Article

On Becoming an Ethnographer

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Abstract

Ethnographers shape a research self as they work through a series of existential choices. Self-defining choices must be made on: the genre or overall narrative logic of the text to be produced, how to handle experiences of awkwardness when interacting with subjects, how to understand one's difficulties in understanding subjects, whether to be drawn into debates in popular culture, whether to assess progress in relation to improvements made or in relation to abstract standards defining perfect knowledge, and whether to understand evidentiary questions within a reflection or a pragmatist logic of truth. These are existential challenges in that, while alternative responses can be equally productive, in each project practical limits press the researcher to choose among inconsistent paths; the alternatives are as consequential when ignored as when reflectively weighed; the choices made implicate the researcher's personality as a whole; and over time the choices shape the researcher's working sensibility. The freedom of ethnographic fieldwork makes it at once an especially democratic methodology, immediately open to all who would advance knowledge of society, and an especially fateful crucible for defining the adult self.

Keywords

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This essay is the product of a nagging question that emerged when co-teaching ethnographic field methods to UCLA graduate students for over three decades.[1] Did something learned in our five-month training course improve the quality of students' subsequent work or careers? Partly the problem is with proving the value of any teaching in higher education but ethnography exacerbates the challenge. Some students who are great in class do not use ethnography as their career method of choice. More discouraging, many great ethnographers seem born that way. The value of our teaching may be nothing more than patting them enthusiastically and sending them on their already ingrained way.

Still I would argue that training in ethnographic field methods is worthwhile, even indispensable in any graduate program in sociology. The training course is valuable, not necessarily for anything substantively learned but for working through existential choices in becoming an ethnographer. At certain moments in the research process, researchers—novices as well as those thoroughly seasoned—must define themselves as they define the research project. Empirically, I am claiming that all ethnographers sense and are shaped by their response to many of the same challenges.

I specify several turning points in the ethnographer's working perspective. The focus will be on decisions that must be made between paths of development that are incompatible alternatives. Whether through reflective choice or in accordance with sentiment, the ethnographer takes a turn that becomes fateful.

"Ethnographic research" as used here refers to the creation of primary data through personal interaction with research subjects in a project of creating or refining generalizations about human social life. "Participant observation fieldwork," a phrase familiar in academic sociology, is included, as is "ethnography" as practiced traditionally by anthropologists who immerse themselves in the settings they study, at least to the extent that they develop original relationships with informants and analyze observations as well as interviews. "Ethnography" may be quantitative or qualitative in the data it produces so long as the data are first inscriptions, which occurs when the researcher develops the descriptive categories and applies them to people personally encountered in naturally occurring social settings. Research is non-ethnographic to the extent that the researcher/author is analyzing what others have already formalized as data sets; or is conducting research in laboratories, interviews, or other social contexts in which what becomes data for the researcher are behaviors pre-defined as research-relevant. Ethnographic research is distinguished by a quality of retrospective and personal discovery in which the investigator realizes that encounters "in the field" can be used as "data" that have generalizing value.

Genre Choice

Most ethnographic research fits into one of three types: iconic, comparative analytic, and modeling. In the first, the researcher gets to know a handful of people in a small number of situations and offers them as representatives of a type of social personality. The author presents a few men as representing "the homeless" (Duneier and Carter 1999); a few women who demonstrate the unpaid, unappreciated work that women do at home (DeVault 1991); a small network of residents who represent "streetcorner boys" (Whyte 1943). The ethnographer creates an iconic image of some part of social life by showing how central characters, who are described as distinct individuals, act in situations that are familiar to them and common in their social world, such as getting money from passersby, preparing the dinner meal, or bowling.

Like religious icons, the portrait that results is a miniature and, if successful, will be revered as glowing with larger significance. Given that the ethnographer selects subjects without any sampling rationale, other than, in a few studies, relying on the preexisting iconic status of the site in popular culture (a notorious "slum," a locally well-known set of "homeless" people, "mother's work"), the generalizations readers are led to draw from the miniportraits are magical. It is impossible to rationalize how a description of three or five people can stand for the lives of hundreds of thousands, even many millions, but this academic genre, which has parallels in journalism and Dickens-like fiction, feeds a hunger that will not be sated. The justification for the genre, which is usually left implicit, is that getting it unusually right about just a few people is an essential complement to most research in social science because most of social science at best explains a very small part of the variation in the behavior of a multitude.

In the comparative analytic genre, the focus is not fundamentally on people but, paraphrasing Goffman, on situations and the identities people enact in them. Lots more subjects are represented, both in terms of the number of people described but also because the subject of analysis changes from individuals to forms of behavior. The researcher focuses on strategies or tactics with which medical examiners work out the cause of death they will attribute to a corpse (Timmermans 2006); everyone works through the interpersonal troubles that arise as they interact with others in everyday life (Emerson 2015); the police deal with the street people they are called to manage on skid row (Bittner 1967); women are trained to serve a helping of gender identity to men (Hochschild 1983).

Boys in White (Becker et al. 1961) became a historical marking point in the development of comparative analytical ethnography. Based on field research in a Kansas medical school in the 1950s, this collaborative research

project was a transition interlinking generations of ethnographic researchers in sociology. *Boys in White* set off a stream of explicit methodological writings on "participant observation" and "fieldwork" by Becker (some coauthored with Geer), beginning in the 1950s (e.g., Becker and Geer 1957; Becker 1958). In the 1960s, Anselm Strauss paired with Columbia-trained Barney Glaser to study behavior in hospitals and to promulgate what they called "grounded theory" methodology, which featured a "constant comparative" way of developing sociological analysis from participant observation fieldwork (Glaser and Strauss 1967). At the same time, without bothering to write up any methodological reflections he may have entertained, Erving Goffman developed the most widely read and longest enduring substantive texts based on combining a comparative perspective (on "total institutions," "cooling the mark out," "stigma," etc.) with analyses of how individuals in given situations shape their behavior in anticipation of how others will perceive it and respond to them. The sociology of Becker, Strauss, and Goffman was essentially comparative and formally apolitical in the tradition of Georg Simmel as conveyed to them by Everett Hughes, a student of Robert Park who in turn had studied with Simmel.

The hallmark of a comparative analytic ethnography is an analysis of how people interact that transcends the conventional, substantive, or official definition of what they are doing. People with all sorts of stigma, whether physical, attributed to race or gender, or associated with moral actions like crimes, develop the same ways of anticipating and guarding against hurtful reactions (E. Goffman 1963). Young black men in cities are shown to use interaction skills that transcend race in order to negotiate passing relations on city streets (Anderson 1990). Students are shown to respond to the demands of educational institutions much as workers respond to employers' demands (Becker, Geer, and Hughes 1968); and workers are shown to respond to employers' demands by making work into games (Roy 1952, 1953).

In contrast with studies in the other genres, an author who follows the dictates of Mead and Blumer or the examples set by Goffman, Becker, and Strauss in their substantive work, will show much more variation in how a given line of action is performed. The resulting data will be a mass of descriptions that as a set show closely nuanced variations in how an analytically characterized type of behavior is conducted. If the researcher wishes, the variations in the data can be exploited for developing and explicitly testing rival causal theories (Katz 2015).

In the modeling genre, the ethnographer tries to show how a social world works. Descriptions are more abstracted from personality than in the iconic genre, and as compared with works in the comparative analytic genre, descriptions of variations in situational enactment may be minimal. Modeling

ethnographies themselves differ as between those that are self-consciously extensions of comparative historical theory (Burawoy 1979), cultural anthropological (Geertz 1972), or in a phrase taken from the Chicago school, dedicated to portraying "collective action" (Becker 1982). In the current academic lexicon, modeling ethnographies show the interconnections of macro historical, meso, and micro processes.

Despite sometimes heated programmatic and methodological statements that emphasize differences, considered as a practical project in developing knowledge across rhetorical divides, "modeling" work shares fundamental similarities. Burawoy reviews changes in the history of finance capitalism, details changes in management practice at the organizational level, and specifies the meaning of his concepts in work-situated behavior that he personally experienced in a factory. Becker reviews the history of musical conventions and the craft of making musical instruments, details practices in organizing art galleries and orchestras, and applies his concepts to socially situated behaviors, some of which he personally experienced when paid to play piano in bars and when taking photographs with classes of students and for his own pleasure. Geertz starts his famous essay on the Balinese way of life by describing his personal experience in visiting a cockfight, details practices in preparing roosters and betting, and draws on his readings and prior research on religion, kinship, and economic and power relations in Bali. Each of these works offers a contribution, not in showing the personalities and life context of particular individuals, not in making fine distinctions among different ways of executing the same line of behavior, but in modeling how people sustain an institution or community by acting in different times and places without directly encountering all the others with whom they interact.[2]

The choice among the three genres is existential because, while equally justifiable, they entail practically incompatible personal commitments. Still, over time a different choice may be made. As an ethnographer's career develops, different genres become practically accessible. Becker's "modeling" work came decades after his many comparative analytic writings. Burawoy later conducted factory research in Hungary, which gave his Chicago-area study a comparative analytic status. Geertz's wide-ranging body of work, some of which details personalities acting in place, forms the context that many readers bring to his cockfight essay. In an empirical review of what ethnographers do over their careers, the oppositions between methodological camps fade, if they do not completely disappear.

In training courses, it is common for the novice either to do an iconic or a modeling ethnography. Either the student gets to know a few people and describes many patterns in their social lives, or he or she integrates a few descriptions of the scenes personally witnessed within a theoretical discussion

based on reading others' studies and theories. Conducting comparative analytic ethnography as a first project is relatively rare, unless the instructor insists on that genre.

