Ethnography

From How to Why: On Luminous Description and Causal Inference in Ethnography (Part I)

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A B S T R A C T ■ Ethnographers often start fieldwork by focusing on descriptive tasks that will enable them to answer questions about how social life proceeds, and then they work toward explaining more formally why patterns appear in their data. Making the transition from 'how?' to 'why?' can be a dilemma, but the ethnographer's folk culture provides facilitating resources for detecting and appreciating especially compelling descriptions. Recognitions of luminous data often implicitly light the path to causal inference. In this, the first of a two-part article, three of seven forms for characterizing the rhetorical effectiveness of ethnographic data are illustrated and the distinctive resources they offer for causal explanation are analyzed.

Over the last 25 years, ethnographic writing has become a major focus in a debunking critical perspective. Literary devices traditionally used for representing social life have been rendered deeply suspect by analyses that reveal how they implicitly construct social realities and subtly claim rhetorical authority. (Noted early contributions include Gusfield, 1976; Clifford,

1983; Clifford and Marcus, 1986; Van Maanen, 1988.) Although useful for opening new lines of inquiry, this reflexive turn continues a long-standing failure to appreciate the logic of the culture in which ethnographers routinely evaluate their data.[1] Perspectives for evaluating ethnographic data cut across writing styles and bridge the anthropology/sociology divide. However undisciplined and vaguely articulated, a community exists in everyday discourse about ethnographies. Even while there may be fierce disagreement about the value of given works, there are still common terms for appreciating ethnographic descriptions as good and for rating given data passages as especially effective.

   To bring into relief the culture for appraising ethnographic depictions, we should put aside terms of approval such as 'significant' and 'a major contribution', which slide attention from a focus on the text to an anticipation of its likely *reception*. We must also put aside terms such as 'creative' and 'carefully done', which commend the author's *quality of analysis* or professional mind. What makes a work 'interesting' is often another matter, as it frequently means that the *nature of the theory* in a text is counterintuitive (Davis, 1971). I wish to focus here on terms of appreciation that are common and distinctively meaningful when applied to the *descriptive* qualities of ethnographic data.

   This culture of appreciation lives in various forms. Readers use it quietly when reading ethnography, as do authors when writing in anticipation of readers' responses. It is easiest to grasp when ethnographies are unselfconsciously characterized with buzz words and rallying cries that praise descriptions of social life as 'richly varied', 'densely textured', 'revealing', 'colorful', 'vivid', 'poignant', 'strategic', or 'finely nuanced'; for showing how behavior is 'crafted', 'grounded', or 'situated'; or as containing 'paradoxes' and 'enigmas' that fascinate the investigator and the reader. Antonyms are used to dampen enthusiasm and fizzle out calls for support. Thus poor or weak descriptions are said to be 'thin', 'superficial', 'abstracted', 'unsurprising', 'redundant', 'not compelling', or as presenting too 'narrow', 'static', 'artificial' or 'conventional' a picture of social life.

   This culture of appreciation contains neglected practical wisdom for guiding ethnographic research and writing, especially for meeting the challenge of moving from 'how' to 'why', or shifting from a focus on gathering descriptions of social life to the analytical re-organization of data into explanatory lines. I do not wish to imply that description is ontologically separable from explanation. It may well be that when refined sufficiently, descriptions of how people act merge seamlessly into explanations of their behavior. The distinction is rooted not in philosophy but in ethnographic practice. In one common form, the challenge emerges when the researcher faces a mass of descriptive data and then seeks to address significant debates that hinge on explanatory problems. Fieldnotes and interviews describing

how people act must be sorted out with relevance for controversies over why people act as they do. As a practical matter, the path to that degree of refinement where description and explanation merge smoothly is often not obvious to an ethnographer until a substantial period has been dedicated self-consciously to description.

It is not easy to avoid this dilemma, for example by gathering data in the first place literally as answers to 'why' questions. Compelling warnings (Mills, 1940; Becker, 1986; Weiss, 1993) alert researchers to the likelihood that asking people 'why' they have done something will shift the focus from what the researcher is usually after, which is information on what subjects did and experienced in times, places, and activities outside the interview setting. Asking 'why?' gives respondents reason to anticipate that the versions of self they express will be reviewed by the researcher and whomever else they imagine the researcher communicates with. Respondents are thus encouraged to exhibit the kinds of comfortable, conventional explanations that presumably will pass muster in the eyes of these fantasized reviewers. Unless one wants to stick closely to conventional rhetoric or to study artifacts of research situations, the data resulting from 'why?' inquiries are likely to be disappointing. (See also Geertz, 1979, on natives nonplussed at questions about why they believe gods are powerful.)

'How?' is generally a better way to elicit responses useful for explanation because it invites a personally historicized, temporally formatted response, while 'why?' authorizes responses formatted in the atemporal and impersonal categories of moral reasoning. Asking someone why they married someone, chose a residence, or took a job often elicits brief justifications that highlight present features of the mate, home, or work situation, features that may well have been discovered since the relationship was established and that, as current realities, are right at hand to provide an impressive documentation of the answer. Shifting the question to how one got that job, found that residence, or got together with that mate commonly turns the discussion toward 'the long story' that traces how networks of social relations and detailed processes of social interaction worked to shape the respondent's present status. It is not uncommon for the researcher to experience a breakthrough toward an explanatory inquiry specifically by shifting the questions that guide the gathering of data from 'why?' to 'how?'

