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Organizational ethnography and methodological angst: myths and challenges in the field

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Abstract

Purpose – The purpose of this paper is to assess the myths and challenges in the field of organizational ethnography and methodological angst.

Design/methodology/approach – This paper is initially written as an invited keynote address for the 3rd Annual Joint Symposium on "Current Developments in Ethnographic Research in the Social and Management Sciences" (University of Liverpool Management School and Keele University Institute for Public Policy and Management, Liverpool, September 3-5, 2008). It explores what might be distinctive about organizational ethnography and how that might be different from "anthropological" ethnography. In particular, it engages a kind of collective methodological performance anxiety among organizational studies scholars without formal training in anthropology who do ethnographic research.

Findings – The paper argues that it is time to be explicit about a variety of forms of professional angst that many ethnographic researchers within organizational studies carry which have not been discussed.

Originality/value – The paper is of value to those willing to consider the myths and challenges that need engaging and perhaps uprooting and casting off.

Keywords Ethnography, Social anthropology, Organizations

Paper type Viewpoint

I have a confession to make: *mea culpa*, *mea culpa*, I am not a card-carrying anthropologist! I come out of a very inter- or cross-disciplinary academic background, having left my undergraduate "Politics" major for, first, a Master's degree in Administration, Planning, and Social Policy, which I took in a Graduate School of Education, and then a PhD in Planning, Policy & Organizational Studies, taken in a Department of Urban Studies and Planning within a Faculty of Architecture and Planning. How can someone with such a background presume, then, to expound on the subject of ethnography?

I find a certain nervousness among those of us who do ethnographic research from departmental and disciplinary bases outside of anthropology, especially when we lack a degree from the "mother" discipline. I will speak for the two fields I know best, organizational studies and political science, the latter including the empirical subfields of public policy, public administration, comparative government/area studies,

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international relations, and national government (at least, as carved up within North American academia). At conferences or workshops where scholars from those two fields who use ethnographic methods come together with anthropologists studying those topics, the former commonly begin their presentations with some nod to their own shortcomings relative to their counterparts' bona fides.

By contrast, I have not seen anthropologists studying schools, museums, hospitals, local governments, Non Governmental Organizations (NGOs) or other kinds of organizations express similar angst. I suspect that our anxiety – meaning those of us doing organizational and political or policy ethnography, my two "disciplinary" bases – is produced by what we learn from the quantitative methods courses running rampant through our curricula. Not that we have "positivism envy"; on the contrary, we typically find positivism-inflected methods largely unhelpful for engaging the kinds of research questions that excite us and for making sense of the complex organizational and policy worlds we inhabit academically. Rather, we envy the clarity that those "other" methods project – onto the worlds they study, but more importantly for us, onto the whole research project, in the classroom, in proposal-writing, in funding applications, in publishing, and beyond. I do not remember encountering anyone doing regression analysis who wondered where to start the process of making sense out of the mounds of data confronting her – or worrying that she had not fully "mined" the data in her field notes from a year-long encounter with some organizational "natives" somewhere. Nor have I ever been asked by someone doing cost-benefit analysis how to make sure that his "truth claims" are legitimate, rigorous, and valid. Our nervousness comes not only from the necessarily improvisational indefiniteness of our methods, which appear not to measure up [sic!] to the imputed rigor of surveys and their statistical analyses, but also from the sense that we are interlopers, borrowing "anthropological tools" that in some way are not "ours," whose use we feel inadequate in defending. The self-denigrating apologies imply that possession of a degree from an anthropology department would eliminate all these problems!

I suspect further that this anxiety of professional identity and practice derives, at least in part, from a lack of clarity about what constitutes organizational – or political, or policy, or some other marked form of – ethnography when it is done outside the realm of anthropology by those without formal training in anthropology departments. In thinking through what might be the possible sources of this collective angst, I have identified a number of myths about the conduct of ethnography within organizational, political, and other studies, held by those conducting such studies, about what makes them distinctive from what I will call "anthropological" ethnography. These join myths held by within-disciplinary "others" not familiar with ethnographic research about what its doing entails, as well as about its methodological justification. Some of these myths we must simply, severally, uproot and cast away. Others pose challenges that we must engage.

