Accounting for Accounts: Crafting Ethnographic Validity through Team Ethnography

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Abstract

Although early classics in ethnographic sociology were produced through research teams, contemporary ethnography is more often described as a personal endeavor, with ethnographers generally entering the field alone and producing single authored texts. The deeply personal nature of the method is viewed paradoxically both as a source of novel insights and as a threat to validity and generalizability. This chapter draws evidence from an innovative ten-year group ethnography of the design and implementation of an Environment, Health, and Safety (EHS) management system across 500 laboratories in a large university. We describe how the team of ethnographers traced the reverberations of the university’s commitment to design an EHS system – as a consequence of an environmental enforcement procedures -- through the daily operations of the laboratories, administrative meetings and decisions, faculty response and resistance, as well as EHS officers’ own work practices. Beyond theoretical insights at the interface of the sociology of organizations, science, and law, the project offers two methodological contributions. First, we distinguish criteria of validity for different aspects of qualitative field research: descriptive, interpretive and theoretical validity are independently distinguished and then associated with conventional requirements for reliability and generalizability. Second, we unpack the intricacies of how groups of scholars gather data and describe what has been observed, analyze and interpret the observations, and then integrate data in ways that leverage the collective’s multiple insights for producing theoretical advance through ethnographic research. Overall, this paper explores efforts to articulate, teach, and critically reflect on ethnographic methods, moving from individual, often tacit and opaque, practices to a collective, visible collaboration.
A number of early “classics” in ethnographic sociology conducted research in teams (e.g. Becker, 1961; Gouldner, 1954). With the recent resurgence of interest in ethnographic research, we see some projects conducted in multi-sited teams (e.g. Anderson, 2000; Burawoy, 1998; Cress & Snow, 2000; Heimer 2008; Newman, 2009), although by and large ethnographers still more often go into the field alone and subsequently produce single authored accounts (e.g. Duneier, 1994, 1999; Espeland, 1998; Blee, 2003; Fine, 2008, 2009; Heimer, 1989; Wacquant, 1998, 2002; Wagner-Pacifici, 2000). The solo ethnographer remains the most common model and yet it may not always be an appropriate model, particularly for those heading into the field for the first time, or for those who wish to study large organizations or complexly coordinated distributed practices characteristic of many contemporary phenomena. In this chapter, we describe our experiences using a team model of ethnographic fieldwork, focusing on its implications for training and, more generally, for improving the validity of ethnographic fieldwork.

According to Van Maanen, fieldwork and ethnography are distinguishable. "Fieldwork usually means living with and living like those who are studied. In its broadest, most conventional sense, fieldwork demands the full-time involvement of a researcher over a lengthy period of time (typically unspecified) and consists mostly of ongoing interaction with the human targets of study on their home ground" (1988:2). Ethnography is the written product of the fieldwork and a standard method for those who wish to describe the culture of a group or organization - how its members "go about their everyday lives" and what it sense they make of their activities (Emerson, Fretz, & Shaw, 2011:1). Ethnographies describe, and often explain “the actual social context and life worlds of those being studied,” in detail and with a depth not often
available in other forms of research (Snow, 1999: 97). As the written representation of a social system, ethnography “carries quite serious intellectual and moral responsibilities, for the images of others inscribed in writing are most assuredly not neutral. Ethnographic writings can and do inform human conduct and judgment in innumerable ways by pointing to the choices and restrictions that reside at the very heart of social life” (Van Maanen 1988:1). In effect, as Van Maanen writes, "ethnographies join culture and fieldwork... [they] are documents that pose questions at the margins between two cultures. They necessarily decode one culture while recoding it for another audience (Barthes, 1972).”

