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Gary Alan Fine

Ethnography 2003; 4; 41
DOI: 10.1177/1466138103004001003

ARTICLE

Ethnography

www.sagepublications.com Vol 4(1): 41–60[1466–1381(200303)4:1;41–60;031886]

Towards a peopled ethnography

Developing theory from group life

■ Gary Alan Fine

Northwestern University, USA

ABSTRACT ■ This article argues for a distinctive form of participant observation which I label *peopled ethnography*. I contrast this to two alternative ethnographic approaches, the personal ethnography and the postulated ethnography. In a peopled ethnography the text is neither descriptive narrative nor conceptual theory; rather, the understanding of the setting and its theoretical implications are grounded in a set of detailed vignettes, based on field notes, interview extracts, and the texts that group members produce. The detailed account, coupled with the ability of the reader to generalize from the setting, is at the heart of this methodological perspective. This form of ethnography is most effectively based on the observation of an interacting group, a setting in which one can explore the organized routines of behavior. I demonstrate the use of peopled ethnography through my own ethnographic investigations, contrasting this approach with classic works from other approaches.

KEY WORDS ■ ethnography, small groups, theory, social psychology, culture

It was not so very long ago that ethnography, at least as it was practiced within American sociology, represented a tightly knit subculture. The relatively few practitioners knew each other, and had developed styles of research that, even if they didn't match perfectly, had recognizable similarities and common understandings. It was a dense network, an 'invisible college' in Diana Crane's (1972) terms – although perhaps all too invisible. Today, with the diffusion and growth of ethnography throughout the social sciences (and, indeed, into professional fields – notably nursing, education, and management, and even into the humanities), the stable, consensual traditions that had once guided what used to be identified as participant observation have frayed (see, for instance, Hammersley, 1992; Denzin and Lincoln, 1994; Atkinson et al., 2001). Indeed, the newly dominant label of Ethnography, swiped from anthropology, although surely as imprecise a description as participant observation had been, reflects something of this newfound cachet. Today ethnography has something of the character of Baskin-Robbins 31 ice-cream flavors, and, in the process, some of the consensus of the standards and boundaries of ethnography, both within and outside of sociology, and anthropology as well, has been lost, a point emphasized by Herbert Gans (1999) in his 'Participant Observation in an Age of Ethnography'.

How can we stand together in a world of multiple ethnographies? How can we map our differences as well as our similarities? The truth is that often we do what we do because we feel in some inchoate fashion that the approach is helpful, and only subsequently attempt to determine the theoretical rationale for our tacit assumptions and practices. At times it is useful to stand back and attempt to assess, perhaps with a certain self-consciousness, the logic and legitimacy of one's practices. My personal need for explication began when a colleague, in a review of one of my manuscripts, awarded me a label that I had not previously considered. In light of the distinction that John Van Maanen (1988) proposed in his influential *Tales of the Field* between realist, confessional, and impressionist 'tales' or representations, I was operating generally within the context of realist ethnography (or, as Gubrium and Holstein [1997], describe it, 'naturalism'). By a realist tale, Van Maanen (1988: 46–54) refers to dispassionate accounts that emphasize the legitimacy or authenticity of the account presented. These accounts assume the experiential authority of the author, a documentary text, asserting transparency, claims about the 'native's point of view', and the validity of the author's interpretations. While such a model does not proclaim 'objective' knowledge, it assumes that the insight of the author is sufficient for the creation of truthful knowledge. I contended that my reports from the field reflected a real, if imperfect, isomorphism between my empirical descriptions and what was actually happening 'out there'. It is not that there were no elements of interpretive or confessional ethnography (the two other categories that Van Maanen presented in his typology), but I insisted upon my earned authority as a narrator. As a trained observer, I have gained a wobbly authority. Further, I maintain that whatever the asserted stance of observers, in practice they are presenting claims about the nature of the social world that they expect readers to accept, even if they choose to evade their responsibility rhetorically. The garb of truth claims cannot so easily be discarded. This is the rock on which I stand.

However, this reviewer proposed something beyond the claim that I was embracing realist ethnography; he claimed that I was presenting a 'peopled ethnography'. This happy, but unexpected, label caused me to start considering what were the characteristics of the model of research and data presentation that I have developed in the course of eight separate ethnographic analyses. It is this label – or at least my interpretation of it – that I explore, contrasting it with other approaches that relate to how we think about qualitative field data and how we think about theory. This article permits me to engage in a retrospective analysis in the late afternoon of my career, a point at which I am attempting to consider the broader implications of the several separate studies that I have completed, both in terms of my methodological choices and in terms of the broader understanding of small groups.[1]