And if we could trust answers to 'why?' questions, would we not be in even worse shape? If research subjects can reliably report why they do the things we want to understand, who would need us? The question often throws the burden of explanation onto the subject, reducing our work to, at best, amplification. The analyst's answer to 'why?' will not necessarily debunk the member's, but it will be more of a contribution when it situates and transcends explanations that members conventionally find satisfactory.

The difference between inquiries about how and why is decisive for

observational as well as interview practices. When we are taking notes on what is going on in a given time and place, describing 'how' is a reasonable if never fully completed objective. Data produced through observations in situ can describe how people work, how they commit crimes, how they struggle with their intimate domestic relations. But questions about 'why' people act as they do logically require observations at different times: such that events at time 2 can be treated as the result of events at time 1; *and* they implicate observations in different places in order to specify the features in a given situation that were meaningful to and taken into account by the actor as contingencies of his or her conduct.[2] When ethnographers claim to see why people act as they do without offering comparative data and analysis, they are vulnerable to the charge that they are either invoking magical powers to 'see into the minds' of their subjects, or 'preaching to the choir' by applying a pre-set explanatory formula that they presume sentimentally sympathetic readers will accept.

Explanations always go beyond the descriptions contained in any single situation represented by a given observational fieldnote, interview segment, or strip of videotape. An explanation is always an hypothesis. Take, as a simple example, filling the car with gas. I can describe how I did that on a given occasion, but why I did it is never really as simple as top-of-the-head explanations suggest, for example, 'because I was low on gas' or 'because I needed gas'. I needed gas before I entered the station; I did not rush to a station the first moment I noticed the gas gauge registering low; and usually I get there without having to push the car in because it ran completely dry. In any case my 'need' for gas would not explain the extent to which I fill the tank, nor why I pay with a credit card instead of cash, nor which of the pumps I choose, nor whether I accept the automatic cut-off as ending the operation or top up with a final squeeze. As the description of how the act is conducted improves, the less convincing becomes the initially obvious answer to 'why?'

Are we, then, imprisoned in the methodological dilemma that the better our descriptions, the more problematic their utility for explanation? The path from describing how people do things to explaining why they so act must initially be sensed more than articulated, because ethnography eschews thoroughly pre-fixed designs for data gathering, such as are used for conducting experiments or surveys, in order to discover questions that are especially salient to its subjects. Yet if the paths from how to why cannot be marked with bright pointers before ethnographic data gathering is under way, they often shine at least dimly in the reader's encounter with the data themselves. They appear in an initially unreflective appreciation of some data passages as particularly effective.

In this article and its sequel, seven sets of evaluative terms commonly used to appraise ethnographic accounts will be reviewed. Underlying each set is

a distinct lead toward explanation, the scaffolding of a bridge from 'how' to 'why'.

1. Data effectively set up a problem for explanation when they are resonant with *enigma, paradox, or absurdity.*
2. Data are appreciated as *strategic* in at least two ways that can make data passages especially valuable. The data set may be *bifurcated* such that each datum explicitly supporting the proffered explanation also implicitly negates a major alternative explanation. Or the flexibility available in participant-observation fieldwork may be exploited to give a temporal structure to the data gathering that *parallels the temporal structure of significant turning points* in subjects' lives.
3. *Rich and varied* data facilitate ruling out rival hypotheses by specifying definitions of explanatory problems and qualifying answers.
4. Data are appraised as *revealing* when they show how forces shaping social life are routinely overlooked, purposively hidden, or ontologically invisible.
5. *Situated* data point to mediating conditions that determine whether constantly present possibilities will be mobilized.
6. When data describe how behavior is *crafted* in ways that are distinctively *aesthetic, colorful, or vivid*, they highlight the obdurate characteristics of social environments, and thus point to the forces that shape social patterns.
7. When data describe *poignant* moments, they capture people humbled by transcending concerns that structure persistent patterns in their lives.

It will be assumed that these terms of appreciation represent a culture that is quite generally used in reading ethnographies. Of course, that is an empirical argument, and it is not unreasonable to expect that an argument of this sort would proceed from formal evidence of the range, depth, and situated mobilization of this evaluative culture. Lacking that base, I trust that these terms, or at least most of them, are in general currency and will be familiar to readers.

But even if I am wrong on that count, I offer a logic for shaping ethnographic texts and for at least partially assessing them. The core argument is that readers' common-sense evaluative criteria point wisely toward methodologically sound links between, on the one hand, the data gathering and data presentation tasks, which aim at showing how social life takes the shapes that it does, and, on the other, the explanatory challenge of making a convincing argument about why social life works as it does. To the extent these criteria are not now used to appreciate the quality of ethnographic data, they should be.