In order to develop a comparative analytic treatment of situated social action, students must narrow data gathering so as to collect multiple variations of a given course of conduct. An example would be collecting data on how people check out (pay) at grocery markets. A mass of data would be created by checking out oneself and observing how others go through the process.

Often novice researchers are not prepared to narrow their research focus to the extent required to set up a comparative analytic study. Students become familiar with different social science theories and wish to explore them. To stay with the grocery market example, the student may appreciate that there are many already politicized issues about grocery markets. These include higher prices for the poor, corporate misrepresentation of product contents, preferential market placement for the products of big capitalism, and less healthy alternatives for marginalized populations. As a practical matter, a data set that would support a comparative analysis of interactions in "checking out" might involve describing 100 instances of the sequence. The student would have to put macro issues aside until they might be discovered buried in the minutiae of quotidian social life. But at that point, the student will have expended extensive efforts to create a large data set on situationally specific behaviors. The student would then be pressed to search for micro-analytic themes and social-psychological issues in order to exploit the value of work already done, that is, to write in the style of a comparative analytic ethnography.

The challenge of choosing genre is more difficult emotionally than intellectually. Preparing to write a comparative analytic ethnography by creating a rich data set on situationally specific variations in "checking out" is hard work in the sense of labor, but describing each retail interaction is a matter of careful attention and detailed write-up. The greater challenge is putting aside the moral/political interests that motivated the project in the first place. Within the data gathering project, the student will have to become involved in craft challenges that are independent of the macro issues which modeling addresses.

The craft requirements for producing data sets displaying nuanced variations in the execution of a given course of conduct are also incompatible with the sentiments and social relations necessary for producing iconic ethnographies. Ethnographers who produce iconic ethnographies appear to stumble into the genre. Whether through personal sympathies or out of a nervous discomfort over the prospect of being thrown out of the site, field researchers are often drawn to follow the concerns of the principal contacts who vouch for them and function as guides. Those concerns are likely to be multiple, to involve relationships and life projects that, if followed by the researcher, will

create a scattering of observational notes such that the resulting data set will be too thin to support the analysis of many variations of any given course of conduct. In order to get and maintain access, the field researcher will need to open himself or herself to subjects in ways that go beyond the truncated perspective used in a comparative analysis of socially situated conduct.[3]

If one wishes to stay rooted in the university world, it creates less strain to try to model the scenes studied. By writing on macro social patterns in order to frame an overall comprehension of subjects' social world, the ethnographer will justify library work and regular discussion with colleagues. To flesh out the human realities of the model, only bits of micro-interaction and short-term field excursions will be needed.[4]

The upshot is an essentially personal confrontation with the choice of genre. At this stage of your life, are you the kind of person who likes to schmooze, hang out, subordinate your interests to those of others so as to "give them voice" in the terms and over the range of involvements they find important? Or do you find it too uncomfortable to sustain a promiscuous dependency on others' lives? Do you "have the patience," that is, can you control your anxieties about making a recognizably significant contribution long enough to enter a social world you do not know and wait for patterns to emerge? Do you find that, to make sense of your life—to tell yourself the story of yourself that will sustain your motivations, to tell your friends and family what you are doing so that they will support you, to tell your colleagues what they need to know to promote you and sustain your research funding—you need quicker analytical "news" from your field interventions? And can you handle the criticism that may emerge if you become diffusely engaged with immoral or criminal subjects?[5]

Rhetoric is often invoked to cover over the existential nature of the choice, with the path rejected disdained as petty or pretentious. But is it more reasonable to treat a few people known intimately as representative of a widespread type; to seek generalizable wisdom by studying variations in the scatterings of social life that happen to fall within a participant observer's situationally narrowed field of perception; or to call on the spirits of Durkheim, Marx, Geertz, or Bourdieu in order to locate a spark that will enlighten relatively few moments in the field? Which course is self-indulgent, which more heroic or timid, which most properly deferential to what scholarly predecessors or the lay people studied already know?

If choice of genre depends on personality, and on the changing fit of personal demands over the stages of an academic career, what is the value of a training course in field methods or discussions of alternative ethnographic methodologies? Not to force square pegs into round holes but so that researchers come to appreciate the reflexive implications, the different demands on self in different ways of working.

The Shame of the Inevitably Naked Researcher

Ethnographers often experience fieldwork as emotionally uncomfortable. If asked to explain why, some confess guilt in taking something of value from subjects without giving back. It matters if they are right. If they believe that to continue ethnographic research one risks an immoral imbalance with subjects, novice researchers may distort their writing to show a deference that puts them in a more acceptable posture, or they may become disdainful of others' ethnographic work and abandon field research.

As ethnographers of ethnography we may consider "exchange imbalance" as one among rival causal explanations for the researcher's discomfort. That the researcher will be getting something of value from the intervention is highly problematic. Perhaps the project will turn out to be a waste of the researcher's time. Even more problematic is the researcher's understanding of what subjects are taking from the transaction. Sometimes ethnographers are surprised to realize what their presence has meant to subjects. John Van Maanen (1983) was appalled at the thought that when his police subjects beat up citizens, they were playing to him. Especially when the researcher comes from a social world that is distant from the subjects, what the subjects get is an opportunity to form an image of the type of person they take the researcher to represent, from his or her appearance, questions, mistakes in navigating their world, etc. When they hear the researcher's questions and feel the researcher's gaze, at least some subjects will find it valuable to glean how people distant from their social world regard them.

At an early stage in a study, and often even after all the fieldnotes are written, the researcher does not know what subjects are taking from the research interactions. After all, the premise of ethnographic field research in the first place is what "we" don't know about "them" or their world. Field research creates the emotional provocation of a Rorschach test. The "moral imbalance" theory projects an ethical monster.

Ethnographic research risks stripping the researcher naked. Not knowing what you are looking for, it is hard to shape your presence as "a researcher," hard to hide behind a researcher guise. The risks can be minimized by entering interactions in the field under the cover of a clipboard or other indicators of predesigned investigative procedures but participant observers have often found that pretending limits the pursuit of curiosity. (This is true of the inverse case as well, pretending not to be doing research at all [Johnson 1975].). Another way to avoid the risk of exposure is to rely on what was learned when one was a full participant in some social world, before becoming a researcher. But most novice ethnographers will either want to enter worlds new to them or, after reading advice on how to write high-quality

fieldnotes (Emerson, Fretz, and Shaw 2012), to reenter with a weird intent to remember and record what had been lived naturally.

The emotional risk of exposure in conducting original field research is not limited to novices. As others have noted, the longer one is in the career, the harder it is to maintain a posture of ignorance. Professors are expected, or at least expect themselves to be expected, to present a confident appearance as a researcher and be guided by a clearly conceived purpose.

Researchers often volunteer to perform some kind of menial or "dirty" work in the scene, in part on the understanding that they are making up for the moral imbalance in their research transactions. Emotionally, doing dirty work may solve the problem of moral imbalance as the student denigrates him or herself to a status lower than that of those studied. But in any case, the volunteer role immediately gives subjects a way of organizing their response to the researcher.

If researchers do not take on a local role, subjects are left to their own devices to figure out what to do with the researcher. How exactly does one "hang out"? From the subjects' side, how does one interact with another who simply wishes to "hang out"? The fieldworker may move into the recognizable role of the interviewer but that role will often provide little cover. The ethnographic interviewer, knowing that he or she does not know good questions to ask, is likely to convey a hesitancy that a sympathetic subject may pick up and take as an obligation to put the researcher at ease. Even if disposed to be helpful, subjects usually have no training in helping would-be ethnographers figure out how to be ethnographers. With impeccable reason, subjects look to the researcher for an understanding of what it means to do sociological research. Appreciating in some dim way that the subject is straining to sustain the interaction, the researcher likely will become more discomfited.

The upshot is a case of professional nausea, mild or severe.[6] Novice researchers find themselves left naked in the gaps of interaction, spinning intellectually in search of some firm identity to hold onto. The researcher stumbles outwardly and falls inwardly. Since time immemorial, a natural response to falling is shame. That experience of full blown dis-ease leads to desperate attempts at escape. Perhaps the most common route is to invent a moral analysis of why the interaction failed: the researcher is ashamed because he or she has no right to be there. That is an elegant confection but not helpful for advancing the research process. However the moment is handled, it must be handled. Perhaps it is best to retreat, taking an anecdote or two in hand, reengage academic discussions of theory, and launch a cultural analysis of the artifacts removed from the scene. Then one can get back into daily interaction with colleagues and transform discomfort in the field into great motivational energy for developing a sophisticated presentation of an

academic self. Or maybe the researcher becomes a lay helpmate to others in the scene, in which case access to certain scenes and people will be compromised. The way taken to cover over nakedness will be a turning point in the shaping of the research self.

Finding the Starting Point for Analysis by Examining the Distinctiveness of Description Already Done

As examined here, ethnographic field research aims at first description. Original description can be qualitative or quantitative, as it often was in the classic "Chicago school" studies in which researchers, in order to show the distribution of a phenomenon over the city, had to work out definitions of what a gang (Thrasher 1927), or suicide (Cavan 1928), or a hotel (Hayner 1936) was. That ethnography's warrant is first description is attested by ethnographers when they justify their work by claiming that, while others have studied various forms of a phenomenon, the current study is "the first" or makes up for the neglect of a version of ethnic, sexual, or occupational identity. No one has gone here before, lived closely with people doing that, or gotten access to this kind of group (Katz 1997). As a general warrant, doing the first study of a given phenomenon makes sense, but then what? How does the researcher begin analysis?