By self-definition I label myself a social theorist, a sociologist of culture, and a social psychologist. These three subdisciplinary labels taken together help to define how I see ethnographic research, and also help to define how the approach that I have taken over the course of my career differs from others. We must do more than report; we are compelled to analyze – to generalize. As an ethnographer I have elected to examine a wide range of research sites: Little League baseball teams (Fine, 1987); fantasy role-play gamers (Fine, 1983); cooking trade schools (Fine, 1985); restaurant kitchens (Fine, 1996); mushroom collecting clubs (Fine, 1998); high school debate squads (Fine, 2001); the world of self-taught art; and offices of the National Weather Service. I am now preparing to observe the world of competitive chess and I imagine that this will probably complete my string of in-depth ethnographic investigations. This range of sites has permitted colleagues to suggest, not unreasonably, that my research interests have been diverse, if not down-right random. After the conclusion of my investigation of Little League baseball teams, some inquired whether I would next be examining hockey, carving out a substantive domain as a scholar of youth sports. However, the claim that my research projects are diverse – perhaps excessively so – misses the core of what I see as being important in my own research. There is an overall rationale and a set of common elements that permit me to build from one study to the next.

From my years in graduate school – 30 years since I was admitted to Harvard's Department of Social Relations – I have focused on the intersection of three core concepts: social structure, interaction, and culture. Today, given salutary changes in the discipline, these concepts have emerged as central building blocks in sociological inquiry, but in 1972, these interests were decidedly novel, if not eccentric or irrelevant. While 'structure' in its obdurate, macro, institutional taken-for-granted form was clearly core to what it meant to be a sociologist, an interest in interaction was distinctly marginal to the profession, stressed by a few symbolic interactionists, but not by many others. Interpersonal behavior seemed too small and unpredictable an area on which to base a science of society (Maines, 1977; Fine, 1991). The agentic power of interactants – their seemingly personal, unforced choices – made it difficult to see generalizations. In his *Theories and Theory Groups in Contemporary American Sociology* (1973), Nicholas Mullins described symbolic interactionists as the 'loyal opposition' in that these scholars embraced their occupational label as sociologists and were, like structural sociologists, interested in the question of social order, but went about their investigations methodologically differently and in light of distinctive core concepts by which order could be understood. Social order is built from the actions, interpretations, and negotiations among actors, and this means that social order is amenable to observation. As an undergraduate trained by Erving Goffman at the University of Pennsylvania, the focus on the 'interaction order' (see Goffman, 1983) – both interaction and order – was bred in my bone. I understood social order as locally constituted, even if structural conditions cannot be dismissed. Indeed, part of my contribution to a newly invigorated symbolic interactionist perspective (Fine, 1991) has been precisely this: that structure matters even on the micro-level of analysis.

Culture was a concept that in 1972 was owned by anthropology and not in currency in sociology. This was prior to the publication of the work of Howard Becker (1974) on 'art as collective action' and that of Peterson and Berger (1975) on the 'production of culture'. This pair of influential articles situated culture as part of the rightful domain of sociology, well before the creation in 1985 of the Sociology of Culture Section of the American Sociological Association institutionalized the concept organizationally. Culture is an essential analytic concept in reminding us that structure and interaction are about *something*; they are not content free. With its emphasis on the importance of meaning, culture connects to traditional symbolic interactionist concerns, and, being part of an interactional system, culture belongs to groups (see Berger, 1995). The place where structure, interaction, and culture come together concretely is in the small group. Ultimately, my concern was not to explore any one of these concepts, but the *nexus of the three*. My doctoral dissertation, under the guidance of Robert Freed Bales, examined the effects of a new member on the behavior of members of the group – an empirical intersection of structure, interaction, and culture. The small group was my window into social order.

My research focus has been to examine the patterns of group life. I consider myself a social theorist, but an unusual one in that I want my theorizing to be deeply grounded in the empirical data of group life. My intent is not the substantive one of describing a class of closely related scenes, but to use one such scene to probe a theoretical arena, and I have utilized ethnography to examine the creation of small group culture, the social organization of fantasy, the role of aesthetics at work, the cultural templates of nature, socialization to social problems discourse, the market politics of authenticity, and the bureaucratic organization of the prediction of the future. Today, we no longer need to be reporters of the exotic, but rather interpreters of the patterns of domestic life.

Peopled ethnography at work

Given these goals, what characterizes a 'peopled ethnography'? Ethnographies can be arrayed on two dimensions. The first dimension is the extent to which field observation attempts to address central theoretical issues, as opposed to providing a substantive analysis of a particular scene. The second dimension refers to the extent to which a rich and detailed account of the world being observed is presented, as opposed to the inclusion of a few instances of data to bolster one's analytical points – in other words data build a case, rather than simply illuminate it.