Myths

Ethnography's origins

> "Ethnography is a research methodology developed originally in the field of anthropology [. . .]" – (Neyland, 2008, p. 1)

> Ethnography, as a research methodology, had its origins in social anthropology [. . .] (McAuley, 2008, p. 89).

Any anthropologist worth her salt would take origins myths as her starting point!

For the longest time, I felt somehow de-legitimate as an ethnographer, seeing as how I lacked the formal education and degree(s) – the credentialing – that would make me a bona fide member of the tribe. And I am not alone: even so central a figure to our enterprise as van Maanen (2001) refers to himself as "ethnographically inclined without *proper* anthropological credentials" (p. 234, n.3; emphasis added). But, as the history of anthropology narrated by social anthropologist Salemink (2003) reminds me, and pace (Neyland, 2008; McAuley, 2008) (in this section's epigraphs), ethnographic methods did not originate with the academic discipline of anthropology. Their origins lie in administrative practices; that is, in empires' needs to manage far-flung and distant outposts – colonial ones, to be sure, with all the paternalism and "Orientalism" (Said, 1978) and racism (and sexism and able-bodied-ism, still largely unspoken of) they entailed, which marked those methods in ways their users are still contending with (more on this below); but administrative nonetheless.

So it is an organizational practice after all!!! Its origins were not only British, where its practitioners were trained to be "community development workers." In France, the professional practice was called *l'animation*. In the USA, it is community organization – one of the subfields today of social work curricula and professional practice, but also developed by Saul Alinsky in Chicago's "Back of the Yards" neighborhood among meat-packers and other workers, and in 2008 in the news through US Presidential candidate Barack Obama's biography. As Salemink (2003, ms. pp. 2-3) writes, in the context of the anthropology of the Montagnards in Vietnam's Central Highlands:

> [. . .] contrary to the now common assumption that ethnography is the descriptive (or even the field research) part of anthropology, [. . .] *professional anthropology is a fairly recent manifestation of ethnographic practice* [. . .] Missionaries, military explorers, colonial administrators, plantation owners, development workers, counterinsurgency experts, government officials, politicians, and indigenous leaders – male and female – construct(ed) ethnographic images of these indigenous groups [. . .] according to their experiences and in order to suit their interests. Those ethnographic representations interacted with the ones of professional anthropologists, who created their representations in dialogue with, sometimes in opposition to, but always against the backdrop of those "non-professional" ethnographic representations[1] (emphasis added).

It is, in sum, says Salemink (2003, ms. p. 10), "better to regard academic anthropology as a specific instance of ethnographic practice than the other way around."

It was my own lived experience as a community organizer in a government corporation in Israel, during three years of working before I went on to graduate school, that drove me to my postgraduate cross- and inter-disciplinary "heresies" (from the perspective of academic disciplines). That very experience is what made me an ethnographer, as I brought it into my developing academic work (initially as my dissertation and then as my first articles and book). So I come by my methods, and my methodological identity claim, honestly. Owing to Salemink's (2003) history, I feel that I can, at last, hold my head up when I talk about my research methods – and I encourage all of us, once and for all, to set aside the *apologia pro vita sua* that we carry with us and to just get on with it!

I am not alone in drawing on something in my personal, pre- or non-academic background for my field research setting. In my case, it was prior work experience. For others, it has been family connections, family origins, even *mamaloshn* – the language of home, of one's community of origin, of the (immediate) ancestors. Although drawing on one's personal background may not be common among survey researchers or experimentalists, it is a commonplace among organizational and political ethnographers. (American anthropologists have long gone to the field setting of what my German-language colleagues call their "*Doktorvater*" (PhD supervisor; I have yet to hear of a "Doctor Mother"), and the ability to place students in the setting of one's own, earlier, field research is one of the reasons one may be hired.) Here is another sort of origin myth to fess up and get over – the extent to which our choices of field research settings do not always originate in the academic literature, are not selected arbitrarily, and yet are no less scientific because of this. Why continue to hide it?

Anthropologists do it with legitimacy!

Related to the origins myth is the notion that, whatever its origins, ethnography – of organizations, public policies, political life – is somehow more legitimate when done by anthropologists than by organizational studies, public policy, educational or other scholars. After all, if they started it, they must, ipso facto, have perfected its performance – as well as the means for judging and evaluating that performance. It is time to leave this one behind, too. Anthropological and "marked" ethnographic research are, I would argue, different kinds of engagements even when the settings are identical or similar; and different, as Minow (1990), among others, has pointed out, need not mean "lesser." In moving beyond this myth, we need to do a better job of articulating what these differences are. What is it that makes organizational ethnography, when done by an organizational studies scholar, distinctive?