Each of the seven sets of criteria highlights a distinct way that ethnographic description can be luminous through subtly promoting the search for causal explanation. All are induced from 'tricks of the trade' (Becker, 1998) by which ethnographers sometimes unwittingly start the move from

description to explanation. Because the terms and relative organization of the seven are not deduced from theory or philosophy, several disclaimers are in order. First, there is an arbitrariness in the selection of examples, in part because a particular data passage may fit one, several, or all of the seven. Second, the examples represent a range of styles and historical periods in anthropological and sociological ethnography, but every reader will have the good sense to be stunned in a different way by the limits of my command of the vast literatures that could be sampled. Despite its length, this essay is only a beginning.

Third, no claim is made that the seven sets of criteria are exhaustive; the following argument is not intended as a statement of *the* logic for assessing ethnographies. As a whole, these categories of appreciation are most useful when applied to descriptions of particular people doing specific things, often with particular other people, in designated places. Many worthy ethnographies never make the attempt. These include: informant-based models of how a community typically functions or how some part of society usually works; structuralist and other interpretations that examine texts independent of the social processes by which members create and read them; and analyses of cultural artifacts as what, in other areas of social science, have been appreciated as 'unobtrusive measures'.

I leave for another day the question of whether there are defensible logics for appreciating ethnographies independent of their value in offering causal explanation. This is one of a series of essays in which I attempt to construct a methodology for ethnography that does not require ethnographers to justify their work with a logic that is either hostile to the causal methodologies of fixed design, quantitative research (survey interviews, lab or field experiments), or that is alien to the logic of humanistic methods (especially as practiced by historians) (Katz, 1982b, 1997). Causal explanation does not require positivism in the form of assertions that one can predict, on the basis of knowing realities at time 1, what will occur at time 2. At a minimum, 'explanation' as I use it here requires 'retrodiction', claims that, if we observe a given phenomenon at time 2, we can state what will have happened earlier, perhaps in a particular sequence of stages that led to the outcome (Katz, in press). Ethnographers may be loathe to put their analyses into the familiar phrases of causal explanation on the common misunderstanding that causal explanation is inconsistent with an appreciation of both the emergence of meaning in people's lives and creativity in cultural development. In fact, many of the most celebrated ethnographies can be comfortably digested as retrodictive explanations, such as the description in Geertz (1972) of all the social alignments that must be put into place to build the stakes that are entailed when holding a cockfight in Bali, and the descriptions in Duneier (1999) of the routine negotiations with passers-by, with guardians of restrooms, with a hostile mayoral administration, and

with inconstant co-workers who must be managed in order to sell books and magazines while living on New York sidewalks. In any case, before thrashing about in arguments over whether we must or must not bring causal analysis to ethnography, we should first appreciate how subtly and usefully we are already doing it.

Part I of this essay treats the first three sets of evaluative terms. Part II, to be published in the journal's next issue, treats sets four through seven.

Enigma, paradox, and absurdity

Ethnographers find numerous common solutions to the definition of an animating problem, including borrowing on a predecessor's achievements by returning to a site to test an earlier, celebrated treatment, or examining the reach of a presumptively relevant existing theory in novel circumstances (Burawoy, 1991). But if we consider how descriptions of social life themselves compel explanatory attentions, one of the most effective ways to move from a description of *how* to an organizing focus on *why* is to present an enigma, a paradox, or an apparent absurdity. If the sense of mystery is not peculiar to the ethnographer, he or she can assume that its eventual explanation will also be of general interest.

Fieldworkers will sometimes be stymied in the natural course of data gathering when they come across a phenomenon that seemingly should have an obvious meaning but doggedly does not. James Ferguson reports such an experience in his recent book on Zambian Copperbelt workers and families.

> I visited the home of a former mineworker and spotted an unusual phrase carefully inscribed on an interior wall of the house. The message was near a windowsill and had been carefully spelled out, in English, in inch-high block letters. It read, 'Asia in miniature'. 'What does it mean?' I immediately wanted to know. 'I don't know,' the young man replied, with no great interest. 'Nothing, really.' But how did it get there? His brother wrote it there, he explained, a long time ago. It didn't mean anything. . . . At last a story emerged. . . . [N]ot yet understanding English, the brother had copied these words from the caption to a photo in a school atlas. (Ferguson, 1999: 209)

Ferguson reports that this enigma long frustrated a sense of having comprehended his subjects well. His description of the phenomenon (what it expressed, how the phrase got on the wall) works as well to provoke our curiosity. Of course, the phrase could be dismissed as insignificant, but it turns out that there was a wealth of non-Zambian, wildly juxtaposed cultural styles that the workers sported in a similarly nonchalant way. Why adopt all these foreign cultural items if indeed they meant 'nothing, really'? Facile explanations, such as that the Zambian urban worker had become

fascinated with Western culture, did not go far, in part because the Zambians not only did not appear to care much about understanding Western culture, but because the items were, in fact, as likely to be Asian or Caribbean as 'Western'.

In the end, Ferguson resolved the enigma in a way that substantively made sociological news but that, as a research experience of solving an enigma, was not. He determined that the mystery was not due specifically to his ignorance, nor to intellectual deficiencies in the culture under examination, but to an ambiguity built into the setting.

