrants to accept, if not seek, a good deal of change in their personal identity and style of life in the process of becoming fully accepted members of an occupation and organization. From this analytic (and somewhat remote) standpoint, the police seemed to be a logical, and downright dramatic, choice. Yet, alternative possibilities were most certainly available—doctors, lawyers, crooks, priests, accountants, professors, architects, railroad workers, and so on. At this point, then, more gritty matters concerning why a specific researcher chooses to study a specific social world must be raised. Of course, to establish a motive, even one's own, is a tricky business. . . .

Three rather personal and perhaps pivotal factors seem best to explain my particular choice to study the police. First, when I began thinking seriously of the police as a topic for research in the

late sixties, the police were prominently fixed in the imagery of the day. Whether damned or praised, they were both participants and subjects in the dramatic and searing issues of public debate. Indeed, the police were visible reminders that the American society was bitterly divided. Second, however, not much seemed to be known about the police. While everyone I knew had cop stories to tell, there remained in all these tales something of a mystery as to why the police acted as they did. I discovered rather quickly that the police-related literature was at the time relatively thin, particularly when it came to describing the actual activities of policemen. Third, the available literature did not seem to square with my own random observations and run-ins with the police. Certainly, with few exceptions, the arid portraits which represented a good portion of the social science literature of the day (circa 1968) did not match my own visceral beliefs. As a young man growing up in a Los Angeles suburb, I had many times been subject to police attention. As a teenager driving a series of unusually shabby but stylized automobiles, it seemed as if I could never undertake a journey of any length without being stopped by the police for some reason or another. I had been arrested several times for minor misdeeds such as underage drinking, curfew violations, petty theft, and fighting. And, of more immediate experience, the cordons of grim, often antagonistic, policemen that demarked the boundaries of every political demonstration I attended could not be easily forgotten. In many ways, I both feared and loathed the police. . . .

My access (into the Union City Police Department) was, to put it bluntly, the result of good fortune. While good fortune does not lend itself well to analytic discussion, a few events in my entry process should be noted primarily to provide context for my discussion of working roles in the field.

Most critical to the entry process was a contact I developed at the University of California, Irvine, while in the midst of seeking a "representative" American police department (i.e., large and urban) within which to conduct my work. After six frustrating months of attempting to gain access, I discovered, almost by chance, a faculty member in the Graduate School of Administration, my school, who had once run a series of encounter group sessions with upper echelon police officers in Union City. I sought out this professor, told him of my general plans and interests, and

asked for any assistance he might be willing to provide. I also told him of the great difficulties I was having getting into a police agency. At the time we talked, I had been denied access to fourteen departments on various and sundry grounds, the most popular of which seemed to be the legal complications that administrators claimed my presence in their particular department would create. At any rate, this faculty member agreed to help and, using the rapport that perhaps only a sensitivity trainer can achieve, was able to persuade the command in Union City of the merits of my planned study and approach.

The rest of the negotiations followed in a rather hurried and *pro forma* fashion. Within a week, I flew to Union City, met with the Chief of Police and several of his aides. After an afternoon of meetings with these men, I was granted access to the department on what could only be called open terms. In the following two weeks, I had a number of telephone conversations to work out some administrative details of my study with the Captain of the Training Division, who was to be my official guide and sponsor in the organization during the period of my residency. The next week I began my work in Union City with a reserve commission (which neatly solved whatever legal complications there were—at least from the police perspective), a slot in the upcoming recruit training class, tentative approval, subject to my graduation from the police academy, for several months of study in the patrol division (which I was able to stretch to almost six months and then renew several times, years later). . . . No editorial control was asked for nor was there any direct discussion of what the police themselves hoped to get out of this initial research bargain. . . .

To penetrate the back regions of police organizations requires a researcher, like any newcomer to the setting, to undergo a lengthy process of examination. As I have described in some detail elsewhere, the novice in police organizations must cross several work boundaries, pass a series of social tests designed to discover something about the prudence, inclinations, and character of the person, and, of course, carve out a few intimate relationships with members of the organization upon whom the newcomer can depend (Van Maanen, 1973, 1974, 1978a).