Beyond the stereotypical image of one “one anthropologist per tribe” (Van Maanen 1988) model, there are two group ethnography models. The first is the collaboration model in which ethnographers disperse to study a particular selected phenomenon across sites. Although they share their field notes and interview transcripts, discussing and analyzing them as a group, they write independent papers about different sites (Bearman, 2009) or may pool their data to generate insights comparatively (Barley, 1996). This model of ethnography has also been used successfully in several major projects in urban sociology (e.g. Anderson, Brooks, Gunn, & Jones, 2004; Newman, 2009). In the second team model, a group of ethnographers observe and interview within the same site, coordinating their observation and interview schedules, and sharing notes, transcripts, discussion and analysis as observation and analysis unfolds. This is the method used in the classical ethnographies by Becker, Greer, Hughes and Strauss (1961) on medical education and by Gouldner (1954) on industrial relations in a gypsum mine. These models differ according to whether there are single or multiple sites (industries, economic sections, and organizations), either unified or distributed, creating different implications for validity and generalizability. Both team models are often used as a means of training students.
Although writing about qualitative methods has proliferated in the last two decades, there has been, according to Huberman and Miles (2002) "no parallel proliferation of studies of the actual process of doing qualitative research." For example, while there are numerous texts that describe how to analyze and then theorize and finally write up accounts from ethnographic data (Strauss & Corbin, 1990; Saldaña, 2012), collecting data is rarely specified with explicit techniques and until very recently even less consistently reported in publications (but see Emerson, Fretz and Shaw 2011). The cause of this inattention to the process is over-determined, a product of ethnography's own history, the epistemological debates among different approaches within qualitative methods, no less vociferous antagonisms between qualitative and quantitative researchers, as well as the heated culture and science 'wars' following the 1980s post-structural turn in the social sciences. As a consequence, the practical skills of data collection and analysis, as well as the distinctions and connections among these, are not well understood, especially among non-practitioners.

Thus, it seems important to place additional focus on unpacking the process and practical skills of field work. This is as important for the collection of data as for the analysis, which in ethnographic fieldwork is always continuous and simultaneous with data collection (Becker 1998; Silbey 2004). One of the most frequently voiced concerns about ethnography is whether a different observer would have come away from the field, independent of the variations in the voice with which the account may be written, with the same basic descriptive account, interpretation, and theoretical contribution. In other words, how reliable is this description of the social world depicted in ethnographies, and how valid is the explanation of what was observed. In ethnographic research, reliability is closely connected and perhaps best understood as a form of validity (Hammersley, 1992, p. 79). Although these terms are conventionally discussed with
respect to quantitative and positivistic research, we use them for qualitative field work to refer to
the ability to produce similar data from multiple observers, and to produce consensually agreed
upon, corroborated accounts (interpretations and theoretical explanations) of a site, person, or
process. With only slight modification from standard understandings of reliability and validity,
we can deploy these terms quite productively for ethnographic research.

Maxwell (1992), for example, proposes five types of validity for qualitative researchers
that offers a useful advance on the usual discussions of reliability and validity. (1) *Descriptive
validity* refers the factual accuracy of an account, that researchers “are not making up or
distorting things they saw or heard” (1992:45). This is the basis for all other forms of validity
and the foundation on which all subsequent interpretation and analysis builds. As Geertz
(1973:17) put it, “behavior must be attended to, and with some exactness, because it is through
the flow of behavior – or more precisely, social action – that cultural forms find articulation.”
This “reportage” function (Runciman 1983) includes descriptions of specific events and
situations, as well as of objects and spaces. (2) *Interpretive validity* refers to the meanings of the
described behaviors, events, and objects *for the actors observed*, one of the central goals and
most common justifications for qualitative research, especially ethnographic fieldwork.
Interpretive validity seeks to capture the participants’ perspective, providing an account in emic
(actors’ rather than theoretical - etic) terms. Interpretive validity “has no real counterpart in
quantitative-experimental validity typologies… [It] is inherently a matter of inference from the
words and actions of participants in the situations studied… grounded in the language of the
people studied, [and] relying as much as possible on their own words and concepts” (Maxwell
1992:48). The interpretation is the barest level of generalization across the described data: what
do these activities, these things, these relationships mean to the actors? The goal of
interpretation is to describe the actors’ “lay sociology” (Garfinkel 1964) or “theories-in-use” (Argyris and Schon 1978), their understandings of their social world. This criterion of interpretive validity distinguishes a form of accuracy that lies between the first form, descriptive validity, resting entirely on observable, consensually validated data and the more contestable inferences of the third type, theoretical validity. While there is “no in-principle access to data that would unequivocally address threats to [interpretive] validity,” the descriptive accounts serve as warrants. In other words, has the ethnographer/observer provided sufficient evidence to substantiate her claim or interpretation of what these events and actions signify to the actors. Consensus should be achievable within the relevant community of actors and/or readers that the interpretation is supported by the reported descriptive data. The terms – language and concepts - of both descriptive and interpretive validity are, in Geertz’s term, ‘experience-near,’ the local language in use among the actors, although interpretive validity might also involve assessments of the accuracy of informants’ reports (to which we will return below with an example from our fieldwork).