Much of the hot debate among ethnographers over the last generation has been about how to answer that question. Young ethnographers have felt pressed to define themselves as "extended case" (Burawoy 1998) or "grounded theory" (Glaser and Strauss 1967) ethnographers. The former get their analysis from existing theories and prevailing debates. The latter are told to enter the field *tabula rasa*, create fieldnotes, then sift to find patterns. The former, while often invoked by sociologists whose politics might be regarded as radical, is a conservative strategy, since it calls for allying with an academic club and rules out finding phenomena that would require wholesale rejection of existing theories. The latter appears to require buying into an indefensible epistemology that imagines a researcher devoid of preconceptions.[7]

A third way to look for the beginnings of analysis recognizes more clearly how ethnographic practice forces a definition of oneself as a researcher. Ethnographic methods for collecting data inevitably require losing oneself in the field. Only in the crucible of fieldwork can the researcher work out who he or she will be to others, and thus what they will reveal; how the researcher will be able actually to present himself or herself, whatever one's pre-formed intentions about the kind of person one would like to be in the field; what will become interesting, and so, something the researcher can naturally observe

and later describe in fieldnotes; what will fall outside of fieldnotes because the researcher failed to perceive it in the first place. . . . These are existential challenges because one cannot plan and control the self that will be enacted when the researcher meets people he or she does not know, in situations never before entered. And the choices are fateful, at least if the researcher honors the hallmark of ethnographic fieldwork, which is to commit to writing comprehensive fieldnotes quickly after leaving each trip to the field.

If the ethnographer goes into the field with the objective of finding out what he or she does not already know, and then follows the practically useful yet absurd injunction to write up everything witnessed, the ethnographer will neither be able to follow pre-recognized themes of interest exclusively nor will he or she be able to "just describe," to remain a-theoretical, to put off analysis until after a set of fieldnotes has been created. The fieldnote-inscription process will already be an exercise in theorizing, a selection of what, according to the researcher's visceral sensibility, the researcher attended to when in the field.

The next step is to raise to explicit awareness the analysis that has already started. For the in situ observer, analysis is implicit in the movement of eyes that catch some things but not others. For the researcher in conversation with subjects, questions will sometimes elicit surprising answers. In the researcher's experience of surprise there implicitly lies a path toward a new analysis.

Most methodological writing on ethnography or fieldwork has been insensitive to the resources for theory building that are built into the existential challenges of description. Thus, on one side, there is a strong tradition that recommends that the ethnographer start analysis by sifting through notes, by hand or with the text-analyzing software that has become available since Glaser and Strauss first recommended "grounded theory." On the other side, under the banner of the "extended case method" that Michael Burawoy has promoted, there is the recommendation to pre-choose a theoretical model and seek exceptions that force revisions in it.[8]

In their substantive statements and in the arguments they have inspired, these two standard views both neglect the path toward developing analysis in the natural way that many have experienced. Whatever the methodological banner they bring into the field, ethnographers tend to focus on the most luminous passages in the data set, the segments of fieldnotes that seem, initially on an emotionally intuitive level, to be especially significant, poignant, displaying amazing creativity by subjects, funny, even absurd (Katz 2001b, 2002a). If one is well read in theory and in the research literature on the substantive area of social life one initially thinks he or she is entering, there is reason to trust that an implicit theoretical sensibility will guide everyday perceptions.

For the novice ethnographer, "grounded theory" and "the extended case" method share a common risk, that of abandoning ethnographic methods either because, in the former case, the craft requirements seem too tedious or in the latter, because the theory carried into the field seems too irrelevant to the distinctiveness of the social life that has been discovered. But with the strategy of starting analysis from "luminous data," the novice may avoid both types of discouragement.

It is notable that the appeal of "grounded theory" and "the extended case method" changes over the research career. Novices may be too diffident about their command of alternative theories to treat fieldwork as an occasion to revise or "extend" a given theory. The "grounded theory" method has the advantage of putting off self-definition as a theorist. With seniority, researchers generally become less inclined to work mechanically through a coding of all their data and more ready to spring into analysis from a particular compelling instance. The single instance will not be enough to sustain a theory but having it in hand changes the researcher's experience of examining a data set, which then becomes a process along the lines of analytic induction, a process motivated by self-protection in which the analyst, leading with chin out, runs into disconfirming data and must, to save face, revise explanans or explanandum to reconcile with the evidence (Katz 2001a). Novices might find ethnographic research less intimidating if they appreciated that theorizing need not emerge from mechanical data inspection or from declaring allegiance with a theory club but is natural to the experience of shaping descriptive attention when in the field.

A Turning Point: Your Problem Is Their Problem

A particularly challenging upshot of the open-ended nature of ethnographic field research is the question: Does the scene not make sense to you because you have not studied it enough or because, although they never acknowledge it, what is going on does not make sense to the people there? The shift from the former to the latter often comes via an epiphany. The researcher realizes that what he or she has been considering a problem in understanding the scene or the people studied is in the first instance "their" problem.

Consider an example from a participant observation study in a Western society at a support agency for Middle Eastern immigrants. The ethnographer enters, knowing that in the population there are multiple national, regional, ethnic/racial, language, dialect and religious backgrounds, and numerous different geo-political historical cohorts. How does she figure out how to classify the subjects, such that she might find that people of one sort act in this way, people of another sort, that way? She is wary of making

attributions based on her perceptions of physiology, dress, or how people speak. If she relies on asking, she won't be able to say anything about those she observes but who escape interview because they are only fleetingly in the site. And those who sit down with her will give an answer but it may be a product of the interview situation: the subject may identify herself in ways that he or she thinks it's best to identify self when talking to someone like the researcher. Yet the researcher can't ignore demographic designation because it is at the essence of the social life in the site.

After agonizing the identifications that must be imputed in order to get on with writing fieldnotes, an agonizing that anticipates the questions about unwarranted labeling that keen colleagues are likely to raise, researchers are tempted to use non-ethnographic methods to code subjects. It is expedient to rely on how subjects have classified themselves when required to fill out a bureaucratic form. But the cost of that solution may be missing the phenomenon that is critical to social life at the site.

The turning point comes when the researcher realizes that the problem of identification is not hers in the first place but a dilemma for those at the site. The research question gets shifted from "How do people of X and Y type act here?" to "How do people here typify others?" How do the people here work out a "good enough for all practical purposes" folk sociological understanding of the demographic composition of the place?

The shift commonly requires a leap of self-confidence. Often a student will be challenged to recognize that her academic or political community had led her to presume that the social world is more "structured" than it is. And the fieldworker may initially assume that respect for subjects requires the presumption that subjects know how their worlds operate. It is often jarring to realize that the problem of figuring out who is whom and how different kinds of people act is due to subjects' ignorance of each other. The matter of attributing identity to others, such that one can anticipate how they will act toward oneself, is the subjects' (everyone's) problem in their relations with each other.

When considering how to make sense of what seems frustratingly inaccessible, ambiguous or elusive about subjects' social lives, ethnographers may wish to think ethnomethodologically. Following the guides they found in Schutz and other phenomenologists, the ethnomethodologists understood that all of social life is founded on tacit practices to sustain intersubjectivity. The point can be stated in infinitely various ways but, essentially, people always act on the taken for granted presumption that "everyone knows what is going on," that "you will understand my action as I understand it from my perspective," that "we share a common history and body of knowledge." Even when people raise questions about whether they and their correspondents share an

understanding, they engage a presumption of intersubjectivity for conducting the interrogation. As it matured into conversation analysis, studies of science, and scattered investigations of the embodiment of behavior, ethnomethodology became the study of the tacit practices that sustain the unstated understanding that "we both understand that it is now your turn to speak," "we both see that blip of light as a star," and that we all hear the same familiar song. But the more "radical" possibilities outlined by the phenomenological tradition include the outsider's understanding that intersubjectivity has no firm ontological basis and is always being finessed into being: people act as if they have a common understanding because to do otherwise would lead to appearing to be incompetent, a troublemaker, or somehow out of it (cf. the formulation in Pollner 1991).

When the ethnographer encounters people acting in seemingly normal ways that follow no norms that the researcher can discover, he or she is faced with the existential doubt captured by the allegory of the naked Emperor. When the ethnographer makes the breakthrough of realizing that his or her problem in making sense of the data is a problem of intersubjectivity that the people studied are finessing, the research may have hit on precisely the magical glue that holds that area of social life together. That the findings of ethnographic research so often are irritating, disturbing, or threatening when presented to the subjects may be due to the inadequacies of the research or to just the opposite, its excellence in hitting upon the tacit agreement to make believe that there is community, there are common values, that a fundamental intersubjectivity undergirds social life in this place (see Vidich and Bensman 1964; Emerson and Pollner 1991).

Shifting from a Holistic Portrait to Causal Analysis

The recognition that your problem, as a researcher, is their problem in living their social lives, is a kind of epiphany. Another kind of transformation is often experienced as wrenching. Audiences or critics may thrust forward the question: What is the variation or difference that you are trying to explain, and with what difference or variation?

Journalists and novelists are not expected to specify explanation but, as contributors to social science, ethnographers are. Ethnography is sometimes justified without invoking explanatory language, much less explicit causal propositions. In one form, ethnographers often warrant their work as a way of giving vulnerable or marginalized people "voice." But why bother doing that unless a lack of "voice" is somehow part of the process of sustaining vulnerability or marginality? That is a plausible causal hypothesis, but without more, only a hypothesis.

The difference between description and explanation is a matter of form more than logic. True, sociology textbooks distinguish between establishing empirical generalizations and inferring explanations. But there are infinite possible descriptions of any scene or set of people. Why describe this versus that feature of the scene unless what is described is more consequential than what is left out? Inevitably, description is implicit explanation.

