The purpose of this chapter is to map out studies that provide rich and contextualized understandings of work, workplaces and occupations through observation, participation and/or immersion – research commonly accepted as constitutive of an ethnographic approach. In order to identify and evaluate what ethnographic field researchers have actually done, and the kinds of claims that ethnographic research can generate, I explore studies that deploy a variety of temporal criteria and methodological strategies, and take a variety of analytic foci, including labor processes, organizations, occupations, industries and combinations of all four.\(^1\)

The list of themes and topics found in social science, ethnographic studies of work is lengthy: Hodson (1998) and Morrill and Fine (1997), for example, have identified a large number of salient themes in this literature, including autonomy, citizenship, informal relations, meaning, environments, ethics and change. In this chapter I focus on three thematic areas, deliberately selecting them to illuminate the advantages of an ethnographic approach: how routine jobs are complex; how complex jobs are routine; and how power, control and inequality are sustained. I emphasize throughout how researchers use their own experiences as a source of understanding and insight in workplace studies.

I then discuss the unresolved dilemmas of time and access, in order to identify the very arduous journey fieldworkers have undertaken to generate these findings. I do this not only to convey a sense of the quite substantial collective investments that have been made to build this important field of research, but also to provide a frank appraisal of the time spent, the anxieties raised and rejections incurred in conducting ethnographies of work. Such an appraisal may deter even the most determined researchers from using an ethnographic approach to study work, but that is not my intention. Rather, such an appraisal should enable ethnographers of work to take stock of the unique barriers to entry to the field, as well as the more universal problem of demands on their time. These barriers are worrisome for all ethnographers; here, I wish to demonstrate the particular ramifications of these barriers for researchers who study work and workplaces.

An Overview

As is true of ethnographic researchers more generally, social scientists who use ethnographic approaches to study work – whether relying principally on participant observation as a mode of enquiry (Burawoy et al., 1991) or privileging particular styles of textual representation over others (Van Maanen, 1988) – cannot be accused of being armchair academics who examine the world at arm’s length. On the contrary, they are an impressively polyvalent and engaged lot, having labored in a spectrum of work sites that encompasses factories, offices, hospitals, restaurants and homes. By becoming paid workers, many have capitalized on an avenue into the research field – getting a job, learning by laboring – not readily available to researchers in other domains.

Fully immersed for often considerable amounts of time, sociologists and anthropologists have been employed as domestic workers in private households (Rollins, 1985), paralegals (Pierce, 1995), food servers and cocktail waitresses (Paulcs, 1991; Spradley and Mann, 1975), lettuce (Thomas, 1985) and strawberry (Wells, 1996) pickers, phone sex operators (Flowers, 1998), nightlife hostesses (Allison, 1994) and automotive repairers (Gamst, 1980). They have toiled as machine operators (Burawoy, 1979), mechanics (Juravich, 1985), furnace stokers (Burawoy and Lukis, 1992), longshoremen (Finlay, 1988), changed the clothes and clothes and moisturized the bodies of the elderly (Diamond, 1952), and trimmed fat and meat off hog bellies (Fire, 1988). They have worked on a variety of assembly lines: auto (Chinoy, [1955] 1992; Graham, 1995), electronics (Lee, 1998), lingerie (Roberts, 1994), auto parts and garments (Salzinger, 1997) and confectionery (Kondo, 1990).

Short of full immersion and regular employment, ethnographic scholars, often quite creatively, have studied work worlds through a prism of organizational spaces, routines and events. They have observed police detectives tending to dead bodies (Jackall, 1997), and doctors performing surgeries (Bozk, 1979) and abortions (Simonds, 1996). They have assisted genetics counselors, consulting with parents who are grieving over seriously ill children or shell shocked at the news that their future offspring might be genetically damaged (Bozk, 1992). They volunteer as reserve police officers (Martin, 1988), attend Tupperware and Amway parties (Biggart, 1989), sit through countless training sessions (Chekhovich, 1997), Leidner, 1993; Pierce, 1995; Smith, 1990; including training in sexual massage, Chapakis, 1997, occupational classes (Diamond, 1992; Fine, 1996) and corporate and workplace meetings (Kanter, [1977] 1993; Kleinman, 1996; Kunda, 1992). They hang out in union halls (Finlay, 1988), bars and workers’ homes (Burawoy and Lukis, 1992; Wells, 1996). In short, they have ‘gained the point of view, the reality-as-experienced’ (Harper, [1987] 1992: 204) of industrial and postindustrial, intellectual-, manual-, service- and sex-based; blue-, pink- and white-collar; semi-professional, professional and working-class workers.

Field researchers who study work conduct their research and write about it in a variety of ways. A close reading of studies of work reveals that while some conform to a model of ethnography based on sustained immersion and participant observation, many others draw on data that can be called ethnographic – observational, interview, experiential - but have derived that data from fieldwork that is intermittent, partial and disrupted. In some, ethnography is simply equated with qualitative research,\(^2\) which may satisfy methodological but not representational criteria (see Clifford and Marcus, 1986; Van Maanen, 1998 for discussions of the claim for ethnography as a process of representing culture in written texts).