(3) Theoretical validity moves the ethnographic account further from the actors’ behavior, language, meanings, and interpretations to a more abstract account that proposes to explain what has been observed in the terms of the scholarly literature. No longer a matter of what the described activity means to the actors, theoretical validity asks what this activity or group signifies to the scholarly audience. What is this an example of and to what other examples should we compare it? What conceptual label shall we affix to this setting and activity?

“Theoretical validity thus refers to an account’s validity as a theory of some phenomenon” (Maxwell 1992:51). Both the concepts used and the relationships proposed are independently assessed for what is conventionally called construct validity (Bernard 2000:50-51) and
inferential or causal validity (Cook and Campbell 1979), although not all theories proposal to offer causal explanations.

The key distinction between the types of validity (descriptive, interpretive, and theoretical) in this schema lies in the “presence or absence of agreement within the community of inquirers about the descriptive or interpretive terms used. Any challenge to the meaning of the terms, or appropriateness of their application to a given phenomenon, shifts the validity issues from descriptive or interpretive to theoretical” (Maxwell 1992:52).

(4) Generalizability, a fourth form of validity, invokes considerations that are common across the social sciences, referring to “the extent to which the particular situation is representative of a wider population” or set (Hammersley 1992:79). There is, however, a level of analysis issue here concerning generalizability that distinguishes internal from external validity. For generalizability (external validity) beyond the particular group or organization, the qualitative researcher must meet the same standards as any quantitative researcher: demonstrate representativeness. For most qualitative researchers, however, internal generalizability is far more important because there are strong arguments for studying outliers and unique cases as existence proofs and means for identifying variation (Small 2009). For internal validity, however, we need to know whether the reported data (activities, statements, documents) are representative of the activities, statements, materials representations of that particular group or organization whether or not the group or setting is representative of some larger set. We need to avoid ‘cherry picking’ examples that support a claim rather than synthesizing across all the evidence and examples. (5) Maxwell offers a final fifth form of evaluative validity, referring to the normative assessment of that which has been described or explained. This category is
perhaps most appropriate for policy studies and ought not to be intrinsically different in qualitative or quantitative studies.

Although some universities provide excellent training in fieldwork and ethnography, it is neither as common nor consistent as is the preparation in quantitative data collection and various modes of quantitative and statistical analyses. Thus, it is not unusual, for example, to meet a graduate student eager to begin fieldwork on a subject about which he has read a great deal and knows a range of scholarly positions and arguments, but does not know how to take the first step in formulating a research design, identifying a field site, or gaining access. Or, one encounters more advanced graduate students about to complete their dissertations based on extensive fieldwork who have never had training in qualitative data analysis, urgently seeking advice about how to make interpretive and theoretical inferences from their data. The conventional pattern of training is most often through one on one mentoring although we may achieve more successful, as well as efficient, training if we work with students in groups. In addition, training students in groups would work to challenge the fetish of ethnographic research as a personal immersion and form of creative discovery.