It is easy but mistaken to see these two dimensions as constituting a single dimension. A continuum with description on one side and theory on the other seems intuitively appealing, particularly in light of the criticism that some made of field research as constituting glorified journalism. However, the issue of whether one's focus should be generalized conceptual development is different from whether one should represent a scene so that readers gain a detailed feel for life in a given social space. Within this hypothetical two-by-two table, research projects that are low on theoretical interest and descriptive content do not much engage our attention, but the other three cells are filled with numerous studies of significance for social science analysis. For ease of reference, I shall label studies that focus attention on theoretical development with empirical description a minor feature as *postulated ethnographies*, and ethnographies primarily concerned with description and local, substantive analysis as *personal ethnographies*, in contrast to *peopled ethnographies*. These terms are hardly precise labels, but the first emphasizes the development of theory, while the second recognizes the importance of the personal descriptions that constitute the heart of the text

Table 1 Models of ethnography

	Substantial data	*Absence of substantial data*
Theoretical development	Peopled ethnography	Postulated ethnography
Absence of theory	Personal ethnography	Empty cell

– it is the personal relationship between observer and observed that vouches for the legitimacy of the ethnographic endeavor in such cases. In contrast the term *peopled ethnography* suggests that it is not the individuals being observed who direct our interest but rather their position within a group or social system: the set of actors and their group 'peoples' the ethnographic analysis and description. A central distinction is that theoretical claims are grounded in detailed observations, rather than being illustrated by them: in the words of Katz (2001), we move from how to why: from close observation to theory. This perspective is in line with Weber's (2001) 'multi-integrative' ethnography, providing primacy to the observation of interactions but always grasping these within structural conditions. The ultimate goal of this writing is to see people in action or, perhaps more precisely, to see people in interaction. Since groups are often the nexus of interaction, such observation scenes are typically constituted as groups or networks of various dimensions.

Before contrasting a peopled analysis with other models, I describe my previous projects and discuss how they constitute peopled ethnography. I use my own projects as exemplars of this approach, but by no means do I suggest that my work is the archetypal example of this model. One might name Michael Burawoy's (1979) *Manufacturing Consent*, Ruth Horowitz's (1983) *Honor and the American Dream*, and Barrie Thorne's (1993) *Gender Play* as exemplary studies of this type. William Foote Whyte's (1955) *Street Corner Society* is arguably the legitimating model of the peopled ethnography with his sweet, rich description and theoretical agenda of understanding the effects of group status systems.

My first ethnographic research study was a three-year observation of Little League baseball teams. I describe myself as examining Little League baseball teams, and not Little League baseball, because my focus was on the team as a site for understanding the process of cultural creation. I came to this project from an interest in small group dynamics in the laboratory. In my work with Robert Freed Bales and Stephen Cohen (1979) and their colleagues, then expanding Interaction Process Analysis into a more complex model of group dynamics called SYMLOG, the Systematic Multi-Level Observation of Groups, I wanted to extend this approach to the analysis of culture in the field. Every group develops, over time, a unique and distinctive culture. I referred to this group culture as an *idioculture* and proposed that 'idioculture consists of a system of knowledge, beliefs, behaviors, and customs shared by members of an interacting group to which members can refer and that serves as the basis of further interaction' (Fine, 1987: 125). While I could have gathered data from numerous social domains to prove the point that groups establish and utilize specific cultures, I described 10 baseball teams in five leagues, hoping to gain generalizability through the observation of multiple sites. The original impetus of this research was not to present a detailed picture of life on preadolescent baseball teams but to examine the means through which culture is created and embedded in small groups.

Of course, the fact that I was examining preadolescents and sporting behavior proved to be central to the analysis. I could have been satisfied with writing an article on group culture; however, I employed the case study to develop other theoretical issues. I described preteen mores in detail – in part to demonstrate that a subculture is constituted by a network of small groups. To this end I presented the content of the preadolescent culture for a more complete analysis of what young boys share with each other: sex, aggression, competition, and morality. The gendered quality of the scene is duplicated in several of my other projects. Further, this was not only an examination of preadolescent boys but of preadolescents playing organized baseball. This led to an analysis of the structure of youth leisure to explore how adults organize the leisure activities of children. Adults impose a moral order on children, which children respond to emotionally and strategically. In the course of doing this research theoretical issues became layered upon each other. In developing understandings of group culture, of preadolescent behavior, and of the organization of children's leisure, I relied on the reality that this was a case study of a set of groups – Little League baseball teams. Site matters, and it was important for readers to be immersed in the behavioral details of Little League baseball, but my intention was not to write a study of youth sport as such. Further, while the data were about the behavior of particular boys – some of whom reappear through the narrative – it was not a study of these *particular* boys. They 'peopled' the analysis, even if the writing was not an attempt to present the particulars of their lives and circumstances. Accordingly I downplayed the role of the 'key informant' and, in exchange, typified the social actors.

The topic of my second ethnographic investigation stemmed directly from the strengths and limitations of my analysis of Little League baseball, continuing to examine local cultures. Youth baseball teams revealed the existence of group culture but some elements of the temporal and organizational structure of these sets made them difficult to demonstrate the lasting and consequential power of an idioculture. Youth baseball teams typically met three times a week during the spring for a few hours each day. These groups, significant as they were to some of the boys, could not compete with other cultural spaces: classrooms, families, and informal friendship groups. A culture did exist on a team, but it was a weak and partial culture. I therefore searched for another research site where the culture would be more all-encompassing. I found such a site – an odd one – in the world of fantasy role-playing games. These groups consist of adolescents (again mostly males) who play games such as Dungeons and Dragons, a manufactured subculture. I spent approximately 18 months with these gaming groups, primarily in a club that met in the community room of a local police station in Minneapolis and in two ongoing private groups of gamers.

These gamers were explicitly interested in creating 'worlds' or 'universes', terms that they shared with academic usage. Put another way, their explicit concern was with *culture building*, and so I was able to explore how the creation of novel and fantastic cultures is linked to pre-existing cultures. This contrasts with the notion that, as fantasy, all cultural themes are possible. I hoped to demonstrate that fantasy was socially organized and patterned, tied to culturally legitimated models. Social organization, as in the case of preadolescent baseball teams, operates on the level of the small group. These young men formed tight and stable groups and their continued interaction contributed to the robust quality of their culture. They were engaged in an ongoing narrative project that extended their particular, local fantasies, giving them a reality within the context of the game activity. The reactions of participants mattered to each other. This desire for acceptance directed the elaboration of their collective fantasies.

As in the previous research, I examined the local context of the players and their activities. These fantasy gamers exemplified Erving Goffman's (1974) theory of frames, as these gamers had to determine the register (the code) of their talk. At times, these actors 'spoke' as their characters, treating the scenario within the game as their primary reality; at other times, they spoke as gamers, referring to the doings of the game as real; and, in other instances, they spoke as natural persons, referring to aspects of the social life outside of the game. The challenge – for both gamer and researcher – was knowing in context on what level interaction was being formulated – a problem that, as Goffman noted, applies to joking, play, deception, and other domains in which ambiguity is possible. What seemed like an odd shard of interaction stood for other domains in which interpretive frames abutted each other.

My third and fourth ethnographic projects dealt with culinary training and restaurant work (Fine, 1985, 1996), and grew out of dual desires. On the one hand, I recognized that in my two previous research projects I had ignored the power of organization, central to the structuring of action. Little League teams and fantasy gaming groups, while organized, floated within an interactional space. It was not that organizational concerns were entirely absent (there is a national Little League organization), but organizations were not a salient reality for these groups. My second concern was that the culture that I had previously examined was 'small C' culture, and that I had been ignoring artworlds that have been so central to the standard sociological analysis of culture. I had been examining cultures distanced from canonized cultures. I wished to examine the localized construction of aesthetic knowledge and the boundaries of aesthetics given organizational and occupational constraints.

For these paired research projects, I examined classes of students learning to become cooks at two Hotel and Restaurant Cooking programs at what were then called Technical Vocational Institutes (today, for purposes of institutional impression management, they are labeled Technical Colleges). The program lasted a year at one school, and two years at the second school. Approximately 15 students were in each class. On the heels of this project, I conducted ethnographic research at four restaurants for a month each. Each site involved a small group – once again predominantly male – engaged in face-to-face interaction, and in each place participants in their mutual talk addressed the issue of the interpretation of aesthetics and the constraints bearing on that perspective. Given the limitations of language for discussing taste and smell, cooks and cooking students had to develop techniques by which they could convey to each other shared assessments of dishes and recipes. In an article, entitled 'Wittgenstein's Kitchen' (1995), I probed the use of metaphorical constructions and incomplete poetics in creating shared group understanding. In a related paper I theorized the 'culture of production' (Fine, 1992), examining the limits on aesthetics operating within an economically and organizationally constrained occupation.

The fifth project, a four-year study of mushroom collectors and the organizations to which they belonged, was linked to those issues of aesthetic discourse raised in the examination of cooks. In studying restaurants, I had engaged aesthetic theory by asking how people addressed philosophical issues in their 'natural' interaction, and probed the boundaries and limits of the applicability of these theories. In my research with amateur and professional mycologists, I investigated how people conceptualized environmental ethics and brought cultural templates to the reading of nature. Focusing on the Minnesota Mycological Society, an organization with some two dozen core members, a small group again constituted my primary research site (although this group was divided between males and females). This was supplemented by observations at three regional and national forays in which groups developed over the course of the week. It was the repeated interaction of individuals, their shared talk and culture, and their behavioral routines that provided an opening to examine the cultural development of environment talk.

The idea of nature is constituted as a cultural template – 'naturework' – or how individuals define the meanings of the environment in light of cultural images and then define their relationship to that environment. Naturework is a rhetorical resource by which social actors individually and collectively elaborate a relationship to the 'environment' (Fine, 1998: 2); it operates through three processes. First, in talk about nature and natural objects – in this case, mushrooms – individuals rely on cultural categories (good, bad, pretty, ugly, male, female) as well as upon elaborated cultural metaphors. Second, the occasions of going into the woods are social events, and even when people traverse the wilds alone, they return with stories to share. Finally, nature is also constituted by the organizations that permit people to gain resources for participation in the wild. These organizations permit the establishment of a politics of trust and secrecy in a world in which mushrooms are alternatively considered rare, desired objects and dangerous, uncertain ones. I was not the first to argue that nature is a fundamentally cultural construct (Evernden, 1992; Lutz and Collins, 1993; Schama, 1995; Ritvo, 1997), but this is the only sustained *ethnographic* observation of a nature world.

Most ethnographic research depends on the attention to talk – often more than to behavior. For my next project I wished to explore the social construction of talk. I searched for a group that talked about talk. High school debate teams constitute such a social scene, and for a year I examined two American high school debate squads, each with about 15 adolescent participants. The issues on which this project was grounded involve what has been termed the narrative turn in social theory (Brown, 1987; Denzin, 1992). How do people learn to communicate so that others can understand, given the uncertainty of language and the decline of cultural consensus? This harkens back to the examination of uncertain aesthetic discourse in my examination of restaurant life. Again, the small group generates a culture through which members routinely discuss issues with the recognition that others will comprehend their meanings. Social problems discourse is based on a model in which argumentation is organized as a game (Fine, 2000). While debate provides a dramatic example of this process, one can see the same forces at work in politics and in law (Schachtman, 1995; Tannen, 1998).

The seventh study, recently completed, examines the development of the market for self-taught art. This represented a return to the concern in my restaurant research with how aesthetic value is constructed, and with the boundaries of this construction. Through self-taught art I investigate the politics of authenticity: how is value linked to the characteristics of individuals and groups? The desire to find authenticity is tied to assessments of many domains of contemporary society, including personal growth and selfhood. In this setting, I have found an inverse relationship between credentials and status – the rarity and value of a work of art is constituted by the characteristics and identity of the creators. The greater the separation between the producers and the artworld, the more desirable the product. Authenticity is used to create the interlinked strands of material and aesthetic value. The features of this world – debates over the proper terminology for the field and ethical discussions about the proper relationships between elites and the impoverished – are grounded on the ideology of authenticity. This five-year study differs from previous research projects in that in it I examine a dense network of individuals, more than a routinely interacting small group. While some groups existed, such as a folk art study group in Atlanta and the national Folk Art Society of America, in general network venues were shows, museums openings, auctions, and the like. As within any network there were clumps of friendship cliques, and I observed several of these groups. Even if the ongoing group life is not as explicit as in the previous studies, the peopled quality of the research – the repeated observations of individuals who served as representatives of their groups – remains constant.

My current research attempts to uncover the boundaries of science, scientific placement within bureaucratic structures, and how prediction and prognosis operate as a social act. For the past 18 months I have been examining three local offices of the National Weather Service, offices that each have approximately two dozen employees. In addition, I spent two weeks at the Storm Prediction Center in Norman, Oklahoma, an office of approximately the same size that generates severe thunderstorm and tornado watches for the United States. I play off the idiocultures of these groups as they connect to the possibilities and constraints of applied scientific practice. Weather forecasters are given the responsibility and the authority to predict the future. They are required twice a day to provide a forecast for the next seven days, claims that the public, government, and business use to organize their activities. Further, they have the authority to warn for the onset of severe weather. How are their consequential decisions made? What are the limits of forecasting weather from models and data, and how are models and data integrated? The National Weather Service is a large government bureaucracy: how are the demands of science – of meteorology – modified and structured within an organization that requires routine and immediate answers? A tension exists as to whether, and when, weather forecasting constitutes science. In this weather forecasting is not so different from other domains of public science, such as genetic counseling, predictions of the effects of climatic change, and assessments of danger from asteroids.

Forecasters are not engaged in gathering data, producing hypotheses, or testing alternative claims. Their task is to take information provided to them from technological inscriptions – like doctors reading X-rays, CAT scans, or laboratory results – and to make sense of this data for themselves and then for others. They must provide for clients a reasonably accurate prognosis. As an organizational matter, there must be some means of judging the validity of the prognosis, and so the organization constructs strategies of *verification*. Success on these measures then becomes the goal for which forecasters strive, rather than reporting what they believe to be most likely. That weather offices are interacting groups affects the doing of work and the final output. They are situated in a world in which the public, the media, and private firms make demands, and this affects the information that they produce. External demands limit occupational autonomy and the doing of science.

Specifying purposes and parameters

Despite their many theoretical and substantive differences, these eight studies provide the basis of my claims for the value of peopled ethnography. I identify seven pillars that collectively support such a style of field inquiry. To be sure, none of these by themselves differentiates this approach from others, but taken together they characterize what I contend constitutes a distinctive form of ethnographic practice.

1. *A peopled ethnography is theoretical.* As ethnographers, we have an obligation to provide for conceptual understanding. We begin with the 'what', move to the 'how', and eventually to 'why' (Katz, 2001). In doing so, we set out scope conditions that allow for a recognition of other scenes that have commonalities, in process if not in substance. Description itself only takes us so far. As I have indicated, in each case my selection of ethnographic setting has been determined by theoretical issues, and these conceptual questions, as transformed through induction gained through systematic observation, are the overarching focus of the analysis. Our ethnographic goal needs to be generalizability to other, comparable contexts.

2. *A peopled ethnography builds on other ethnographies and research studies.* We need to base our work in previous literatures. I reject the notion that we should enter the field ignorant, smugly confident that *something* will turn up. My studies are linked in their theoretical concerns, as one study links analytically to the next. In addition, several themes run throughout the research, as each project reflects a working out of recurrent problems that I see as sociologically significant. The questions raised or limitations found in one study are addressed by the subsequent ones. Similarly, each study is tied to a body of literature which, even if it is not based on the same scene, addresses related concepts. In my case, much of the research examined the development and perpetuation of small group culture, particularly expressive forms of culture such as humor and gossip, and how group cultures are linked together through networks to form subcultures. Another topic that has infused much of my work is how cultural boundaries are organized and how ostensibly non-cultural issues are made cultural, whether that be manual labor, the natural environment, or atmospheric conditions.

3. *A peopled ethnography examines interacting small groups.* We should examine those places in which people talk and act, and where they do so on

a continuing basis. We need to explore ongoing scenes in which interactants actively work out their problems and concerns, rather than focusing on anonymous moments in which actors by chance 'bump into' each other. Research on behavior in public spaces often lacks an emphasis on shared culture and on lasting consequences, because individuals do not mean much to each other, and the assumption is that interaction is not part of a ongoing *relationship* (Lofland, 1973; Edgerton, 1979). We must examine the ongoing linkage of the construction of meaning and the outcome of events. As a result of this belief, my research focuses on groups engaged in continued interaction. Thus, these studies of culture are fundamentally micro-sociological, even when the theoretical issues are grounded in more macro-sociological concerns. In this, perspective groups represent the laboratory for the examination of natural dynamics.

4. *A peopled ethnography relies on multiple research sites*. Every group has its local distinctiveness. Even if they belong to the same 'class' of social scene, there is no single template for group action. The researcher who focuses on a single interacting unit may discover as a 'conclusion' some unique peculiarity, a function of the characteristics of members or setting. To avoid basing generalizations on idiosyncrasies, ethnographers should examine multiple scenes to gain a measure of confidence that findings characterize the class of groups (and sites), and not just the particular group (or site). Each of my studies involves the observation of several small groups, transcending the dilemma of uniqueness.

5. *A peopled ethnography is based on extensive observation*. Ethnography is hard work; by its nature, rigorous field observation is labor-intensive. Hour upon hour; day after day; year by year. While there is no rule for the length of time required, ethnography takes longer than one might wish! One needs to become an *expected* participant in group life, and not an *ethnographic tourist*, appearing when convenient. One stays as long as one keeps discovering the new. I participated in my field sites for months or years, continuing to observe until I find that my learning curve is no longer increasing and at which time I can predict what will happen next in the setting. Put another way, I observe until I feel myself increasingly a full member of the group, rather than marginal to the interaction; when members of the group begin asking me questions about how their group operates it is time to leave. At this point I can joke with the members of the group and can gossip meaningfully, a mark of acceptance (Goffman, 1989). I find my field notes decreasing in length and my theoretical categories saturated. While researchers differ in the length of time they remain in the field, one of the changes with the growth of ethnography as a widely accepted methodology is that many ethnographic studies have become

briefer and less intense – perhaps not in the best of the studies, but in too many. I self-identify as a *working ethnographer*, and this implies the need to spend years in the field. Fieldwork is my career. Works that do not adhere to this principle may be insightful sociology, but they are not in my eyes exemplary ethnography.

6. *A peopled ethnography is richly ethnographic*. Field notes – and their publication – are our stock in trade. As ethnographers, we must do more than claim: we need to *show*. Theory is developed through the presentations of empirical data; it is in the *glittering instance* that theory becomes developed. Our data must be luminous (Katz, 2001: 443). We must create theory from action and talk, a collection of composed set-pieces, reflecting the activities of social actors. Each of my studies involves a detailed presentation of field data; my books are filled with instances that support my claims. I strive for an ethnography awash in behavior. This is a branch of realist ethnography but with the fundamental goal of making an argument. Our goal must be to expand theoretical and conceptual knowledge. This happens most effectively while providing a detailed accounting and exploration of a social scene, proof as strong – or stronger – than statistical measures.

7. *A peopled ethnography distances researcher and researched*. It is tempting to believe that we are capturing the lives and personalities of individuals – to enshrine their nobility as persons, placing them on a literary pedestal. Yet, such a stance makes us into biographers or into psychologists. If we embrace the centrality of generalizability, these figures, however much we might care for them personally, are merely present to make a case. They stand, not for themselves, but for many others. Particular individuals should not become defining figures in the text. As a result, in my representations, I strive to maintain an analytic distance from those whose actions I recount – the traditional ethnographic stance of remaining on the periphery (Adler and Adler, 1987). The goal of ethnography is not to meet people, but to depict action and talk of sets of participants. In my writing – and in my thinking about my writing – I do not imagine my subjects as heroic or oppressed, as romantic or malign figures. My writing does not involve the enshrinement or abasement of subjects, but in treating them as morally neutral, as a good ethologist might treat observed primates. Behaviors matter more than soul. As I compose, I strive to be marginal, to maintain an *ironic detachment* from informants. In some regards, ethnography is a sociological comedy of manners.

By spotlighting these seven features, I make a case for what a peopled ethnography looks like and for how ethnography should be conducted. My model emphasizes *theory building*, *detailed* observation and data presentation, a focus on continuing *group interaction*, and the downplaying of individual actors and individual scenes to fulfill the need for *generalizability*. I do not claim that these themes necessarily must be packaged together, but for me each contributes to the goal of research to create systematic and substantiated knowledge. In my ethnographic career, I have not attempted to plow in the same narrow substantive row. While several of my projects dealt with youth, the arts, and leisure worlds, the projects were not intended to build on each other *substantively*. However, while the groups were diverse, most involved middle-class white Americans, often male. Each ethnographer has a *personal equation* that encourages the examination of some groups, while avoiding others. The reality that our equations diverge means that all groups will find an ethnographer who deserves them and whom they deserve.

Contrasting types of ethnography

I contrast this stylized model of a peopled ethnography with two other approaches which I label *personal ethnography* and *postulated ethnography*. I wish to illustrate each by reference to an exemplary model, Robert Jackall's *Wild Cowboys* (1997) and Arlie Hochschild's *The Managed Heart* (1983) respectively. These two works are among the most important ethnographic documents of the past two decades. My work does not read anything like either of these classic texts, nor would I have it do so, much as I admire the achievements of my colleagues. My claim is not that they violate each of my principles – they don't – but they, and similar, less effective ethnographies of their type, do not represent the features that I have proposed for a peopled ethnography.

Clearly Jackall has the eye. His account of New York City police and prosecutorial responses to Dominican narcotics trafficking and murder in the Bronx presents an insider's view more powerful – and more real – than any television drama could ever be. He brings us with him – through his spare and objective style – inside cop cars and courtrooms as these agents of social control fight the seeming tide of disorder and disorganization. Reading with him, we come to know these scenes and these peoples. We learn of the plight – the thoughts and deeds – of Detectives Mark Tebbens and Garry Dugan, Assistant District Attorneys Dan Rather and Dan Brownell, and by their words and documents the miscreants that they are chasing. Jackall wants us to get to know these beleaguered professionals – not as stick figures but as multi-dimensional people in their own right. Jackall presents us with a set of powerful moments about detective work and the way that the criminal court system operates. Yet, in doing this, Jackall explicitly rejects theorizing. This is pure unabashed narrative description. The text is filled with people and their incidents, but no ethnographer. He provides us with no overt clue as to what sense to make of urban decay and the legal system that he depicts. Implicitly (and from the subtitle) we might well see the battle between urban marauders and the forces of order – but what are the policy implications? For many it is that more resources are needed to support policing and prosecuting. For others that the inner city is a dense jungle. Perhaps a few would see the structural conditions that make drug trafficking something other than the choices of immoral, indecent men. Jackall is calling for order, but we cannot be sure since he absents his own voice. Although he provides us with a set of searing images, he presents no searing ideas, and so we are on our own in the urban wilderness. The explicit theorizing that I am calling for – perhaps bringing in concepts relating to the reproduction of inequality or the demands of difficult occupations – would have enriched the analysis and provided a set of contentions with which others might argue.