I neither attempt to resolve the issue, a perennial one for ethnographers across the board, of what might constitute a ‘true’ or ‘best’ ethnography, or to untangle whether ethnographic studies meet what Van Maanen (1988: xi) worries are the ‘overrated’ criteria of reliability, validity and generalizability, issues that have been amply addressed elsewhere (e.g., Friedman and McDaniels, 1998; Hammersley, 1992; Hodson, 1998; Morrill and Fine, 1997). Rather, I map out a broad spectrum of studies that exemplify what ethnographic approaches can tell us about worlds of work. For this reason I avoid exclusively using a strict and narrow label of ‘ethnographies of work’, a label that suggests that there is a singular type of ethnography. In order to reflect the field itself I deliberately use multiple labels for the studies I consider, calling them, for example, ‘ethnographic approaches to work’ as well as ‘ethnographies of work’, and calling ethnographers ‘ethnographic field researchers’, or simply ‘field workers’ as well as ‘ethnographers’.

Highlighting How Routine Jobs are Complex

No single approach to the study of work has been more effective than the ethnographic in uncovering the tacit skills, the decision rules, the complexities, the discretion and the control in jobs that have been labeled routine, unskilled and deskilled, marginal and even trivial. Researchers working to this end have debunked hegemonic conceptions of the unskilled job, challenging the idea that the ‘truly’ skilled job is an industrial or professional one, or that it is a job held only by a male worker.\(^3\) They have shown how assumptions about what constitutes an unskilled or routine job have been socially and historically constructed, and that how managers describe such jobs may have little relation to the skills the job in fact entails.

Researchers have used the ethnographic method to dissect how workers do their job: the conceptual tools and the strategies workers use to accomplish their work when faced with mechanical failures, bottlenecks, speedups, defective materials, or the need to take shortcuts to finish their work in a timely way; how they reconcile the contradictory demands between efficiency and quality; and the individual-and-group level processes by which workers maintain dignity and control over and against supervisors and customers. Observing workers and their interactions with co-workers, managers and clients over extended periods of time; talking endlessly with workers about how they make decisions about what they do; and actually working in order to experience the organizational arrangements of and social relations in work that shape lived experience and construct workers’ interests, are just some of the ways that ethnographers have advanced social science knowledge about work.

One approach to this issue has been inspired by the work of Marx, by way of the critical analyses of Harry Braverman (1974) and Ken Kusterer (1978). In his now-classic argument about deskilling, Braverman suggested that under conditions of monopoly capitalism, employers and managers wrest planning and control from workers, deprofessionalize, marginalize and otherwise exploit them, in order to profit from their labor. Kusterer (1978) soon thereafter pointed out that such overly-deterministic assertions about deskilling ignored the degree to which nearly all types of jobs, even those that appear to be mindless and highly routinized, require some degree of worker consent, initiative and insight gained through time and experience (a point about the importance of tacit skill corroborated by Manwaring and Wood, 1984). Often as participant observers, subsequent researchers tackled these claims, investigating whether or not capitalists continually deskilled and degraded workers, robbing them of opportunities for involvement, for decisionmaking and for personal meaning (see Smith, 1994 for a review of their findings).
Juravich’s (1985) study of ‘National’, which focused on industrial jobs often regarded as repetitive, meaningless and sometimes considered to be marginal or trivial, enabled him to understand the complexity of thought and action workers – in the case of medical workers, those who deal with illness, death, ethical dilemmas, individual and family catastrophe, day in and day out – accommodate to and live with their jobs, how they depersonalize the deeply traumatic personal circumstances of others. A second concern has been to translate and demystify professional work, to give what seem to be chaotic, challenging, uncertain work settings a sense of order, of familiarity and repetition. Boek, for example, sought to understand the ‘shared and socially patterned ways that surgeons treat deaths and complications’ (1979: 31), serving as a participant observer for 18 months in two hospitals. He was a gofer, he scrubbed and assisted on operations as needed, observed meetings where cases were evaluated, and served variously as a ‘sounding board’, a ‘referee’, and a ‘historian’, a source of organizational memory for the groups of surgeons he studied. This intense engagement and the high trust he earned in the process enabled him to observe patterns in the ways surgeons routinely distanced themselves from their own and their colleagues’ professional errors.

Chambless (1996) uncovered how nurses detached themselves from even the most emotionally charged deathbed situations and even objectified the dead, turning death into an ‘organizational act’ rather than experiencing it as a human tragedy. Sludowe (1967) had drawn similar conclusions about the strategies of doctors and nurses who worked in wards for the terminally ill. The doctors that Fox (1959) observed for more than 10 months in a research hospital experienced a moral conflict between their professional imperative to heal patients and their organizational mandate to dispense experimental drugs and conduct experimental tests. They coped with this dilemma by kidding, waging on patients’ diseases, test outcomes and probabilities for surviving, and ‘counter transferring’ to their patients by showering special treatment on them. These routinely enacted mechanisms enabled them to stabilize their everyday practices and reconcile their two very different orientations to the practice of medicine.