The answer is rather straightforward on the face of it: organizational studies scholars are interested in those scholarly domains that make that field its own "discipline" – issues in organizational theory, such as organizational forms, structures, and design (whether new or old) and reorganization; problems in organizational behavior, such as management control methods, employee motivation and morale, employee involvement, empowerment, work restructuring, patterns, and conditions of employment (whether ongoing or changing), and the effects of these on job satisfaction, commitment, and job security; overarching issues, such as problematics in leadership and change, auditing and information systems, gender, workplace politics, organizational culture, organizational identity, organizational learning, and such classically non- (or anti-) management topics such as knowledge/power, oppression, exploitation, alienation, and subjugation, workplace ethics–a list derived from the Call for Papers for the 2008 Liverpool-Keele ethnography conference. Or, to take another taxonomy, studies of:

- Organizational processes and informal relations, including chains of command, standard operating procedures, decision-making, communication, definitions of offices (tasks), levels of responsibility, trust, competition, cooperation, presentation of self, routines, and teamwork.
- Organizational identity and change, including new technologies, governance structures, internationalization, performance evaluation, mergers, and acquisitions.
- Organizational "environments," including institutions, networks, industries.
- Organizational morality and conflict, including disputes, illicit practices, illegal organizations, and corporate misconduct.

The four lead entries are van Maanen's (2001, pp. 240-253); the subsequent terms are culled from those pages and the organizational studies discipline as a whole. We might do a better job of articulating the distinctiveness of an organizational studies approach to ethnography if we were better able to draw out the relationships between theorizing and ethnographic data.

But there are more subtle differences, as well, between organizational ethnographies and anthropological ethnographies of organizations. They are so hard for me to articulate that I fear I have to resort to paraphrasing US Supreme Court Justice Potter Stewart's definition of pornography (in his 1964 concurring opinion in the *Jacobellis* v. *Ohio* 378 US 184 obscenity case): I know it when I am reading it! There is a quality of analytic insight in organizational ethnographies (and I would say the same about political and policy ethnographies) that I miss in "purely" anthropological ethnographies. As scholars grow into a disciplinary community, they adopt that community's ways of seeing – the shared sense of the field's key theoretical concerns – along with its ways of knowing – its shared sense of how to go about addressing those concerns. (This was one of Kuhn's (1970) central points in describing how scientific communities resist change). And so reading, or perhaps even just reciting, "Van Maanen (1991)" and "Orr (1996)", or "Kunda (1996)" and "Barley (1986)", or "Czarniawska (2002)" and "Rosen (2000)" – our shared language within this discipline – gives me that "cozy" feeling of being at home among scholarly concerns with which I am familiar, while the chapters in Gellner and Hirsch (2001), with the notable exception of Malcolm Chapman's (2001), left me ill at ease, wondering why the volume's anthropology-trained authors appeared surprised by the problematics of organizational forms and structures, hierarchies and bureaucratic politics, turf wars and control, and other organizational studies topics that they "discovered" and discussed.

Let me sketch out some other facets of the mythological domain before engaging this point further.

Organizational ethnography is new

> Ethnographic approaches to the study of organizations is a newly developing field [. . .]

I have made up this quote, as I, maddeningly, cannot put my hands on the many book chapters, articles, and papers that I have read in the last few years which advance this claim. Organizational ethnography is decidedly not new – but knowing that requires a decidedly non-corporatist, non-presentist, and perhaps even non-US-centric approach to the field of organizational studies. In the USA, since the late 1970s and the growth of the corporate sector, academic studies of organizations have largely been colonized by management faculties and business schools, away from political science, sociology, educational studies, and other social scientific departments where they once comprised major foci of research. The US Academy of Management has increasingly become an academy of corporate business, to judge by the content of presidential addresses given there in the last decade or so. And it has been difficult in recent years to find, on the pages of US organizational studies journals – the *Academy of Management Review* and *Academy of Management Journal* chief among them – and those that ape them treatments of other than corporate organizations: nonprofit organizations, public sector organizations, health-education-and-welfare sector organizations, as well as family firms and other entrepreneurial configurations that do not fit the large-scale, corporate model.

