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Ethnography as Work: Career Socialization, Settings and Problems

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Whatever else it may be, ethnography is work. This reality and its implications for the doing of and institutional support for ethnography[1] has largely been neglected. None the less, in addition to being a form of cultural critique (Clifford and Marcus, 1986), a set of literary or rhetorical traditions (Atkinson, 1990; Hunter, 1990), a research tool for policy-makers (Akins and Beschner, 1979) and a set of techniques for gathering and analysing data (Agar, 1980; Becker, 1970; Emerson, 1981, 1983; Spradley, 1980; Strauss, 1987), ethnography is, we repeat, ultimately a kind of work. However, only rarely is ethnography *central* to the job descriptions of practitioners, whatever their disciplinary or institutional affiliations. Therein lies the rub. Almost never does one answer an advertisement for 'Ethnographer'. There are, to be sure, exceptions to this rule, such as in the growing world of evaluation and applied research (Loseke, 1989; Patton, 1990; Steele and Iutcovich, 1997) or for those under contract to governments, private foundations or industrial employers (Baba, 1998; Fetterman, 1989; Riley, 1967).

Some ethnographers embrace the more direct connection to social practice and grounded theorizing afforded in various kinds of 'action' research (Cancian, 1993; Lyon, 1997; Whyte, 1984, 1995). But, the occupational dilemmas in this choice are encapsulated by the very term – non-academic research – which has traditionally been used to denote activity not primarily oriented to publishing and developing theory. Here we detect a fateful career contingency in fieldwork, a tension that Wright (1967) found to be strong among graduate students: a reformist versus a scientific orientation. Our discussion of role problems suggests that this tension reflects the institutional and political pressures to which researchers are subject as conditions of employment. These tensions are especially salient among ethnographers who, like the theater's Blanche DuBois, must depend on the kindness of strangers. Being dependent on informants' consent fosters empathy, as well as ambiguous obligations of reciprocity.

For most in academic jobs, however, concerns about the practical impact of their research – Robert Lynd's still troubling question, *Knowledge For What?* (1939) – are less pressing than the problems of conducting and publishing research based on fieldwork, and gaining respect from disciplinary peers. Hence the perennial sub-text of much writing about ethnography is, in Clinard's (1970) phrase, the 'quest for respectability'. In our insistent digging into the underside of social ideals and institutions, and our alliances with those at society's margins, we may be discredited by association (Stinchcombe, 1984). Ironically, though central to the public image of social research (Gans, 1997) and to its appeal for the undergraduates who help subsidize academic sociology, fieldwork may be derided as the academic equivalent of 'dirty work' (Hughes, 1984: 338–47).[2]

Our agenda is: (1) to identify an approach, concepts and empirical problems relevant to understanding ethnography as work; (2) to show how an ethnography-as-work perspective helps one to connect separate streams of writing about practitioners and their careers; and (3) to help delineate an agenda for future research.

We need to clarify our scope at the outset: first, while our focus is on fieldworkers, we readily concede that many problems we discuss arise in different forms in the careers of other kinds of researchers, including quantitative ones (see, for example, Szenberg, 1998, on craft in economics);

secondly, our discussion reflects the normative and institutional conditions for doing research in the United States, and may not apply elsewhere, given different systems of training and promotion. Problems we discuss below, such as incorporating community groups and agendas into research, are probably smaller in nations where tenure is rare and scholars receive more sponsorship from outside the academy. We hope that this chapter prompts those working in different styles, and in other places, to contrast their experiences.

ETHNOGRAPHY AS WORK: A NEGLECTED PERSPECTIVE

By taking this perspective we subject our practices to the same scrutiny that fieldworkers have applied to other occupations. In Barley's (1989: 41–65) review of the 'Chicago School' of work, revolving around Everett Hughes, he argues that its essential (if often implicit) contribution was to reveal the recursive interconnections between careers, identities and institutions through which society itself is sustained and transformed. An occupational analysis of ethnography sheds light on the history of the method (Vidich and Lyman, 1994; Wax, 1970: 21–41) and on dilemmas that practitioners are likely to face in the future. Fieldwork cannot be understood by an exclusive focus on its internal logics, which, as Burawoy (1998: 12) shows, have often been invoked to provide justification in a ritualized dispute between reflexive and positivist 'models of science'. This dichotomy, useful for generating methodological discourse and occupational networks, obscures the institutional constraints felt in common by diverse researchers, regardless of method.

As in other occupations, ethnographers' ideals and practices coincide and diverge over time, depending on the business at hand and the interests (and power) of observers. Ideals are desirable, even essential, for occupational communities. But understanding work as a 'going concern' (Hughes, 1984: 52–64) requires that one pay equal attention to the *drama* of work: its informal organization (for example, the constraints on and conflict over resources); cooperative ties linking practitioners, sponsors, clients and 'regulators'; frustrations and thrills that animate encounters among these participants; and the forces that produce patterns in the diversity of individual careers. Also, occupational groups are neither static nor unified, but forever *in process*, through changes in internal specialization and external alliances (Bucher and Strauss, 1961).

Sadly, journals that publish ethnography pay scant attention to recurring problems of work; ethnographic monographs include methodological appendices, but these tend either to be defensive (regarding problems in research design), topical (elaborating generic fieldwork problems), or personally confessional (Van Maanen, 1988). Not even in the autobiographical writings of prominent researchers (such as Berger, 1990; Hammond, 1964; Reinharz, 1984; Riley, 1988) do we learn much about mundane pressures, pettiness or collegial sustenance in employing institutions (but see Shaffir et al., 1979).[3] Like other workers, we are often blind to organizational dynamics shaping our careers (Rosenbaum, 1989). Consulting the index of Denzin and Lincoln's (1994) *Handbook of Qualitative Research* reveals virtually no reference to the categories 'work', 'funding', 'occupation', or 'career', nor do the articles indexed (but see chapters by Greene, Morse, and Punch). Promising exceptions to this neglect in American social science can be found in feminist narratives of academic life, centered on the women's movement (Laslett and Thorne, 1997; Orlans and Wallace, 1994), but here, too, attention to the practice of research is peripheral. The richest cache of data on managing fieldwork and other demands is, as Rabinow writes, still to be found in 'corridor talk'. But, 'the micropractices of the academy might well do with some scrutiny ... When corridor talk becomes discourse, we learn a good deal' (1986: 253).