Furthermore, the student of the police, again like any rookie patrolman, must also come to terms with some rather concrete

and pervasive emotional issues. In short, there are personal qualms about one's own safety to quiet. Indeed, much of the occupational talk of the police carries the tune of violence. Danger, whether real or imagined, is a constant companion to the police. And, fear is consequently an emotion every researcher who spends time in the field with the police must face.

Fear, to an observer of the police, stems from several sources. Certainly, by associating closely with the police, it may come from the ever present danger existing in city streets. I can recall feeling as if I had a bull's eye painted on the side of my head the first few times I rode in the front seat of a patrol car. Fear may also arise from the police themselves. I once witnessed a bar fight between two officers, each believing the other had embarrassed him in the eyes of a Captain. The police, of necessity perhaps, are not gentle, impassionate sorts who can easily tolerate a deviant in their midst. The working style of an ethnographer is sure to reflect this. Of course, one cannot know until the moment arises how he will handle these fears. But, the police will certainly be watching closely to determine, on the one hand, whether or not they can "depend" on the researcher, and, on the other hand, whether or not they can "take the researcher out" without adverse consequences arising should the need arise.

At another level, the police adhere to an organized format in going about some of their daily tasks. This format is rigid in some cases, such as the police academy, and relatively loose in other cases, such as roll calls and street work in the patrol division. A researcher, in either context, is conspicuous to the degree he does or does not fit the format. In the academy, for example, a researcher who did not participate in the program would have been so conspicuous as to preclude him from asking questions that might uncover the attitudes recruits might be forming toward each other, the staff, the department, or the work itself. On the street, however, there is considerably more leeway for a fieldworker to fashion a research role for himself without following a rigid format.

In my study, I entered the police academy as a self-acknowledged researcher who, I wanted made known, would stay with the class through graduation and spend some time working with the recruits after they had left the academy. During training, I consciously avoided establishing obvious links with the academy staff.

When asked, I turned down offers to sit with staff members at lunch, visit their offices on breaks, or go drinking with them after work. I felt this appropriate since a very strict formality normally obtains between recruits and staff members. Similar to the industrial workers studied a generation ago, police recruits (and patrolmen in general) were particularly sensitive to the possible connections a researcher might have with their bosses. On several occasions, when I had chanced to have an extended conversation in the hall with a staff officer, I was immediately quizzed on my return to the recruit areas as to what the conversation had been about. Early in the training program I was asked on a few occasions to plead a special case on behalf of a particular recruit to our academic superiors. I replied on those occasions that as far as the staff were concerned I carried no more weight than they themselves (which may or may not have been true)—although I usually said after my disclaimer that if they felt my talking to the Sergeant in charge of our particular class would do some good, I would do so. When it became apparent to the men that my nominal interventions were of little or no assistance to them, I was not asked for more special favors.

The police academy, with its strict discipline, prescribed calendar, and enforced lines of authority, was an environment clearly at odds with the patrol division. Yet, without doubt, my 13-week stint as an academy recruit helped immensely when it came to building an observational role among working patrolmen. During my first six weeks in the patrol division. I always worked with a recruit I had known in the academy and his assigned veteran partner, called, in Union City, the Field Training Officer (FTO). On virtually every occasion, I was introduced by my recruit colleague to his FTO with a tag line that went something like, "This is John Van Maanen, he's OK, he went through the academy with me."

Following the initial period in the patrol division, I decided to begin to focus my fieldwork in two sectors and, in particular, with two squads, thus, confining my work to one shift (7 PM–3 AM). Several reasons were behind this choice. First, the shift I chose was the most active in terms of dispatched calls. Second, the sectors I selected were thought to produce the most "police work." One sector took in the skid row and downtown business district and the other sector included a large part of the black ghetto in Union

City. Third, several of the men with whom I had developed the closest ties in the academy were assigned to the squads I picked and a disproportionate number of men from my academy class worked in the same sectors on overlapping shifts. Finally, by restricting my range, I hoped to be able to build firmer, more trusting relationships with the officers, both rookies and veterans, of the two squads. Although I sometimes worked outside of these two squads, I spent at least four of the five working shifts each week with these two squads.