For organizational ethnography this has meant a kind of amnesia concerning a body of work developed before the behavioralist-survey research-computer revolution of the 1960s and beyond, which, under the influence of 1930s-1940s Chicago School-style research, took an ethnographic approach to the study of a wide variety of organization-based and other work-life. Exacerbating this amnesia has been the growing extrusion and segregation of public administration programs – the natural home of bureaucracy (and anti-bureaucracy) studies – in the US from "organizational studies" departments and schools and the ghetto-ization of their curricula and research in self-standing schools and departments, journals and conferences.

At one time, both of these parts of the organizational studies world traced their origins to Max Weber's writing; and studies of bureaucracies and bureaucratic work practices, many of them done using ethnographic methods, were a mainstay of the discipline's research. I have in mind, for starters:

- Blau's (1955) *The Dynamics of Bureaucracy: A Study of Interpersonal Relations in Two Government Agencies*;
- Kaufman's (1960) *The Forest Ranger: A Study in Administrative Behavior*; and
- Selznick's (1949, 1957) *TVA and the Grass Roots* and *Leadership in Administration*.

These and more drew on those same methods described and discussed today in organizational ethnography papers and the growing numbers of text- and other books treating it both methodologically and substantively. French (2008) has detailed a similar erasure of the methodological past among Industrial and Labor Relations programs, another member of the organizational studies institutional family.

It is time to put an end to this disciplinary amnesia, reclaim this heritage, and celebrate it.

Organizational ethnography is a single "n" study

This amnesia may be a willed reaction to methodological critiques contributing to an overall malaise concerning interpretive and qualitative methods, organizational ethnography among them. Yet those critiques are based on a key methods myth prevalent among scholars doing quasi-experimental or survey research – that ethnography is a single "n" study, and therefore not rigorous and its "findings", not reliable or valid – which is used in attacking ethnographic and other interpretive research and which has largely been internalized by scholars doing the latter.

What all of the classic works named above have in common is a large number of events, persons, acts, and interactions observed, people spoken with, documents read; minutes, hours, days, weeks, and months, if not years, spent on site. This puts the lie to the charge that organizational ethnography is a single "n" study:

> [. . .] it is a fallacy that small "n" studies entail a small number of observations: they may entail a small number of research sites – one is not uncommon outside of explicitly comparative work – but field studies of communities or organizations or polities entail large "n" data points in their sustained observation [with whatever degree of participation] over extended periods of time, often in and of various locations within the research site, extended and repeated conversational interviews, and/or multiplicity of agency, policy, or other documents read and analyzed. One might imagine counting, for example, the large number of hours of engaged observation, the number of conversations held, the number of interactions,

> and the ensuing number of segments of observation and/or conversation and/or interaction analyzed over the course of the research project – any one of which would yield a large "n", indeed (Yanow and Schwartz-Shea, 2006, p. 15).

Take one example. In her study of a single organization, a single "n" study by traditional reckoning, Kanter (1993/1977, p. 337) spent over 120 "personal on-site contact days" and conducted over 120 "more than momentary conversations" in which she asked her interlocutors to describe other people (uncounted) and their situations (also not tabulated). This accounting:

> [. . .] omits the countless hours of observing and "momentary" conversations. In some sense, each one of these constitutes an "observation," although not necessarily as that term is used in [experimentation or in methodological discussions of quantitative analyses] (Yanow and Schwartz-Shea, 2006, p. 15).

Organizational ethnographers need to stop feeling defensive against this "single 'n'" charge which implies that their work lacks rigor; doctoral students and others in organizational studies need to stop avoiding doing such research because of this accusation. A fuller grasp of ethnography's methodological and presuppositional grounding provides sufficient ammunition to refute these charges.

Organizational ethnography lacks "rigor"

It is a constricted sense of what it means to be rigorous that is invoked to dismiss organizational ethnography as non-trustworthy and therefore nonscientific[2]. Our research may not be rigorous in the sense of unbending, step-wise "progress" – the sense of "rigor" as enacted in the form of the scientific method laid out in methods texts. But ethnographic research is systematic – or can be, given methodological requirements to carefully anticipate, consider, and plan for the days, dates, and times of a study, to provide a detailed account of "exposure" to and in a field setting, to "map" views across various levels and/or departments and/or actors and/or time periods within an organization, to be systematically reflexive about one's positionality and other elements of knowledge generation, and so forth (Schwartz-Shea and Yanow, 2009; see also the various chapters in parts II and III of Yanow and Schwartz-Shea (2006)).