> [A]s urban workers in the 1950s and 1960s gained a greater ability to live independently of their rural allies, they became increasingly able to shrug off the rural-based obligations of wide kin networks, remittances, bridewealth, visits home, and localized 'home-folk' sociality, and along with it (in the long run) the cultural style that signified the acceptance of these obligations. (Ferguson, 1999: 231)

By adopting a bizarre mixture of 'foreign' styles, Zambian Copperbelt workers celebrated their (historically temporary) freedom from local ties.

We need not evaluate the substance of Ferguson's explanation; our primary concern is to grasp the broadly applicable methodological wisdom in his research experience. A single data passage describing what a scene looks like and how it came to be can effectively launch a curiosity to provide the answer why. An innocently confronted, transparently described enigma effectively drafts the reader into the explanatory game. The answer suits the form of traditional social science explanation, although it is not presented in a formal or abstracted fashion. Ferguson argues that a spontaneous, collective fascination with 'foreign' styles rose and fell, rising as ties to home villages were loosened and then, in the undertow of a significantly declining economy, giving way to a re-involvement in local cultural themes.

If Ferguson profited from a sudden confrontation with a neatly packaged, emphatically underlined question mark conveniently carved into the landscape, for others the shift into a search for explanation will arise more gradually over the course of fieldwork. Jean Briggs was struck by a fascinating enigma only after years of study prepared her to confront a paradox in what she had come to know of the Inuit. In her early field trips to study Eskimo shamans, Briggs was challenged by a failure to find any. Then she was challenged by a sense that something was deeply wrong in her field relations: she discovered that her subjects discreetly viewed her as impulsive, irrational, and aggressive. She explained her interpersonal problems by viewing the Inuit as governed by expectations of 'superhuman control of antisocial moods and superhuman benignity' (Briggs, 1987: 9), and she explained their culture in her first book as influenced by values of love/pity

for mankind and of rationality (Briggs, 1970). By modeling these values, and without refusing young children their wishes, the Inuit managed to produce five-year-olds who were very 'good' and 'undemanding'.

Years later she realized that some patterns she had always observed were in tension with her previous view of Inuit people as benevolent and non-aggressively disposed. Indeed, they often seemed sado-masochistic with their children. There were:

> 'games', little interpersonal dramas, that adults stage all the time with children . . . 'Why don't you kill your baby brother? Like this!' [demonstrating]; 'Bite me – harder! Ouch! Isn't that fun?'; 'Who's your daddy? *You* don't have a daddy.' (Briggs, 1987: 12)

Note that the paradox is not simply the common experience of finding a contradiction between patterns in one's data; it is a tension dramatized in the subjects' interaction. That grounded paradox then poses an enigma for the researcher. Why would a people who are especially distinguished in their 'superhuman control of antisocial moods and superhuman benignity' engage in play that encourages their children to be aggressive and that dramatizes pleasure in inflicting, witnessing, and suffering violence?

Here the question 'why do they do that?' emerges vividly when, after the publication of her first, successful book-length treatment, the ethnographer reconsiders her original materials and appreciates a neglected pattern of inter-personal, and intra-personal, conflict. For Briggs, the key phenomenon is not a conflict of cultures (compare the story of Cohen in Geertz, 1973). Nor is it a culture of conflict; this is play, and, as 'just child's play', it could easily be overlooked (compare a dramatization of conflict that is put on regularly, in designated sites, at high stakes, and for public display, Geertz, 1972). Conflict by itself may be universally fascinating, but it does not make explanation problematic. Like violence in movies and TV shows, researchers who describe cultures in conflict and cultures of conflict are likely to draw attention to their narratives, but as with entertainment conflicts, one can easily leave a reading without having exercised explanatory curiosity. Descriptions of conflicts within culture insist more powerfully on answers to questions why.

Briggs suggests that the moments of dramatized conflict in child-rearing prepare the way for routine stretches of overtly tranquil living, and that, conversely, the ambience of sensitively polite social equanimity is a kind of cultural skin that subtly detects and represses wild, destructive forces. The analysis is reminiscent of the 'axes of variation' that Kai Erikson (1976), in a few brief but provocative pages, offers as characterizing an Appalachian community, and as explaining how the social world in Buffalo Creek was turned upside down by a flood occasioned by negligent mining practices. If one follows up the theoretical leads suggested (so gently as to be almost

unnoticeable) by Erikson and Briggs, ethnographic descriptions of self-conflictual social practices should reliably contain far-reaching explanatory keys.

It remains to note how *absurdity* can also play a powerful role in shifting an ethnographer's descriptive energies into explanatory channels. (For a classic anthropological example of an explanation of a ceremony attended to initially as a striking absurdity, see Bateson, 1938.) The most direct use of absurdity to set up social research questions has been by the ethnomethodologists. Continuing a perspective rooted in the writings of Edmund Husserl, elaborated for social science by Alfred Schutz, and diffused through American sociology by Harold Garfinkel and Aaron Cicourel, the ethnomethodological researcher understands that all social life is based on a continually bootstrapped presumption of inter-subjectivity.