Exposing and Explaining Power, Conflict and Inequality

Ethnographic research also has had a profound influence on our understanding of social-relational dynamics and lived experiences related to class control and inequality. Fieldworkers have observed relations between workers, between workers and their managers, and between managers. They have participated in everyday shopfloor and office relations that reveal the drudgeries and satisfactions of job tasks, as well as unsanctioned, informal activities (following classic studies of informal work groups done by Blau, [1955] 1963; Dalton, 1959; Roy, 1954). They have been squarely positioned to detect how power is exercised, control asserted and maintained, conflict and resistance expressed, and social inequalities manipulated and recreated.
Laboring side-by-side workers in their natural settings has enabled fieldworkers to experience the same emotional reactions, bodily pains and injuries, personal humiliations, compromises, ambivalences about mobility and resentment about blocked opportunities. Fieldworkers’ shared experience itself thus has been an important and unique source of insight and data. Fink (1998) worked in a meatpacking plant for five months, tapping into the degradation to which assembly workers in a ‘new breed’ of meatpacking plants were subjected. She discovered, nearly having a physical breakdown in the process, how management’s unrelenting and coercive control, the brutalities, the speed and the arduousness in the job of butchering hogs, created a near-inescapable cycle for the working-class labor force, trapping them in a life of economic and spiritual impoverishment. As a front-line worker Fink directly observed the ways in which management at this ‘new breed’ plant, which was rural and non-unionized, mapped its coercive practices onto the organizational stratification system, exploiting primarily non-white, newly immigrated and women workers.

Other researchers have observed and experienced the costs to workers’ dignity, authenticity and sense of self, when they are required to labor and perform, not so much physically, but interpersonally and emotionally, in jobs that require significant levels of interactions with customers (Leidner, 1993). Making home visits with insurance salesmen and attending their training seminars enabled Leidner to explain how the potential dehumanization that salesmen might feel – from having to make repeatedly hard-hitting, patently manipulative sales pitches to clients who frequently deflected their goal of making a sale – was offset by their hope that eventually they would profit handsomely from these questionable interactions and that they would move up into management positions. Graham (1995) found that, when she worked on the line in an auto plant where a participative work model had been introduced, she was pressured to develop both new physical, productive skills and new interpersonal skills. Her direct experience provided a core insight about the confusing and destabilized nature of control and domination inherent in a model that many call progressive: she and her co-workers felt, at various points, embarrassed, resentful, critical, but at the same time immobilized, ‘chained psychologically to the line’ (1995: 113).

Immersion, participation, observation have also yielded our most enduring typologies for understanding class control. Engagement on shop and office floors over extended periods of time gives researchers a sense of the depth of particular strategies for control, as well as the distinctiveness of patterns across diverse work sites. Theories of coercive and hegemonic control emerged from Burawoy’s (1979) study of blue-collar machine operators; autocratic control from Juravich’s (1985) study of blue-collar assemblers; paternalistic and craft control from Vallas’ (1993) study of operators, clerical workers and craft workers in A T&T; and bureaucratic control from Jackal’s studies of bank branch clerical workers (1978) and corporate middle managers (1988), and Kantor’s (1977) 1993 study of managers and secretaries in a huge bureaucratic firm: all studies based, if not on sustained participant observation (Burawoy, Juravich, Kantor), on extensive observation and interviews (Vallas, Jackal).

Notions of cultural control have increasingly gained currency from fieldwork conducted in, to name a few: ‘High Technologies Corporation’ (Kunda, 1992), ‘American Security Bank’ (Smith, 1990), ‘Ethicon-Albuquerque’, a Johnson & Johnson subsidiary (Grenier, 1988), and a Subaru-Izuzu plant in Indiana (Graham, 1995). Looking at everyday work practices and interviewing workers about their subjective impressions of new cultural norms, in-depth field researchers have been particularly successful in uncovering the disjuncture between rhetoric and experience, as ‘progressive’ cultural frameworks, introduced by managers to improve organizational performance, fail to map onto existing cultures and elicit unanticipated forms of resistance from corporate employees.

The counterpart to understanding systems of control has been the identification of modes of conflict and resistance. Because conflict and resistance are dynamic social processes, apprehension of which requires ongoing observation of action and interaction, and interpretation of meaning, ethnographers can claim a near-monopoly on this issue. Fieldworkers have been well positioned to observe, wait out, listen for and experience the dissonances between formal systems of control and the reactions of workers to them. Virtually every study mentioned above has looked at workers’ individual- and group-level resistances to management’s efforts to control their bodies and their minds. Ethnographers have uncovered how workers refuse to do what supervisors and managers tell them to do, do their jobs differently from the methods dictated by management, withhold information from supervisors and engineers about the most efficient method of working, sabotage production processes, play games on the job, and collaborate with fellow workers to finish their work.

