appendix

Methods: A Confessional of Sorts

> Jesus! I've t'ought about dat guy a t'ousand times since den an' wondered what eveh happened to 'm goin' out to look at Bensonhoist because he liked duh name! Walkin' aroun' t'roo Red Hook by himself at night an' lookin' at his map! How many people did I see get drowned out heah in Brooklyn! How long would it take a guy wit a good map to know all deh was to know about Brooklyn!
>
> Jesus! What a nut he was! I wondeh what eveh happened to 'im, anyway! I wondeh if someone knocked him on duh head, or if he's still wanderin' aroun' in duh subway in duh middle of duh night wit his little map! Duh poor guy! Say, I've got to laugh at dat, when I t'ink about him! Maybe he's found out by now dat he'll neveh live long enough to know duh whole of Brooklyn. It'd take a guy a lifetime to know Brooklyn t'roo an' t'roo. An' even den, yuh wouldn't know it all.
>
> —Thomas Wolfe
> *Only the Dead Know Brooklyn*

This study belongs to the genre known as "ethnographic realism."[1] This identification says much about presentational style, little about the actual research process. The descriptive style of this genre presents an author functioning more or less as a fly on the wall in the course of his sojourn in the field—an objective, unseen observer following well-defined procedures for data collection and verification. It requires no great insight, however, to recognize that ethnographic realism is a distortion of convenience. Fieldwork, as all who have engaged in it will testify, is an intensely personal and subjective process, and there are probably at least as many "methods" as there are fieldworkers.

It is the task of the methods section to balance the potentially misleading implications of the realist style as adopted in the text with a backstage glimpse of the actual research process. Often reading like a confessional, the fieldwork account emphasizes (along with proof of one's intimate familiarity with the subject matter) shortcomings, potential for bias, and the random nature of fieldwork. Such a discussion serves a number of purposes. First, it conforms to the conventions set by more stylistically scientific genres. The methods section provides the reader with procedural information, and, for the more sophisticated, it introduces the issue of observer subjectivity into a consideration of the scientific process and its limitations. This, it is hoped, should allow a qualified reading (replication having fallen on bad times, even in experimental circles).

Second, and more interestingly, a methods confessional serves to establish a kind of ethnographic credibility; here self-criticism not only exposes weaknesses and qualifies assertions, but allows a demonstration of the breadth, depth, indeed the relentlessness, of an ethnographic incisiveness seemingly so powerful that it is applied most scathingly to oneself. Thus, although it reads like a confessional, it is in fact a self-application of one's scientific tools, a "realist ethnography" of the research process.[2]

However, as an ethnography of ethnography, a confessional—no matter how dramatic, how insightful, how excruciatingly honest—falls short, a victim of its own interpretive logic. One is writing of oneself; and beyond the human conventions and constraints of self-presentation, one runs afoul of a basic epistemological dilemma inherent in interpretive logic: how is one to know oneself? Techniques for verification, for introducing multiple voices, for turning the object of meaning around and repeatedly lighting it with evidence from apparently independent sources (what the more mathematically minded would refer to as "triangulation") are not applicable. Self-analysis has opened the writer to the criticism of informant knowledge that is the essence of the ethnographic enterprise: it is only "experience-near"; it is only "first-order"; it lacks the distance required of a valid interpretive effort. The question, then, looms large: how is one to break through the vicious cycles of one's own interests, distortions, and misperceptions?

There is no clear answer.[3] Nevertheless, since I believe that such contextual information may be helpful and perhaps interesting to readers, I will offer some observations and comments on the background of the study, the nature of my activities in the field, and the process of data analysis and writing. What follows should be regarded primarily as an informant-produced text; as elsewhere in this study, it is offered with the recurring caveat: let the reader beware.

Why Fieldwork? The original research for this book was done in 1985 as part of a doctoral dissertation at the Sloan School of Management at the Massachusetts Institute of Technology. In retrospect, it seems that a number of factors—above and beyond the theoretical justifications specified in the preceding chapters—led me to do fieldwork at Tech. For my thesis I wanted to study a large business corporation. I had experience as a researcher in public sector people-processing organizations (Kunda, 1986) and as a participant in military, psychiatric, and educational institutions in Israel. I had little firsthand knowledge of the business world (though I was in a school of management), and there were few secondhand sources that seemed trustworthy. I felt that an extended sojourn in this world was necessary for a student of organizations.