The “Governing Green Labs” project began in 2002 when Professor Susan Silbey initiated a study of the development and implementation of an environmental, health and safety management system at Eastern University, a large research university in the United States. While much research tries to determine if regulation works, and whether it is cost effective, too few studies have looked at the ground level - inside the organizations, at the shop floor level - to trace the behavioral and cognitive threads between the routines of daily work and myriad regulations. Most studies look at the macro level of organizations, tracing variations, for example, in emissions, air quality, volumes of waste disposal, costs and benefits for the organization
conceived as a single entity or actor. Rarely have studies looked at the organizational and cultural conditions that create empirically observed compliance or resistance to regulation within the work routines that aggregate to constitute the organization (Gray and Silbey 2014). By observing the invention of a new management system from day one of the commitment to create such a system, its implementation, and dissemination across the University, the research sought to unpack the black box of regulatory performance. By mapping the ways in which local organizational processes and sub-cultures produce environmental, health and safety practices and create, where it might exist, responsive regulation (Ayres & Braithwaite, 1992), the research hoped to discover the conditions for sustainable improvement in environmental conditions. Alternatively, the research sought to understand how good intentions may nonetheless produce unwanted or unanticipated outcomes and not improved environmental sustainability. The research sought to understand how diverse institutional resources and constraints influenced, if they did, organizational performance - in this case, performances mandated by law. Through participant observation, interviewing, and inductive analysis, Silbey intended to capture the variations in interpretation and consciousness of legal regulations that are sedimented in and through organizational cultures (Silbey 1992; Ewick and Silbey 1998, Edelman and Suchman 1997). “Rather than rest on either side of an unfruitful divide (individual person v organization as actor),” we looked for “the sources of variation in organizational governance” that may derive, for example from “the hierarchical structure and distribution of resources (i.e. autonomy, expertise, experience), and thus bridge the gap between an individualist, micro account of organizational failure as propounded in the bad actor models and macro accounts of the organization qua organization as the actor or unit of analysis” (Gray and Silbey 2014), and in the process describe the production of green laboratories.
The project included Ph.D. students from four interdisciplinary graduate programs at MIT where Silbey is a member of the faculty: Urban Studies and Planning in the School of Architecture and Planning; History and Anthropology of Science, Technology and Society in the School of Humanities, Arts and Social Sciences; Behavior and Policy Studies in the Sloan School of Management; and Technology and Public Policy in the School of Engineering. The diversity of students’ background knowledge, training programs, and distinct research interests was simultaneously exciting and challenging. Depending on their interests, students were assigned to collect data via participant-observation and interviews within a particular group, or department in the organization: senior administrators, department managers, legal-technical specialists, and laboratory researchers. Extending over ten years, the project involved two generations of students. Beginning in the spring of 2003, the research group met every Wednesday afternoon during the summer and Friday mornings during the term to exchange information and begin to piece together a complicated field site. Students were asked to send in weekly field notes. Selections of these field notes were discussed collectively and coded during team meetings. This routine helped aspiring ethnographers develop a discipline that, as we describe below, improved the descriptive, interpretive, and internal validity of the data. At the outset, the single most notable consequence of the group activity was its success in overcoming individual reticence about sharing one’s experiences, appearing inadequate to the task or displaying ignorance.

*Descriptive validity.* Researchers exchanged field notes, reading and discussing not only the substantive content of the notes but critiquing the quality of the text of the notes. Was this thick description? What assumptions were embedded in particular words and concepts? What needed to be known to understand a reference? Were phenomena and actions situated locally
as well as in relation to alternatives not present or possibly seen elsewhere? What was missing? Was the observer noticing silences and omissions? Was the researcher’s focus too close or too distant? Most of these conversations concerned how the ethnographer could know or apprehended what was being described, striking out statements that were impressionistic rather than empirically observable, shifting linguistic terms to stay as close to the native categories and terms as possible and offering local definitions where necessary. Through these discussions, the ethnographers in training were challenged to write detailed descriptions of the scene, sometimes attending to the material conditions, the aural and aromatic sensations, and at other times focusing on language and affect. Sometimes two or more researchers attended the same meeting, learned of the same laboratory accident or injury from different sources, and heard the same gossip circulating from one group to another. Field notes were compared, discrepancies and omissions discussed along with emerging commonalities. These discussions were an opportunity to learn the discipline of constructing an empirical account and testing its descriptive validity against other empirical accounts and multiple forms of data (e.g. looking for paper records, disseminated memos, email trails). Field workers also became aware of their tendencies to report some types of data (e.g. conflict or non-compliance) and gloss over other observations (e.g. agreement or compliance) and were able to correct these tendencies, taking in the full picture, over time. Descriptions generated more questions than answers, pushing the researchers in diverse directions to seek out more informants, to visit archives and organizational records, and to search the scholarly literature for comparisons and variations. This ongoing reflexive engagement about what could be known informed the practice of collecting data immediately and continuously improved the descriptive validity of the data.
Interpretive validity. While the accounts that our subjects gave were taken at face value, scenes were triangulated to extract a richer empirical reconstruction that often demanded that participants’ divergent accounts be explained and reconciled. The data collected in one lab or one series of meetings was put in the context of other labs and meetings. For example, at different meetings with different groups, fieldworkers heard stories that the Dean of Science had closed a laboratory because of failures to meet environmental, health and safety regulations, in other words, because the lab had consistently ‘failed’ inspections. We collected all the different accounts heard by different researchers and assessed the ways in which they differed and collided. Together these accounts told us about how the actors understood the authority of the Dean, the limits of academic freedom, and place of law in the organization. However, it was also important to understand whether, in fact, the Dean had indeed closed a laboratory for failing an inspection. We pursued these two avenues collectively, and in parallel; that is, we collected all the stories and noted in what situations they were narrated; we also traced the story back to the actor’s in the stories. In doing so we became sensitive to the differences between descriptive and interpretative validity but also the analytical opportunities offered by our recognition of the discrepancies in the stories.