It is unusual for ethnographers to think explicitly about explanandum and explanans. The words themselves may trigger a rush to the dictionary. Still less are ethnographers likely to formalize the ruling out of rival causal hypotheses. Usually, practitioners rely on warrants for their research that are based on differences in description between what they find and what others have said or written about the people studied. They contradict prejudices by showing "what is really going on." Some ethnographers will be content to write for readers for whom it is enough of a contribution to show how a distant, curious, enigmatic social world works.

In recent decades, a strong warrant for ethnographic research has been acknowledged by quantitative researchers who build explicit models of causal explanation. Understanding that variable-based explanations are not adequate as descriptions of social life, they welcome ethnographic descriptions. The beckoning terminology has recently been "mechanisms": ethnographers can show "how" explanations of "why" work. But, if "mechanisms" carves out a home for ethnographers, the risk is conceding ownership of major explanatory power to "structural" explanations: "all work on social mechanisms assumes that mechanisms are the gears in some social machinery and thus stand in a relationship of lesser to greater vis-à-vis the causal effect they bring about" (Gross 2009, 362). But if, as Blumer insisted, all of social life is processual, every moment of social life is a mechanism. Suggesting obdurate character and mindless repetition, "mechanism" is imprecise at best.

Ethnographic descriptions are not just valuable for showing how or why explanations work, they offer unique causal explanations, albeit as emergent social psychological processes or in varying "iterations of meaning making" (Tavory and Timmermans 2013). Take the example of evicting tenants. One can produce a great ethnography by showing how people who are evicted go through and experience the process, and how through interactions with landlords the collectively arranged social process of eviction works. But why should readers care? There are various answers but the most tempting is that the eviction process is consequential for "reproducing" poverty, that is, for making those evicted poorer than those not evicted or poorer than they would have been had they not been evicted.

But how to show that evictions themselves promote poverty? Most evictions are the result of not paying rent, which is itself a consequence of

poverty. It is a nontrivial rival hypothesis that whatever led a poor tenant not to have the money to pay the rent also continued to undermine the evicted tenant's post-tenancy economic status. Why, then, do we need rich descriptions of variations in how people get evicted? Perhaps to set up a social psychological explanation. Tenants with similar pre-eviction statuses (economic, demographic, history of tenancy and prior evictions) may have different fates, some evicted, some not, because landlords are more patient with some tenants than with others. The relations between landlords and different tenants come into play when some cannot make the rent and ask for more time; when landlords hear complaints about tenants from neighbors, the police, or other third parties; when landlords deal with costs in repairing or maintaining units, etc. Landlords' differential inclination to "work with" different tenants and tenants' differential inclination to show consideration for (or bribe or elicit sympathy from) landlords may be because of some emergent personality expression on each side or a unique interaction of personalities: because since the start of tenancy the tenant and/or the landlord's biography has moved into a new phase and so sets up a different perspective when interacting with the other; because, independent of tenant and landlord personalities and biographies, third parties (the police, creditors, relatives, other business commitments, etc.) change the pressures on landlord and/or tenant; because some tenants more than others are interconnected with other current, past and potential future tenants, so that some can secure replacement tenants, find alternative housing, and leave voluntarily when they can no longer pay; because some tenants or landlords misperceive what the other is doing, thus building an "iteration of meanings" leading to eviction; etc. And these differences among tenants, once they have emerged during tenancy, may continue to differentially affect the fate of those evicted and not evicted. If so, an eviction reflects but does not create a causal change that has already occurred.

The ethnography alone is likely to be insufficient to prove that eviction itself, independent of any other influence, contributes to poverty. Other forms of data will be necessary. In particular, to show that eviction rather than something else caused negative post-eviction consequences, data might be worked up to show that differences in well-being for those evicted/not evicted (or the same people in pre-eviction and post-eviction life phases) hold up when biographical or ecological background factors representing rival explanations (income, age, education, employment status, family structure, criminal record, prior eviction and tenancy history, psychological status, etc.) are held constant.

Even then, the very excellence of an ethnographic description tends to undermine faith in background factors as measured by the bureaucratized social tracking institutions that produce the data used in statistical analyses.

Judicial records, measures of educational achievement, determinations of economic and social welfare need—all these official descriptions recognize individuals in atomized fashion rather than in their networks. They ignore personality idiosyncrasies and extract individuals from waves of contextual pressures. Paradoxically, it is by systematically ignoring just what ethnographic fieldwork is uniquely qualified to describe that various bureaucracies produce the fragmented descriptions of individuals' lives which will form the basis for the variables that may be controlled. By showing how much can be learned by looking at the personalized interactions that lead to eviction or decisions to pass over possible eviction, a good ethnography will show that what determines who gets evicted is something in the pre-tenancy or pre-eviction lives of tenants that organizations' case records fail to capture. Once that possibility is contemplated, the next thought is that the differences among tenants that emerge during tenancy do not end when the eviction process is completed. If so, even after using standard background variables to control for differences in tenants' pre-eviction lives we would expect to see differences in well-being as between those evicted and those not, or the same people before and after eviction, independent of any effects of the eviction itself (cf. Desmond 2012, 2016; Desmond and Kimbro 2015).[9]

In short, if the rich data in observational fieldwork ethnographies are explicitly addressed with causal questions, those questions will not easily be put to rest. Asking students to restate their claims as causal propositions—claims not about what subjects are doing or even how they are doing it but claims about *why* subjects act as they do—is likely to be painful. But at some point in their careers, perhaps at the worst possible first time—when confronted in job interviews by quantitative researchers—ethnographers may be asked questions along the line, "What is the variation you are trying to explain?"

Ethnographers define themselves not so much by the answer they give as by whether or not they work up a response. In fact many have found that it is not necessary to answer. Many prominent careers have been developed by ethnographers who, following the iconic genre, let their descriptions of a few people radiate out in implications for causal issues that readers sense but are not called to explicate. (For an extreme example, see Williams 1992.)

More commonly, the ethnographer will address causal questions as a secondary matter, taking them up in introductory or concluding sections and in tangential arguments pursued in footnotes. Or, instead of proposing generalized causal explanations, the author will use ethnographic description to propose a political-moral, emotionally evocative characterization (exploitation, symbolic violence, the reproduction of inequality, the carceral society). Or the author may organize the ethnographic narrative to negate prevailing

explanatory ideas. If the ethnographer shows that in all the lives she or he describes, poverty is *not* due to moral failings, or that the better overall characterization of the neighborhood is that it is *not* a socially disorganized "slum," the ethnographer offers something significant, even without expressly applying alternative hypothesized causes across variations in outcome to show the superiority of the author's understanding of why some subjects but not others are homeless, committing crimes, or obnoxious with neighbors. A contribution to explanatory social science is offered, but not in a way that may satisfy colleagues who understand progress as offering a new explanation of why people do what they do.

Culture Lies. Where Does Your Loyalty Lie?

At any given time, some part of the population of every society is being demonized. The ethnographer can make a guaranteed scholarly contribution by documenting that the realities of the lives of those stigmatized, stereotyped, or otherwise collectively denigrated do not fit the images conveyed by entertainment, news media and politicians.

There is every reason to believe that the systematic distortion of social reality to serve political and popular cultural institutions will keep ethnography eternally alive as a valuable contributor to knowledge. But the very security of that mandate makes it a great challenge for the researcher to decide whether to oppose, and thereby allow one's life to be shaped by, the rhetoric of demagogues, the distortions in marketing campaigns, and the tropes about social life that journalism sends to mass audiences. The battles are worthy but do they define the maximum contribution you can make?

The choice, whether made reflectively or not, is real and the problem is severe not just because culture lies but because in lying it masks other patterned realities. Over against the sociology of social life invoked by popular culture—the typifications of people, the causal claims invoked to bolster policy stances—there is a sociology of social life to be discovered by the researcher. Devoting oneself to undermining the stereotypes of popular and political culture risks passing up the possibility of discovering a social reality that culture fails to acknowledge, not just the social reality that culture distorts by sensationalizing or politically exploiting but one that goes on through everyday social processes untouched by the debates of the day. One can counter images of "reefer madness" by describing the normality of marijuana users or sort out evidence on users' histories to locate the stages in becoming a marijuana user (Becker 1953). The latter not only does more, by offering a positive causal explanation, it is also a cooler (emotionally reserved) way to achieve the former objective.

The choice is whether to study phenomena in relation to the folk categories of popular and political or to study phenomena even if, in the academic, political, and social viewpoints of the day, they are insignificant. When devoting yourself to showing that people live in ways that undermine popular prejudices, are you being presumptuous in thinking your work will matter or humble in joining a movement which, while speaking truth to power, already has many others on the march? If you try to answer causal questions that no one is asking, are you being arrogant in prizing your own intellectual concerns or humble in working on matters of no apparent political/moral significance? Whether political relevance is the path to sainthood or the devil's temptation is for each to decide. [10]

Orienting to the Mistakes Corrected versus to the Perfection Yet Achieved

In one way or another, the fantasy of a future state of perfect knowledge guides research that proceeds from fixed designs. Researchers invoke an image of perfect knowledge when they describe exactly how much variation has been explained or state correlations as a finite point somewhere between 0 and 1, where 1 would be a un-exceptioned relationship. Similarly for those offering statistics that display significance at the .05 level, which is not perfect but is to be understood in relation to a conception of infinite specificity. Similarly also for a controlled experiment, which may only be approximated in quasi-experimental conditions but which still guides the assessment of what has been achieved. Quantitative researchers do not expect to reach perfect knowledge but their methods are shaped so that they can assess, with formal precision, how far they have come toward that imagined resting place.