My personal background seems to account, in part, for the specific directions this study took. As an Israeli who had come to the United States in order to pursue graduate studies, and therefore a foreigner (albeit one in a rather accelerated process of assimilation, and, but for the accent, almost perfectly bilingual), I was already in an ethnographic mode. "Learning the culture" was a real-life experience. Formal "fieldwork" seemed an opportunity to discover more of America, and particularly to observe some of the manifestations of its power. In Israeli slang, "America" stands for everything that is advanced, powerful, comfortable. Things American carried (and still do) an ongoing fascination for me, whether found in Fenway Park, on Route 128, or wherever I chanced to stumble, like Thomas Wolfe, with my map. In some sense, they came to represent an authentic cultural source of the secondhand artifacts that flood and tantalize the rest of the world. For an Israeli, growing up in a premeditated and designed culture, "authenticity" was a never-ending quest.[4] For many Israelis, moreover, "America" is both a dream and a threat, representing an option not taken by one's grandparents, and always posing the dangerous temptation either to "Americanize" Israel or, more drastically, to commit the ultimate betrayal and emigrate.[5] As a resident alien in the United States, I was already suspect on both counts. Ethnographic exploration of corporate America was an excuse to follow the sirens, examine them up close, and in the process turn the tables on the historically one-sided anthropological enterprise.

Another factor was my (somewhat militant, at the time) stance vis-à-vis methodological debates in the field of organization studies.[6] I wanted to do "qualitative research," "see for myself," get involved firsthand, test my methodological beliefs concerning the importance and feasibility of interpretive methods, and challenge what I took to be the dry and unexciting procedures (and findings) that characterize much of the research on formal organizations. I was armed with much (perhaps too much) previous reading and some ideas about culture, ideology, identity, and interpretation; the specifics of high-tech engineering never really attracted me—and they still do not, although I did develop an advanced layman's working knowledge of some of the technical issues (having succumbed to and overcome an addiction to computers in the days of Fortran, punchcards, and anxious overnight waits for output), as well as an ongoing curiosity about the social worlds built around them and a grudging respect for the skills involved. But, ultimately, I was after a generic business corporation as an American microcosm and as a methodological proving ground.

My background seems to have influenced my theoretical preferences as well. Those familiar with Israeli culture will understand my preoccupation with the relationship of ideology and the self: it is a central and salient part of the experience of my generation. Israel is the product of Zionism, an ideology that held, and still holds, a central place both in public discourse and in the private concerns of Israelis.[7] Zionism not only defines the Israeli collective but also makes heavy demands on its members; interpreting and coming to terms with its significance in the various arenas of social life is an ongoing and often intense activity. As those who follow the news from the Middle East are well aware, the historical interpretive debate over Zionism is far from being resolved, and, for those whose lives are affected, the outcome is often experienced as a matter of life and death. Upon rereading this study, it seems to me that it may be read also as an allegorical discussion of certain aspects of my own society: the theoretical edifice that I erected—such as it is—can quite easily accommodate the tension between the demands of Zionism and the emergence of an Israeli identity. In this sense, doing ethnography is also a process of self-exploration and discovery.[8] I do not recall thinking of these matters at the time, but looking back, they seem to account for a good deal.

Finally, I might add that there are solid, rational reasons for taking an interpretive approach to research. Of central importance is the fact that the subject matter is elusive and highly context-dependent, inseparably intertwined with the way people understand their reality and reflect on it. Research requires some intimacy in order to access conscious constructions, and close observation of behavior to uncover tacit ones.[9] However, many interesting but fruitless methodological debates have convinced me that there is more than rationality at stake in methodological preference. These rationales become clichés hurled back and forth. The best one can do, then, is to let the work speak for itself.

In the Field Fieldwork was characterized by continual ambiguity with regard to my role vis-à-vis the company and its employees. The first contact was made through MIT. I was approached by members of a staff organization seeking consulting help. Intrigued by the idea of combining and perhaps comparing ethnographic and clinical approaches to research, I decided to explore this possibility.[10] How and why it failed is another story—one about which I have only partial data. In essence, the staff group had completed a study documenting the shortcomings of a specific engineering project and wished to introduce me to an engineering development organization to help "implement" some of the conclusions. The engineers, however, were clearly not interested, viewing this as a political move by the staff group, for whom they had little sympathy and no respect. Nevertheless, one of their managers was willing to accept my presence as "an MIT sociologist" interested in "the culture." In return I promised to make a presentation about my findings.[11] "I am interested in what you write, but I want you to know that it might also make this group look good to have someone like you," he told me, with the bluntness that characterized many managers at Tech. He was the new manager of a group that in the past had been seen as "closed" and "paranoid." I, presumably, was to be one of his "signals" that times were changing.