A critical point needs to be made in this regard. By allowing myself to be closely identified with the patrolmen, I was purposely making a choice about the data I would gather. My self-imposed isolation from the managers of the organization and the other enclaves of special police interest very clearly biased my study toward the perspectives of those at the street level. In the police system, as perhaps in any social system, those of the lower caste (in this case, the patrolmen) are thought to be subservient and differential to those of the higher caste (in this case, from sergeants on up), who, in turn, balance the system, theoretically at least, by showing a paternalistic regard for the lower caste. In the police world, the power of the higher caste holds the system relatively stable, but there is a good deal of tension and conflict existing not far below the surface. To a field worker, this usually means that the members of the lower caste will make better informants (reveal more). Not only do they have less to lose objectively, but they are under less strain to appear faultless to either their internal or external audiences. . . .

My appearance while on patrol was tailored after the plain-clothes officers in the department. My hair was closely cropped, I wore loose fitting sport jackets that did not make conspicuous the bulge of my service revolver. I wore hard-toed and heavy shoes, slit or clip-on ties, and carried with me a flashlight, chemical mace, rosewood nightstick, handcuffs, various keys, and sometimes a two-way portable radio. Several patrolmen, at various times during the study, gave me (for no doubt mixed reasons) fist loads, sap gloves, and an assortment of jacks to carry with me on patrol. And I did carry a few of these tools of the police trade although departmental regulations prohibited their use. One officer insisted I carry a second gun, a "two incher," in my coat pocket in the unlikely

event, he explained, we were to be disarmed. This too violated departmental regulations. Even my 357 magnum revolver was against departmental regulations. This was a gift from my academy classmates, given to me formally during the graduation exercise in front of the police command, members of recruits' families, and local television news cameras. Even the ammunition I received through regular departmental channels was officially taboo. While I was something of a walking talking rule violation, so, too, were my colleagues. . . .

On the street, I encountered little overt hostility from patrolmen, although a few veteran officers refused to allow me to work with them. One instance bears mention because it sheds light on the research process itself. I was working the "last out" shift (11 PM-7 AM) with an academy classmate when we received an assignment to check on a possible "break" in a warehouse closed for the evening. We were some distance away and when we arrived at the call, several other units were already on the scene. In fact, a few officers were already inside the warehouse, flashlights in hand. As we got out of the car to enter the building, another officer came over and, after asking who the hell I was, told my partner to clear the call as unfounded, there were no burglars on hand, just an open door. If anything had been taken, the manager of the warehouse would make a report in the morning. We did as we were told, stayed on the scene for a short time, but left before the other officers departed. During the next hour or so, my partner enlightened me about what might have been occurring in the warehouse when we arrived. "Those fucking mopes," he said, "trying to make off with as much as they can get and on my call yet! You can't trust anybody in this outfit." . . .

[In summary] To some officers with whom I worked, I was a sort of "acceptable incompetent," capable perhaps of shortening the long hours on patrol through talk but incapable of doing anything remotely connected to the job itself. To most officers, I was more the reserve officer, a "friendly helper" sort who could, when called on, handle some light paper work, the radio, conduct an interview at, say, the scene of a fender-bender traffic accident, but, nonetheless, required continual supervision and could not be assumed to know what to do should an occasion arise in the field that called for "real police work." To a very few officers, two or

three at most, I was more or less a “working partner,” albeit a temporary one.

As an acceptable incompetent, I sat in the backseat of a two-man unit, taking no part in the decision being reached in the front seat, save those decisions about where and when to eat or take a break. On these shifts, I rarely spoke with anyone but my police guides. I did no police work other than to occasionally keep a personally protective eye on a prisoner who might happen to share the backseat with me.

As a friendly helper, my time was split somewhat evenly between one-man and two-man units. In this role, I was delegated tasks such as keeping the log or calling radio for a license plate check on a vehicle that just might turn out to be stolen. Other times, I would be asked to post myself at the corners of buildings when checking out a potential burglary or prowler call. In this role, I was also expected to physically or otherwise assist and back up an officer if any altercation arose during the tour.