The 'Chicago School', Work and Method

The roots of fieldwork in sociology can be traced to the Chicago School and, in turn, to its connections with social problems and social reform (Bulmer, 1984; Fine, 1995; Turner and Turner, 1990). In the resurgence of ethnography among sociologists in the 1950s and 1960s, scholars developed methodological rationales for fieldwork in social research. This was partly a response to Merton's (1968: 39–72) ecumenical call to develop theories of the middle-range, as well as to the growing number in government agencies and foundations willing to fund social research but unsure about its validity. In papers such as Becker's (1970) 'Problems of inference and proof in participant observation', Bensman and Vidich's 'Social theory in field research' (1960) and Gold's (1958) 'Roles in sociological field observations' sociologists analysed fieldwork and its relation to theorizing in ways that anthropologists, as carriers of an oral tradition, had generally not done (for example, Freilich, 1970; Golde, 1970). This literature enhanced both the practice and prestige of field research.

For sociologists of work, however – especially those in the tradition exemplified by Hughes and his students during the 'Second Chicago School' (Fine, 1995; Solomon, 1968) – no attempt to understand an occupation, nor the careers of its members, could get very far by uncritically accepting lofty ideals, or ignoring the 'dirty work' of making a living. Superiors and clients must be kept at bay; autonomy and honor are seldom won for good. If Hughes and

his colleagues seemed subversive, asking the same questions of the 'proud' professions as of those in more 'humble' lines, it was not an exercise in irony or contrarism. Rather, the core insight of the Hughesian approach is that occupational ideals, routines, achievements and indignities are shaped by institutional arrangements, disparities of power and the legacies of local and societal cultures. To a puzzling degree the work of ethnographers has escaped this venerable kind of scrutiny. Perhaps the problem is that the tasks and issues 'internal' to ethnography – gaining access, forging roles and relationships, constructing and recording data, ethics, analysis, writing – have been treated with such care separately that authors seldom have integrated them with other features of work and careers. Writings about ethnography have tended to remove and abstract particular work problems (for example, gaining access, ethics, data collection, writing/rhetoric), and to subject them to philosophical or methodological scrutiny. Yet, these topics have seldom been integrated or discussed in the context of 'external' career constraints or contingencies. This lack of *occupational* self-reflection among ethnographers is striking, however, after a decade defined by the most thoroughgoing and reflexive critiques of this research genre.

From Work *in* Methods, to the Work *of* Methods

One, parallel line of analysis has explored the social relations and constraints *in* (as opposed to *of*) field methods. The thrust of these writings has been methodological – that is, rejecting or responding to criticisms about ethnography *as science*. So, Katz (1983: 147) describes fieldwork and data analysis as a social system, in which researchers, informants, and (later) readers jointly define and interpret findings. He notes that 'In its present state, the methodological literature assumes that reactivity in participant observation is a contaminating problem. But if we examine how research procedures shape the meaning of the study to members, we may conclude that field research without a formal design makes interaction between researcher and member into a substantive data resource'. Wellin and Shulman (1995) argue that placing field data in theoretical 'frames' involves negotiation between researchers and those – including mentors and reviewers – with the authority to judge. In these encounters the validity of field data, central to realist claims, is bracketed; fieldnotes and interview transcripts become vehicles for demonstrating ethnographic competence, creative induction, or knowledge of substantive domains. Others analyse conversational practices, emergent meanings and coding decisions of survey researchers (Hak and Bernts, 1996; Holstein and Staples, 1992; Maynard and Schaeffer,

1997). These authors elaborate Garkfinkel's argument that:

> The investigator frequently must elect among alternative courses of interpretation and enquiry to the end of deciding matters of fact, hypothesis, conjecture, fancy, and the rest, despite the fact that in the calculable sense of the term 'know', he does not and even cannot 'know' what he is doing prior to or while he is doing it. Fieldworkers, most particularly those doing ethnographic and linguistic studies in settings where they cannot presuppose a knowledge of social structures, are perhaps best acquainted with such situations. (1967: 77–78)

These and similar studies implicitly provide evidence about recurring work problems facing ethnographers. But revealing the fluidity of meaning within research encounters is different from documenting the obdurate institutional contexts in which such fluidity is either glossed over or resolved in routine ways. This paradox is common in occupational sociology: for instance, we recognize the discursive construction of medical diagnoses (for example, Waitzkin, 1991), but do not ignore the question of how institutional authority and procedures order work lives in hospitals.

Salient Concepts in the Sociology of Work and Occupations

Three concepts and processes basic to the sociology of work and occupations are helpful in analysing ethnographers' work: occupational socialization and culture, tensions of bureaucratic (and disciplinary) organization, and careers.

Socialization and the Legacy of 'Classical' Anthropologists

For ethnographers, idealized work images and identities are inherited partly from cultural anthropologists, whose rigorous process of penetrating others' cultural and language groups is extended metaphorically to fieldwork in one's 'own backyard'. Paradoxically, for anthropologists doing fieldwork has been at once more central to occupational socialization and identity, and less subject to critical reflection, than among sociologists (Freilich, 1970). We do not refer here to the broad, political and literary critiques of ethnography and its linkages to colonial power, in which anthropologists have been in the vanguard (Clifford and Marcus, 1986). Cultural anthropologists treat initiation into fieldwork with the deep affect and autobiographical nuance befitting its status as an occupational rite of passage. However, because the rite is culturally sanctioned as a solitary, transformative ordeal in an anthropological career, reflections by neophytes have produced little in the

way of collective, institutional definitions of work problems, let alone solutions to the problems of how to sustain fieldwork throughout a career.

The guiding metaphors by which anthropologists define and transmit problems in ethnography often reinforce individualistic, quasi-mystical imagery: one is made to 'sink or swim', in an encounter that commonly demands total and prolonged immersion in a 'foreign' culture and language. The author of an influential textbook chapter on ethnography as method and product acknowledges that the neophyte:

> has little advance preparation for the methodological and technical problems which will confront him in his field research. This is partly because of the subtlety of the ethnographic research process ... and partly because, until very recently, it was widely assumed that the process need not, and perhaps could not, be taught, that it was an ability or knack which came naturally or not at all. (Berreman, 1968: 340)

Similarly, in a memoir Laura Nader reports having received a grant to support nine months of fieldwork in Mexico. Though she lacked even a 'textbook' knowledge of fieldwork, she writes that her advisor, Clyde Kluckhohn 'told me that he thought I had been in the library too long ... I accepted the grant ... I packed several good ethnographies, a copy of *Notes and Queries* (written for nonanthropologists), and a pack of medicines, and off I went' (Nader, 1970: 98).