Rookie ethnographers often face an overwhelming amount of stimuli in the field. When a researcher enters the field, much is different from what was expected. We are not blank slates; we are, after all, competent mature social actors and come with an abundance of tacit as well as explicit assumptions about how the world works. We are social scientists after all and have read a great deal about the site and phenomena we are studying. We deal simultaneously with our interpretations and our subjects’ interpretations. Although our initial naïve interpretations must give way to the data and analysis process, they can be stubborn and overshadow those of
our subjects. The continuous discussion about our subjects’ interpretations helped to excavate and subsequently submerge our own perspectives, becoming more open to local understandings, as well as to the need to triangulate and validate empirical observations. Every ethnographer must do this to succeed; the group process seemed to accelerate the development of this fieldwork skill.

As it turned out, while previously serving as a Department Head, the Dean had shut down a laboratory. It was not, however, in response to safety or environmental violations but because the principal investigator – the professor – had overspent his research accounts and had not responded to requests to reign in his spending. Nonetheless, an apocryphal account that miscreant laboratories could be shut down was circulating as a warning to lab managers and scientists. The message was clear: This Dean and these new regulations carried consequences should they become lax and inattentive to the safety requirements. At the same time as we were able to record the circulating stories and track the accurate historical event, we were discovering the hierarchy of organizational concerns. Although creating safer and cleaner laboratories was a high priority at the university, garnering abundant resources, financial and scientific misconduct called forth more immediate and serious sanctions: shutting down a scientist’s laboratory. This story, what is sometimes referred to as an atrocity tale (Best 1990), allowed us to locate the various regulatory regimes and organizational practices within an overarching account of the university’s values and enacted priorities. We could offer a more accurate interpretation of the story for the various and differentially located actors.

Theoretical validity. Newcomers were always presented with an overview of the project, a history of what had been done to date, an opportunity to explore published and unpublished accounts, and initial questions to consider for their own work, questions posed in the form of
“What is this site or activity a case of?” For instance, initial questions centered on the role of monitoring systems in organizing working relations and the effects of professional authority and autonomy on responses to regulation. The project also included a database with relevant readings on science, safety, and knowledge sharing in organizations. This initial scoping helped individual researchers focus on theoretically relevant categories of interest. Discussions around “What is this site a case of?” provided an array of topics to investigate. The questions and associated readings allowed researchers in the team to focus observations, see data in a broader context and build on contemporary debates in sociology, legal, science and organizational studies. While researchers eventually deviated from these orienting questions as they developed their own interests, the students were provided with an initial heuristic with which to engage conceptually meaningful categories. Theoretical validity can be challenging when members of the team are drawing from diverse literatures and are unfamiliar with the theoretical debates and advances in a field. However, exploring shared yet diverse avenues of inquiry and attempts to theorize the data together helped the research team to discard theoretical preoccupations that were valued in some disciplines and fields but were not salient in the field site. Textual coding of fieldnotes and interview transcripts was done on a continuous basis and early cohorts of field workers did this collectively. However, because each team member came with different theoretical background and interests, planning to produce a scholarly work relevant for the field in which he was seeking a degree, attempts to create common conceptual codes was a struggle and was eventually abandoned.