As my role as a passive observer in the development group emerged, my fortunes with the staff group changed. In the course of my entry, I had established good ties with a number of the members of the group, some of whom became valued informants. Nevertheless, when the nature of my role became apparent—an unstructured observer rather than a free management consultant—the staff group manager considered asking me to leave. By then, however, my ties with the engineering group were established, and rather than make waves, he chose to tolerate (at arm's length) my presence in his group as a participant-observer and as someone vaguely associated with the "SysCom space." Consequently, I wound up with access to both the staff group and the engineering development group referred to as SysCom in the previous chapters.

The staff group was located at corporate headquarters. It consisted of twenty to thirty people encompassing training (both technical and behavioral), communications (the various publications and newsletters generated in the organization), some technical consultants, and marketing research. It also had a number of "individual contributors," including Ellen Cohen, the full-time "culture" expert. The manager of the group reported directly to Dave Carpenter, the vice-president. The group had relatively low status (as do most staff groups) but was quite central. Through this group I gained access to the various training affairs and also got a bird's-eye view of the entire organization and particularly of senior management. I was given my own office space, a computer terminal with access to electronic mail, whatever administrative assistance I needed, and a free run of headquarters.

SysCom consisted of about six hundred people housed mostly in the Lyndsville facility (see Chapter 2). Here life was harder. I was given grudging access to three projects (one of which was considered to be "in bad shape"), temporarily vacant office space, another terminal, and permission to initiate interviews with anybody, with the understanding that they had permission to refuse.

Once formal access was negotiated and my presence became relatively legitimate, I was left to my own devices. In the staff group my role evolved into that of an "individual contributor" functioning in my own "metaspace" (a role that, as in progressive mental institutions, evokes much overt tolerance and just as much covert backbiting). I also possessed some credibility as an academic with a perceived specialization in "management." In SysCom I was "overhead," with what some considered the redeeming features of an uncharacteristic and rather wild-eyed thirty-second appearance on *Eyewitness News* resulting from my private political involvement in Middle Eastern matters, an inexplicable (to many) MIT affiliation, and a last-minute overtime goal (also uncharacteristic) in the SysCom Olympics soccer game.[12] But to many, my true motives and the exact nature of my work remained unclear. This was caused not only by my own vagueness, the tension between the two groups with which I was associated, and the general air of high-pressure ambiguity that characterizes Tech, but also by the widespread suspicion of the consultants and academics who are a familiar—and to many not always a welcome—sight at Tech.

Between January and June of 1985, I was a full-time participant-observer in the staff group, averaging three to five days a week. I participated in all public activities and a variety of private ones, and established a number of informants as well as various acquaintances. During this time I also used the group's help in gaining access to SysCom.

Between June and December I spent most of my time at SysCom, working the same three- to five-day schedule, but spending a day a week with the staff group. At SysCom, I began by initiating rather extensive interviews (one to two hours long) of the sort known as conversational.[13] First contact would usually be made at my initiative, by requesting permission to talk. In reserve I had a note from the group manager suggesting that "it was all right." Responses varied dramatically, from friendly acceptance to a complaint to the personnel manager that my request constituted harassment. From these initial interviews, I developed a number of informants and friends, formed many casual acquaintances, and learned of many people who seemed to consider my presence there a problem. I made an appearance at all public activities: talks, group meetings, summer sports, training sessions. I enrolled in anything that indicated open enrollment: workshops, sporting events, and so forth. I also managed, with the help of friends, to get invited to a number of more private affairs: staff meetings, design meetings, review meetings, and the like. Although some participants seemed to find my presence disturbing, others were quite willing to share their thoughts and concerns about the proceedings. Over the last months of my fieldwork, I initiated day-long observations of managers and engineers with whom I had established relationships. They would choose a day, and I would tag along, going to meetings, having lunch, asking questions when possible, and disappearing when necessary. On some occasions I offered myself as a driver; several interesting discussions took place on the road with a captive informant beside me.