Coffey and Atkinson survey the dense domain of meanings associated with 'fieldwork' in anthropology, based on interviews with a graduate student and faculty member. They find it connotes both ordeal and reward in the process of training; fieldwork is central to 'semantic relationships' that organize (for both parties) the student's emerging identity along dimensions of place, inclusion, product, emotions and time (1996: 92–107). These meanings anchor researcher identities both projectively – as aspirants await validation through the crucible of fieldwork – and retrospectively – as one narrates theoretical insights in terms of biographical detail and personae. As Geertz (1988: 79) concludes, 'To be a convincing "I-Witness" one must first, it seems, become a convincing "I".'

The possessive identification, common among earlier cohorts of anthropological fieldworkers with *their* tribe or community, is analogous to the therapeutic relationship in psychoanalysis; this helps explain the almost mystical aura around cultural translation and personal transformation of the neophyte from a student of culture to an anthropologist. There was, in this tradition, little emphasis on prescriptive training or protocols since, after all, 'How can one program the unpredictable?' (Freilich, 1970: 15).[4]

This theme in the culture of anthropological fieldwork endures. In a revealing essay entitled 'I am a fieldnote', Jean Jackson explores the feelings and careers of seventy cultural anthropologists she interviewed, most of whom reported never having before publicly discussed this central activity in their work lives. In their accounts, fieldnotes themselves, for all their personal idiosyncrasy and context-dependence, embody the practice of ethnography. On the one hand, they signal the acceptance of the collective ritual of initiation into fieldwork, a membership defined by a rejection of uniformity:

> A general pattern for most interviewees is to couch their answers in terms of how much their fieldwork – and hence fieldnote-taking – differs from the stereotype. In part, this signals a defensiveness about one's fieldwork not living up to an imagined standard ... A substantial number of interviewees expressed pride in the uniqueness of their field sites, in their own iconoclasm, and in being autodidacts at fieldnote-taking. (Jackson, 1990: 19)

Anthropologists accept, even celebrate, the resistance of their practices to routinization. In turn, they reinforce the image of ethnography as an elusive combination of theoretical orientation, spontaneous insight and bodily presence. This individualistic image of ethnographic practice makes a virtue of the necessity of going it alone, and continues to define the ways in which new practitioners assess their performance and are seen by teachers. Because these evaluations are 'characterological', many ethnographers perceive their methodological stance as more salient than their discipline or topics of interest. As, Kleinman, Stenross and McMahon (1994: 4) argue, those using non-ethnographic approaches see them as 'techniques rather than [as] identities ... Field workers are more likely to identify with their method, and the perspective that underlies it, than with substantive areas. This occurs because each new study might bring us into a different substantive area.'

Sociologists have inherited the anthropological ethos that ethnography is a creative, ineffable accomplishment, borne, by necessity, of long, solitary removal from one's familiar haunts – especially given that teamwork in field studies is uncommon, despite such well-known exceptions as *Boys in White* (Becker et al., 1961) and valuable discussions like those by Olesen et al. (1994), Mitteness and Barker (1994), and Shaffir et al. (1980). The ordeal of fieldwork is seen not as a pedagogical or institutional shortcoming, but as inevitable. As a result, a connection exists between the silence in the literature regarding ethnographic careers, and the prescribed images by which aspirants embrace the role of the ethnographer. To acknowledge social and institutional constraints in ethnographers' work lives is to reveal the benign, humdrum, perhaps arbitrary nature and consequences of such constraints for ethnography itself.

Training, Identity Formation and Early Career Problems

As Pavalko notes, occupational socialization can be understood with reference either to aspirants' patterned subjective appraisals of work, or to salient features and demands of their social context. But, as he reminds us, 'Clearly, not all occupations have elaborate, formal training and socialization processes, and in many occupations socialization occurs on the job' (1988: 117). For ethnographers, socialization is embedded in a process of academic apprenticeship which, though intimate and subject to negotiation, is also regulated by evaluations and dependence on mentors. Thus, for ethnographic researchers, the learning of the craft is: (1) idiosyncratic across aspirants; (2) dependent on immersion in 'the field' at a distance from schools and mentors; and (3) equated symbolically with competence and occupational membership. These conditions, in turn, reinforce the solitary ethos of the work, the strong subjective identification with ethnography, even among many whose daily work lives all but preclude ongoing involvement in the method (Kleinman et al., 1994); and the absence of accurate 'anticipatory socialization' or of collective strategies that might help students cope with uncertainty in ways similar to those Becker and colleagues (1961) found true of medical students.

These conditions of uncertainty regarding performance, evaluation and temporal markers of progress compound the emotional paralysis or 'hang-ups' that Stinchcombe (1986) discusses as characteristic generally of graduate training. The tendency to become identified with method is also a product of the delay in graduate training before one can tackle a concrete project. Uncertainty about one's substantive direction is managed partly by the acquisition of research skills, which, in turn, is often a basis for matching students with mentors. The minority status of fieldwork students in most disciplines and departments reinforces the need to justify and identify with the method.

The degree of disciplinary consensus regarding goals and evaluative standards influences the strength and timing of methodological commitment, as it does the reception work receives. Where scholars are chronically divided over research approaches – as in sociology, education, psychology – choices about method are tantamount to career choices (Schatzman and Strauss, 1974: 3). Furthermore, role conflicts embedded in training or employing institutions are overlaid by those inherent in field relations (Adler and Adler, 1987; Wax, 1957). Negotiating field roles involves both practical and psychological demands. For instance, we must reconcile our schedules to those of our informants; and we must choose field settings in which we are 'allowed' if not comfortable, given our age, ethnic

and gender statuses.[5] To these demands is usually added that of learning the specialized knowledge or language required to be a competent member and observer of the social world.[6] And, as Wax argues, we must teach our informants to assume roles that will allow us to learn (1957).

Moving from graduate training these logistical and emotional demands produce a 'hangover' effect following the completion of ethnographic dissertations, coinciding with the pressure, certainly in academic jobs, to begin a new project. Also, any new project will be measured against the dissertation by peers plotting one's research trajectory, despite junior professors' inability to invest the time and single-minded devotion to research demanded of graduate students. There are long-term costs of failing to manage these pressures – not to mention those of family life – in the transition from student to professional. Although books are often favored by ethnographers, they take longer to complete and are subject to more variable criteria of evaluation than are articles (Clemens et al., 1995). Thus, in the early career, one finds an especially large part of ethnographers' occupational identity and hopes riding on a single product.