Morrill’s (1995) innovative ethnography of executive action in private corporations examined conflict, not as an expression of class conflict per se, but as a reflection of the ways different organizations structure hierarchy and authority at their highest levels. Over the course of two years of fieldwork in three different firms he observed and interviewed high-ranking managers, extensively studied grievance patterns, and attended a variety of executive meetings. Only prolonged exposure to diverse organizational contexts could have generated this compelling comparative account of patterns of conflict enactment and management. Prolonged observation and participation in the field similarly made possible Kleinman’s (1996) vivid understanding of gender- and occupational-based conflict in an organization of holistic health workers.

Finally, ethnographic studies have effectively pinpointed how gender and race are central categories upon which the workplace is organized. Arguing that it is insufficient to study work and the labor process through the lens of class hierarchy alone, researchers have found that gender and race constitute parallel systems of control, often inextricably bound up in class power and authority relations. Kondo’s (1990) brilliant examination of gender, family and economic organization in Japan demonstrated how gendered conceptions of identity formed an enduring foundation for the sexual division of labor and for unequal modes of participation in paid work. Biggart (1989) explored the work/family linkage in the direct sales industry in the United States; Roberson (1993) also studied the work/family linkage as a participant observer in a Japanese metals firm, as did Roberts (1994), who spent 12 months working on a female-dominated tingei packing assembly line in Japan. Allison (1994) ‘hostessed’ for four months in a nightclub in Tokyo, examining how women’s sexual and work identities intersected with and were exploited by large corporations’ efforts to colonize their male workers’ lives.

Ethnographers also have uncovered how work sites recreate gender and race stratification over time, thus explaining how the workplace acts as a major institution in the persistence of inequality. Exploring Acker’s (1990) claim that work organization jobs, compensation schemes and interactional expectations are structured differently for women and men, many have traced the depth to which work organizations are gendered, explicitly and subtly. Hossfeld (1990), Hsu (1996), Lee (1998), Pierce (1995), Salzinger (1987) and Thomas (1985) found that gendered and racialized discourses were constructed, manipulated and incorporated into the way jobs were defined, compensation determined, members valued and workers controlled. Salzinger (1997), for example, conducting extensive observations in three plants in Mexico and working on the line in two, documented how shopfloor managers appropriated gendered assumptions and stereotypes quite flexibly to control female assembly workers, advancing our understanding of how pervasive yet how malleable social categories such as gender are, and how readily available they are as a source of control and social organization.

Researchers have tapped into the ways that male police officers discourage and even jeopardize the lives of female officers in routine practice and in crisis (Martin, 1980); how women firefighters precariously navigate through a deeply masculinized work culture – built on intense gender unity between men of different racial groups counterposed against a woman of any color (Chetkovich, 1997); and how, in workplace meetings and interactions, the expression of emotions is privileged when done by men but devalued when done by women (Kleinman, 1996). In so doing, they have facilitated our understanding of why jobs, occupations and positions of formal authority that appear to be opening up to women continue to discourage and block them from participating on terms comparable to men. Participant observation, interviews and sustained observation enable research to go beyond numbers that indicate women’s occupational mobility and success, to see continued inequalities within aggregate categories. Precisely for this reason, Reskin and Roos (1995) used a set of ethnographic case studies to document the ‘integration-regressive’ process: how formerly male-dominated occupations – officially opening up and showing greater statistical representation of women – continued to resegment women workers into the lowest, less prestigious levels of each occupation.

In short, ethnographers of work, like ethnographers writ large, have problematized what we often take for granted. By highlighting the complex in the routine and the routine in the complex, and by examining the reproduction of power and inequality, they have made enduring and unique contributions to the social science understanding of the dynamic nature of workplaces. These insights would not otherwise be available from study methods that cannot go deeply into organizations and occupations, study process, experience relationships and events firsthand, listen for voices, hesitations and silences, unpack and interpret meaning, and account for the effects of historical context.

In key respects the substantial size and the integrity (Hodson, 1998) of the population of ethnographic workplace studies is surprising given a set of inextricably connected obstacles
researchers have faced getting into work sites and spending significant periods of time in them. I complete the mapping ... they can spend there, researchers face professional, community and familial obligations that restrict one’s ability to...

Methods appendices and fieldwork reflections are replete with examples of the appreciable amount of time it can take simply to get permission to enter a particular work site or set of work sites. It took Thomas (...). His difficulties were two-pronged: first, I spent many discouraging months seeking approval from a number of firms to go in and conduct research. I was on the verge of being granted permission to work on the shopfloor as a temp-worker in one computer manufacturing plant when the site manager who had authorized my access left the company to take a better position in a rival firm. Despite his assurance that the person assuming his position would be delighted to have me conduct this research (music to my naive ears), his successor, to the discouragement and surprise of no one but myself, never returned my phone calls.