In between scheduled events, there was much free time. I spent these long hours in a variety of places: in the library, poring over trade journals, in-house publications, and company videotapes; in the cafeteria, eating and eavesdropping, sometimes feeling lonely and at other times relieved that, unlike most members, I could easily disengage from the pressures of corporate life; in front of my computer terminal, exploring the public files or reading my technet messages and mail; or wandering aimlessly through the labyrinth of cubicles, trying to present myself to those whom I encountered as someone with a purpose in mind (on the way, I read and memorized the various signs, decorations, comments, and comic strips adorning the offices). It was during these walks that I established ties with members of Wage Class 2 and temporary workers, many of whom seemed curious about my activities, friendly, and eager to talk.

Toward the end of the year, I stepped up my staff activities again, largely because contact with the staff was easier, and my role of observer–confidant–interesting guy seemed to work. The group was undergoing a rather painful disbanding, and a friendly ear seemed to be appreciated. There is nothing as seductive for the fieldworker as being made to feel like an insider, like someone with something to contribute, particularly in an environment where "value added" is the ultimate measure of a person's worth, and worthlessness is very unsubtly communicated. I responded to invitations eagerly and developed what often seemed a quasi-therapeutic consulting role with a number of people.

Studying a formal organization surfaced two major concerns that stayed with me throughout my fieldwork. First, the problems for ethnographic work posed by a hierarchical system. As was to be expected, the extent of my access was inversely related to hierarchical level. One indicator of power is the ability to preserve privacy, and my interactions with the pinnacle of power were limited to some interviews, observation of presentations, and continuous and often frustrating contact with protective secretarial gatekeepers. A number of senior managers took an interest in my work and made themselves somewhat more available. Toward the end of my fieldwork, the vice-president responded to a request I made in a moment of recklessness and surprised me by inviting me to observe some of his activities. I sat in on a few of his staff meetings, wondering what had held me back earlier.[14] Most of my contacts, however, were engineers and managers in the middle range and in my age (thirty-three, at the time)—and possibly status—group. With them, my main goal was to transcend their suspicion of my ties with more senior managers or with other groups, and avoid colluding with whatever organizational purposes they might have. In addition, those who were somewhat different, or marginal, seemed to find their way to me: minorities, especially those with an interest in my Israeli background, those who were failing, unhappy, or "burnt out," and those who wanted to distance themselves from the "nerd" and "Techie" images. I have no way of evaluating my success other than by intuition and "clinical" skill and the fact that people seemed interested in talking and thinking about their experience, even when it was apparent that doing so involved no benefit—and might even involve some danger—for them. For people at the lower organizational levels, I seemed to be a curiosity, an anomaly, someone close to having a Ph.D. yet marginal in organizational terms. My marginality seemed to attract some of the disaffected in this group, while I also appeared to represent an easily accessible (even openly grateful) contact with the class of people from which members of Wage Class 4 hail.[15]

Second, my access to the dense social network and the informal aspects of life at Tech was limited. Some of the events that were of interest to me occurred in inaccessible places: off-site meetings, private, after-hours discussions, secret one-on-ones, and so forth. My access was further curtailed by the nature of my involvement. By limiting myself to relatively standard working hours and to the main working facilities and their close environment, I restricted the range of events that were accessible for direct observation. This decision reflects the difficulties inherent in the research process, the rather segmented social lives many people at Tech lead, deficiencies in my "networking" and socializing skills, and, to some extent, my own family constraints. Consequently, participation in certain kinds of events was relatively rare. I was invited to only three homes over the course of the research, and I did not travel with members, many of whom spent considerable time in airplanes, hotels, and conference centers. For what transpired outside my view, I pieced together hearsay, gossip, and stories.

Despite these constraints, I was swamped with information. Throughout my year in the field, and despite the advice I frequently received, I did not consciously define what I was after. Everything was interesting, and my discussions, interviews, and observations usually focused on whatever was occurring at the time and on the particular interests and concerns of the people involved.[16] In the course of this process I generated thousands of pages of fieldnotes and interview transcripts (produced each day from the fragmented notes hastily scribbled during and between events and interviews), collections of archival material, computer output, newsletters, papers, memos, brochures, posters, textbooks, and assorted leftovers. Internally produced statistical evidence landed in my lap on a number of occasions, along with explicit caveats or dark hints about their "political" nature, "sensitive" quality, and questionable validity. I also made some informal counts through the interviewing process (educational background, personal background, employment status, and so forth). As it should be clear by now, however, the strength of my argument does not rest on data of the quantitative sort.