WORK SETTINGS AND DILEMMAS OF OCCUPATIONAL IDENTITY

Few scholars work primarily as ethnographers. The majority of ethnographic researchers are hired for positions in which teaching, advising, publishing, consulting, or service are the practical activities that must be performed. This reality, now taken for granted, is anomalous given the roots of sociological fieldwork in social reform and the emphasis in anthropology on ethnography as immersion. Shaffir and colleagues introduced their collection of essays on fieldwork processes and problems by claiming *marginality* to be an 'especially well-suited adjective that describes the social experiences of fieldwork' (1980: 18). They view fieldworkers as marginal in several ways: in our desire to know the situated and subjective realities of people, we stand outside of their communities, suspected of being spies or inept. In turn, we are marginal to the social sciences and closer to the humanities. In rejecting positivism, we are marginal to standards that have regulated much academic research and evaluation. Excepting anthropologists, ethnographers are often marginal to their own disciplines.

Ironically, the ethnographer-as-marginal theme coexists with a counter-theme, based on the protected and quasi-elite status we enjoy by virtue of class, educational and institutional affiliations. Joined to postmodern enquiries into the method's colonial roots, this critique cast ethnographic practice

and writing into a period of deep, even crippling, introspection through the early 1990s (e.g., Gans, 1999). The postmodern movement has none the less given rise to a vigorous and cohesive community of scholars, whose writings and exegeses of earlier texts sustained ethnography as an intellectual genre, based in academe. Here we note Nader's (1998) injunction to distinguish ethnography as practice (that is, the sustained, first-hand study of cultural meanings and processes) from scholarship that is *ethnographic* (also see Wolcott, 1990). An anthropologist, she is among those who regard the growing popularity of the genre as, ironically, concurrent with a dilution of ethnography's standards and aspirations. For Gans, the introspective, postmodern genre of ethnographic writing, unlike sustained fieldwork, is

> a nearly perfect adaptation to today's academic economy. [It] can be done by one person, working at home, and in bits and pieces between teaching one's classes – or even in class ... Moreover, the ethnographic product can be turned into articles ... In this respect, ethnography is similar to today's computerized quantitative research, which, at the acceptable level of quality required by its peer reviewers, can also produce the number of refereed articles needed for tenure. (1999: 7)

In *Living the Ethnographic Life*, Rose (1990: 10) too is concerned that 'our corporate way of life constrains our pursuit of ethnographic knowledge'. While this might be dismissed as romantic naivete, kindred researchers do give up their protected status as organizational employees. Diamond (1995), for example, took leave from a tenured faculty position – and his health insurance benefits – partly to experience the vulnerability that millions of Americans face in securing medical coverage in the hodgepodge of care provision. This commitment to bodily immersion in the field harkens back to such classical realist studies as Nels Anderson's *The Hobo* (1923), the 'research' for which began as the author rode the rails between work in mines and lumber camps, years before he wrote of 'Hobohemia' as a student at the University of Chicago. Goffman (1989: 125–6), reflecting on the essence of fieldwork, offered that:

> It's [a way] of getting data, it seems to me, by subjecting yourself, your own body and your own personality, and your own social situation, to the set of contingencies that play upon a set of individuals ... You are in a position to note their gestural, visual, bodily response to what's going on around them, and you're empathetic enough – because you've been taking the same crap they're taking. To me, that's the core of observation. If you don't get yourself in that situation, I don't think you can do a piece of serious work.

These scholars occupy a place on a continuum whose other pole is planted firmly in the worker's institutional setting and roles, including those who rely on 'one-shot' interviews, 'hit and run' fieldwork, or narrative analysis of extant texts. However, we are more than any of our field roles; these images obscure fieldworkers as workers who, like other workers, must negotiate and justify multiple tasks, roles and relationships. Depending on the task and audience, workers invoke multiple rhetorics, each reflecting and seeking to sustain occupational identities (Fine, 1996).

Work Problems and Role Conflict

Writings on *membership roles* in field research (e.g., Adler and Adler, 1987; Gold, 1958) have placed more emphasis on how they shape the collection and interpretation of data than how they may conflict with or disrupt other roles in researchers' daily lives. Studies of occupational socialization examine the development of and conflict between work roles (Pavalko, 1988: 84–120). Some studies reflect a 'structural' conception of roles, following Merton and Ralph Linton; others, an interactionist one, following Mead, Blumer, Becker, and others (Colomy and Brown, 1995; Hewitt, 1984). In structural terms, Merton (1957, 1968) argued that we do not occupy single roles, but are members of *role sets* with their competing expectations. In theory, conflicting pressures in role sets are attenuated by differences in the importance attached to various roles. But, fieldworkers' most immediate and visible *institutional* roles – teacher, departmental citizen, advisor – may be less central to identity than one's subjective commitment to that of ethnographer. Much interactionist attention to role conflict centers on the *dynamics of inclusion and distance* in field relations (Emerson, 1983: 235–52). These tensions, of intimacy, trust and translation, can only be managed, never resolved. This can result in a *dual-consciousness* regarding our work lives. A parallel is that which Dorothy Smith and others long proclaimed was true for women scholars, torn between their lived experience and the abstract concepts dominant in sociology (see DeVault, 1999: 46–55).

The character of and investment in academic roles varies based on method, which can be seen across the settings in which ethnographers work. One's institutional affiliation can, depending on one's field milieux, be irrelevant or even harmful to the maintenance of field relations (as when 'low status' informants are alienated or threatened by researchers' associations with the government or with elites). One class of career problems among fieldworkers are, then, rooted in the combination of their reformist orientation, topical concerns (institutional power, social inequalities) and reciprocal obligations with both sponsors and informants. Fieldworkers often reflect the grievances and hopes of those whose worlds they enter. This is why realist claims – which some postmodern critics reject as

quaint or trivialize as a literary trope – are better seen as social facts, imposed on us by all sides in our work. Both oppressed people, who see one as an instrument for articulating their critique, and more privileged informants, seeking absolution for institutional failure, see the fieldworker as a tangible embodiment of the more abstract promise implicit in ethnography: that empathic understandings can matter in exposing and shaping realities.

Further, as fieldworkers we assume complex burdens in our institutional roles. As mentors, we supervise beginning fieldworkers in a system of apprenticeship. To guide students in field projects responsibly requires much time and talk; issues of access, field roles, data sources and 'making sense' of material is far less amenable than most research genres to prescriptive advice (Schatzman, 1991). Often, students are drawn to topics in which they feel a personal stake, thereby adding to mentors' pedagogical role, a therapeutic one.