Once having made a connection with an individual in the type of site I was seeking, who both had the power to let me in and was enthusiastic about my research interests, it took five more nerve-wracking months before all the details of my access had been hammered out. Her delayed delivery of pertinent phone numbers that I could call to begin my work was sandwiched in between her staggeringly busy schedule as a personnel director, her need to clear my proposal with one of the corporate lawyers, and her desire to brief some of my prospective interviewees about my project. It was at this time that I began seriously to ponder research projects that would leave me less vulnerable to the inescapable realities of the corporate world, realities that seemed to thwart my goals at every turn.

Some writers convey the sense that obtaining access was seamless and effortless, that the researcher simply decided what site or sites she or he wished to study, asked for permission, and received it with nary a rejection (e.g., Fine, 1996: 240–5). But the preponderance of evidence suggests that organizational gatekeepers tend to deny and delay researchers because they are concerned – not unreasonably from their point of view – about the uses to which the research data will be put. They may worry, for example, that research reports will be used to expose company practices to the public, or be used in lawsuits against the firm. They cite the need for confidentiality, both for individuals and for firms. They worry about their liability for company practices that might be revealed in the course of the research. Such issues might be potentially explosive, such as when researchers uncover evidence about sex or race discrimination, about violations of labor law, or about the use of informal policies which run counter to official company regulations (Friedman and McDaniel, 1998). In the course of my research on workplace flexibility I have been required to sign non-disclosure forms, addressing company managers’ desires to protect details of products and specific technology innovations, and to avoid having these details revealed in articles or books, an agreement that Thomas (1994) also made with managers in the companies he studied.

Obviously, gatekeepers’ resistances to researchers present a story or set of data about the organization itself. As Burawoy (Burawoy and Lukács, 1992: 4) noted about his travels getting into Hungarian firms, ‘As so often happens in fieldwork, the genealogy of research – entry, normalization, and exit – reveals as much about the society as the research itself. Resistance to novel and potentially threatening research, such as that we undertook, exposes deeply held values and interests of the actors – both the ties that bind and the conflicts that divide.’ Yet such insight and potential can be of little reassurance to the field researcher whose time clock is ticking, whether because a leave from teaching is coming to an end, a summer break is almost over, a grant is about to expire, or repeated failure has battered self-esteem and sense of mastery.

Some might argue that the obvious, even desirable solution is to enter companies covertly, unencumbered by any obligation to people or persons with power. In fact, the number of researchers who conduct their studies anonymously and covertly is small. That most researchers obtain permission and do their research overtly reflects a constellation of factors. First, in the United States the American Sociological Association’s Code of Ethics identifies a very limited number of conditions under which sociologists can conduct covert research and as a general guideline advocates obtaining informed consent from research participants (American Sociological Association, 1997: Section 12; on covert research see Section 12.05). Then, researchers understandably worry they cannot get where they want to go inside the work organization, and thus will not be able to develop a picture with meaningful depth, unless they are authorized. Working covertly, for example as a paid employee, can restrict researchers’ access to a narrow range of interactions, events and relationships. Lack of depth can compromise one of the main advantages of ethnographic research, which is to grasp faithfully the meanings that individuals hold, the factors shaping those meanings, a full rather than partial perspective on work organizations, and the dynamic nature of work life.

It is, nevertheless, a fine line to walk. Although researchers may accomplish their goals with official authorization they also run the risk that they are being allowed contacts with and glimpses of people, situations and events carefully selected by company managers. Struggling with and overcoming this tension is a significant source of labor - strategizing, negotiating – for all field researchers. Ethnographers of work, though, often strive to descend well into organizations in their studies. They worry that they may have only partial views into one area or one workgroup, and so strive to supplement or cross-check their participant observation or observational data with other types of data. Ethnographic fieldworkers extensively draw on in-depth interviews and focus group data with a variety of participants from the setting they are studying. They have done surveys (Kanter, 1977; Vallas, 1993). Many companies have on-site libraries open to their employees and to the public, filled with publications for general audiences about the business world in general and more specialized publications and reports and documents – internal to the firm itself. However, this archival source, I have found, is vulnerable, hence unreliable. When I conducted a qualitative case study of the Bank of America in the mid-1980s, for example, I initially used their corporate library extensively, but arrived one day to discover that the library had been closed to the public without advance warning. The official explanation was that this was necessitated by reduced resources for serving the public, but since the bank was in a period of major financial crisis, its history, its practices and its mistakes scrutinized daily in the local, national and international press, it seemed plausible to me that corporate-level managers had become wary of making their internal documents conveniently available to the public. This reversal of company policy, and its implications for my study goals, underscores Buchanan, Boddy and McCalmont’s (1988) observation that opportunism is an asset when doing field research. Fieldworkers never know what organizational door will close in their faces, what meeting will be convened to which researchers are spontaneously invited, or what change in organizational fortunes may lead investigators down new avenues of enquiry. For these reasons, many ethnographic scholars self-consciously approach the field using multiple research tactics to develop broadly sketched, multi-layered portraits of work.