Ethnographers often feel blindsided when reviewers ask that they justify their claims. Implicitly the discussion becomes shaped by touchstones of perfect knowledge. The ethnographer is likely to be nonplussed, and for good reason: a perspective on endpoint perfection makes no methodological sense in ethnographic field studies.

How are ethnographers haunted by images of quantitative evidentiary standards? An ethnographer may be faulted for bias in what he or she formally or effectively offers as a sample. A participant observer who wishes to make claims about social life in a neighborhood typically will not have drawn a sample by procedures that gave everyone in the area an equal or weighted chance to become subjects. "Sampling" by participant observers is usually done by chance, convenience, "snowball" or other improvised methods. It is increasingly common for ethnographers of crime and poverty to be criticized

for sustaining negative stereotypes, even if or precisely because they had not make explicit claims about the population to which their findings should be generalized (Small 2015).

Consider an ethnographic study that describes the social lives of a handful of subjects as pervasively shaped by the criminal justice system. What are the researcher's options for finding out if the portrait that fits the observed subjects also fits neighborhood residents more generally? The researcher, with the assistance of one subject, knocks on doors to ask about outstanding warrants, probation, and other criminal justice oversight, using the most casual procedures. The resulting "sample" cannot logically be compared to a "population" since no effort was made to ensure that each member of the population would have an equal or weighted chance to enter the sample. In most cases of informally conducted ethnographic "surveys," the population will only have been vaguely suggested (see Cohen 2016, commenting on A. Goffman 2009).

The survey methodologist directs data collection with a fixed design that sets up a comparison between the sample selected in fact and what an infinite number of samples might reveal about the population sampled. For a researcher immersed in social life, innovating contacts, questions, observational positions, and other means of data collection on the fly, situation by situation, under the pressing need to keep up with evolving narratives in subjects' biographies, the practical choice is not one of drawing a sample that can approximate a randomly executed survey, which would require formally defining the meaning of households and the criteria for counting someone as a resident, controlling when and how doors are knocked, training interviewers to ask the same questions about criminal justice contacts the same ways, etc. The researcher might abandon ethnographic data gathering for an extended period to work up formal survey research procedures, but subjects' rapidly changing lives might not accommodate. And most academic ethnographers, in particular novices, will not have command over resources to pay others to do the survey for them.

The practical choice is whether, in order to understand the patterns in a population beyond the handful of people who are the main subjects, it is better only to sniff clues from in-depth fieldwork contacts or, in addition, to knock on as many doors as is feasible. Doing the latter will almost always revise the researcher's picture of the area, negating some hunches, revealing some limitations in what the people one knows through participant observation know about the area. That's progress, if not what survey researchers would recognize as evidence. But is it not wiser to knock on doors, however unsystematically one can, even at the risk of criticism for not having properly drawn what specialists would consider a "sample"? In fact the casual

knocking on doors is no different than the fieldworker's other methods of data collection, which are not governed by a pre-fixed design and yet pay off in recognitions of prior misunderstandings.

In effect, the fieldworker's discoveries are alterations of a model of the scene or of the population studied that he or she brought into the research. That initial model, which may never have been formalized beyond sketches in proposals for funding or admission to PhD candidacy, is likely to be forgotten as soon as the researcher encounters the exigencies of interacting with subjects. Yet changes in the model, if they were to be examined as such, would indicate progress. Implicitly, each change rules out rival hypotheses or models of the scene. If explicated, as has only occasionally been done (see the classic of analytic induction, A. R. Lindesmith 1947), the modifications formally define the stages of improvement. They also change the search for the next case that will contradict the prevailing model, thus undermining the pre-fixing of data collection methods that is logically necessary to produce a data set which can be precisely assessed by measuring the distance from perfect knowledge.

When considering evidentiary questions raised by the 4 Rs of standard methodological discussion (representativeness, reliability, reactivity, replicability), the ethnographer's challenge is to shift focus to the distance that the research has in fact traveled (Katz 1982, appendix). The same turn is constructive when ethnographers find themselves haunted by qualitative images of a perfect reflection of empirical reality. Fieldnotes often pale in comparison to transcripts of audiovisual recordings of social life (see the improvised in-class experiment reported by Fine 1993, 278). If the ethnographer examines subjects' conversations as transcribed from recordings, the utterances as described in ethnographic texts will at best be truncated and ripped out of context. Should the researcher concede that when quoting subjects whose talk was not transcribed from a recording, he or she "lies"? (Fine 1993). If observational field research is such a dirty business, why not choose a less morally compromised way to study society?

Questions about the accuracy of fieldnotes need not be addressed by the ethnographer through an assessment of the distance between talk as recorded in fieldnotes and talk as reviewable in an audiovisual recording of the event. The alternative is to retrace the improvement of hearing that the ethnographer has achieved over the course of the research. Initially, the ethnographer may not have heard or appreciated the significance of "crock," a phrase that medical students often used when describing patients. Following the pragmatist's suggestion that culture is created to solve problems, the researcher comes to understand the systematic meaning of the phrase. The patient whose complaints are a "crock" (of shit) is useless for the learning of physical medicine that students seek (Becker 1993).

There is no escape from questions about how well ethnographers gather and present their data, whether they lie, produce a good sample, sufficiently show how they interact with subjects so that their influence in producing subjects' behavior can be assessed, etc. But, unlike other forms of social research, it is unnecessary and unproductive to evaluate ethnographic methods independent of an examination of the substantive claims being made. A survey, an experiment, a conversation analytic study based on recorded data can be assessed as good or bad, independent of the substantive questions addressed. Not so in ethnography. In addition to how well descriptions fit the requirements of social ontology (Katz 2002b), how well an ethnography represents a conversation depends on assessing how well the ethnographer has improved upon earlier versions.

And for good substantive reason. People in everyday life often use indirect, vague, metaphoric, even seemingly self-contradictory language to speak about problems that they cannot avoid but have collectively agreed not to resolve. Not being more explicit, not confronting tensions is often the key to holding a community together. (For a historian's appreciation of the methodological challenges of documenting what was carefully never directly said, see Certeau 1988.) As opposed to the researcher who studies talk only through recordings or transcripts of recordings, the participant observer's hearing can draw on knowledge of how subjects, in uttering certain phrases, are drawing on distant situations and phases of biography. Having been with subjects in other times and places, the ethnographer can hear what is said against the background of what might be but is not said. As noted above, the researcher's realization of why he or she had not been able to grasp the significance of a local phrase will often be a decisive turning point: the ethnographer appreciates that what had been presumed to be the researcher's problem is precisely the way subjects finesse a problem central to the social organization of their lives.[11]

Something similar distinguishes ethnographic from fixed design interviews of the sort used in survey research. The ethnographic interviewer listens to what is said but also listens for moments in which the interviewer becomes elusive, rhetorical, generalizing . . . specifically not saying something. The ethnographic interviewer risks the embarrassment of long pauses so that pressure will increase on the subject to bring out what just now had been hidden. When something surprising is revealed, the substantive concerns that disaster the researcher initially brought into the interview may be put aside or even abandoned. Listening guides the improvisation of next questions; hearing becomes a progressive, interactive achievement (Weiss 1994).

Subordinating a Mirrored to a Pragmatist Understanding of Truth

Ethnographic field researchers will find more encouragement if they focus on the prior steps in the research path and the progress they have made from earlier formulations, and resist focusing on the gap that remains to reach a perfect state of knowledge. But it is too simplistic to claim that ethnographers are more oriented to the research past then to the future. In another sense, the ethnographer is well-advised to subordinate a focus on what happened in the field and give preference to the future trajectory of the research.

About one hundred years ago, in one of the most contentious and misunderstood areas of his thought, William James argued against a "reflection" and for a pragmatist theory of truth (James [1909] 1975). The reflection theory of truth focuses on the relation between a proposition and the past social life it purports to represent: how well do these descriptions mirror what happened? Judgments about the validity of elections depend on a reflection theory of truth: vote tallies claim to reflect a collective will as expressed in a given time and place. Newspapers that aspire to be "journals of record" also claim to mirror reality: if a news article contains quotes, the words should accurately reflect what was said. Criminal trials work toward verdicts that find what the defendant did in a particular past time and in another place, "beyond a reasonable doubt." Elections, journalism, and law courts conventionalize and institutionalize versions of social reality that seek to reflect a past state as an end in itself, without regard to what the victor will do with the power that has been authorized, what readers will think of the news report, what sentence the judge will pronounce after a guilty verdict. The mirroring of a past reality is central to what we mean by honest democracy, objectivity in the news, justice. Three cheers for the reflection theory of truth!

James argued that the scientific way is a pragmatic search for truth, which in the final analysis shows its value not in specifying how well a proposition reflects a past reality but as a tool useful in better comprehending and working through the next problematic situation encountered. If some researchers formalize their procedures so that they will be able to claim with quantitative precision that their data reflect the world they had entered, they do so as a means to an end, as a strategy for developing confidence in future applications of their findings. For a knowledge-building enterprise that works toward generalizations beyond the time/space of any study, what is the point of showing that relationships described in a "sample" represent, at a specifiable confidence level, the time-spaced defined "population" that was sampled? To increase confidence, to a quantitatively unspecifiable level, in the application

of the tested proposition to the "universe" of persons or events encountered in the future (Morrison and Henkel 1970). If ethnographic field researchers cannot formally report how they selected subjects or produce their fieldnotes for inspection, they are still concerned to discipline the reflections in their texts of the scenes they studied because they have no other basis for confidence for the verification, in future applications, of their claims about how social life operates.