ETHNOGRAPHY AS WORK, BUREAUCRATIZATION AND DISCIPLINES

Even when the products of ethnography, such as publications, reports, or policy recommendations, fit readily into the system of occupational duties and rewards, few institutional allowances are made for the demands of this labor-intensive method of enquiry. But, on the other horn of this dilemma, compromising standards of methodological rigor to those sponsors accept may offend one's scholarly integrity and bring the scorn of fellow ethnographers. However central to one's identity and research program ethnography is, accomplishing it is peripheral to the bureaucratic timetables and record-keeping that govern work in employing institutions.

In a historical essay, 'Professionalization of sociology', Janowitz (1972: 105) argues that, 'To speak of sociology as a profession is to focus on a relatively neglected aspect of the organization of the discipline, namely, its clients and the dilemmas of client relations ... But strictly speaking, the clients of the sociologist, as researcher, are relatively ambiguous.' Janowitz (1972: 106) concludes that sociology is best seen as, 'a staff-type profession based on the fusion of research and teaching roles most effectively institutionalized in a university structure.' Adler and Adler (1995) confirm this pattern among fieldworkers. They tallied characteristics of authors who had submitted work to the *Journal of Contemporary Ethnography* (over 800 manuscripts) between 1986 and 1994, and reported that roughly 90 per cent were academically based. We have, then, the anomalous situation of a craft-like activity that has no direct constituency, and is almost entirely dependent upon an institutional infrastructure.

The overall benefits of this arrangement for the establishment of academic disciplines have been costly for field researchers. Thus situated, social researchers have been subject to bureaucratization (Mills, 1959; Sjoberg and Vaughan, 1993), both in the allocation and management of research support and in their evaluation for tenure and promotion. This trend has had distinctly different effects on scholars, depending upon their styles of research: theorists or those who rely on archival data are better suited to careers inside academic institutions, provided that library resources are readily available. The bureaucratic context is favorable too for those who analyse large-scale survey research, in line with what Sjoberg and Vaughan (1993: 5) term 'the natural science model':

> Grantsmanship in sociology is closely-interwoven with commitment to the natural science model ... By relying on established data sets, the researcher can anticipate, in general terms, the results of the findings. These are defined by the nature of the questions included in the survey. This situation greatly reduces the risk of failure, and the funding agency generally can be assured that numerous publications will result from the project.

Efforts to control costs in higher education – pervasive in the United States – may promote the natural science model, independent of the status of various research styles (but see Lidz and Ricci, 1990 for advice). One sign is the increasingly common requirement that academic job-seekers demonstrate 'a track-record of securing external funding'; these data sets and funds are, in turn, important in the training of graduate students in many research universities, and cannot but shape their own research ideals and practices.

Disciplinary Contexts and Career Problems

Many bureaucratic controls in academic life are mediated by departments and their affiliated disciplines. Field researchers are especially vulnerable to a set of career problems arising from work being subject to *disciplinary* evaluation and politics. As in academic life generally, the 'politics' are both institutional (selecting personnel and allocating resources) and ideological (shaping knowledge production). Whether separately or in tandem, these have myriad effects on ethnographers. Rather than having our work appraised and our status assigned primarily by those within the 'guild' – the *sine qua non* both of traditional craft control and of professional dominance (Freidson, 1970) – ethnographers must translate their work into terms that are acceptable to disciplinary peers who may have little direct experience with the craft of ethnography. Moreover, promotion and tenure decisions are based on criteria that are even further removed from research or

teaching practice, that rest on indices of reputation among disciplinary peers (citations, publications in prestigious journals, grants), that committee members and deans outside the field can accept.[7] Stinchcombe (1990: 338) argues that universities 'rent reputations, ... by paying [faculty] to do research whose value to the university will come mainly when they are senior scholars who are known to be first-rate by a wide community of scholars outside the university'.

This administrative fact makes a virtue of disciplinary consensus, which (at least in sociology and anthropology) is weak. Under these conditions, evaluators and grant-review committees can either fetishize those methods that have gained the highest status, or, they can rely for their judgements on more transient and morally charged criteria, such as 'interestingness' (Stinchcombe, 1994). Whereas emphasizing method works to the disadvantage of fieldworkers (as Plattner et al., 1987 found of NSF funding in anthropology), the latter may be a benefit, as long as the work is not defined as trivial or 'popular'. In a world with increasingly polycentric disciplines (Becker, 1986: 209–20), where innovative thinking spawns 'hybrid' fields (Dogan and Pahre, 1989), we ethnographers have carved out spaces with our own institutions, such as scholarly societies and publishing outlets. But, in the United States, the most esteemed journals featuring field studies, the *Journal of Contemporary Ethnography* and *Qualitative Sociology*, solicit and feature work from a wide range of disciplines, thus confusing the reception authors may enjoy among departmental peers.[8] And these spaces are themselves under pressure by researchers favouring different goals and criteria of evaluation.

Careers of fieldworkers in academic jobs and the fortunes of the method more generally are also tied, then, to factors that interact with and transcend disciplinary affiliations. Three factors we see as important are:

1   *Institutional contexts*: employing institutions differ in their definition and ranking of work products in promotion decisions; Clemens and her colleagues (1995) found that in large state and research universities, journal articles are 'the coin of the realm', whereas liberal arts colleges are more accepting of books, even or perhaps especially those which find a broad scholarly and lay audience. The same contradiction between the public and academic reception given ethnography and other qualitative work is apparent in Gans' (1997) review of 'best-sellers in sociology', whose virtues and readership often overlap with those of journalism (Ragin, 1994: 17–24).

2   *The timeliness of research topics* and their linkage with social problems agendas advanced by government agencies and foundations. The connection between field research and social problems agendas is especially apparent when one considers that a great many of the 'classic' post-war American studies – of poverty, family, ethnicity, and cities – were sponsored by such federal agencies as the National Institutes of Mental Health (NIMH), as part of their commitment to an expanded model of the etiology and treatment of mental illness (Duhl and Leopold, 1968; Felix, 1961). More recent federal policy to wage a 'drug war' and, it is assumed, stem street crime, has led the National Institutes of Drug Abuse (NIDA) consistently and generously to sponsor field research. Whatever its impact on the policies in question, and granting the moral and political hazards this dependence may pose for researchers, knowledge of fieldwork and of society have been enhanced by this support (e.g., Agar, 1973; Akins and Beschner, 1979; Weppner, 1977). Many in the postwar cohorts are nostalgic for the days when funding for field research was more plentiful, because it afforded them time away and relief from the academic career pressures discussed above.[9]

3   *Movements within social theory* create change and dynamism in careers. Ethnographers have both instigated and benefitted from movements that bridge and transcend disciplinary discourse. For instance, narrative analysis has infused enquiry in history, psychology, linguistics, even clinical medicine, which, in turn, has stimulated interest in pinning down linkages between narrative and context via ethnography (Riessman, 1993). The same synergy can be seen between ethnography and the dramatic growth of research on gender, and constructionist studies of social problems and of science.