Because the team included members from diverse disciplines and professional communities, the theoretical analyses addressed multiple phenomena and theoretical questions, for example, concerning organizational change and regulatory compliance; disciplinary
variations in doing science; institutional and normative ordering, and produced theories of newly discovered phenomena, e.g. relational regulation (Silbey 2011; Huising and Silbey 2011) and role performance, e.g. sociological citizen (Coslovsky, Huising and Silbey 2009).

External and Internal Generalizability. The team project is the most effective means of dealing with questions of external and internal validity. Obviously, multi-sited ethnography can produce accounts that apply across a larger population, offering generalizability that one site cannot. More often than not, research teams offer more extensive and deeper multi-sited ethnography than can be produced by one field worker (Marcus, 1995). So, for example, Burawoy (2000) collated studies conducted by his students of the responses of individuals and organizations to processes of globalization. In our study of the development and implementation of a management system for laboratory hazards, we did not seek external generalizability through multiple organizational sites, although several students worked in laboratories in more than one university. Rather, the overall project was an in-depth case study of one university, over time tracing the processes of organizational change as enacted by participants across all levels of the organization and variations in departments and disciplines. Without question, working as a research team enhanced the internal validity because data was shared across diverse analysts and because analysis proceeded systematically by multiple researchers. There could be no purposive selection of evidence that was not corroborated across organizational locations and across observers.

Challenges of Team Ethnography. One essential, practical challenge for team ethnography is the need to balance data sharing and individual contributions. Although students were invited into the project to address questions Silbey had identified in her research proposals, students in the project were asked to write their own papers, allowing them to
develop their own theoretical and empirical interests. For instance, while the project had an initial emphasis on how regulation filters through large and complex organizations, fieldworkers developed other themes such as how professional and occupational struggles shape regulatory compliance or how experimental materials shaped laboratory practice. The theme of safety regulation functioned as a lens revealing other organizational dynamics and by this expansion of focus enabled multiple contributions.

Early writings and attempts at theorizing also functioned as a heuristic for later contributors. New observations and interpretations were compared to existing works and created an impetus to build on extant interpretations. For instance, while early works focused on the role of EHS personnel in mediating regulatory compliance (Huising and Silbey 2011, 2103, Huising 2014a, 2014b), later works explored how ground level personnel (researchers and technicians) creatively recombined legal rules with local practice (Evans and Silbey, 2014), and struggled with moral dilemmas engendered by research strategies (Evans 2014). Some papers pursued lines of analysis related to the use of technologies to audit regulatory compliance (Silbey and Agrawal 2011). Others considered the place of law in the laboratory (Silbey and Cavicchi, 2005), and controversies in science (Silbey and Roosth, 2008).

Conclusion

Contemporary scholarly norms limit considerably the time frame within which researchers can collect, analyze and publish their results. Professional expectations for a greater volume of publication as the standard of respectable accomplishment, coupled with the increasing importance of journal articles rather than book-length manuscripts, mitigate against the traditional practices of ethnographic research: one scholar totally immersed in one location
for a long period of time. In this new hyper-productive scholarly universe, working in teams becomes ever more attractive and thus group ethnography a more interesting research option. In a sense, transforming ethnography from a personal exploration to a research collaboration mimics the evolution of scientific research practices which were the subject of this project. Contemporary experimental science is almost exclusively a collaborative group effort. Not only is team ethnography more efficiently productive, but as we indicated above, it offers increased validity across all dimensions – descriptive, interpretive, theoretical and generalizability.

For those interested in studying organizational processes, especially complex processes and distributed organizations characteristic of our historical time, a fieldwork team allows deep, spatially and temporally extended involvement to study not only the entire organization from top to bottom but often from the beginning to the end of a project or organizational change. Team work permits members to enter and exit, be replaced or supplemented to create continuity where one or few researchers could not.