Ethnomethodologists never have developed an ethnographic craft, and indeed when they consider the matter, they are sometimes hostile.[3] The problem with ethnography that they stumble upon is real. As a matter of field practice, ethnographers cannot afford *not* to bridge gaps in understanding. Garfinkel's (1967) students, carrying out his instructions to go home and, without announcing their purpose, act like a guest or ask family members to explicate all that was formally ambiguous and implicit in their speech, could experiment with temporarily disrupting their family relations, but fieldworkers need to build and maintain rapport in order to gather data.

Ethnographers have the common sense to anticipate that it would not be propitious to indicate constantly to every person one meets that he or she does not obviously make sense. Indeed, like anyone using a new language in everyday-life foreign settings, ethnographers inevitably learn a great deal about a new social terrain by faking it; that is, by frequently pretending to understand what they really do not. Although personalities must differ widely on this score, it is common for ethnographers to exercise substantial care before pointedly raising specific questions. They fear discovering something that might be at once wonderful substantively but disastrous practically, that their subjects have taken deep offense that something beyond question has been put up for critical inspection. Garfinkel's students could afford it if their families reacted with anger or tears because what they expected to do the next day was to go back to class, not to the field.

If ethnographers are reluctant to create absurd scenes, still, finding a naturally occurring scene that seems absurd, the ethnographer should be delighted. In it there is likely to be a powerful invitation to try to explain why members can treat the scene as perfectly sensible. Here is an example from a study of how lawyers provide civil legal assistance to poor clients.

> For at least 25 minutes, L. [lawyer] and Cl. [client] fight over whether L. or Cl. would go to eviction court and show the judge receipts which, L. claims, make out a solid defense. Cl. argues that she wouldn't be paid the same

respect by the judge as would L. L. privately insults Cl. by thinking her unstable (as his comments later show), all the while explaining in a therapeutically calm voice, 'I refuse to insult you by handling this case. You're fully capable of doing this yourself.' Cl. retorts angrily: 'This is Legal Aid; I'm a poor person, so I'm your boss; and if I want you to insult me, you'll insult me.' (Katz, 1982a: 221–2)

One might characterize this standoff as a double double bind, a Catch 22 squared. The lawyer in effect asserts that the prospective client only thinks she needs service; by arguing so powerfully for service, she shows that she can take care of her problem by herself. His services are available only to people who are so needy that they do not think that they need his services. For her part, the prospective client so forcefully and shrewdly claims incapacity that she appears capable of taking at least initial steps to resolve her problem.

Such an event suggests that there are indeed deep gaps between the social worlds of the workers and clients at this place. Because absurd events are rare, the social organization of the place must somehow contain a genius that routinely and invisibly operates to avoid the appearance of gaping openings. When descriptions are backlit by the strange glow of the absurd, they pulsate with demands for explanation.[4]

To review, various types of enigma in ethnographic description guide the researcher toward a search for explanation. These include: (i) descriptions of matters in social life that are inexplicable both for the researcher and for members; (ii) paradoxes that appear as frustrating obstacles when the researcher attempts to weave descriptive patterns into a coherent narrative; (iii) self-contradictory actions, like the 'double binds' thought by some to lead to schizophrenia, that are collaboratively sustained; (iv) moments of absurdity, such as when two people sustain a prolonged interaction by insisting that they cannot. When ethnographers describe the operation of these enigmas, paradoxes, and little overt lies, they provoke curiosity about the big sociological 'why?': what explains the sense of apparent coherence in the lives of the people studied? What makes it possible for them to take for granted that they live in a common social world? Why is social life not apparently coming apart at the seams constantly?

Strategically organized data

If ethnographers do not collect data according to detailed or inflexible protocols in research designs, at the outset of a project they can often still anticipate explanatory debates they will inevitably enter. In response they may think strategically about how to organize data collection. In any era of

research and in every field, some omnibus explanations will be circulating. However the research problem is ultimately defined, the ethnographer can reasonably anticipate explanations that many readers are likely to bring to the text.

Thus, Lisa Frohmann (1991), in setting out to investigate how gender discrimination might work its way into the prosecution of rape cases, was strategic in dividing her participant observation time between prosecution offices in a substantially affluent, white community, and in a low-income minority city. That her findings were essentially similar in the two offices enhanced their power. Although ethnographers often appear to abhor the standard rhetoric of scientific research, what Frohmann did was to try to control some factors that predictably would be argued as alternative explanations for whatever she found.

Indeed, there were differences in the rape cases handled in the two offices that reflected the different social ecologies. In upscale 'Bay City', the charges often took the form of 'date rape'. In low income 'Center Heights', rape cases usually had a gang, prostitution, or drug dealing connection. Frohmann's focus, however, was on the process of reviewing and constructing cases, and in both offices the prosecutors were involved in essentially the same work of constructing believable stories, not in judging the truth. They were constrained by the same anticipated problems, which included 'ulterior motives', discrepancies in the accounts given by complainants at different times or to different officials, and variations from typifications of how sexual assaults presumably occur.

Prosecutors, it turns out, are not so much judges as story tellers. Whatever their personal values, by and large they are geared toward drafting scripts that will work down the line, when spun in negotiations with defense counsel, when recited to grand juries (which decide on charges), or when dramatized before adjudicating authorities. In effect, the institutional working environment creates a personality for prosecutors that is substantially independent of gender and community context. They develop an almost tangible habitus in which interactions with complainants are managed with a prismatic eye that looks backward to what might have happened to the complainant, forward to what audiences hearing the tale are likely to opine, and over the heads of the people immediately involved to the reputation the prosecutor is building in his or her work circles. The extreme differences in the social ecologies of the two offices function as a backlight to every data passage. Each time we read of how a 'gang' rape case was handled in Center Heights, we implicitly look beyond the boundaries of the case to how 'date' rape cases are handled in Bay City, and vice versa. It is this double-layer in the reading process that gives the data their luminosity.

In order to recognize the potential in this methodological strategy, we

should note how the data passages become luminous specifically because of the logical (not racial) blackness in the background. By varying the offices represented by the data passages included in the text, Frohmann quietly negates the rival hypothesis that race, poverty, or other community-distinguishing characteristics dominate case processing. For example, a case from Bay City and then another from Center Heights are described as manifesting a discrepancy from a 'typical' rape because there was a lack of variety and experimentation in the sexual activity. In one case the victim described 'just intercourse', in the other, 'all three acts are the same . . . he is grinding his penis into her butt' (Frohmann, 1991: 217). Frohmann does not explicitly stamp each case as showing that demographics or social ecology do not control the outcome. And she probably should not, because she does not want to distract the reader from her positive thesis, which is about the narrative tasks by which prosecutors discredit complainants' allegations of rape. But the negative points, the negation of a multitude of reasonable rival hypotheses, come across, and they are made with even more power because they are made subtly.

Similarly, the rival hypothesis that male bias leads to rejection of victim complaints is quietly but powerfully negated by varying the genders of the prosecutors in the cases described. The upshot is the implication that if there is a problem of injustice in the ways rape cases are handled by prosecutors, the problem is deeply implicated in the professional work identity that is taken on by both female and male prosecutors. The problem is either non-existent or, as she argues in a later piece, more subtle in that gender, class, and race bias are 'inadvertently' reproduced through the interpretation of the discrepant moral character of victims' and jurors' locales (see Frohmann, 1997, which, however, draws only on Center Heights cases).

It is also useful to bifurcate the data set on the matters to be explained. Liisa Malkki's study of Hutu refugees is divided on 'outcomes', or what might be taken as the 'dependent variable'. Massacres by majority Hutus of minority Tutsi people, and Tutsi attacks on Hutus, led to mass refugee movements. Some Hutus settled in towns. Many resettled with the aid of international organizations in designated camps. Malkki documents the Hutus' identity work by presenting 'panels', or lengthy, indented quotations from individual interviews:

> [C]amp refugees saw themselves as a nation in exile, and defined exile, in turn, as a moral trajectory of trials and tribulations that would ultimately empower them to reclaim (or create anew) the 'homeland' in Burundi. . . . In contrast the town refugees . . . tended to seek ways of assimilating and of inhabiting multiple, shifting identities – identities derived or 'borrowed' from the social context of the township. (1995: 3)

It is tempting to call Malkki's study a 'natural experiment', but she does

not and the temptation is wisely resisted. Not only is there no evidence of a random assignment of refugees to town or camp, there is also no measure of pre-refugee identity. It may be that more of the camp refugees had been involved in slaughters of Tutsi masses and were committed to a more militant ethnic version of their identities before they arrived in the camps. Moreover, in experiments, not only is the independent variable manipulated by the researcher, its identity is presumably known before the subjects live out different fates. In this study, precisely what the 'independent variable' is, or what specifically about town and camp life explain the different identity processes, remains to be explored.

Malkki does not set up the study as a causal inquiry into the contingencies of ethnic identification, just as Frohmann did not set up her study as a test of community ethnic composition, SES, and gender bias in criminal justice processes. But in both cases, the organization of the data into moieties relevant to causal debates was strategically useful, given readers' likely responses. Had Frohmann studied only the poor minority setting and found bias in case processing, her findings might be dismissed by some readers as masking race and income biases; had she studied only the more affluent white setting and found no bias, her findings would have faced objections that the affluent, politically mobilized community context of the prosecution office masked gender biases operating elsewhere.

In Malkki's study, the magnitude of the tragedy behind these refugee experiences irresistibly draws initial attention. The irony of the historical events adds interest: international aid established camps to ameliorate the sufferings that were brought about by impassioned ethnic nationalism, and camps appear to have recreated the problematic phenomenon. But what sustains interest is not just these compelling outline themes but the additional irony that, however indisputably tragic the backgrounds of all the refugees, the tragedy lacked a power sufficient to determine the identities taken on by the town inhabitants. Something else, something apparently very powerful, did.