There is no fundamental difference between quantitative and qualitative researchers in this regard. Both seek to perfect past-oriented reflections as a means to increase the chance that others will find their claims valid in future applications.[12] But because methodological rhetoric is almost always about the reflective truth of a study—because the *raison d'être* of methodological conventions is to underwrite claims that a proposition mirrors what existed in a given time and place—it is easy to forget that the essential warrant for a social science text is to set up a means–end relationship between reflections of prior social life and future applications.

"Replication" is especially informative in pointing out the subordinate status of a reflection theory of truth in research, whether the data are developed from fixed research designs or through interventions and observational perches improvised in the field. Replication is not a matter of turning back time or creating a biography of the research project, much less administering the survey again or bringing the same subjects back into the same lab and running the protocol again. If after applying implications of the study to new data, replication fails, then, and usually only then, a new investigation into the original research procedures may be launched to discover if something significant was not described in the formal protocols.

Now consider how the standard of replication applies to the work of Erving Goffman. Goffman offered a multitude of propositions about social life. For example, he discovered the "with," a relationship that people in public perceive as organizing the movement of others and use to organize their own response (E. Goffman 1971, 19). Pedestrians comment to each other about and tailor their movements around others they perceive as "with" each other without knowing the origins or nature of the "with," which may have been formed in family, work, neighboring, romantic, or short-term utilitarian relations. Characteristically, Goffman does not report where or when he observed whatever gave rise to his concept. Goffman left a few scant reflections on fieldwork practice (e.g., E. Goffman 1989), which were in the nature of "how to do it," not methodological arguments specifying, much less justifying, how he worked in any specific study.

We cannot replicate his finding in the sense of going where he was and lying in wait to observe the "with" he witnessed. We intuit that it would not

make sense to try to track down others who were present and verify that those Goffman observed were indeed "with" each other. We care whether he is right or wrong, but we do not care that there is no evidence to show that his description reflects a prior time and place.

We validate his image of society by using it in situations we encounter (or recall) after reading his text. I am confident he is right because, when I encounter a number of people walking in my direction on a mountain trail, I greet the first but not the others—I don't in machine-gun style execute a series of nods or salutations to each as he or she passes by. I organize my conduct on my understanding that they will have the understanding, as they register my response to the "first" and my nonresponse to the others, that I understand they are "with" each other. Goffman's finding works for me; it is so readily applicable to situations post his observations that any documentation he might work up about where, how, how often and with whom he observed a "with" would be wasted. The utility of his "discoveries" is such that we put aside conventions for documenting how he developed his data set. We don't need to see his fieldnotes. The "fact checking" that journalists demand often makes no sense in ethnographic work.

It matters that the ethnographer's portrait reflects what went on in the scenes studied, but usually not as an independent matter of establishing the truth of the claimed reflection. The researcher, whether qualitative or quantitative, looks back at what happened with care because that is the only resource for better anticipating what will be found in the next scene encountered. The daily newspaper, the criminal trial, and quantitative measurement instruments are social institutions that are operated by people who agree to put aside generalizing to future times and places, at least temporarily, in order to use standardized conventions like fact checking, cross examination and the systematic implementation of a pre-fixed research design, to document what happened in a particular past time and place. The ethnographic fieldworker generally lacks conventions for representing the relation of his or her portrait and the scenes of fieldwork immersion, even as he or she develops resources for anticipating what will happen in the next instantiation of that social world.

Ethnographers inevitably will find themselves in a crucible in which they are pounded by two conflicting pressures. On the one side, they will be tempted to neglect the language of generalization in favor of composing a text that mirrors the people they came to know. It will have taken so much work, intellectually and emotionally, to develop working relationships with subjects that one is tempted to treat them as precious, to put in secondary position the original, generalizing rationale for the research and make one's priority the composition of a text that holistically and in humanistic detail reflects the people and scenes observed. On the other side, ethnographers will

be tempted to care about potential future uses as they contemplate the tool their text may become when picked up by other hands, whether those others are in the author's research community or in the general public. A future orientation tempts ethnographers to compromise the readers' abilities to judge how well the portrait given in the text reflects the scenes studied. An ethnographer may leave out details about how he or she did the research, on the belief that writing a more detailed history of the data gathering will be unnecessary and distracting for future tests; or for fear that otherwise the text would be misused to reinforce negative stereotypes. Anticipating the risk of harm to individual subjects, perhaps the author will destroy fieldnotes so that they cannot be subpoenaed. And there is always the temptation to suppress disconfirming evidence to avoid undermining acceptance of the ethnographer's generalizations.

Such compromises of the ability of readers to determine how the text reflects the people and places studied put a heavy responsibility on the author and/or the research community to determine through subsequent research whether the portrait works in other times and places. With Erving Goffman's studies of behavior in public places, that burden was easy for next researchers to carry. With Alice Goffman's immersive study of how young men in a high-crime area lived "on the run" from police, lovers, and peer antagonists, not so much.

The ability to replicate findings in other times and places is highly variable across studies. In one respect, the ability to replicate depends on what the reader takes the generalization to be. For readers of Alice Goffman's book, is the takeaway that the police illegally persecute young black men; that in many urban low income areas, a significant number, a lot, or a small but especially impactful subset of young men live lives "on the run"; that in such areas there are networks of young men who resist and succumb to law enforcement pressures as a collectivity, so that urban crime rates are constantly shaped by wavelike pressures? There are different practical challenges in replicating each of these possible generalizations. Another, related variable is whether the type of event or behavior that is the subject of generalization happens often or rarely. Minimally, Goffman is claiming that in virtually any large US city, a researcher will be able to find a subset of young black men living lives "on the run." Other studies, conducted from other perspectives, provide support.[13]

Putting more weight on a reflection theory of truth and doing fact checking makes more sense when events are rare or unpredictable. A fatality-producing heat wave as studied by Eric Klinenberg (2012), for example, does not happen predictably. It is difficult to set up a study to witness what happens in the next. The possibility of replicating Klinenberg's study through application to subsequent heat waves was also impaired by the scandal which the

success of his book helped promote. City leaders in Chicago and elsewhere were pressured to put into place protective measures that did not exist in 1995, when the ethnographer was in the field. So it made special sense for Mitchell Duneier to do "fact checking" almost ten years later. Duneier (2006) personally inspected particular addresses and interviewed people who knew those who died during the 1995 heat wave and obtained death certificates in order to see whether physical conditions and social relations were consistent with claims made by Klinenberg that the deceased had died alone.

It is notable that the academic field of "history" by definition is committed to a reflection theory of truth, even for those who understand that over time and in response to contemporary pressures, the mirror itself is always changing. Some ethnographies are essentially historical works because of the uniqueness of the events they study. For example, Diane Vaughan (1996) wrote an exceptionally detailed historical ethnography of a rare event, the Challenger space craft disaster. Reconstructing exactly what happened leading up to the catastrophe was the indispensable basis for developing a sociological explanation. Vaughan could anticipate that any errors in reflecting records would predictably be noted by highly motivated, organizationally powerful readers outside of the sociology research community, in the federal government's space administration and in congressional oversight committees.

The recent development of a large public audience for academic ethnographies means that the ethnographer may find herself writing for a research community that will assess the text as a tool for ongoing and future research *and* for a public not engaged in ongoing, communal research in the substantive area she addresses. The public audience will be familiar with the reflection theory of truth, to some extent from exposure to the formalities of quantitative research but more powerfully from political-legal controversies focused on—to recall a phrase from the Watergate era—"what did he know and when did he know it?" Ethnographers may become dizzy if they do not anticipate the different demands for anchoring their text in the past and for facilitating application of their images and explanations in new times and places. Making the pressures especially difficult to manage, it is the novice more than the senior researcher who is likely to be spun around in the crucible of ethnographic writing.[14]

Ethnographers in the Crucible: Existential Challenges Shaping the Research Identity

"Existential challenges" in research encompass the choice among equally valid genres as well as fateful decisions that cannot be worked out in advance but must be made on the fly, in nonreflective ways, within the contextualized

pressure of social interaction. Fatefulness develops in one of several ways: by changing the lens that will make all future challenges appear differently; by creating commitments that cannot be escaped; by abandoning opportunities that cannot be recovered. Ethnographers recognize these challenges repeatedly, albeit indirectly, in the wrenching process of leaving a contemplative environment to enter the field and then again when reversing the process. These shifts can be made mechanical and timed to a calendar but they entail gut decisions: they cannot be rationalized in advance. No one else can authoritatively decide that you are spending too long in the field or at the writing desk, the two settings between which the ethnographer must repeatedly migrate. Existential challenges require acts of faith because they draw on the entirety of one's personality.

Whether to be reflective about alternative pathways is a constant challenge in doing ethnography. At each stage of the research project, there is the possibility to plan explicitly or to act spontaneously. One can select a site by comparing census descriptions (St. Jean 2007) or by striking up relationships in a place encountered incidentally when living everyday life (Duneier 1992). Narrative themes can be carried into the field from a formalized prospectus that is used to guide observations, culled out of a set of written fieldnotes through an electronic search, and/or progressively teased out of the stories that elicit the greatest interest when related to friends.[15] The inclusion of a proposition can be assessed qualitatively, through judging that when no further exceptions have been encountered the data has reached "saturation" (Morse 2004) or by numerically weighting evidence according to an explicit criterion (Becker 1958).

Ethnography's greatest challenge is the methodology's distinctive freedom. It is possible to work successfully as an ethnographer without reflectively choosing among alternative genres; without working up a defense of one's evidentiary base that quantitative colleagues will understand; without recounting the revisions in hypotheses that were made on the way to the final formulation; without knowing or showing where analysis began; without understanding where shame-provoking awkwardness in the field comes from; without explicating the turn from obsessing on one's own deficient understanding to documenting subjects' deficient understandings of each other; without making explicit how the text facilitates replication.