Finally, as a working patrolman, I was put in the role of what Union City police called the shotgun partner. I played this part only with officers working solo beats and during these tours I was responsible for radio communications, paperwork (often signing my name to the log, arrest reports, field investigation slips, etc.), back-up responsibility on traffic stops (positioning myself just outside the passenger door on the patrol car), and for the shotgun carried in most police cars should its use be required (hence, the Union City tag for the role, “you’re shotgun tonight, I’ll drive”). On calls such as the various sorts of disturbance calls, I would help separate the quarreling parties, restrain them if need be, and usually take a share in the decision about what, if any, police action was to be taken. On no occasion, however, did I drive a vehicle on routine patrol. This was probably for the same reason few rookies do much driving of prowl cars—veteran officers do not trust the novice driver who, first, does not know the district, and, second, is unaccustomed to the unpredictable ways other motorists react when spying the police black-and-whites.

What comes through as a result of this cursory overview of these three somewhat distinct roles played by one fieldworker is the inconsistency associated with the ethnographic research role. At times, I frisked suspects, put handcuffs on prisoners, wrote as-

sault reports, while at other times I simply stood in the shadows and watched the police go about their tasks or, less frequently, but more discretely, “did a train” and slipped from view entirely. . . . In the academy, I helped cover for tardy classmates by concocting what I thought to be reasonable tales to tell superior officers. Several times I cheated on exams by passing my answer sheet around the back of the room (as I too looked at others’ answers sheets). These mostly mundane matters would hardly be worth mentioning were it not for the fact that they point to the difficulty, if not impossibility, of maintaining a clear cut and recognizable observational or participatory research role. At least in the police world, the variation existing in the environment as well as that among the people studied, requires a situational and very flexible set of guidelines not easily categorized—even when writing with the luxury of hindsight.

Confessional Tales in Perspective

The confessional tale has become, as I argued earlier, an institutionalized and popular form of fieldwork writing. The confessional attempts to represent the fieldworker’s participative presence in the studied scene, the fieldworker’s rapport and sensitive contact with others in the world described, and something of the concrete cultural particulars that baffle the fieldworker while he learns to live in the setting. It is necessarily a blurred account, combining a partial description of the culture alongside an equally partial description of the fieldwork experience itself. Since the authors are writing of their own sightings, hearings, and interpretations, the soft subjectivity of the fieldwork experience begins to slip into fieldwork confessions in a way it does not in realist versions of a culture. Missing data, incompleteness, blind spots, and various other obscurities are admitted into the account. The avowed purpose, of course, is to lift the veil of public secrecy surrounding fieldwork.

Unmasking fieldwork is a relatively recent phenomenon. A generation (perhaps two) of fieldworkers, in both anthropology and sociology, apparently felt no great urge to enlighten their readers as to what canny tricks of the trade carried them through their respective research projects. For the most part, they were

willing to simply state something to the effect, "This study is based on two years of fieldwork" and leave it at that; allowing readers to judge the adequacy of the method by the final result. No more. Several reasons can be generated for the current popularity of the fieldwork confession.

First, much of the traditional authority claimed for fieldwork by its early promoters and justified by them on the basis of their establishing ethnography as a human and behavioral science, akin to the observing natural sciences, has worn thin. Some confessions are therefore an attempt to shore up the fieldwork craft as a still scientifically valid one. They attempt to show how a reader might work back from a display of the conditions under which the fieldwork was accomplished to some assessment of how reliable and valid the realist ethnography itself might be. Presumably the claims, anecdotes, and personal jitters contained in my confessional tale might inform the reader who worries about the trustworthiness of my stationhouse sergeant depiction. Because realist accounts are methodologically silent, because they adopt the conceit that data must be cleanly separated from the fieldworker (implying, no doubt, that virtually anyone would see, hear, and think the same things were they in the fieldworker's shoes), and because they offer only the fieldworker's tightly packaged account of the culture studied, confessions are necessary.