The lasting value of many ethnographic works has relied primarily in the author’s ability to convey with clarity and nuance and make familiar the everyday experience of unfamiliar groups and cultures. As Alexander (1989) notes: “Because [social science’s] object is life, it depends on the [social] scientist’s own ability to understand life. It depends on idiosyncratic abilities to experience, to understand and to know.” Alexander (1989) suggests two ways in which this knowledge distinguishes itself: through the interpretation of states of mind and through the reconstruction of the empirical world. Although ethnographers may bring unique sensitivities to their fieldwork, the skills needed to create detailed, subtle observation and interpretation and for the careful reconstruction of raw and disorderly empirical worlds can be
made explicit and developed more effectively through collaborative strategies. Rather than being idiosyncratic, it can be a shared set of highly skilled techniques as well as developed sensibilities. Team ethnography allows for this collective construction of sociologically meaningful categories of interest, the development of more complex and complete accounts, and ultimately the transformation of a personal understanding of a field to generalizable social science.

Figure 1: Collecting, analyzing and theorizing accounts through team ethnography
Table 1: Examples of team ethnographies

<table>
<thead>
<tr>
<th>Reference</th>
<th>Research context/phenomena</th>
<th>Types of group allocation and engagement with research context/phenomena</th>
<th>Outcomes/results of methodological approach</th>
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</table>
| Burawoy (2000)             | Globalization             | Multiple industries  
Multiple organizations  
Multiple roles within and across organizations  
Collection of independent student projects                                                                                     | Diverse examples across varied sites to develop a general theory of the implications of globalization on work, communities, and organizations.                                                                                               |
| Stark (2009)               | Creative Work             | Multiple industries  
Multiple organizations  
Multiple roles within and across organizations  
Three ethnographies of different organizations engaging in creative work                                                       | Diverse examples across varied sites to develop a general theory of the role of competing accounts of worth on creative work                                                                                                                                   |
| Barley (1996)              | Technical Work            | Multiple industries  
Multiple organizations  
Same role in different industries and organizations  
Nine mini-ethnographies of different technician’s Occupations                                                                         | Large sample of same role across varied sites to develop general theory of technician’s work                                                                                                           |
Multiple clinics  
Three types of medical protocols (clinical practice guidelines, rules for the conduct of research, and governance protocols)  
Ethnographies in matched AIDS clinics in the US, Uganda, Africa, and Thailand.                                                             | International sampling allows for institutional comparison of the intersection of law and medicine.                                                                                                                                                               |
| Bearman (2009)             | Unique occupation: Doormen in New York City | Single industry (real estate)  
Multiple organizations of same type (housing)  
Same role in different organizations within single industry  
Sample of dispersed members of an occupation in a large city | Large sample of same role across similar organizations to develop in-depth, detailed account of a role, an occupation                                                                                         |
| Becker, Greer, Hughes and Strauss (1961) | Medical education and practice | Single industry (medicine)  
Single typical organization  
Same role at different stages of professional development  
Engaged full organizational population, no sampling                                                                                      | Efficiently observed four year process in one year, holding historical context constant; in-depth account of a single institution (medical education); proposed general model of a social process (professional socialization).                                                                 |
| Gouldner (1954) | Organization of industrial production | Single industry (mining)  
Single typical organization  
Multiple roles across single organization  
Engaged full organizational population | Able to observe entire organization over time to map change processes within limited historical frame; in-depth account of a common phenomenon (bureaucratic organization); proposed general model of the phenomenon. |
|-----------------|----------------------------------------|-------------------------------------------------------------------------------|
| Huising and Silbey (2011) | Risk management  
Regulatory compliance  
Scientific practices | Single industry (university research science)  
Single typical organization  
Multiple roles across single organization  
Engaged across full organizational hierarchy, stratified sample of disciplines/departments, and random sample of labs within disciplines/departments | Able to observe entire organization over time to map change processes within limited historical frame; in-depth accounts and models of multiple phenomena (organizational change; regulatory compliance; disciplinary variations in doing science; institutional and normative ordering; proposed model of newly discovered phenomena (relational regulation) and role performance (sociological citizen). |
References:


