Elaborating analytic ethnography
Linking fieldwork and theory

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Abstract
Among the diverse styles of qualitative methodology is what John Lofland referred to as ‘analytic ethnography.’ In contrast to the traditional interpretive style that attempts to get at the crux of ‘what is going on’, to the more formal approach that seeks to identify the cognitive rules undergirding behavior, and to the more recent postmodern preoccupation with individual experience and voices, analytic ethnography seeks to develop systematic and generic understandings and propositions about social processes. In this article we elaborate analytic ethnography beyond Lofland’s original statement by articulating how it can contribute to theoretical development through conceptual refinement and theoretical extension as well as through the more traditional development of grounded theory. We discuss and illustrate these three strategies for theoretical development by drawing on various field studies, including our own, and by suggesting the conditions propitious to each strategy.

Keywords
Theory, conceptual development, analytic induction, extension, refinement, ethnography
Our objective in this article is to elaborate what John Lofland (1995) christened ‘analytic ethnography’ by articulating how it can contribute to theoretical development through refinement and extension as well as through the more traditional development of grounded theory. Analytic ethnography seeks to produce systematic and generic propositions about social processes and organization through attention to a number of overlapping research principles. Lofland’s initial codification of this style of ethnography focused attention on the means or methodological stratagems and dictums facilitating the elaboration of generic understandings and propositions, but said little about this objective insofar as it relates to theoretical development. We focus specifically on that issue in this article.

This effort is prompted not only by the insufficient attention given to theoretical development in Lofland’s statement, but by two criticisms that have persistently dogged ethnographic research. The first concerns the tendency for ethnographers, with but a few exceptions (e.g. Glaser and Strauss, 1967; Comaroff and Comaroff, 1992; Burawoy, 1998), to neglect the theoretical relevance and potential of research; the second bemoans the relative dearth of systematic procedures for analyzing field data in a fashion that facilitates theoretical elaboration across sites. We believe that Lofland addresses the second criticism more successfully than the first; so, in this article, we seek to elaborate a number of ways that enhance the theoretical relevance and potential of ethnographic research. We think there are a number of warrants for doing so. One is that ethnography and theory are mutually informative in that theory focuses and sharpens ethnography while ethnography grounds theory in the richness of social life (Prus, 1996). A second warrant is that theoretically-engaged ethnography facilitates dialogue across fields and methods by providing a trans-situational language. Third, theoretically engaged ethnography can also promote ethnographically-based contributions to policy or intervention. In the pages that follow, we elaborate three ways through which theoretical development can occur via ethnographic/qualitative research. Before doing so we discuss the sources of this neglect within the ethnographic enterprise.

Sources of analytic neglect

The tendency among ethnographers to neglect the theoretical relevance and potential of their research projects is rooted, somewhat ironically, in the traditional objectives and rationales for taking to the field in the first place. The first and most widely articulated traditional objective is to discern, grasp, and understand the world at hand from the standpoint of its members or practitioners; to acquire an insider’s view so that, in the words of Geertz (1973: 6), one can distinguish between a wink and a twitch. Discernment
of the webs of meaning and significance that characterize and bound one
social world from another is not regarded as the most salient research object-
by all ethnographers, however. For some, the central objective is to
uncover the structure of rules that enable one to behave as a member, irre-
spective of the meanings attached to the repertoire of associated behaviors.
This second central objective is articulated most forcefully by Goodenough's
(1956) directive that the proper aim of fieldwork should be to describe what
one needs to know in order to ‘pass’ as a member of the world they are
studying.

Although the traditional pursuit of either an insider's perspective or the
rules for passing clearly facilitates understanding of the relevant contexts,
it seldom beckons others to assess or extend theoretically the resultant
findings or claims. There are a number of reasons for this neglect, including
becoming too close to one's informants, side-stepping relevant theory in
favor of accenting popular assumptions about one's informants (Wacquant,
2002: 1523), or making ‘known previously unknown or misknown social
worlds’ the primary objective (Emerson, 1987: 72–3).

Readers may nod and acknowledge the intuitive interest of an ethno-
graphic report inasmuch as it reveals aspects of a social world that most of
us are not privy to or with which we are unfamiliar. But such acknowl-
edgements are also often followed by dismissive statements such as, ‘Inter-
esting, but so what?’ ‘Where do we go from here?’ ‘What's the theoretical
question here?’ or ‘Nice read, but no theoretical implications.’ In other
words, although ethnographic reports founded on one or the other of the
above objectives may be empirically grounded and informative, they too
rarely generate compelling theoretical formulations or link clearly and
cleanly to extant theory. In acknowledgement of this shortcoming in a 1987
special issue of the Journal of Contemporary Ethnography on ‘The Past and
Future of Ethnography,’ Emerson, a former editor of the journal, bemoaned
the lack of attention given to theoretical issues and development within
ethnography. Similarly, Hammersly has noted, in a book-length critical
ey on the problems with ethnography, 'while there is no shortage of
promising theoretical ideas in ethnographic and historical work, there are
few examples of explicit and sustained attempts at theorizing . . . ' (1992:
39). And most recently, Wacquant laments the 'problematic relationship
between theory and observation' in contemporary urban ethnography on

We are not arguing that ethnographic inquiry is ever truly devoid of
theoretical assumptions, but too often there is a lack of awareness of these
assumptions and their implications. Nor are we arguing that ethnographic
reports that highlight detailed accounts of local social contexts should be
dismissed as having little intrinsic value. On the contrary, such accounts can
be useful in their own right for theoreticians, practitioners, and policy
makers who need rich, detailed knowledge of people’s activities at particular places and times. We believe, however, that ethnographers too often enter the field with only the goals of description and interpretation to guide them, treating theoretical development as a black box or ignoring it altogether, and forgetting that much of scientific and humanistic scholarship is concerned with the relationships between particularistic accounts and more general understandings of the world around us.¹

The theoretical black box to which we allude may be encased in an even larger black box in the ethnographic tradition that lies somewhere between what Van Maanen (1990) calls the first ‘moment’ of ethnography, getting started and data collection, and its second ‘moment,’ the construction of ethnographic text. In this interstice are the practices that ethnographers employ in order to transform their field data into narratives. A rather extensive and fairly coherent set of anthropological and sociological discussions exist for the first moment of ethnography, including conceptualization of the field, directives for getting started, negotiating access, developing and maintaining fieldwork roles, writing field notes, and even leaving the field (e.g. Wax, 1971; Snow et al., 1986; Adler and Adler, 1987; Emerson et al., 1995; Lofland and Lofland, 1995; Gupta and Ferguson, 1997; Weber, 2001). A critical literature on writing up qualitative fieldwork has also developed, calling into question and, in some cases, offering alternative literary strategies for presenting ethnographic text (e.g. Clifford and Marcus, 1986; Van Maanen, 1988; Denzin, 1992).

But often neglected is the analytic moment or phase of the research process. With the exception of a handful of field researchers (e.g. Glaser and Strauss, 1967; Miles and Huberman, 1984; Comaroff and Comaroff, 1992; Burawoy, 1998), many ethnographers either gloss over the practices and procedures of analysis or fail to articulate them in their final reports. This is unfortunate in two respects: it generates the impression that qualitative analysis is a haphazard enterprise; and it undermines the prospect of theoretical development, since it is in this moment of qualitative research that such developments will most likely occur, if at all. That some level of analysis takes place is clear, but how ethnographers do it across the board remains vague and relatively unarticulated. As one well-regarded fieldworker reported to the first author when queried about how he coded his field data: ‘Oh, I just start looking through my notes for a pattern. And once I find one, then I use that as the needle to thread other things together.’ We suspect that the rather haphazard, fortuitous process referred to here is commonplace rather than atypical.

It is interesting to note that Lofland (1970) referred to this analytic black box three and a half decades ago in an article entitled ‘Interactionist Imagery and Analytic Interruptus.’ In this article, Lofland argued that interactionist sociology in general, and the work that flows from field research more
specifically, is ‘conceptually impoverished’ (p. 37). Furthermore, this impoverishment, Lofland argued, is due in large part to ‘analytic interruptus’ (pp. 35, 42). That is, field researchers do not follow through on the analytic tasks they set out to perform during and in the aftermath of fieldwork and thus they do not really engage in the time-consuming, detailed data analysis required for conceptual development. We agree with Lofland that ‘analytic interruptus’ fittingly describes a part of what ails qualitative field research, at least in US sociology. But what is the primary road to conceptual fertilization and theoretical development? Lofland suggests that the creation of concepts that abstractly capture and typologize social life is a necessary condition. We have no quarrel with this directive in some research contexts and for some purposes, but we do not regard it as sufficient for theoretical development in general. Instead, ethnographers need to be oriented toward larger theoretical concerns from the outset of their projects by, at the very least, being sensitive to the range of theoretical relevancies of their orienting research questions and of the alternative paths through which those questions might be linked to theoretical development. We specify three of these paths below in the context of ethnographic qualitative research.

We use the notion of theoretical ‘development’ to connote the dynamic processes by which theories emerge, change, and grow in scholarly work. But what do we mean by ‘theory’? Consensus does not exist about the nature of theory, but definitions of theory encompass one or more of these four basic elements:

1. a set of logically interrelated propositions;
2. an openness to subjecting propositions to empirical assessment and falsification;
3. a focus on making empirical events meaningful via conceptualization; and
4. a discourse that facilitates explanation of empirical events.

Beyond these four fundamental elements, theories also provide links between empirical studies and allow researchers to interpret the larger meaning of their findings for themselves and others (e.g. Hoover, 1980). Although theory can constrain meaningful dialogue by obfuscation, at its best, theory provides a common language through which discourse across subfields, problems, and levels of abstraction can occur. A particularly useful distinction along these lines is that between formal and substantive theory. Substantive theory is theory developed for particular empirical domains of inquiry, such as juvenile delinquency, medical education, hospital organizations, or dying. The goal of substantive theory is to illuminate and explain the phenomenological aspects of distinct situations, processes, or contexts. In contrast, formal theory focuses on ideal-typical domains of inquiry, such as deviant behavior, socialization, formal
organization, or status passage (e.g. Glaser and Strauss, 1967: 32). The goal of formal theory is to abstract the more common or universalistic features of situations, processes, and contexts for more fundamental theoretical discourse. With these ideas in mind, we turn to an elaboration of three paths to theoretical development.

Theoretical discovery

In the context of sociological ethnographic research, the most well-known form of theoretical development is ‘discovery,’ a term that takes on different meanings depending on one’s methodological strategies and analytic bent. Lofland (1970: 38) cites Goffman’s taxonomies and mini-concepts as pointing toward a key analytic task for ethnographic work, arguing that ‘Goffman has been to Chicago interactionism what Merton was to Harvard functionalism’ in that he ‘propounded many “middle range” concepts’ (1970: 38) that provide a partial analytic or theoretical framework for understanding social interaction. Lofland advocates for an exploratory, inductive approach to discovery with the goal of developing ‘mini-concepts’ and generic propositions through detailed coding and emergent constant comparative analysis of observational data. Such an approach captures the most important element of Glaser and Strauss’ *The Discovery of Grounded Theory* (1967): the crescive interpenetration of data and analysis. Since all ethnographers inevitably bring both conscious and unconscious assumptions and interests with them to their research, the development of taxonomies and concepts does not occur in vacuo, but analytic understandings are discovered in the sense that they emerge in large part from detailed examination of observational fieldnotes, and are then tested and revised in a constant comparative process.

The Discovery of Grounded Theory (1967) and its subsequent elaborations (Glaser, 1978; Strauss, 1987; Strauss and Corbin, 1990) have provided a handy rationale for doing qualitative field work, so much so that it also has some currency in quantitative circles. At the same time, the notion that ethnography is primarily a path toward theoretical discovery has blinded ethnographers to alternative forms of theoretical development.

We also suspect that very few ethnographers actually engage in theoretical discovery in the systematic manner Glaser and Strauss had in mind, and in a fashion Charmaz (2001) has clarified. This failure, combined with the widely-held assumption that theoretical development derives solely from the deductive application of theory, and the fact that all research is embedded in a theoretical substrate of some kind, has led some scholars to dismiss the idea of generating grounded theory. Yet both anthropological and sociological ethnographers have long recognized the importance of openness, or
what Ruth Benedict (1948: 591) referred to as ‘surrender,’ in which the researcher allows conditions of the field or interests of the informants to guide foci and methods of investigation, and many exemplary ethnographic works are known more for their seminal theoretical contributions than for their particularistic findings. Certainly in Asylums (1961), Goffman generated highly influential theoretical formulations regarding ‘total institutions’ and organizational social control and resistance. His sociological conceptualization of total institutions provided a striking alternative to the medical paradigm on which official rhetoric and practice of mental health therapy were based. Further, the mini-concepts he developed here— including (among others) sociological conceptualizations of ‘underlife,’ ‘mortification processes,’ ‘moral careers,’ and ‘secondary adjustments’— have since been widely extended to other empirical domains.