At the same time, the social scientific rigor that is held out as a virtue (if not a fetish) is, from an ethnographic perspective, more of an error. Ethnographers need to (be able to) respond "in the moment," as theatrical improv has it (Yanow, 2001), to what their interlocutor has just said in a conversational interview or to the suggested invitation just extended by the copy or coffee machine. They need to "yes – and" (drawing once more on improv theory): to take the conversational "move" and build on it, rather than throwing a "block" that will stop the interaction. Here, too, is systematicity of a sort, but it rests on training in recognizing such openings as what they are and for what they offer, rather than in following a predetermined schedule step by step.

Organizational ethnography should be hidden under a barrel

Rather than seeing that organizational ethnography rests on its own not inconsequential set of philosophical presuppositions – and therefore need not bow its head to quantitative research any more than it does to anthropological ethnography – some advise researchers to hide what they are doing or downplay the role of their qualitative evidence in producing research findings. I would argue that practicing "closet qualitative research" (Sutton, 2007) is not only not "virtuous"; it is outdated – in light of contemporary methodological argumentation, unenlightened, and misguided. This advice:

> [. . .] is unenlightened in that qualitative methods of an interpretive sort have long been a part of the organizational studies world and methodologists have worked to articulate and promote the standards and criteria that are appropriate for evaluating such [. . .] research [. . .] [It] is misguided because interpretive researchers have moved far beyond defensive modes to exploring new methodological terrain that re-positions that research firmly within the canons of science [. . .] (Schwartz-Shea and Yanow, 2009).

Such advice proceeds from an assumption that only positivist-informed research is legitimate as science, denying:

> [. . .] the historical standing and significance of the contributions of qualitative-interpretive science. Moreover, a qualitative-interpretive science that tries to conceal the character of its evidence can only be a weak version of quantitative-positivist work [and vice versa]: the presuppositions informing the conduct of each are simply too different for such a masquerade to be successful (Schwartz-Shea and Yanow, 2009).

This and the charge that organizational ethnography lacks rigor can only exist in a situation in which ethnographic (and other interpretive) methods are unknown or misunderstood. Presuppositional argumentation concerning its ontological and epistemological grounding aside, organizational ethnographers still need to figure out the character of the mythic beast and do a better job of making explicit its entailments and processes. Let us consider two of these central issues.

Organizational ethnography = writing?

These days, ethnographers and other interpretive researchers insist on reflexivity as one way of handling our awareness of our positionality in its many forms – and our greater awareness of the ways in which both research and writing entail choices, themselves "ways of worldmaking" (Goodman, 1978) with respect to our topics of study (Clifford and Marcus, 1986; Geertz, 1988; van Maanen, 1988, as well as Brower *et al.*, 2000; Golden-Biddle and Locke, 1993, 1997; Richardson, 1994; Yanow *et al.*, 1995). Perhaps, under the influence of this turn toward textuality, many are wont to argue that, looking at its etymological origins, "ethnography" means a form of writing – and that this is its distinctive hallmark. The text, however, cannot be separated from the fieldwork that produced it. As van Maanen (2001, p. 235) put it, "The work of ethnography, then, goes on in much the same way as it did before textuality came into fashion."

Salemink (2003, ms. p. 20) comments on the present state within anthropology:

> There is a peculiar see-saw historical movement here: whereas Malinowski tried to subsume ethnography to the professional's claim to intensive fieldwork as a scientific method, now fieldwork seems to be subsumed under the practice of writing culture. If one wishes to understand the processes of production and consumption of ethnography, this subsumption needs to be resisted.

"Writing" alone, in other words, does not an ethnography make. In order to resist this equation, we need more explicit delineations, once again, of what "the work of ethnography" entails.

There is yet another move that needs resisting, and that is the reduction of organizational (and political and policy) ethnography to interviewing.

Organizational ethnography ≠ interviewing

Yet another treatment of ethnography sees it as only a set of tools, each one capable of being isolated from the others in the tool box.