Second, some confessional writers are not at all interested in reestablishing and confirming orthodox views on the scientific charter of fieldwork. In fact, some confessional tales are written explicitly to question the very basis of ethnographic authority and to transform ethnography, insofar as possible, into a more philosophical, artistic, phenomenological, or political craft; a craft sensitive to matters thought by these writers to be more relevant and important than what ethnography provided to readers in the past.¹¹ In skilled hands, the personal voice can be a gift to readers and the confessional becomes a self-reflective meditation on the nature of ethnographic understanding; the reader comes away with a deeper sense of the problems posed by the enterprise itself. In unskilled hands, a wild and woolly involuted tract is produced that seems to suck its author (and reader) into a black hole of introspection; the confessional is obsessed with method, not subject, and drifts toward a single-minded, abstract representation

of fieldwork. Yet however involuted some confessional accounts may appear, the reader who wonders why the confessional writers don't do their perverse, self-centered, anxiety work in private and simply come forward with an ethnographic fact or two are, quite frankly, missing the point.

A good deal of recent confessional work rests on what many (myself included) take to be a fundamental turning point in American social thought. No longer is the social world, as mentioned in chapter 1, to be taken for granted as merely out there full of neutral, objective, observable facts. Nor are native points of view to be considered plums hanging from trees, needing only to be plucked by fieldworkers and passed on to consumers. Rather, social facts, including native points of view, are human fabrications, themselves subject to social inquiry as to their origins. Fieldwork constructs now are seen by many to emerge from a hermeneutic process; fieldwork is an interpretive act, not an observational or descriptive one (Agar, 1986). This process begins with the explicit examination of one's own preconceptions, biases, and motives, moving forward in a dialectic fashion toward understanding by way of a continuous dialogue between the interpreter and interpreted (see Rabinow and Sullivan, 1979).

Some confessionals suggest that the acute self-consciousness brought on by working through such a process can lead to something of a paralysis (e.g., Jules-Rosette, 1976; Thorne, 1983; Krieger, 1983; Tyler, 1986). There is obviously a need for balance between introspection and objectification. When only the former is involved, a sort of "vanity ethnography" results, in which only the private muses and demons of the fieldworker are of concern. Conventions of confessionals offer some aid, if not comfort, for fieldworkers trying to grasp occurrences in the field empathetically, but to stand away to situate them in other contexts, both social and personal. The textual organization of the standard confessional tale may be of some help for fieldworkers who regard participant-observation as a metaphor best reformulated in hermeneutic terms: a dialectic between experience and interpretation.

There is, as exemplified in my confessional, something of a norm about what constitutes a minimally acceptable table of contents for an account of fieldwork. Authors must discuss their pre-understandings of the studied scene as well as their own interests

in that scene; their modes of entry, sustained participation or presence, and exit procedures; the responses of others on the scene to their presence (and vice versa); the nature of their relationship with various categories of informants; and their modes of data collection, storage, retrieval, and analysis. To work through such matters deeply forces on the fieldworker a private encounter with some very basic hermeneutic issues, an encounter which may become public. As fieldworkers consider and report their practices, confessional tales grow more complex and sophisticated.¹²

In this vein, when I consider my own confession I find it now a rather flat, traditional, and unremarkable one.¹³ All the conventions discussed in the introductory section of this chapter are present. The authority is highly personalized. It is certainly the case that it is my own point of view that is at issue in the tale and not that of the police. The naturalness of the data is implied by the various ways I document my acceptance into police circles as a quasi member in good standing. On this matter, the unsaid but unavoidable implication of the writing is that these world-weary policemen ignored me as a researcher and paid attention to me only as an awkward novice or easy friend who was seen as reliably on their side; they went about their mostly unmertry way in much the same fashion as they would had I not been there. Certainly this is the message I wished to convey at the time I wrote the confessional, and in a sense it is my hope that it still represents at least a partial truth.

But I must admit I am far less certain or confident now about the veracity and faithfulness of either my confessional or my realist tales than I have been in the past. Both kinds of writing are highly conventionalized in both a representational and a stylistic sense. Both, as I know only too well, leave more of my knowledge out of the accounts than they put in. Both close off too early (and too casually) what remain rather open matters. Fiddlesticks. I am, in short, still very much in the process of coming to understand my materials—which continue to develop each time I revisit Union City, talk to my friends there, read the newspapers, review articles and books by others relevant to my materials, or sit and consider old writings or notes of my own.

I am also troubled by my rather strait-laced and straight-faced

handling of informants for whom I unproblematically claim to speak in my tales. I know full well that the understanding I have of their talk and action is not only incomplete, but rests fundamentally on the contextual matters that surround my coming together with particular people, at particular times, for particular purposes, in particular places, and so on. Thus I put forward the meaning of such talk and action untruthfully in my writing without also considering (and representing) the various contexts within which it occurs. In what is rapidly becoming something of an in-group term in fieldwork circles, both informants and fieldworkers are “interlocutors” in cultural studies and are therefore jointly engaged in making sense of the enterprise (Clifford and Marcus, 1986). The line between what informants and fieldworkers make of the world is not an easy one to locate (Van Maanen, 1979, 1981b).

At issue is the fact that there are always many ways to interpret cultural data. Each interpretation can be disputed on many grounds. The data fieldworkers come to hold are not like dollar bills found on the sidewalk and stealthily tucked away in our pockets for later use. Field data are constructed from talk and action. They are then interpretations of other interpretations and are mediated many times over—by the fieldworker’s own standards of relevance for what is of interest; by the historically situated queries put to informants; by the norms current in the fieldworker’s professional community for what is proper work; by the self-reflection demanded of both the fieldworker and the informant; by the intentional and unintentional ways a fieldworker or informant is misled; and by the fieldworker’s mere presence on the scene as an observer and participant.

Fieldworkers are increasingly conscious that the so-called data they produce and carry away from the field have already been thoroughly worked over. “Textualization” is Ricoeur’s (1973) term for the process by which unwritten behavior, beliefs, values, rituals, oral traditions, and so forth, become fixed, atomized, and classified as data of a certain sort. Only in textualized form do data yield to analysis. The process of analysis is not dependent on the events themselves, but on a second-order, textualized, fieldworker-dependent version of the events. The problem here is how to crack

open the textualization process itself. As we shall see, several possibilities are being entertained in the more experimental forms of ethnographic writing.

Nonetheless, despite growing discomfort among many fieldworkers with these apparently intractable dilemmas facing their craft, if they are to write at all about their research, they must get on with it or retire from the sport entirely. Two forms of practical resolution have been discussed thus far. In gross form, realist writings take what the authors know (or at least think they know) as their subject matter and, by and large, ignore how such things came to be known. In equally crude fashion, confessional writers take the author or knower as subject matter and by and large bypass what it is that the author knows as a result of fieldwork. Each treats the other as supplemental.

In chapter 5 another class of fieldwork tales are examined. I call these "impressionist tales" and argue that they are an attempt to explicitly bring knower and known together in representational form. Currently, impressionist tales of the published sort are often buried within realist or confessional ones and are thus something of a subgenre and a marginal type of ethnographic writing. More frequently, however, impressionist tales are told to little gatherings of friends, colleagues, students, and other interested groups. While they rarely make it into print, impressionist tales are the backstage talk of fieldwork. Telling them is a familiar enough occurrence in fieldwork circles to warrant closer inspection. There are, of course, important differences between the spoken and written tales. I've chosen, however, to join the two in chapter 5, with only a ritual nod given now and then to the distinction between them.

NOTES

1. Until the 1960s, fieldwork was with few exceptions simply done and not much written about or analyzed. Critics of ethnography delighted in pointing this out. To some, fieldwork became known, with a certain condescension, as the "anthropological method;" by others it was thought of as preparatory to the main business of social research and was hence called a "pilot" or "exploratory" study; and to the most vehement fieldwork was merely "pseudoscience" (Hughes, 1960). Early confes-

sional tales attempted to set their critics straight by demonstrating the sanctity and worth of their "timeless way of knowledge." These legitimizing works were celebratory in tone (sanctification by grace), and while exposing some of the warts of the activity, fieldwork came off splendidly in the text, as might be expected since the fieldworker was doing the writing (e.g., Casagrande, 1960; Adams and Preiss, 1960; Maybury-Lewis, 1965; Vidich, Bensman and Stein, 1964; McCall and Simmons, 1969; Spindler, 1970). These forms have now hardened into the genre presented in this chapter. As I suggest, however, confessional tales, like realist ones, are being modified now as the power of observation slips away as the unique ethnographic strength. Marcus and Fischer (1986:33-44) discuss the direction such changes are taking under the label "interpretive anthropology."

2. I have in mind such examples as Junker, 1960; Freilich, 1970; R. Wax, 1971; Kimball and Watson, 1972; Bogdan and Taylor, 1975; Loffland, 1976; and many of the essays in Van Maanen, 1983b. This list is, however, a drop in the bucket. Apparently the quip attributed to Evans-Pritchard (quoted in Clarke, 1975): "Anybody who is not a complete idiot can do fieldwork," seriously underestimates the felt need of fieldworkers for more explicit guidance.

3. Examples here would include Agar, 1986; Emerson, 1983; Hammersley and Atkinson, 1983; Ruby, 1982; Schwartz and Jacobs, 1979; Rabinow and Sullivan, 1979; and Douglas, 1976. This literature continues to grow.

4. Confessional tales do not always praise the trickster image. In some, fieldwork is presented as a moral trial having anguish and ambivalence as the felt result. In my own work, for example, I was once thanked by some of my police acquaintances for coming to the funeral of one of their mates. I still feel like a hypocrite recalling the incident, since I was at the funeral to unravel a cultural rite and not to pay my respects. Hypocrisy is always at issue in fieldwork, and these (and other) inner experiences mark the confessional tale (e.g., Powdermaker, 1966; Henry and Saberwal, 1969; Gans, 1982; Habenstein, 1970; Thorne, 1980; Punch, 1986; R. Rosaldo, 1986a).

5. From this standpoint, theory would seem to have most relevance at the second moment of ethnographic production, the writing phase, where an author's selection and choice of ethnographic facts and arrangement of them create the text. Theory matters in the field only insofar as similar kinds of fieldwork experience for very different fieldworkers are given contrasting readings and weight. The same informant's account or activity can be seen as an example of false consciousness or as situationally appropriate and creative behavior. Theory doesn't determine

fieldwork experience, but it may provide the dictionary with which it is read. See Feyerabend (1972) for a useful, though polemic, treatment of this matter.

6. As noted earlier, a relatively new school of fieldwork practice is emerging in sociology under the existentialist banner. This group argues cogently for more intimate involvement (“become the phenomena”) in order to personally experience emotion and meaning in the life world studied. Fieldworkers within this school regard both discourse and observation as inadequate devices for getting past the fronts, duplicity, and secrecy that often surround certain settings (e.g., nude beaches, message parlors, drug dealing, adult bookstores). See J. Douglas (1976, 1985) for a statement of aims and theory and Adler and Adler (1987) for a useful review of some of the ways the existentialist desperadoes of fieldwork are putting their views (and feelings) into practice.

7. This matchmaking sense of ethnography resembles Giddens’s (1976) idea that different cultural realities are, insofar as they are aware of one another, frames of meaning always in the process of mediation. Thus, fieldworker and native frames of meaning meet in an ethnography which presents the results of a mediation process. These results could, of course, represent the triumph of rationality, delusion, or coercion in fieldwork. Readers have only the final product on which to reflect and surmise.

8. Things are somewhat more in flux here than the text suggests. A part of the confessional literature also debunks the previously unquestioned (and charming) myth of fieldwork rapport. Malinowski (1967) was again path breaking in this regard (posthumously). More recently, it has become fashionable in some circles to speak of a confrontational form of fieldwork where from the outset of the study little faith is placed in the innocent attainment of rapport as the necessary precondition to unlocking cultural knowledge. Clifford (1983b) points out that there is always a certain amount of violence involved in fieldwork if only because the fieldworker’s presence is manifestly an intrusion. Confrontational fieldworkers no longer avoid mention of such violence, so they attack the assumption of rapport and with it the dream of an unobtrusive ethnography. In the hands of some sociologists confessions read like debriefings after a battle in the social combat zone with accounts of how informants were bullied, how tactics of coercive persuasion were employed, and how the weaknesses, disunity, and confusion of the natives were exploited (e.g., J. Douglas, 1976, 1985; Humphreys, 1970:185–92; Bulmer, 1982; Punch, 1986).