One can point as well to other classic ethnographic field studies in anthropology and sociology that can be said to have generated significantly new theoretical approaches or sensitivities. Included in such works might be Malinowski’s Argonauts of the Western Pacific for the functional study of culture and economy (1961[1922]), Bateson’s Naven for the study of interpersonal and ritualized communication (1958[1936]), and Whyte’s Street Corner Society for the study of stratification, mobility, and social organization at the level of the neighborhood or community (1973[1943]).

In each of these cases certain concepts and theoretical principles are stimulated by or emerge from ethnographic observations rather than being imported or derived from extant theory, which cuts to the core of theoretical discovery. However, we believe that most ethnography oriented toward theoretical development takes two rather different paths than the one leading to discovery.

**Theoretical extension**

One such avenue is what we call ‘theoretical extension.’ In this process, one does not discover or develop new theory per se, but extends pre-existing theoretical or conceptual formulations to other groups or aggregations, to other bounded contexts or places, or to other sociocultural domains. As such, theoretical extension focuses on broadening the relevance of a particular concept or theoretical system to a range of empirical contexts other than those in which they were first developed or intended to be used. In this sense, theoretical extension preeminently involves the ‘transferability’ of theory between at least two contexts (Lincoln and Guba, 1985: 124).

Simmel’s formal sociology helps further elaborate theoretical extension. Formal sociology, as Simmel defined it, is the identification and elaboration
of trans-situational social forms. As Simmel explains in discussing the logic and conduct of formal sociology:

It . . . proceeds like grammar, which isolates the pure forms of language from their contents through which these forms, nevertheless, come to life. In a comparable manner, social groups which are the most diverse imaginable in purpose and general significance, may nevertheless show identical forms of behavior toward one another on the part of individual members. We find superiority and subordination, competition, division of labor, formation of parties, representation, inner solidarity coupled with exclusiveness toward the outside, and innumerable similar features in the state, in a religious community, in a band of conspirators, in an economic association, in an art school, in the family. However diverse the interests are that give rise to these sociations, the forms in which the interests are realized may yet be identical. (1950: 22; italics in original)

Thus, attention shifts from the search for factual novelties and the peculiarity of actual events to trans-situational patterns; from the concrete, to the search for formal patterns across a multiplicity of situations or contexts (Zerubavel, 1980). Simmel, in turn, focused on three kinds of social forms: social processes, social types, and developmental patterns. The two of greatest concern, or at least most accessible for qualitative researchers, are social processes and types. Social processes refer to relatively stable configurations of social interaction, whereas for social types, attention is shifted to the typical or modal characteristics of persons when engaging in various social processes. Examples drawn from both sociological and anthropological ethnography help to illuminate theoretical extension involving both social forms and social types.

As an explicit approximation of theoretical extension involving social forms, consider Morrill's (1995) ethnography of conflict among top managers in private American corporations, especially his work on 'vengeance.' From the 1940s onward, anthropological fieldworkers identified vengeance as a relatively stable social form that organizes disputing and competition among neighboring fraternal groups in a variety of horticultural and preliterate societies located in Eastern and Mediterranean Europe, the South Pacific, and parts of Africa (Roberts, 1979). Perhaps the most distinguishing characteristic of vengeance, as a social form, is 'reciprocal aggression' (Black, 1990: 44) regulated by a code of honor that 'specifies the rules of challenge and riposte, including when, where, and with whom' such aggression should occur (Morrill, 1995: 195). Although the contexts in which Morrill conducted his fieldwork ostensibly appear very different from those in which the initial fieldwork and theoretical formulations on vengeance occurred, Morrill identified similar interpersonal and intergroup dynamics that organized meetings and other formal gatherings
among high-level corporate managers. Although managers hurled invectives instead of spears or firing guns (he did record some instances of physical violence), the essential form of their interactions mirrored what had been studied in the traditional locales associated by anthropologists with vengeance. Moreover, Morrill found that some of the same social conditions that anthropologists argue facilitate vengeance in horticultural and preliterate societies – namely, relatively equal status groups with strong internal solidarity and weak external ties, plus a lack of authoritative third parties who can legislate settlements – appear in corporate contexts where vengeance occurs. Thus, although the specific contents of the actions observed are different, their social forms are quite similar.

Moore (1978) offers another explicit example of theoretical extension in which she transferred a concept developed from her own fieldwork to another field site. The concept in question is the ‘semi-autonomous social field,’ which Moore argues is an appropriate and useful construct for the study of normative regulation in ongoing social life. Moore first developed the concept of the semi-autonomous social field based on two years of fieldwork in the late 1960s among the Chagga, a Swahili-speaking ethnic group living at the base of Mt Kilamanjaro in Tanzania. In many ways, notes Moore, the Chagga were a ‘model of successful development’ (p. 65): at the time of her fieldwork, they were considered the ‘most prosperous and worldly tribe’ (p. 65) in Tanzania, due in no small part to their sales on the world market of the coffee grown in their collective farms. Although the Chagga had been proselytized by Catholic and Protestant missionaries for 80 years, and had experienced a long history of externally imposed formal laws and rules (including the official abolition of landed private property and tribal chieftainships by the Tanzanian state in the early 1970s), they retained local kin-based ‘bounded units’ (Moore, 1978: 71) that encompassed several sets of families and community groups in which senior male members mediated disputes involving property, family, and neighborhood matters. To outsiders, the Chagga did indeed seem to approximate model socialist citizens. To insiders, the Chagga continued to use informal rules and regulations that had endured over time (with various modifications), thus retaining a measure of autonomy even in the face of dramatic shifts in macro institutions (especially the shift from colonial to state socialist law).

In the 1970s, Moore extended the concept of the semi-autonomous social field cross-culturally to characterize the internal normative relations in the garment industry in New York City devoted to producing the ‘better dress line’ for women (1978: 59). Moore argued that this niche of the garment industry – despite its regulation by state and municipal authorities, as well as its impingement by national unions – nonetheless retained a set of informal, sometimes illegal, rules and obligations that regulated the core actors in the field: designers and contractors (who actually make the
dresses). Despite the dramatic differences in context, Moore contends that semi-autonomous social fields (i.e. the New York high-end garment industry and the Chagga) are able to preserve their internal relative independence by remaining outwardly committed to and flexible vis-a-vis broader institutional arrangements, yet resistant to those same external forces.

The previous examples illustrate the extension of theoretical perspectives and concepts across vast cultural and social distances. One need not travel so far, theoretically or geographically, to engage in theoretical extension. Hunt and Benford (1994), for example, extended theoretical work that had originally been developed to analyze the interactional bases of personal identity to understand how talk among members of the American peace movement during the 1980s generated collective identities. Ponticelli (1999), likewise, extends theories of religious conversion to understand how lesbians are 'converted' from their homosexual lifestyles to 'straight' orientations.

Theoretical extension involving social types appears to be less prevalent in the ethnographic literature than that involving social forms. A recent, albeit less explicit, example of theoretical extension involving social types comes from Duneier's (1999) ethnography of urban street vendors. Early on in his fieldwork, he encountered a street vendor who claimed to be a 'public character' - a previously identified social type associated with some urban settings. Jane Jacobs first introduced this concept into urban ethnography in her work on the quality and aesthetics of urban social life. Jacobs (1961) argued that public characters, who typically are thought to be middle class, facilitate social order in urban settings through their interactions with a wide variety of individuals, their knowledge of the informal, public 'news' of the neighborhood, and their vigilance regarding public crimes (especially vandalism) and other types of deviance. As a result, the social structure and culture of public urban life turns, in no small part, on the activities of public characters. Duneier was surprised to find that one of his informants, a street vendor, considered himself a public character. From the perspective of police and not a few residents, street vendors were an eyesore, if not a potential source of disorder. As his fieldwork proceeded, he came to take the street vendor's claim seriously as he witnessed the public character activities in which his principal informant engaged. He ultimately used the concept of public character to represent and analyze the street vendor's role on the street he studied and extended the concept to other vendors whose activities seemed to fit the role.

In all of these examples, the logic of theoretical extension is consistent with the Simmelian project with regard to a focus on forms rather than particular contents. The basic idea is to explore the possibility of transferring concepts and theory representing social forms or types across diverse contexts. Among other things, such extension enables us to examine closely the conditions under which particular forms and types manifest themselves.
Theoretical refinement

A third avenue to theoretical development is ‘theoretical refinement.’ This refers to the modification of existing theoretical perspectives through extension or through the close inspection of a particular proposition with new case material. This could be considered as overlapping with analytic induction or negative case analysis inasmuch as the theory is modified on the basis of new evidence. Again, this can occur independent of theoretical extension or in conjunction with it.

Burawoy’s ‘extended case method’ (Burawoy et al., 1991; Burawoy, 1998), or at least its application, can be construed as a prototypical example of theoretical refinement via extension. Refinement is accomplished through a sequence of extension processes – intervention (from the observer to the participant), process (over space and time), and structuration (from process to force) – eventually resulting in the reconstruction of extant theory. The objective is not the discovery of grounded theory, Burawoy writes (1998: 16), but the elaboration of ‘existing theory,’ adding that ‘(w)e do not worry about the uniqueness of our case since we are not as interested in its “representativeness” as its contribution to “restructuring” theory.’ For purposes of illustration, he revisits and re-elaborates his earlier research on Zambia (Burawoy, 1972).

If theoretical extension and refinement are conceived as overlapping Venn diagrams, then Burawoy’s extended case method falls into the area of overlap. But not all refinement is predicated to the same degree on extension for the simple reason that extant theories vary in their scope conditions and range of theorized applicability, with some theories being of broader generality than others.

A case in point is Stryker’s role-identity theory (1980), which is intended to apply to all situations where alternative lines of action are available to actors. The theory’s orienting premise is that identity and roles are linked isomorphically, with its fundamental proposition hypothesizing that the choice of behaviors associated with particular roles will depend on the relative location of the identities associated with those roles in what Stryker terms an identity ‘salience hierarchy’ (1980: 60–1). Although other concepts, such as commitment, figure in the elaboration of the theory, the basic idea is that the identities we project or invoke are grounded in the roles we play. While Stryker’s theory constitutes a theorized refinement of the interpretive variant of symbolic interactionism associated with Blumer (1969), among others, Snow and Anderson’s (1993) study of identity work among the homeless provides an ethnographically-based refinement of Stryker’s theory. Inasmuch as identities are internalized positions or role designations, we might expect the homeless to be imprisoned within personal and social identities of little self-worth. This not what Snow and
Anderson found, however. Instead, their fieldwork revealed a great deal of variation in the avowal of identities among the homeless, from those who strongly embrace their street identities, such as dumpster diver, to those who categorically distance themselves from stereotypical social identities imputed to the homeless, such as bum, and even from the general role-identity of a homeless street person - as when a homeless informant explained in response to a query about talking with him, 'If you want to know about street people, I can tell you about them, but you can't really learn about street people from studying me, because I'm different' - to those who resort to fictive storytelling that enables them to embellish their past or current identities or to fantasize about their futures and imagined identities. Their findings suggest that under some conditions social structure does not 'bestow' identities upon role incumbents, but rather alternative, contrary identities are asserted - even among those individuals situated at the bottom of status systems. In showing that for some individuals there is greater indeterminacy between identities and structurally-based roles than role-identity theory suggests, Snow and Anderson's research calls for a modification of that theory based on a series of propositions specifying the conditions under which there is likely to be a positive or negative articulation between roles and identity, and the conditions which affect the latitude one has to construct and impute or avow alternative identities.

Snow and Anderson's findings also suggest a refinement of the assumption, held by many social scientists, that identity-related concerns, such as the need for self-esteem, are secondary to more physiological survival needs. Rooted in Maslow's (1962) well-known hierarchy of needs, this thesis has become almost a cliché even though research bearing on it is scant, ambiguous, and typically at the aggregate level. However, the finding that identity-related concerns can be readily discerned in the talk of homeless street people, clearly among the most destitute in terms of physiological and safety needs, suggests that such concerns are not necessarily contingent on the prior satisfaction of more physiological survival requisites but may, instead, coexist even at the most rudimentary level of human existence.

Other examples of theoretical refinement are fairly abundant in other domains of substantive inquiry as well. For example, in his ethnographic study of the professional socialization of mortuary science students, Cahill (1999) refines theorizing and research on secondary socialization through the introduction of the concept of emotional capital. He found that one of the key factors distinguishing successful from unsuccessful students in the training program, in which he participated, was the emotional responses of the students to the work of funeral direction. He conceptualized the different emotional reactions as emotional capital and argued, based on his observations and interviews, that they were derived from the biographical backgrounds of the students. His analysis broadens theorizing about
secondary socialization by countering the human capital and cultural capital biases in this stream of work through the suggestion that variation in repertoires of emotional capital can affect socialization processes to some occupations just as variation in human and cultural capital.

It is reasonable to wonder how one might proceed to conduct ethnography in a fashion that produces theoretical refinements. Of the several examples of refinement summarized above, clearly Burawoy provides some directives if the aim is to refute existing theory (1998: 20). However, other steps or procedures can be culled from the above works. One is to approach the field with familiarity with a repertoire of relevant substantive and formal theories, but without having those theories function as the sole determinant of what is attended to and seen. From this vantage point, we view theories more in terms of repertoires than blueprints. Second, we contend that one should become as immersed in the context as possible, but also have an eye open to the possible generality of what is being observed. Third, in examining one’s field observations, one should allow the data to speak as loudly as the theories, so that they mutually inform each other. And fourth, as refinement of one or more theories proceeds, it is imperative that the researcher revisit his/her notes, and perhaps even the field, with the objective of capturing data that further substantiate the evolving refinements.

Conclusion

Ruth Behar writes (2003: 16) that ‘the beauty and mystery of the ethnographer’s quest is to find the unexpected stories, the stories that challenge our theories’. She then asks, ‘isn’t that the reason why we still go to the field . . . ?’ We certainly agree with her recognition of the link between ethnography and theory, as our foregoing discussion makes clear. However, the language that ethnographers use to engage theory has been somewhat impoverished and largely limited to discussions of either grounded theory or haphazard discovery. It is in response to these limitations that we have suggested three alternative paths for theoretical development. The examples we provide in this article illustrate these different paths, even though they were not conceived or written with this language in mind. The broader theoretical potential of ethnographic studies has been demonstrated within sociology in recent years by several scholars who have mined multiple studies in specific substantive areas to discern trans-situational patterns and develop corresponding theory unrealized or underdeveloped in the original ethnographies. Examples of this genre include Katz’s (1988) imaginative discourse on the emotionally compelling attraction of “doing” crime, Hodson’s (1998) synthetic analysis of work in organizations, and Schwalbe et al.’s (2000) identification of generic processes in the reproduction of
inequality across multiple ethnographic contexts. Taken together, such ethnographic endeavors not only demonstrate further the theoretical potential in much ethnographic research, but they also sensitize us to the utility of explicit theoretical development. But to maximize the potential for such advances, we believe it is imperative to create discourses that articulate and clarify ethnographically relevant strategies for theoretical development. We would not presume that discovery, extension, and refinement are the only pathways or languages that might be used, but these are three clearly effective approaches that can be derived from some of the ethnographic literature with which we are familiar.

In advocating for these (and other) pathways to theoretical development, it is important to consider the conditions that facilitate it or encourage it. One set of conditions operates at the level of the researcher. Among these factors is a systematic approach to fieldwork and data analysis – such as analytic ethnography – that promotes the linkage of field data to relevant theoretical traditions. A second element that facilitates theoretical development at the researcher level is a conscious and explicit comparative agenda that seeks to identify the conditions under which extant theoretical concepts or perspectives on generic social processes may be extended or refined. A final condition for ethnographic theoretical development is a deep familiarity with one or more substantive or formal theoretical traditions, including familiarity with the theory’s application in the research literature. In general, we would hypothesize that the more conversant the ethnographer is with a range of theoretical perspectives, the greater the likelihood for identifying opportunities for theoretical development.

A second set of conditions, beyond the level of the ethnographer, involves the contexts in which social science research and theorizing are conducted. One set relates to the degree of similarity between the setting in which a theory was initially developed and the site(s) of current research. Lincoln and Guba (1985: 124) argue that theoretical ‘transferability’ is more likely the greater the similarity between two contexts because of the greater ease of extending concepts across congruent settings. However, we contend, based on our own research and the illustrations offered in this article, that there is a curvilinear relationship between theoretical development and contextual similarity. More explicitly, the likelihood of theoretical development is greatest under conditions of either pronounced contextual similarity or dissimilarity. In the case of contextual dissimilarity – as in the previous discussions of the relevance of theories of vengeance to both horticultural societies and the board rooms of Fortune 500 corporations, and identity work among strikingly different sets of actors in highly discrepant situations – the discrepancy is likely to heighten the ethnographer’s theoretical sensitivity as s/he attempts to bridge and render understandable the observed and presumed differences.
A second set of contextual considerations concerns historical factors. It has long been understood that theories are historically embedded, even if it has not been as well understood how particular opportunities or constraints for developing theory are linked to these conditions. But whatever the linkage, it is clear that social research and theory are facilitated and guided by the available language and discourse within the social science disciplines. So, for instance, one of the reasons for the proliferation of grounded theory studies over the past three decades has been that Glaser and Strauss provided a systematic and (at least to some) compelling articulation of this approach that other scholars were then able to apply to their own topics and settings. No matter what the pros and cons of the grounded theory approach, it has flourished in part because of the absence of other well-articulated models for linking ethnographic research and social theory. The point of this observation is not to denigrate grounded theory, but rather to emphasize that the linkage between ethnography and theory will not develop further unless we provide richer discursive articulations of the ways in which they may be connected.

Ethnographic practice today is diverse and frequently rancorous. Recently, for instance, debates have swirled around differences among ethnographic styles. In some circles simply mentioning auto-, feminist, realist, or global ethnography is enough to fuel the fires of contention. Aspects of such debates are not without merit (although they often generate more heat than light). But when such discussion dominates ethnographic discourse, the goals that most ethnographers have in common too frequently get lost, including the important goal of ethnographically-based theoretical development. One result is that ethnographers lose the opportunity to have a theoretical impact in the broader disciplines and fields to which they belong. Yet unless strategies to achieve such impact are explored and articulated, ethnographic theory development will continue to be too often a hit-and-miss affair dependent largely on individual intuition and serendipitous insights. We have written this article in the spirit of providing ethnographic researchers with a clearer understanding of several strategies for better achieving the theoretical relevance that their studies deserve. We hope that other scholars will also take up this challenge and that collectively we can expand and clarify the theoretical ‘tricks of our trade.’ We believe that social theory in general, and ethnography in particular, will benefit greatly from such endeavors.

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Notes

1 This tension is not new of course, but reflects old schisms in the ethnographic tradition that have been expressed in terms of 'emic' versus 'etic' analysis or the 'nativist' versus the 'theoretical' impulse (e.g. Boas, 1943; Goodenough, 1956). But this tension has been inadequately addressed beyond vague directives to grasp the native's point of view and then relate that to some larger theoretical question (e.g. Pelto and Pelto, 1978: 63).

2 This is not to say that some research questions or hypotheses may not emerge during one's fieldwork or that all of an ethnographer's expectations about what they will find in the field must be worked out a priori to doing fieldwork. However, we do believe that successful theoretical development requires ethnographers to be oriented theoretically at the outset of their research rather than intentionally delaying or bracketing theoretical thought until well into the fieldwork or after leaving the field.

3 Hoover (1980: 30), for example, defines theory as 'a set of related propositions that suggests why events occur in the manner that they do.' Sjoberg and Nett (1968: 30) offer a slightly different definition of theory as a 'set of logically interrelated propositions or “statements” that are empirically meaningful.' Glaser and Straus (1967: 3) focus more specifically on the functions of theory as 'a strategy for handling data in research . . . [and for] . . . providing modes of conceptualization for describing and explaining.' And Fararo (1989) has argued that sociological theory generally takes two forms: the 'positivist' (e.g. hypothetico-deductive systems) and the 'instrumentalist' (e.g. hierarchies of increasingly abstract concepts useful for reasoning about phenomena).


References


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