One of the tools, not typically listed in qualitative methods texts (which commonly focus, instead, on observing), is language. What the anthropologist began to study before heading to the field setting was its language (pursued further on site), as well as learning something about its "culture" through reading previously published monographs about its kinship structures, rituals and ceremonies, and so forth. The organizational ethnographer needs to learn general business, government or nonprofit terminology in order to converse intelligently in such field settings, as well as the language of the industry within which the study is situated – museum-speak, or high tech, or physics, or cooking, or copier terminology – in addition to the language particular to the organization being studied and the theoretical field being interrogated. Moreover, organizational history – or political, societal, cultural or other history – may be important for understanding the present-day characteristics and actions of and in the subject of study. For example, my students' and my study of The Netherlands organizational museums has required us to delve not only into the history of museums in general, but also Dutch political and museum history, specifically, in order to understand the place and role of organizational museums in contemporary The Netherlands life.

The communities of traditional anthropological ethnographic study did not commonly produce extensive written histories or other documents that the anthropologist could use to prepare his a priori knowledge prior to or upon arrival. Here, is where organizational research is distinctive: "organizations" produce documents – annual reports, meeting summaries, informal notes, and, etc. depending on the kind of organization and the research question, research-relevant newspaper, magazine, and other sorts of print and aural and/or visual journalistic coverage, not to mention internet materials, may also exist; and these can be important sources of background information, as well as evidence for analysis. Furthermore, one cannot count on encountering all key participants by hanging out at the water-cooler, coffee or copy machine, or "loo" and being able to strike up conversations with them; it can be – and usually is – necessary to set up appointments to interview such people, although terming these "interviews" need not signal that they are closed-ended, structured affairs.

As qualitative methods, let alone ethnography *per se*, are not commonly taught in organizational studies (read: business, management) curricula; as survey research, which is taught, is often equated with "interviewing," itself rather widely known as a qualitative research tool; and as ethnographic research is "clearly" qualitative, *sic ergo* "ethnography" must mean "interviewing" – or so it seems from a large proportion of the manuscripts I have been asked to review over the last three years (I cannot cite them due to blind review practices). But if we reduce ethnography to interviews, we lose sight of its specific character as a way of knowing. I hold with Gay y Blasco and Wardle (2007, p. 12), who advocate "respecting this specificity". Moreover, in reducing ethnography to interviewing, we risk diminishing the importance of something that is central to its practice: that definitionally-elusive ethnographic sensibility (Pader, 2006; Ybema *et al.*, 2009).

Challenges

The challenges facing those of us doing organizational ethnography, then, are at least four fold. One, we need to be able to answer the question, what is ethnography, as done from an organizational studies perspective? I urge us to resist the equation of ethnography with interviewing, a practice that appears to be prevalent, especially among newer scholars. But at the same time, we need to be able to articulate the ways in which organizational ethnography is a sensibility and to work to delineate this more clearly.

Two, we also need to be able to answer the question, What is an organization? In ways that make clear that organizational ethnography is not only about corporations. This means not only including public sector services, local authorities, and the like. But also means engaging virtual and new media-mediated organizations; and the prospect of studying second life-based organizations and others, using their own and other technologies, has strong implications for the third question: how do traditional methodological concerns need to expand in order to encompass methods appropriate to contemporary organizational ethnographies?

Traditional ethnographic inquiry rested on the unspoken assumption of face-to-face interaction while immersed in a single, bounded community: the anthropologist went to great lengths, and often great distances, to make this happen. This is the traditional idea of ethnography, which comes to many of us through reading anthropological research monographs and which continues to survive in our methods textbooks and discussions. New forms of organization challenge the methods inherent in both of these assumptions. Ethnography needs to be able to give a methodological account of a range of surrogates for in-person relationships, from voice (e.g. telephone or VOIP/Skype interviewing) to writing (e.g. e-mail, SMS, and "Chat" IM interactions) to avatars that can be entirely fantastical, that are increasingly in use in today's organizations. Other organizational innovations challenge anthropology's long-standing assumption of single-setting research: "multi-sited ethnography" is a marked term (i.e. it is the "lesser" version of the unmarked term, "ethnography")! Organizational ethnographers are positioned, in terms of both their theoretical armamentarium and study setting, to engage these issues and anticipate methodological turns that traditional anthropology (other than "applied anthropology" in high tech firms, hospitals, and the like) has, on the whole, not yet considered and developed an account of.

There is a second methodological issue that all ethnographers, regardless of disciplinary base, need to engage: how to rid field practices and their conceptualization of the colonial heritage passed, unconscious and unthinking, through the language of ethnographic methods? The old formulations, ranging from admonitions about "going native" to injunctions to join "emic" with "etic" and "insider" with "outsider" positions, are outdated, themselves having emerged from a particular construal of "natives" and researchers, and of their relationships, that almost every ethnographer, in every discipline, today rejects (Nencel and Yanow, 2008). And yet the terms carry these outdated ideas, and organizational studies ethnographers have absorbed many of them from their anthropological sources. We need a heightened, critical reflexivity on their use.

Along with this, we need a more conscious and explicit consideration of power relationships between researchers and organizations; and this leads to the fourth challenge, the last I will mention: how does organizational ethnography fit into current higher educational structures?

In the USA, ethnographic research does not fit the Institutional Review Board (IRB) paradigm for doing science, which is based on an experimental research design model in which research participants come into the researcher's domain (e.g. the laboratory). Ethnography, however, is based on researchers going out to participants' domains – and this constitutes a different wrinkle in power/knowledge relationships (Yanow and Schwartz-Shea, 2008). Senior scholars in several disciplines in the USA – sociology, legal studies, political science – are concerned that IRB policies threaten to end participant-observer, ethnographic research as we know it. US IRB requirements have the potential to curtail the cross-national collaborations that have been growing among organizational scholars exploring "globalization," entrepreneurial diasporas, and the like.

Ethics review boards exist in other countries, as well[3]; but the UK has a different policy that threatens ethnographic research, in a different way – the Research Assessment Exercise (RAE). The view is widespread among business and management academics that books do not count in assessing research outcomes. Although this may not be the actual case[4], the perception (rampant in The Netherlands as well) is leading many UK scholars to turn away from this customary outlet for ethnographic research – one that makes sense given its data presentation requirements, in terms of page-lengths, for situational, historical, and other contextualization. To the extent that management programs adopt the mathematical model of doctoral dissertations – three or four published articles collected between covers with a single title page – rather than the traditional social scientific book-length manuscript, ethnographic research comes further under attack.

Parting words

It is not my intention to instantiate turf wars that pull up drawbridges between sociologists or anthropologists and organizational studies scholars. In addressing the organizational studies world, I seek to challenge its members not to forget their rich ethnographic heritage. And I am calling on organizational ethnographers, specifically, to rid themselves of the inferiority complex they seem to carry with respect to anthropology and on all of us engaging in such study, regardless of disciplinary or departmental affiliation, to join forces in developing a more explicit articulation of what ethnographic practices require – with all the transparency that methodologists currently call for.

There are many other substantive-methodological concerns that pose challenges to the conduct of organizational ethnography today. Organizational studies scholars doing ethnographic research cannot do full justice to any of these issues if we continue to look over our shoulders, not with worry that we are not doing as good science as the organizational psychologists and organizational economists, but with anxious concern that we are not doing as good science as the anthropological ethnographers. Let us take care to develop what is distinctive about organizational ethnography and to be as explicit as we possibly can be about what that entails. Let us continue to look "inside organizations" as organizational studies scholars, with an appreciation for that discipline's distinctive concerns and possibly distinctive ways of being ethnographers "at work."

Notes

1. Salemink (2003, ms. p. 9) later adds: "To a large extent, the professionalization of fieldwork in British anthropology depended on the tactical denigration of both missionary and administrative ethnographies". He notes there Talal Asad's argument (in his 1973

Anthropology and the Colonial Encounter) that it was "not the complicity of anthropologists with colonialism, but the location of anthropology in the colonial context" that was the crucial issue.

2. This section builds on Yanow (2006, 2009).
3. The Netherlands pan-university *gedragscode* for the use of personal data (*persoonsgegevens*) is equally inappropriate for ethnographic research; but so far, this has drawn little attention within the national anthropological association, let alone outside it (Yanow and van der Haar, 2008).
4. Rick Delbridge (personal communication, September 4, 2008) assures me that RAE examining boards do count books. My understanding that they do not comes from conversations with several UK colleagues over the last few years and from my own experience of being told (by a colleague on a UK university's hiring committee) that I would not be considered for an appointment because my books were not countable. The perception may be inaccurate, but it is, nonetheless, out there, widely held, and enacted.

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