Once in the field, ethnographers have structured their research time in a number of different ways. Some work or are involved full-time in a research setting for long periods of time, while others do fieldwork part-time and continuously, or part-time discontinuously. A great many of the studies considered for this chapter are based on fieldwork carried out for longer than six months, and a not-insignificant minority were carried out for several years. Months and years can pass in between the completion of one case study and the beginning of another.

Fieldwork appendices and texts reveal that the diversity of approaches is not due to insensitivity to ethnographic standards, to flaws in research designs, or to methodological sloppiness. Instead, very often they reflect the real constraints governing the conditions under which researchers can and cannot conduct qualitative field research. Here, difficulties with gaining access merge with constraints on the time that social scientists can spend doing uninterrupted fieldwork. In addition to aspects of work organizations themselves that limit when social scientists can get into them, the pace at which they can collect their data, and how long they can spend there, researchers face professional, community and familial obligations that restrict one’s ability to...
commit to sustained fieldwork, particularly to fully immersed participant observation. Researchers rarely articulate the stories of how personal life – the births of children, the deaths of friends and family, physical illness and emotional upheaval (both of self and others), breakdowns of family and friendships, changes in job fortunes -shape the conduct of research and the writing of books and articles. We glean these stories from reading between the lines, usually in authors’ acknowledgments.

It is the rare researcher who has maintained a consistent, steady track record, continually immersing themselves in the ethnographic fieldwork enterprise for long periods of time over a matter of many years, but it is instructive to look at those who do. Burawoy, to use one example, visited Hungary two and three times a year over the course of seven years, at times spending entire semesters off from teaching (Burawoy and Lukáš, 1992: xiv) for his co-authored research monograph on Hungary’s transition from socialism. He has continued this pattern of immersion in the field in more recent research in the Soviet Union (personal communication). This model of work is impressive but obviously difficult to sustain.

Zussman (1992) diagnosed this problem and its implications in his own methods appendix. His frank, lucid reflections, expressing his weariness after several years of fieldwork in two hospitals, are worth quoting at length.

Although the claim would be difficult to document, it is my impression that (with a good number of notable exceptions) an unusually high proportion of social scientists who have produced superb first books based on fieldwork have then either failed to produce second books, taken a very long time to do so, or turned to different methods. I suspect that most of us, myself very much included, simply find fieldwork too exhausting, too time consuming (especially if undertaken in conjunction with a full-time teaching position) and too inefficient to justify the effort. (1992: 231)

The time-consuming nature of using an ethnographic approach to work is reflected in one direct indicator: the length of time between the beginning of fieldwork and the publication of the fifty-three research monographs considered for this chapter. The average length between the start of fieldwork and publication of the fifty books for which information was provided was 8.14 years.⁶ (To be sure, this length of time is extended by the nine anthropologists in the sample; their average was 10.7 years. Taking out the anthropologists, the average is still as impressive at 7.6 years.) This figure would be more striking (alas) so if I were able to calculate the amount of time from the design or inception of the project to publication, since a significant amount would have to be added for the period of time during which field researchers were trying to get into workplaces to do their research.

Clearly, the time conceptualizing, planning, researching, coding and analyzing, and writing, is a considerable amount to wait to see the fruition of one's work. A more indirect indicator is an observation about the origins of the books and articles I have reviewed here. Of the 57 authors whose work resulted in a book or article considered in this chapter, 32 (56%) indicated that the study originated in their dissertation research, thus done during a stage in one's academic career where individuals have greater flexibility and latitude to stay in the field (compared to the time when one is on a tenure track and must contend with, not only research and publishing pressures, but teaching, advising, administrative and committee work).⁷

Ethnographic field research in general is notorious for its time- and labor-intensiveness. Ethnographic researchers, whether immersed in communities, in social movement organizations, in the military, or in laboratories, all must struggle with the time and access dilemma. Why in particular does the time-consuming nature of ethnographies of work – the research and the production of texts – matter? As Boek (1992) pointed out, studying work and work processes can have a time-delimited aspect to it. Writing about his research on genetics counselors, he noted that with the passage of the ten years between doing his fieldwork and publishing the book, new technologies, testing procedures and scientific knowledge itself had changed enough that he worried whether his conclusions would still hold. Much field research in work organizations is historically specific, trying to document how particular forces and trends in the larger political economy shape and reshape work structures and relationships. Studying current trends – organizational (restructuring or flattening), technological (the effects of computer technology), demographic (the entrance of white women and men and women of color into the labor force and diverse work settings), or labor market (the explosion of temporary work) – is problematic for scholars whose data may not be as relevant or whose conclusions will be dated if published (especially if undertaken in conjunction with a full-time teaching position) and often relegated to margins and the back pages of scholarly texts, remain unresolved but central to this field.

Ethnographic field research is general is notorious for its time- and labor-intensiveness. Ethnographic researchers, whether immersed in communities, in social movement organizations, in the military, or in laboratories, all must struggle with the time and access dilemma. Why in particular does the time-consuming nature of ethnographies of work – the research and the production of texts – matter? As Boek (1992) pointed out, studying work and work processes can have a time-delimited aspect to it. Writing about his research on genetics counselors, he noted that with the passage of the ten years between doing his fieldwork and publishing the book, new technologies, testing procedures and scientific knowledge itself had changed enough that he worried whether his conclusions would still hold. Much field research in work organizations is historically specific, trying to document how particular forces and trends in the larger political economy shape and reshape work structures and relationships. Studying current trends – organizational (restructuring or flattening), technological (the effects of computer technology), demographic (the entrance of white women and men and women of color into the labor force and diverse work settings), or labor market (the explosion of temporary work) – is problematic for scholars whose data may not be as relevant or whose conclusions will be dated if published (especially if undertaken in conjunction with a full-time teaching position) and often relegated to margins and the back pages of scholarly texts, remain unresolved but central to this field.

Finally, it should be noted that too often researchers only hint at these difficulties rather than acknowledge them explicitly. There is a wide range of representational styles, including accounts that deeply implicate the self of the researcher in the story of work (e.g., Diamond, 1992; Kondo, 1990; Swedlow, 1998), those that do not place the researcher at center stage yet tell the story from deep within organizations and labor processes (e.g., Burawoy, 1979; Juravich, 1985), and those whose authors were less involved observers but use their observational data with rich and vivid effect (e.g., Hoosfield, 1990). Representations of methods, the confessions of fieldworkers, similarly vary from the straightforward (‘I did this, then I did that’) to the critically self-reflexive; from standard methods appendices that serve a kind of scientific legitimating function, in which researchers justify each methodological tactic and account for all time spent (e.g., Morrill, 1995), to cases in which reflections in and on the field are fully and fluidly part of the text itself (e.g., Bosk, 1992).

But as Van Maanen (1988) has pointed out, the great majority of ethnographic studies are written in a realist voice, a style of writing about fieldwork that implies unquestionable authority, objectivity, detachment and confidence about the research and the writing of the text. Although few writers are assuming or objectifying enough that they discuss their field roles in the third person, as was more characteristic of earlier generations of qualitative case studies,⁸ objectivist, realist voices pervade the majority of ethnographic studies of work. These voices convey to the reader a sense that the researcher’s observations are clear-cut, that they are imparting a truth about a knowable entity, the organization and social relationships of work. Importantly, realist accounts rarely acknowledge the uncertainties, the flaws, the confusions, and the ambivalences that authors feel about the process of their work.

Conclusion

Ethnographic studies have been invaluable for the contemporary understanding of work. Researchers have mined the situations and perspectives of workers through their own lived experience as participant observers, both as workers and as witnesses (Bosk, 1992: 12). By engaging in the same social processes, confronting the same organizational, technological, and administrative structures, and being implicated in the same relations of power and control, ethnographic field researchers have acquired a type of data that is simply unattainable using other modes of enquiry. They reveal to us things that we cannot know by conducting a survey, by interviewing individuals out of context, by doing archival research, or by performing experiments in carefully controlled settings. In particular, field-workers using ethnographic approaches convey vivid, dynamic and processual portraits of lived experience.

I have outlined three key areas which ethnographers have pioneered. But I have also suggested that there is reason to be concerned about how effectively this enterprise can be maintained. Between the restrictions placed by those guarding the gateways to businesses and work organizations, on the one hand, and the pressing demands of professional and familial obligations, on the other, researchers’ ability to conduct sustained observation and participation seems to me to be in jeopardy. This is especially troubling when thinking about doing research that gets at how work, occupations, labor processes and work organizations are changing, and how those changes affect different groups of workers who ordinarily stand to benefit from the insights of ethnographic research.

One of the major goals of social science research on work, I would argue, should be not merely to describe, but to explain, to determine how modern work organizations change opportunity structures, serve as vehicles of inequality, and transform the nature of power and control. Trends in work arrangements in postindustrial workplaces are reconceptualizing production arrangements and employment relations in fundamental ways (Smith, 1997b). If field researchers – with a keen eye toward understanding both structure and agency, the ways in which action is situated, objective constraints and subjective experiences – cannot fully explore these trends, we will have a partial view, a view that will keep us from pinpointing causal forces, identifying ameliorating policies and theorizing alternatives. We may miss out on how inequalities are maintained, or, conversely, how workplace participants embrace, in surprising ways, new forms of work, participation, or employment.