Arlie Hochschild's writing has a different set of strengths. She wishes to test theoretical postulates through observational depiction. Perhaps the description is too limited to be considered an ethnography, but so it is often described. Hochschild (1983: 14) attended classes at the Delta Airlines Training Center and spent countless breakfast, lunches, and dinners observing the flight attendants. She also observed Pan American's recruitment of flight attendants in San Francisco. The book, an important theoretical argument about the role of emotion in labor and organizational life, is often described as being *about* airline flight attendants (stewardesses), although it also depicts the work conditions of bill collectors. Hochschild argues the theoretical point that by enacting an emotion, that emotion can come to be real for the player, and this is an important claim, even when we recognize the exploitation involved. Yet, it is striking how little we learn about the routines and interactional patterns of flight attendant trainees. We barely meet these women, only briefly consider their conditions of work, and do not learn of their interaction partners. The group and its interaction patterns are largely external to the argument. Hochschild presents us only what is necessary to build her broader theoretical argument. This is a minimalist ethnography; one might even consider it to be miserly in its refusal to present us with the richness of the work lives of flight attendants. The stewardesses are poster girls for building Hochschild's sociology of emotions. I confess that I am envious of these two works and their rhetorical power. Yet, the one lacks a detailed theoretical armament – an analytical raison d'être; the other lacks an ethnographic generosity: missing in it is the domain of actors and their acts. Neither is a peopled ethnography. For Jackall the unwillingness explicitly to confront the conceptual and theoretical implications of his scenes places him at some distance from his own text; for Hochschild, we cannot separate her claims, however plausible, from what she saw in her days with her informants.

Concluding thoughts

Let me end gently. My claim is not that a peopled ethnography is inherently superior, but that it has justifications – and, of course, limitations. Ethnography is demanding. It requires time and effort. We should all be working ethnographers. While there are some virtues in texts that describe distant, exotic, or taken-for-granted scenes, we should do more than provide verbal pictures: we need to provide explanations as well. A peopled ethnography calls for these pictures and for these explanations. These pictures are to be found where people talk and act in ways that permit us to gain an understanding of concepts on which we wish to build explanations of the possibility of social order. An emphasis on the power of group dynamics can justify the use of ethnographic detail for social theory, not moving too far from the detail or the theory, and in recognizing that our data are action and interaction. With their routinized, continuing, self-referential, and embedded activities, groups provide the spaces in which meaning is generated and in which explanations are therefore possible. As theorists, we may need to go beyond the group in our explanations, but that is where we begin.

A peopled ethnography is surely limited in that it downplays what cannot be easily observed – the hidden webs of power in a world-system, not readily discernible given constraints on group access. Large forces may be missed when the groups in which these forces are enacted are closed to ethnographers. Similarly, a peopled ethnography directs attention away from the nobility of the person, in its insistent emphasis on the group. Ethnography, classic and contemporary, should depend on making sense of the group. It is no wonder that so many classic works of ethnography in sociology – whatever their political stance – have fixed on the street corner: Whyte's *Street Corner Society* (1955), Liebow's *Talley's Corner* (1967), or Anderson's *A Place on the Corner* (1976). The image of the corner calls forth a small, intimate community whose interaction is socially situated within a broader, powerful structure and whose connections permit both members and observers to see the significance of their interaction. The world is filled with corners, clubs, teams, offices, and cliques. The world is filled with uniqueness and with regularities. By studying the former we discover the latter. In this – in disclosing the link between the routine and the rare – peopled ethnography can be a model to embrace, and not merely something onto which a naive observer happens to fall.

Note

1 This analysis constitutes the results of my consideration of my ethnographic approach. I should emphasize that by this self-presentation and self-justification I do not suggest that other approaches are necessarily deficient;

however, they do not reflect the ideal by which I feel that ethnography should be conducted. They simply diverge from my own ethnographic program. While this is a subtle difference, it is an important, if, some might suggest, disingenuous one.

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■ **GARY ALAN FINE** is Professor of Sociology at Northwestern University. He is the author of several ethnographies, including, most recently, *Morel Tales: The Culture of Mushrooming* (1998) and *Gifted Tongues: High School Debate and Adolescent Culture* (2001). His current research is an ethnographic study of government meteorologists and the sociology of prediction. *Address*: Department of Sociology, 1810 Chicago Avenue, Northwestern University, Evanston, IL 60208, USA.
[email: g-fine@northwestern.edu] ■