Writing It Up Ethnographers describing their craft, and I am no exception, often cultivate the aura of heroism associated with their activities. In comparison with the armchair efforts of their tamer colleagues, fieldwork, they claim, is an adventure. Ethnography's tribulations however, are found not only in the unknown jungle, tropical or corporate, but also, I submit, in the seemingly unexciting task of analyzing and reporting one's findings.[17]

Having returned to safer shores, I discovered that, chained to a desk like the mythical hero, I was forced to relive the essence of the dangers and pain of the field adventure over and over again: facing the unknown, the incomprehensible. Masses of facts, stories, vignettes, numbers, rumors, and endless pages of fieldnotes documenting the observed trivia of everyday life—their sheer volume offered daily testimony to the seeming impossibility of making any valid statement at all. And, ironically, the more conscientious one is as a fieldworker, the more impossible one has demonstrated one's task to be. Moreover, the less adventurous and closer to home the field experience, the more difficult the secondary one, for one is not the sole owner and interpreter of the particular culture one has studied. Everything, it often seems, has been said; all is already known and, if anything, overdocumented.

I began the analysis and writing during the last months of my fieldwork, and completed the thesis close to a year later. The first step was reading and cataloguing my fieldnotes, creating, combining, redefining, and discarding numerous categories and groupings. It was in the course of this process that the main analytic categories—ideology, ritual, the self—emerged. Next, I wrote a short ethnographic description of Tech as part of a co-authored paper (Van Maanen and Kunda, 1989). This became the basis for a rather frenzied, apparently directionless, yet satisfying process of writing descriptions that I engaged in after the fieldwork was (arbitrarily) terminated. The final version of the thesis emerged after repeated writing and rewriting, and under pressure from readers to move from pure description, with occasionally disguised theoretical insinuations ("illustrated diatribes," in the words of one advisor), to an explicit analytic framework. This transition, the heart of the ethnographic procedure, is also the stage most difficult to specify. In my case, this difficulty reflects not only the inherent problem of generalizing from ethnographic data, of combining the general with the specific, but also my own deep suspicion of any general theoretical statement. A careful reader might detect in the book the traces of a struggle with standard presentational requirements and accepted forms.

Responses to the thesis from Tech were limited—largely, I believe, because of my preference for a low-key withdrawal from the field and my decision to reduce my general discomfort with my role and its implications by severing contact with the company. My promised feedback session never materialized, forgotten or considered unnecessary by management, and gladly ignored by me. There were no responses to the copy I sent by mail about one year after I left, and I did not stay in touch with any of the people I had worked with in the field. The only formal response was from a senior manager in the Human Resources Department, who had received a draft from one of my advisors and who sent a note back indicating that the findings could be used to help plan whether and how to spread Tech culture to foreign subsidiaries as the company became increasingly multinational. Tech employees who read various drafts generally confirmed the validity of the findings and added comments ranging from "Yes, but why the negative tone?" to "You should really let them have it!"

Transforming the thesis into a book occurred over three years of intensive reanalysis and rewriting. Some of the empirical data were discarded, others added; the theoretical sections were rewritten and the analytic framework restructured and sharpened. During this period I did not return to the field, nor did I contact any of my informants. I did, however, follow the company's fortunes closely. Writing the book occurred under conditions of personal flux, as I was making the decision to leave the United States and return to Israel, and during the first two years of my return; throughout I was troubled by the implications of my choice in light of recent events in the Middle East and continuously concerned with my own identity, my responsibility to my family, and my stance toward the ideological underpinnings of my own society. Whether and how these concerns are reflected in the book, and whether these are at all relevant questions, I leave to the reader.

I regard this study as far from finished. Each completed sentence represents, to paraphrase one of Max Weber's biographers, "a tenuous victory over the infinite complexity of the facts." Such victories are short-lived, and the battles must be fought again. If, as Thomas Wolfe, himself a student of detail, suggested, "only the dead know Brooklyn," then the living can only continue to sketch and follow their own maps.