In other words, if we are confined to talking to workers at the end of the day, or to managers and personnel directors who tell selective stories about the causes and consequences of particular work arrangements, we lose the ethnographic edge and thus lose knowing what is transpiring at work. Not all fieldworkers must get jobs in the organizations they study, but my reading of the field strongly suggests they should have the ‘broad and relatively unrestricted access to people’ that Thomas (1994) held out for in his multi-case study of technology systems. These dilemmas, discussed intermittently and often relegated to margins and the back pages of scholarly texts, remain unresolved but central to this field.
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Notes

1 Because of spatial constraints I don't consider ethnographic monographs that shed considerable light on work but focus primarily on other institutions and social processes such as family (Ong, 1987; Stacey, 1990; Wolf, 1992; Zavella, 1987), community (Halle, 1984), social movements (Blum, 1991; Fantasia, 1988), secondary schools and labor markets (MacLeod, 1987; Powers, forthcoming; Weiss, 1990; Wills, 1977), and professional schools (Becker et al., 1961; Granfield, 1992), to name just a very few. It is worth noting that there is a substantial literature on work and occupations that is ethnographically evocative, which uses primarily interviews, surveys and documentary analysis to generate thick descriptions of work: some examples include research on domestic workers (Constable, 1997; Dill, 1994; Glenn, 1986); blue-collar women (Eisenberg, 1998); longshoremen (DiFazio, 1985); men in female-dominated occupations (Williams, 1995) and women in male-dominated institutions (Zimmer, 1986); and industrial workers (Dudley, 1994; Milkman, 1997). Vaughan's (1996) innovative historical organizational ethnography, a study of engineers and managers at NASA, is similarly evocative. See Schwartzman (1993) for a history of workplace studies with an ethnographic orientation or component.

2 Although some explicitly disavow such an equation; e.g., Manning (1977) emphatically noted that his qualitative fieldwork study of police officers was not in and of itself an ethnography.

3 For this reason, the studies discussed in this section have a strong affinity with comparable worth studies. The latter deconstruct the ways in which definitions of what is more and less skilled have been infused heavily with implicit biases within work organizations that value men's job tasks and qualifications more highly than women's (Blum, 1991).

4 Another organizational variable, one that lengthens the time spent trying to get into work organizations, is the notorious difficulty of making person-to-person contact with organizational gatekeepers – middle managers and personnel staff – in order to broach the topic of doing research in their firm, and seeking their permission for the project. In large companies, it is an axiom that these individuals do not over answer their own telephones unless you have a prearranged phone appointment. Even then, secretaries usually answer the phones and transfer the call to the correct person. Researchers don't often write about the wait involved as they play a long game of phone tag, leaving multiple messages on voice mail, speaking to secretaries, as well as the wait involved for the time and day, usually weeks away, that the individual can't fit you into their frantic schedules for a phone appointment. All this is only the prelude to making an in-person appointment to talk about research possibilities, usually scheduled a few weeks down the road. I have learned never to rely on a person to return my call, and instead, pursue him or her as aggressively as possible. I find that keeping a phone log is quite useful, which I use to track when I have phoned someone and to remind myself of when to call them next.

5 Human subjects review committees also discourage fieldworkers when they prohibit covert research because it might put subjects at risk or violate their privacy.

6 Authors usually indicate the year, and often the month of the year, in which they began their fieldwork: in the text, in methods appendix, acknowledgements, footnotes or in tables summarizing data collected by the author. In some cases dates of fieldwork are not included but can be approximated from the timing of key events that are mentioned in the data analysis (for example, Boek (1979) noted that he brought newspapers to the surgeons he studied during the Watergate affair, dating his research at approximately the early 1970s), or from the timing of earlier publications on the research. Technology facilitated my search for information about this since I was able to e-mail some people directly to ask them when they did their fieldwork and whether or not their research started as a dissertation.

7 Additional, anecdotal evidence supports my point. Four authors included in the population of studies I reviewed for this chapter have multiple research monographs (Boek, 1979, 1992; Burawoy, 1979; Burawoy and Lukás, 1992; Jackall, 1978, 1988, 1997; and Thomas, 1985, 1994). One of these authors published their second book 9 years after the first (Thomas), one, 10 years after the first (Jackall), and two, 13 years after the first (Boek, Burawoy), all fairly substantial amounts of time. Needless to say, all four published other things in the intervening years (articles and edited collections), but the studies listed above are the monographs reporting the results of their major ethnographic research projects.

8 See Blau's ([1955] 1963) comments, for example, about how people reacted to his observer role in two government agencies: 'In both agencies the observer was introduced to the staff as a sociologist by a senior official … Many believed he was a member of a government commission … and not a social scientist, as he claimed' (p. 3; emphasis added). It is profound to compare his dislocated voice to the involved voice of someone like Diamond (1992), whose description of his anxiety and care in helping a nearby-100-year-old woman slip on her sweater, delicately 'coaching her eggshell-brittle, pencil-thin arms into sleeves' (p. 140), as well as many other instances of caring for the frail and the sick when he worked as a nursing assistant, so vividly conveys the lived experience of the participant observer.

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