Is the message of Malkki's case the depressing power of evil to reinvent itself, or the inspiring possibility that people can always escape what others would cruelly make of them? The human freedom implicit in the variation in the outcomes, despite the blinding horror in the near-background, compels detailed attention to the evidence of how identities evolved in the two settings, and underwrites Malkki's effort to demonstrate the constructed nature of the impassioned version of ethnic identity promoted by the camp refugees. The description of any individual's outlook in the camp is made more luminous through the ongoing juxtaposition with outlooks in the town, and vice versa. In effect, each of the two parts of the data set frames the other. Whenever camp refugees speak with passion about their national rights, quotes implying the purposive construction of identity

magically appear around the passages due to the reader's background appreciation of the relative indifference of town refugees.[5]

Malkki's study illustrates an essentially cross-sectional approach to setting up explanatory questions by documenting simultaneously existing, non-random contrasting patterns. A complementary appreciation for a data set may develop when one can describe how identities change over time. The possibility of developing diachronic data is inherent in virtually all fieldwork projects, at least to the extent that they involve extended time commitments to describe a given segment of social life. In most studies, the fit between the temporal structure of data gathering and the temporal structure of the examined processes of personal and social change is poor. The ethnographer does not arrive at the scene at the point at which members first get there, or if he or she does, the matters of interest develop on time schedules that are inconvenient to the researcher. Ethnographers use informants not only because informants possess a cultural perspective or insider's insight that the researcher lacks, but also simply because they are there: they were there before the researcher arrived and they may remain there as the researcher alternates career phases at the university and in the field.

Ethnographers, particularly those working out of sociology departments, have often structured the temporal course of their data gathering in parallel with the temporal course of the social processes they have investigated. This has usually occurred for practical reasons, without any express theoretical conviction leading the way. The genre of 'becoming a . . .' became common in American qualitative research at mid-twentieth century, in particular in the work of the 'second Chicago School' of ethnographies carried out by the students of Everett Hughes and Herbert Blumer. Some of these studies involved stints of participant observation in which graduate students would become novice employees in field sites in order to finance their way through school, a temptation not traditionally open to anthropologists.

In addition, ethnographers in bureaucratically organized societies can easily find research sites where cohorts of new members are regularly brought in on a precise calendar schedule and kept together in cohorts for extended processes of personal transformation: probationary employees, classes of students, employees in seasonal work. As with rites of passage in anthropology, the careers of institutionally arranged cohorts offer the strategic attractions of describing change that runs its course in front of the ethnographic eye.

Social control agencies, government service administrations, and even private clubs commonly process people in the form of cases of biographies-to-be-defined, and often the careers of cases are shorter than the researcher's involvement. Thus, within a two-year fieldwork project, one often can reliably project an ability to collect a series of patients screened for admission, treated, and released; inmates arrested, jailed for months, and sent on to

prison; would-be boxers entering a gym, becoming committed amateurs and then professionals who fight for a purse.[6] On a shorter temporal scale, in many formal work organizations that manage the provision of services, a sizeable set of cases begins and ends every day, as clients, patients, or customers are received, their demands or needs defined, and they are sent off. Much contemporary service work is billed by the hour and done in gigs, on service calls, through presentations, and in other work units that have two methodologically attractive features for the ethnographer who would describe them. They require worker attentions that are shorter than a day in length from start to finish, and they require the worker, and sometimes the client or customer, to remain continually and more or less exclusively engaged until the 'job' is done.

The descriptive challenges of phenomena of these sorts are tempting. The researcher can expect to describe the course of a large number of cases that, in temporal structure, if not in substance, are homogeneous from the subjects' perspective. Encountering a phenomenon that appears to have natural (member-organized) temporal structures, the ethnographer's descriptive tasks seem attractively manageable. In turn, the charming appeal of producing data that are naturally organized in temporal form has frequently inspired natural history-like causal explanations.

With Darwin not far in the background, the 'natural history' perspective was a central theoretical perspective in the formative years of American sociology at the University of Chicago (Park and Burgess, 1924). Early studies of the 'Chicago school' often focused on 'life histories' and on 'careers', for example Cressey's (1932) study of the careers of women paid in dance halls to entertain immigrant men. Howard S. Becker's dissertation, written under Everett Hughes, was on 'The Career of the Chicago Public School Teacher' (Becker, 1951), and his 1953 article, 'Becoming a Marijuana User', became a widely used research model. Ethnographies of particular settings, such as social life in a home for unwed mothers (Rains, 1971), carried on the tradition; and theorists drew on multiple ethnographies to conceptualize vast areas of social life within a framework on transformations of the self, from studies of personal change in organizational settings (Brim and Wheeler, 1966) to 'becoming deviant' in the ubiquitous clutches of state authority (Matza, 1969). Erving Goffman's 'The Moral Career of the Mental Patient' (1961) was an especially bright model for the study of systematically organized sequences that forced powerful transformations in personal identity.

Anthropological research has a long tradition of studying rituals and ceremonies guiding liminal phases of personal metamorphoses and, perhaps more recently, historical turning points in personal and collective identities (Sahlins, 1986; Comaroff and Comaroff, 1992). The parallel topics in the West have been more elusive because personal transitions are managed by bureaucratic institutions that profess to overcome any mystery about the

contingencies of status passage with explicit criteria, thus contributing to a culture in which members generally hide their magical beliefs from themselves. Thus we think 'competency' and 'ability' refer to real things, not to mystical notions, even though these conceptions of essence are never quite caught in the socially situated conduct that makes up our visible social lives (see James, 1907).