Is ethnography's openness experienced as daunting, a frightening inability to control your working identity? Or as liberating, an inspiring opportunity to treat yourself as the most important tool you take into and develop in the field? Or is treating the study as a challenge to refine one's perceptive sensibilities a route to self-indulgence and hubris? Negotiating between pressures and temptations to treat subjects or oneself as too precious, the ethnographer finds field research to be a constantly formative crucible.

Among research methodologies in the social sciences, ethnography condemns its practitioners to the burdens that come with its uniquely democratic nature. Anyone can become an ethnographer. No need to arm oneself like Perseus with powerful tools fashioned by the research community before seeking to rescue truth from political demagoguery, from the sophistry of academia's theorists, or from the obscurantism that passes for social science when formalized research conventions are taken as ends rather than means for advancing knowledge. Perhaps no need for formal training of any kind.

The dramas that grow out of lives shaped in the nitty gritty of ethnographic fieldwork coalesce into a generation's tales. Is there more wisdom or more madness in contemporary ethnographic research than there was in 1970, in 1920? The answer might well be more significant than the resolution of any substantive academic question that individual authors address.

For one hundred years, the journey out and back that is participant observation research continues to draw young investigators. Becoming an ethnographer is now a biographical process that is, to an unprecedented extent, subsidized by academic institutions and attended by public audiences fascinated with how fieldworkers honor and abuse their privilege. We have more reason than ever to examine the careers of experience forged in the crucible of ethnographic fieldwork.

Notes

1. The bedrock for the course was Emerson, Fretz, and Shaw 2012. Most of the time I taught with Robert Emerson, but once with Mel Pollner and in recent years several times with Stefan Timmermans. I apologize for the inevitable intellectual theft and, to the extent of my powers, grant immunity to my colleagues.
2. A fictional case can be used effectively for modeling. Rosett and Cressey (1976), a sociologist and a prosecutor-become-law professor team, collaborated to

create a composite case which they follow through the criminal justice system to show the highly interactive, multistage process through which plea bargains are worked out. In effect, they model the modeling of the system done by each actor, from police officer to prosecutor to judge to probation officer, as each acts on an understanding of what other officials, whom they may never meet, have done and will do with the case.

3. In his iconic book on unhoused used book sellers, Duneier includes a chapter that describes and analyzes finely differentiated, gender-charged interactions with female passersby. Notably, this comparative analytic chapter (Duneier and Molotch 1999) was written with a collaborator and was more suitable for journal publication than was the wide-ranging study as a whole.

4. As sociology has become more international over the last generation, through an increase in foreign-born students enrolling in US graduate programs and in funding for overseas research trips, it has become common for sociology's ethnographers to gather data abroad. Relative to the access enjoyed by ethnographers when they study sites nearby their universities, for ethnographers abroad long immersions in fieldwork are more difficult to arrange. For fieldworkers who can only arrange a semester's absence from professorial obligations for immersion in a foreign research site, as opposed to the lengthier personal involvement that PhD-seeking ethnographers put into firsthand domestic data gathering, the modeling genre in the form of an "extended case study" has become especially appealing. If the model is sufficiently strong, it may not be necessary to learn the local language. See, e.g., Burawoy and Lukacs (1985), where two months of participant observation in a Hungarian factory was sufficient to set up a comparison with data from ten months of working in a US factory to the end of advancing the prevailing models of how capitalist and socialist work is socially organized. The more limited the anticipated involvement in the field, the riskier it is to rely on "the discovery of grounded theory."

5. Doing fieldwork in the iconic genre makes one especially vulnerable to morally compromising pressures, which become intense when the fieldworker is studying criminals (Venkatesh 2008; A. Goffman 2014) or police (Van Maanen 1983). How these are handled, by the researcher in the writing and after publication by reacting readers, has become a powerful contingency in ethnographers' careers.

6. "Field observation has always been a torture. . . . I have usually been hesitant in entering the field myself and have perhaps walked around the block getting up my courage to knock at doors more often than most any of my students (I have been doing it longer)" (Hughes 1984, 497).

7. This limitation is avoided by Charles Peirce's concept of abduction, which avoids both the solipsism of deductive research and the naïveté of inductive method (Tavory and Timmermans 2014).

8. The scholarship behind Michael Burawoy's (1998) version of the "extended case method" is curious. "Extended theory" was coined by Max Gluckman, whose research career developed before the formulation of "the discovery of grounded theory strategy" but closely resembled Glaser and Strauss's approach. Gluckman followed the inductive logic of jurisprudence as developed in the

Anglo-American common law tradition. Trained as a lawyer, Gluckman sought to find patterns in masses of laboriously collected descriptions of conflict cases as adjudicated in Africa outside of colonial courts (Gluckman 1955). Van Velsen, another source claimed by Burawoy, is an equally perplexing precedent. In parallel with the challenges that ethnomethodology and social interaction analyses were posing for structural-deterministic theory, Van Velsen (1967) emphasized the importance of focusing on emergent situational dynamics. An image of a researcher entering the field with a favored theory and concentrating on finding cases to extend it does not describe the inductive methods that Gluckman and Van Velsen practiced.

9. If the background analysis shows the causal effects of eviction, why, beyond stirring readers' sentiments, do we need the ethnography? Excellence in ethnography depends on registering actors' understandings as they change in social interaction. Excellence in statistical research relies on the fragmentary characterizations of individuals' lives made not for the purpose of capturing the meaning of events for those counted but for the institutions creating the official records. Ethnographers face the challenge of either taking a doctrinal position on the compatibility of the two forms of inquiry or showing in detail how to reconcile ethnographic and bureaucratic descriptions in the particular cases studied.

10. Along with Howard Erlanger at Wisconsin, I faced this challenge early in my career when studying lawyers who were assisting poor people in the United States. The then–vice president, Spiro Agnew, was denouncing the lawyers as "radicals," which our research contradicted (Erlanger and Klegon 1978; Katz 1978). Donald Trump is only the latest in the endless series of public figures who threaten to limit intellectual freedom by seducing ethnographers into opposition.

11. For an example of an ethnographer making a breakthrough by resolving a haunting paradox, that people in a neighborhood who claim "I stay to myself" in fact do not, see Murphy (forthcoming).

12. If ethnographers talk with their quantifying colleagues they might be relieved to find that at least some survey, experimental, and demography researchers are sensitive to Whitehead's fallacies, which, applied in this context, would include treating a study's reflection of a past reality as an end rather than a means (Whitehead [1925] 1967)

13. See Jill Leovy's (2015) book of investigative journalism which shows similar patterns of offenders living on the run from interrelated pressures emanating in a web of peer, ex-lover, and police antagonists. Leovy's investigations in South Los Angeles were done at about the same time as A. Goffman's but show a view from the police side, which helps overcome the doubts that A. Goffman's account was distorted by her anti-police sympathies with offenders. And, if the generalization to be tested is illegal police manipulation of offenders' vulnerable status, a recent (early 2018) revelation about an abusive police unit operating in Baltimore adds to a lengthy list of confirming cases.

14. Why? Because the dissertation career phase is especially accommodating of long, deeply immersive field research, as was the case with A. Goffman, Klinenberg, and Vankatesh. And because novice ethnographers may draw on phases of life

they lived through before they became "ethnographers," i.e., before entering graduate school, as was the case with Contreras (2013), Becker writing about marijuana use, and Irwin (1970), among others.

15. My dissertation analysis (published as Katz 1982) was shaped by my appreciation of Howard S. Becker's work on adult careers; an effort to develop interaction sensitivity by reading Erving Goffman; the charm of one of Simmel's essays (Simmel 1971 [1908]), which was too abstract to be useful as a daily guide; a long and tedious process of manually annotating photocopied fieldnotes in ways that software has since made incalculably more efficient; and, most decisively, by the stories from the field that made John Kitsuse laugh.

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References

Anderson, E. 1990. *Streetwise: Race, Class and Change in an Urban Community*. Chicago: University of Chicago Press.

Becker, Howard S. 1953. "Becoming a Marihuana User." *American Journal of Sociology* 59 (November): 235–42.

Becker, Howard S. 1958. "Problems of Inference and Proof in Participant Observation." *American Sociological Review* 23 (6): 652–60.

Becker, Howard S. 1982. *Art Worlds*. Berkeley: University of California Press.

Becker, Howard S. 1993. "How I Learned What a Crock Was." *Journal of Contemporary Ethnography* 22 (1): 28–35.

Becker, Howard S., and Blanche Geer. 1957. "Participant Observation and Interviewing: A Comparison." *Human Organization* 16:28–32.

Becker, Howard S., Blanche Geer, and Everett Hughes. 1968. *Making the Grade: The Academic Side of College Life*. New York: Wiley.

Becker, Howard S., Blanche Geer, Everett Hughes, and Anselm Strauss. 1961. *Boys in White: Student Culture in Medical School*. Chicago: University of Chicago Press.

Bittner, Egon. 1967. "The Police on Skid-Row: A Study of Peace Keeping." *American Sociological Review* 32 (5): 699–715.

Burawoy, Michael. 1979. *Manufacturing Consent: Changes in the Labor Process under Monopoly Capitalism*. Chicago: University of Chicago Press.

Burawoy, Michael. 1998. "The Extended Case Method." *Sociological Theory* 16 (1): 4–33.

Burawoy, Michael, and Janos Lukacs. 1985. "Mythologies of Work: A Comparison of Firms in State Socialism and Advanced Capitalism." *American Sociological Review* 50 (6): 723–37.

Cavan, Ruth S. 1928. *Suicide*. Chicago: University of Chicago Press.

Certeau, Michel de. 1988. *The Writing of History*. New York: Columbia University Press.

Cohen, Philip N. 2016. "Survey and Ethnography: Comment on Goffman's 'on the Run.'" SocArXiv. June 5. osf.io/preprints/socarxiv/hdtgg.

Contreras, Randol. 2013. *The Stickup Kids: Race, Drugs, Violence, and the American Dream*. Berkeley: University of California Press.

Desmond, Mathew. 2012. "Eviction and the Reproduction of Urban Poverty." *American Journal of Sociology* 118 (1): 88–133.

Desmond, Mathew. 2016. *Evicted: Poverty and Profit in the American City*. New York: Crown Publishers.

Desmond, Mathew, and Rachel Tolbert Kimbro. 2015. "Eviction's Fallout: Housing, Hardship, and Health." *Social Forces* 94 (1): 295–324.

DeVault, Marjorie L. 1991. *Feeding the Family: The Social Organization of Caring as Gendered Work*. Chicago: University of Chicago Press.

Duneier, Mitchell. 1992. *Slim's Table: Race, Respectability, and Masculinity*. Chicago: University of Chicago Press.

Duneier, M. 2006. "Ethnography, the Ecological Fallacy, and the 1995 Chicago Heat Wave." *American Sociological Review* 71 (4): 679–89.

Duneier, Mitchell, and Ovie Carter. 1999. *Sidewalk*. New York: Farrar, Straus and Giroux.

Duneier, Mitchell, and Harvey Molotch. 1999. "Talking City Trouble: Interactional Vandalism, Social Inequality, and the 'Urban Interaction Problem.'" *American Journal of Sociology* 104 (5): 1263–95.

Emerson, Robert M. 2015. *Everyday Troubles: The Micro-Politics of Interpersonal Conflict*. Chicago: University of Chicago Press.

Emerson, Robert M., Rachel I. Fretz, and Linda Shaw. 2012. *Writing Ethnographic Fieldnotes*, 2nd ed. Chicago: University of Chicago Press.

Emerson, Robert M., and Melvin Pollner. 1991. "Difference and Dialogue: Members' Readings of Ethnographic Texts." *Perspectives on Social Problems: A Research Annual* 3:79–98.

Erlanger, Howard S., and Douglas A. Klegon. 1978. "Socialization Effects of Professional School: The Law School Experience and Student Orientations to Public Interest Concerns." *Law & Society Review* 13 (1): 11–35.

Fine, Gary A. 1993. "Ten Lies of Ethnography." *Journal of Contemporary Ethnography* 22 (3 October): 267–94.

Geertz, Clifford. 1972. "Deep Play: Notes on the Balinese Cockfight." *Daedalus* Winter:1–37.

Glaser, Barney G., and Anselm L. Strauss. 1967. *The Discovery of Grounded Theory: Strategies for Qualitative Research*. Chicago: Aldine.

Gluckman, Max. 1955. *The Judicial Process among the Barotse of Northern Rhodesia*. Manchester: Manchester University Press.

Goffman, Alice. 2009. "On the Run: Wanted Men in a Philadelphia Ghetto." *American Sociological Review* 74 (3): 339–57.

Goffman, Alice. 2014. *On the Run: Fugitive Life in an American City*. Chicago: University of Chicago Press.

Goffman, Erving. 1963. *Stigma: Notes on the Management of Spoiled Identity*. Englewood Cliffs, NJ: Prentice Hall.

Goffman, Erving. 1971. *Relations in Public*. New York: Basic Books.

Goffman, Erving. 1989. "On Fieldwork." *Journal of Contemporary Ethnography* 18 (2): 123–32.

Gross, Neil. 2009. "A Pragmatist Theory of Social Mechanisms" *American Sociological Review* 74 (3): 358–79.

Hayner, Norman S. 1936. *Hotel Life*. Chapel Hill: University of North Carolina Press.

Hochschild, Arlie R. 1983. *The Managed Heart: Commercialization of Human Feeling*. Berkeley: University of California Press.

Hughes, Everett C. 1984. *The Sociological Eye*. New Brunswick, NJ: Transaction Books.

Irwin, John. 1970. *The Felon*. Englewood Cliffs, NJ: Prentice Hall.

James, William. (1909) 1975. *The Meaning of Truth*. Cambridge, MA: Harvard University Press.

Johnson, John. M. 1975. *Doing Field Research*. New York: Free Press.

Katz, Jack. 1978. "Lawyers for the Poor in Transition." *Law & Society Review* 12 (2): 275–300.

Katz, Jack. 1982. *Poor People's Lawyers in Transition*. New Brunswick, NJ: Rutgers University Press.

Katz, Jack. 1997. "Ethnography's Warrants." *Sociological Methods & Research* 25 (4): 391–423.

Katz, Jack. 2001a. "Analytic Induction." In *International Encyclopedia of the Social and Behavioral Sciences*, edited by Neil J. Smelser, and Paul B. Baltes, 480–84. Oxford: Elsevier.

Katz, Jack. 2001b. "From How to Why: On Luminous Description and Causal Inference in Ethnography" (Part 1). *Ethnography* 2 (4): 443–73.

Katz, Jack. 2002a. "From How to Why: On Luminous Description and Causal Inference in Ethnography" (Part 2). *Ethnography* 3 (1): 63–90.

Katz, Jack. 2002b. "Start Here: Social Ontology and Research Strategy." *Theoretical Criminology* 6 (3): 255–78.

Katz, Jack. 2015. "Situational Evidence: Strategies for Causal Reasoning from Observational Field Notes." *Sociological Methods & Research* 44 (1): 108–44.

Klinenberg, Eric. 2002. *Heat Wave: A Social Autopsy of Disaster in Chicago*. Chicago: University of Chicago Press.

Leovy, Jill. 2015. *Ghettoside: A True Story of Murder in America*. New York: Spiegel & Grau.

Lindesmith, Alfred R. 1947. *Opiate Addiction*. Bloomington, IA: Principia Press.

Morrison, Denton E., and Ramon E. Henkel. 1970. *The Significance Test Controversy*. Chicago: Aldine.

Morse, Janice M. 2004. "Theoretical Saturation." In *Sage Encyclopedia of Social Science Research Methods*, edited by Michael S. Lewis-Beck, Alan Bryman, and Tim Futing Liao, 1123. Thousand Oaks, CA: Sage.

Murphy, Alexandra K. Forthcoming. *Where the Sidewalks End: Poverty & Race in an American Suburb*. London: Oxford.

Pollner, Melvin. 1991. "Left of Ethnomethodology: The Rise and Decline of Radical Reflexivity." *American Sociological Review* 56 (June): 370–80.

Rosett, Arthur I., and Donald R. Cressey. 1976. *Justice by Consent: Plea Bargains in the American Courthouse*. Philadelphia, PA: Lippincott.

Roy, Donald. 1952. "Quota Restriction and Goldbricking in a Machine Shop." *American Journal of Sociology* 57 (March): 427–42.

Roy, Donald. 1953. "Work Satisfaction and Social Reward in Quota Achievement." *American Sociological Review* 18 (October): 507–14.

Simmel, Georg. 1971 [1908]. "Group Expansion and the Development of Individuality." In *Georg Simmel: On Individuality and Social Forms*, edited by David M. Levine, 251–93. Chicago: University of Chicago Press.

Small, Mario L. 2015. "De-exoticizing Ghetto Poverty: On the Ethics of Representation in Urban Ethnography." *City & Community* 14 (4): 352–58.

St. Jean, Peter K. B. 2007. *Pockets of Crime*. Chicago: University of Chicago Press.

Tavory, Iddo, and Stefan Timmermans. 2013. "A Pragmatist Approach to Causality in Ethnography." *American Journal of Sociology* 119 (3): 682–714.

Tavory, Iddo, and Stefan Timmermans. 2014. *Abductive Analysis: Theorizing Qualitative Research*. Chicago: University of Chicago Press.

Thrasher, Frederic M. 1927. *The Gang: A Study of 1,313 Gangs in Chicago*. Chicago: University of Chicago Press.

Timmermans, Stefan. 2006. *Postmortem: How Medical Examiners Explain Suspicious Deaths*. Chicago: University of Chicago Press.

Van Maanen, John. 1983. "The Moral Fix." In *Contemporary Field Research*, edited by R. Emerson, 269–87. Boston, MA: Little, Brown.

Vaughan, Diane. 1996. *The Challenger Launch Decision*. Chicago: University of Chicago Press.

Velsen, Jaap van. 1967. "The Extended-Case Method and Situational Analysis." In *The Craft of Social Anthropology*, edited by Arnold L. Epstein, 129–49. London: Tavistock.

Venkatesh, Sudhir A. 2008. *Gang Leader for a Day: A Rogue Sociologist Takes to the Streets*. New York: Penguin.

Vidich, Arthur J., and Joseph Bensman. 1964. "The Springdale Case: Academic Bureaucrats and Sensitive Townspeople." In *Reflections on Community Studies*, edited by Arthur Vidich, and Joseph Bensman, 313–49. New York: Harper & Row.

Weiss, Robert S. 1994. *Learning from Strangers: The Art and Method of Qualitative Interview Studies*. New York: Free Press.

Whitehead, Alfred North. (1925) 1967. *Science and the Modern World*. New York: Free Press.

Whyte, William F. 1943. *Street Corner Society: The Social Structure of an Italian Slum*. Chicago: University of Chicago Press.

Williams, Terry M. 1992. *Crackhouse: Notes from the End of the Line*. Reading, MA: Addison-Wesley.

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