APPLIED, EVALUATION AND INDUSTRIAL ETHNOGRAPHY

We would be remiss if we did not say that much of the growth in ethnography – both in numbers and in its influence on practice – is in non-academic settings. The traditional denigration of 'applied', versus 'basic', research, as well as the insularity of academic life, are to blame for the collective ignorance of the promise and problems in such settings (but see Baba, 1998; Fetterman, 1989; Lyon, 1997; Patton, 1990). Lyon examines and refutes conventional concerns that have impeded the growth of applied ethnography, concluding that: 'Given the increasing acceptance, and frequent advocacy, of practice- and policy-related ethnography, it is remarkable that it is not more widespread' (1997: 23).[10] Indeed, Mobley and Spitler (1998: 24) report that 'a majority of those with sociological training — at both the undergraduate and graduate level choose

to work in applied settings upon graduation'. According to a 1984 'Career Guide for Anthropologists', 'a majority of practicing anthropologists report that they are employed in areas of administration, management, and service, rather than in research' (Chambers, 1984: 338). In the United States then, applied ethnographers are both more numerous and more informative for understanding the changing role and market for the method than is generally reflected in the literature.

Given their academic training, it is understandable that many of the problems facing ethnographers in non-academic settings have to do, first, with role and identity problems (Fetterman, 1983; Mobley and Spitler, 1998; Riley, 1967) and, secondly, with the demands of justifying the method to those whose expectations are vague or irrelevant to the logics of field research. Many practitioners 'straddle the fence' between academic and applied research, finding problems distinctive to their location. Those in non-academic settings tend to have an easier time forging collaborative relations with co-workers and in seeing how their work can inform practice. Yet, as Brownstein (1990) explains from his position as an analyst in a government agency, if they are to survive, qualitative researchers must demonstrate the utility of their approach for addressing pre-existing organizational questions; and they face a greater need to 'sell' both the efficacy of the methods and themselves as practitioners, than is true in academic jobs. Fetterman (1983) elaborates on problems ethnographers, working under contract as evaluators, are likely to face. In addition to a shortened time-frame (which may involve the need to report findings on-site), contract workers often must mediate between contending organizational factions. If under government contract, they are seen as agents of government, which, in turn, is liable for the actions of researchers. As a result, ethnographers must negotiate 'up-front' and explicitly the terms, boundaries and results of research (including whether and in what form findings may be disseminated). As with those linked to government and its construction of social problems, applied researchers may be judged among informants as 'guilty by association' with institutional authorities and goals.

In contrast, those conducting applied or evaluation research from an academic base, face logistical and status problems in translating this work into the reward structure of tenure and promotion. For example, contract researchers may accept agreements regarding confidentiality, such that limits on the use of data or potential revelation of the case undercuts scholarly publication. In addition, the short time-frame of contract research may yield a level of ethnographic depth or understanding that is too superficial to inform theoretical articles. Finally, research reporting on the fate of a particular policy intervention in ways unflattering either to the sponsors of the

research or to powerful stakeholders, may expose the investigator to public, even legal, challenges that, in turn, could threaten relations with academic and private employers. Finally, when the goal of qualitative evaluation is to assess the effectiveness or efficiency of programs – that is, an 'engineering', as opposed to an 'enlightenment' model of research (Weiss, 1977) – fieldworkers inherit the positivist burden of demonstrating validity, in response to concerns that, 'This is an interesting portrayal of the program, but where are the hard facts?' And, 'With all these different views of the program, how do I know which one is true?' (Greene et al., 1988: 353; Morse, 1994). Solutions to these troubles, on which fieldworkers depend for their continuing acceptance in this career niche, can involve external audits, in which outside researchers inspect both data and findings for threats for bias or sloppiness. Such practices collide with the researcher-as-instrument ethos and may ultimately produce a convergence between ethnographic methods and those of private detectives who uncover 'dirty data' in their investigation of lies and deception (Shulman, 1994), with as yet unexamined occupational and ethical implications.

Lyson and Squires (1984) acknowledge that applied and contract research offers an alternative 'career niche' during times when competition for secure academic jobs is high (Hartung, 1993). But they detect a danger that such research will appropriate the methods and prestige of social science without either enriching theoretical understanding or altering relations of power. None of these problems is insurmountable (Weiss, 1977), nor are they sufficient to dissuade those (Bogdan and Taylor, 1990; Loseke, 1989; Lyon, 1997; Mobley, 1997; Whyte, 1995) who see qualitative evaluation work as uniquely well positioned to reveal the moral and organizational dimensions of social problems, in more direct ways than are possible when we translate the problems into abstract theory.

Ethnography in and for Industrial Settings

There is also a long-standing tradition of ethnography in industrial work organizations (Baba, 1998; Burawoy, 1979b; Sachs, 1999; Schwartzman, 1993). Occupational problems in this tradition have turned on the relationship of fieldwork and managerial control. Between roughly 1930 and 1960, and including the 'Hawthorne Studies' of informal work organization, ethnographers were consulted by and worked with the highest levels of industrial management. This period was followed by the critical, Neo-Marxian line of enquiry that Braverman (1974) established with *Labor and Monopoly Capital*. In this period (circa 1960–1980) attention to applied questions was rejected in favor of documenting forms of managerial exploitation and the

presence (or lack thereof) of worker resistance (Baba, 1998; Burawoy, 1979a, 1979b).

Since the early 1980s we see a new intellectual paradigm and a new occupational niche for ethnographic research in industry. This research has been animated by questions central to established theoretical schools. For example, Orr's (1996) study of oral culture among Xerox copier technicians, applies the concerns of ethnomethodology to work in which formal (and textual) authority is largely irrelevant. Barley and Orr's (1997) edited book on the new technical labor force, relies heavily on workplace ethnography to re-think labor divisions, politics and theory in relation to this fast-growing sector of workers.