9. In this light, to publish a confessional tale is often something of a reward given the fieldworker for having first presented a realist account

deemed interesting enough by one’s colleagues to warrant another account of how such sterling work was apparently done. Much confessional writing helps to establish the respectability of the ethnographic work that preceded it, either by showing how the traditional canons of practice were followed in the field, or, conversely, by showing why traditional canons were inadequate to produce the worthy tale the confession indexes.

10. I should also note that while confession was partly on my mind when I wrote this article, so, too, was a rather blatant attempt to smuggle in some of my police material that I found more difficult to represent in the realist tradition. This secondary objective is hardly atypical of confession, and, as I noted earlier, some ethnographers (and their readers) find the confessional format perfectly tuned to their own theoretical, philosophical, and personal commitments. It therefore serves them as a favored form of fieldwork reporting.

11. The fieldworkers of interest here are likely to consider ethnography more an art form than a science (see Geertz, 1983; Marcus and Fischer, 1986; Rabinow and Sullivan, 1979). They often chastise their more scientifically oriented colleagues for what they regard as failed prophecies, trivial research, and little progress toward any iron laws of behavior despite the constant whine for more research on a given topic. Not only do the critics of traditional ethnographic aims draw on interpretive theories for inspiration, but this bolting from the fold occurs, as Clifford (1983a) suggests, at a time when colonial authority has vanished and most liberal democracies are said to be in a crisis of conscience (partly as a result of the social upheavals of the 1960s and 1970s). In this climate, the institutional role of fieldwork has been attacked, sometimes savagely, for being but a special branch of the queen’s secret service, serving mainly to inform the crown during those long, hot summers when the natives are restless. A new form of ethnography is therefore required on moral grounds—one with a more dispersed form of authority and less claim to possess the correct interpretive stance. Strong statements urging a more active and politically savvy role for ethnography are found in Hymes, 1972; Dwyer, 1977; and Thomas, 1983b.

12. While complexity and sophistication may indeed grow, there are limits to the genre as well. Confessions, endlessly replayed, begin to lose their novelty and power to inform. In the extreme, they also lose their way altogether by tacitly suggesting that fieldwork is a better method for learning about the fieldworker than it is for learning about the culture the fieldworker went to study. It may be that standard-form confessional tales have exhausted the possibilities for improving what remains a necessarily uncertain and risky task. New ways of understanding fieldwork may be required in order to look more closely and critically at the prestudy as-

sumptions and practices that govern the production and dissemination of ethnography. Both history and literary criticism are models for the kind of work that is needed. Movement along these fronts is visible (e.g., Rock, 1979:178–217; Gusfield, 1981:83–108; Stocking, 1983; Geertz, 1983; Clifford, 1983a, 1983b; Bulmer, 1984; Clifford and Marcus, 1986; Marcus and Fischer, 1986; and Becker, 1986a, 121–25.

13. A striking example of this is the fact that I made no mention in this confession of my simple desire to do fieldwork. This seems a curious oversight in retrospect, because I was very much committed to getting beyond the university and trying my hand at what I was beginning to regard as “real research.” At the time, my only exposure to what the craft entailed were two hurried observational projects, one in a commercial bank, the other in several local city halls. I had, however, read enough about fieldwork to prefer my image of it to other thesis prospects of mine, such as standing over an IBM machine in the computer center running data or hanging out in the library talking to myself. A very real motive behind my commitment to fieldwork was (and I suppose still is) that it seemed like fun. A good part of my imagery came, of course, from the lively confessionals I was then reading. The irony of all this is that, as mentioned in the Preface, when all was said and done, my thesis, despite the lengthy fieldwork, still put me in front of the IBM machine cranking out survey results and running back and forth to the library to develop some comparative framework for my numbers. I was not yet confident, nor had I learned to write it up. Writing, not fieldwork, turned out to be my problem. Becker (1986b) provides some much needed advice and insight on the most practical problems of deskwork in sociology. >