These researchers can as likely be found in ethnographic 'shops', like those at Xerox (Suchman and Orr, 1999), NYNEX (Sachs, 1999), Sun Micro-Systems or 'E-Lab', than in academic departments. Whatever their scholarly concerns, these practitioners must also justify their work in terms of direct benefit to product design or process innovations, for employers with whom they often do not share even the tenuous connection that university-based fieldworkers have with disciplinary or departmental colleagues. New forms and uses of data strain against academic standards and rules guiding ethnography. The *Financial Times* (5 December 1997) reports that:

> Observational research ranges from the distant to the intimate. For some projects, studying footage of people browsing in a shopping mall or negotiating their way through an airport can be appropriate. For others, researchers spend time with subjects as they use the product at home or work ... One factor driving the growth of observational research is technology. Advances in photography and video recording make it easier to obtain and analyse the observations – increasing the research's value.

Finally, industrial ethnographers have increased pressure to master such technical skills as are used in the workplaces they study, and are vulnerable, once producing detailed accounts of local work practices, to the managerial goals that such knowledge may inform (Sachs, 1999; Suchman and Orr, 1999). On the positive side, industrial ethnographers tend to have significantly more time and continuity in fieldwork projects (often spanning years) than even the most privileged academics. They also enjoy great autonomy, since few in their immediate milieux are apt to question or even care about the details of how data are collected or interpreted, so long as the work, in the aggregate, informs product or process innovation (Suchman and Orr, 1999). In this sense, ethnographers in industrial settings 'make out' by finding spaces for spontaneity and independence, much as those in the machine shop Burawoy studied (1979a); and like workers in the arts, they may trade a measure of organizational status for greater continuity

and autonomy in the practice of their craft, seen holistically (Wellin, 1993).

Career Lines and Cumulation of Knowledge

Many authors of important ethnographic books, having made widely acknowledged contributions to theory, move on to new settings and topics. In doing so they violate the expectation, embedded in processes of promotion and tenure, that scholarly enquiry and individual careers be marked by continuity and cumulativeness. As Kleinmann et al. (1994) show, ethnograhers' focal concerns tend to be defined broadly, in terms of social process, or identity formation, or organizational change, rather than by discrete substantive areas. Still, given our orientation to work careers, it bears mention that such catholic scholarship is likely to be better accepted (and rewarded) from senior scholars than from junior ones, who need to establish credibility in substantive niches. Although we write of ethnographic careers, it is sobering to recognize that relatively few ethnographers sustain this research activity after completing a dissertation.

This is surely not to denigrate the quality or impact of work by younger scholars. Consider as an illustration the genre of workplace ethnography in the United States: Chinoy's *Automobile Workers and the American Dream* (1955), Gouldner's *Patterns of Industrial Bureaucracy* (1954), Blauner's *Alienation and Freedom* (1964), Bosk's *Forgive and Remember* (1979), Burawoy's *Manufacturing Consent* (1979a), Halle's *America's Working Man* (1984), Smith's *Managing in the Corporate Interest* (1990), Kunda's *Engineering Culture* (1992), Leidner's *Fast Food, Fast Talk* (1993), Morrill's *The Executive Way* (1995) and Nippert-Eng's *Home and Work* (1996) – each an important if not a classic study – were revisions of doctoral dissertations, published by top university presses. Each has contributed to theory, been a valuable book for teaching, and provides a model for ethnography.

A significant implication of the tendency for ethnography to be conducted and published by graduate students and junior faculty, however, is that the pool of practioners is especially beset by those pressures that rest most heavily on people early in their careers (that is, new or large classes, demands for publication and for university service). Even junior scholars who manage to revise their dissertations for publication – itself becoming harder, given the fiscal pressures in academic publishing – may require heroic efforts to conduct a second ethnographic project. Finally, despite encouraging recent changes, those writing articles based on ethnography have traditionally strained against the stylistic and methodological preferences of the editors of the prestigious journals.

Clearly the conditions of graduate study can provide a combination of compulsion and support – material and intellectual – that is both conducive for sustained ethnographic research and difficult to re-create afterward. The fact that ethnography is labor-intensive, while not being capital-intensive (in contrast to survey research) makes it compatible with graduate training. While there are, of course, ethnographers who continue to practice the method later in their careers, they are exceptional.[11] A worthwhile project would be a collection of statements of how, or under what personal and occupational circumstances, they have managed this feat.

In occupational terms, this realization has several important implications. First, it is notable that so many exemplars of any craft should, at the same time, be relative neophytes; rather few of the writings by which ethnography has its impact on social theory and policy are products of cumulative experience by seasoned scholars. This minimizes comparative research across related settings, and longitudinal research or retrospective interpretation of earlier fieldwork, which is more common among anthropologists (e.g., Nader, 1990; Wolf, 1992).[12]

Conversely, the scope and richness of ethnography that reflects more extensive, cumulative experience suggests the method's even greater potential contribution, were more researchers to continue doing fieldwork (see Wiseman, 1987). Both Burawoy and Lukács' (1992) research on industrial work organization through the transition from state socialism to capitalism in Eastern Europe, and Nader's (1990), on how a range of local institutions and cultural practices mediate global expansions of power, are inspiring examples of how seasoned scholarship and ethnography can inform one another. Adams reports a fascinating account of her years-long odyssey, studying followers of the American rock band The Grateful Dead (1998). Along with her increasing involvement and visibility in this 'community', (to the consternation of some colleagues and public watchdogs), she discovered that her ideas, access to 'data', modes of teaching and effect on the wider public expanded and informed one another.

CONCLUSIONS

As in most case studies of work groups, ours betrays both a conceit and a moral stance. The conceit is to impose typicality on career stages and responses which are (as we make clear) complexly varied. The stance is avidly sympathetic; and, while essential for seeing problems from workers' viewpoints, it is all the more natural because we share those problems. In addition to the inherent interest in revealing work problems (which here will be most keen for those who are non-ethnographers),

we hope to have shown the collective impact of these pressures for practioners and for the stock of knowledge that is available to inform social theory and practice. There is a distinctive value to research that immerses one bodily and morally in others' social worlds. But this work activity places considerable demands, not only on practitioners, but on the institutions and personal networks in which the researchers are lodged.

Though ethnography is not an occupation in the strict sense, we regard problems facing practitioners as *occupational*; our warrant for the term is justified, first, by workers' subjective identification with the method, which (identity) is a basis for career choices and patterns; secondly, we have seen that such workers are sought out and hired by a range of employers who somehow rely on the distinctive practices and knowledge fieldworkers provide.

Like other work communities, ethnography is defined by ideals, as well as by drudgery; by the sacred and the mundane; even by self-serving myths or lies (Fine, 1993) that aim to preserve reputation and the tenuous mandate ethnographers enjoy *vis-à-vis* sponsors, employers, students and consumers. Institutional work problems that ethnographers share are important – for individuals and for the larger 'guild' – however obscured they are at times by abstract debates over epistemology and representation. Still, in their backstage moments, ethnographers are commonly preoccupied by just such workaday problems. Examples abound: typically out-numbered – if not isolated – in their departments, ethnographers struggle with the burden of practicing and teaching a labor-intensive research 'craft' in bureaucratic institutions, among colleagues whose understanding and support may be limited. A similar tension arises with granting agencies or human subjects committees, whose demands for certainty about research methods, timetables and outcomes may collide with the ethnographer's injunction to maintain an inductive and flexible posture regarding data and theory.

Counter-balancing these pressures on ethnography as work are others that help sustain the enterprise. If practitioners are few and have marginal status in their departments, their occupational networks are relatively strong and resourceful. At the university level, ethnographers are joining, across disciplinary boundaries, colleagues from psychology, communication, education, nursing, social work and performance studies. Combined with the recognition in policy circles (notably those concerned with AIDS, drug abuse, homelessness, and educational reform) that ethnographic knowledge is critical to major public concerns, the future of ethnography appears hopeful.

For ethnographers, occupational and organizational membership bring with them distinctive challenges. First, field research places ethnographers in

practical and moral worlds outside the academy, where demands and obligations can be as compelling as those within it. Secondly, fieldwork tasks, such as discussing tentative ideas with informants or colleagues, and 'open coding' with data (Strauss, 1987) are so idiosyncratic – between workers and across projects for the same worker – as to strain conventional definitions of what the word 'work' means. Aside from inhibitions regarding 'not knowing what's going on' in a project, one often avoids sharing such tasks with peers due to ethical concerns about confidentiality or pragmatic ones about preserving access to research sites. But share we must, since managing fieldwork problems and developing theoretical narratives are, no less than fieldwork itself, social processes. Thirdly, the 'packaging' of ethnographic knowledge to fit into pre-established theoretical categories and sub-fields is more complicated than in survey research, where questions are more explicit and circumscribed at the outset.

Ultimately we claim that ethnographers as social scientists need to recognize that they are workers, and that the concepts and theories that they have applied successfully to other domains of labor apply within the scholarly workforce. As we began, so do we end, whatever else it may be, ethnography is a form of work, interpretable as such.

Clearly, we have merely charted, rather than mined, the territory involved. We hope to have sharpened interest in further reflections on and empirical investigation of problems herein. Among them: *To what extent, and how, do career and role conflicts shape the topics ethnographers study?* One would expect important gender differences here – given the unequal division of domestic labor – though we found little public discussion of this problem. *How do different national, historical and disciplinary contexts provide distinctively different problems (or opportunities) from those we discuss?* And, finally, with Adams (1998), we ask, *How can we foster career conditions and rewards which recognize the process, as well as the products, of field research?*

NOTES

1 We use the terms 'ethnography' and 'fieldwork' interchangeably. Both convey sustained first-hand involvement in research settings, which we distinguish from research based solely on interviewing or the analysis of audio or video tapes.

2 Similar status problems, arising in work groups that mediate between abstract theory or discourse and local practices, have been noted by Barley and Orr (1997) and Orr (1996) with respect to technicians in the labor force.

3 An important exception is Orlans and Wallace's (1994) collection of essays on Berkeley Women Sociologists. There, the goal of revealing gender barriers,

faced by an important cohort of scholars, overcame the tendency publicly to narrate one's career in cosmopolitan terms. Perhaps it was only after these authors became generally celebrated, however, that they determined such candor was possible without reinforcing stereotypes about women's marginal status in academic institutions.

4 Fieldwork culture in anthropology has not been static. Prior to the postmodern critique, there were efforts to codify practice, as in Campbell and Levine's (1970) 'Field Manual' aimed at facilitating replication of prior studies in elaborate schemes of cross-cultural comparison. Smith and Crano (1977: 364) conducted factor analyses, based on ethnographic data from over 800 societies, 'for the dual purposes of sorting out spurious results attributed to particular methods of analysis and also of developing an empirical model of the dimensional structure of culture'. Such a model of ethnography as a basis for a formal, cumulative body of knowledge, is unusual (Noblit and Hare, 1988).

5 Of these, age has been notably absent in writings about fieldwork (but see limited treatment, e.g., in Delamont, 1984 and Honigman, 1970). In addition to increasing career demands over time for individual fieldworkers, are constraints rooted in the age-grading of social life in Western societies, in which it is 'deviant' for older people to take part in many groups and activities of interest to researchers. This is especially true given that the social problems many field studies address (e.g., drug use, informal economy, occupational socialization, schooling) predominantly involve young people. Of course, younger investigators may have poorer access to some elite settings.

6 The acquisition of local competence in the doing of fieldwork is rarely discussed. In academically oriented fieldwork, the threshold is minimal – to know enough to 'pass' with informants, and to confirm or refine theory during fieldwork. For ethnographers in program evaluation or under contract in industry, the standards of competence, needed to collaborate in large, diverse teams of practitioners, are higher.

7 These career problems are exacerbated for sociologists by what Dean (1989) has shown is a shortage of available publication space, relative to other disciplines.

8 This same dilemma has been evident for writers on women's studies, who have often found their publications in specialty journals discounted in the eyes of disciplinary colleagues.

9 This point was confirmed through discussions between the first author and several members of that cohort, including Howard S. Becker, Herbert Gans, Lillian B. Rubin and Leonard Schatzman. We appreciate their help.

10 For valuable insights into the problems of informing policy with such work, see Rist, 1994.

11 In trying to confirm and amplify this trend, we had helpful correspondence from two experienced and respected sociology editors. Douglas Mitchell of the University of Chicago Press, and Naomi Schneider of the University of California Press. They estimate that between one-third and one-half of their ethnographic books are revisions of doctoral dissertations.

12 In a self-fulfillng prophecy, this tendency is taken as confirmation by those (including funders) charging that ethnographic research is overly descriptive, ahistorical and micro-oriented. In turn, fewer resources and allowances are made available to support fieldwork, *vis-à-vis* other, supposedly more scientific approaches.

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