Yet ontological transformations, or shifts in essential nature, are matters of intense preoccupation in contemporary lives. We might usefully recall a common method for teaching a child to ride a bike. Someone runs behind the would-be adept, stabilizing his balance by holding onto the seat, and after an unpredictable number of runs and at some unannounced point, the guide lets go, a fact that the novice often discovers only sometime later, by turning around. At which point, having learned that he has already mastered the new competence, he can no longer manage his anxiety and falls down.

Such events become luminous memories because there is something specifically ineffable at work. For virtually any competency valued in contemporary society, there are manuals specifying how to acquire or improve one's ability. But even when such explicit instructions predictably work, the experience of acquiring competency has a magical quality that is not itself cognitively captured. Thus, one can learn to play piano by regularly following a course of lessons, but when competency is acquired, the experience will be of getting a feel for the activity, not of knowing something cognitively but of obtaining new 'ways of the hand' (Sudnow, 1978).

If courses of personal development cannot be grasped by subjects cognitively, retrospective interviews are not likely to be satisfactory methods for studying them. But in their participant and observer roles, ethnographers can give a temporal structure to data collection that may put them as close to inherently elusive causal contingencies as any research can. Sociological ethnographers have repeatedly stumbled upon subjects' experiences of magical transformations by finding emotionally powerful frustrations in their subjects' efforts to move from performing given lines of action to taking for granted that they have acquired talents, abilities, or competencies. Instead of writing about magic, liminal states, and taboos that keep experiences beyond words, sociological ethnographers appear to have adopted the imagery of existential challenge as the most comfortable vocabulary for conveying experiences with the ineffable. Here are two leading examples in contrasting traditions and styles.

When Becker et al. (1961) conducted their now classic observational study of socialization in medical school, they found the students preoccupied with the problem of how to limit what they should try to learn. So taken were they by what we might call the charisma of medical practice, and by the teachers whose knowledge seemed both indispensable and encyclopedic, the 'boys in white' could find no justification for limiting their

studies. It turned out that to the extent that students worked out reasonably comfortable solutions, it was through relying on thoroughly non-rational, sentimental means. In particular they discovered the utility of a shared common culture, for example the domestic culture they shared as married students or fraternity culture borrowed from days in college. What Becker and Geer wrote of as 'latent culture' provided a collective basis for working out common limitations on effort (Becker and Geer, 1960). The key contribution of latent culture is that it supplies people in the most rationalized regions of technologically advanced cultures with that most elusive and precious quality, trust.

By entering a ghetto gym in Chicago as a participant observer and investing three years in becoming a boxer, Loïc Wacquant (2000) shaped a strategic parallel between the temporal structure of his data gathering and the temporal structure of his own process of development. While most of his data described others' careers, his own experience continually provided a sensitizing perspective on issues of profound personal change. Wacquant's data quickly establish that there is something to explain. The gym is a place of rigorously disciplined civility; the novice boxer comes to see himself in a new moral profile in contrast to his ghetto counterparts outside. With respect to practice, the boxer takes on a new habitus, one celebrated in a newly acquired aesthetic:

> Pugilistic beauty resides in the practicalities of the fight itself, not in what it signifies, as the following comment by Jeff makes obvious: 'Bein' able to deliver a punch jus' the way you picture it in your head, how you gonna do it, you know: I mean *that's an art*. Jus' the *right timin'*, the *right speed an' everythin'*, it's, [raving] it's a helluva feelin' after you been trainin' all the time an' hit somebody with that perfect punch'. (Wacquant, 1995: 514 and 2000: 47–60, 67–72, 202–4)

What explains the process of change? Many of the crucial influences go beyond questions of strong will. How, after all, does a boxer know he has 'heart', and when does he know that he has lost it? Why do some come to assume that they are 'ready' for competition while others do not? Critical turning points occur through participation in the collective world of the gym. When Wacquant somewhat disingenuously asked whether one could learn how to box by reading books or could get into shape by working out at home, he was told emphatically that it would be impossible. His own experience as a novice boxer and his fieldnotes describing others' careers show how interaction with others in the gym guides the process of personal change in numerous ways. Others are observed as models of styles to imitate; various corporeal rhythms become familiar, thus minimizing potential surprises in the ring. As colleagues, others pace and push one to exceed limits, limits which, as a kind of existential mystery, are never objectively

knowable. The older men in the gym, in particular the head coach, Dee Dee, who arranged matches with promoters, give out measured judgments as to whether it is likely that given boxers ever will be ready for a fight, and then, if they have the potential, precisely when it is that they have realized it (Wacquant, 2000: 99–125, 137–43).

Data are strategically well shaped to locate sociologically significant phenomena when they track how people move through an anxiously moni-tored transition from one state to another. Ethnographic data that vividly describe *how* people make such a transition are likely to find that the work-ings of a spiritual, magical, or sentimental culture is a key contingency, one that explains *why* some make it and others do not, yet without imputing determinism. One may never be able to predict which students will develop a comfortable study regime in school and which will persist in or even succumb to anxiety, how many poverty lawyers will burnout and how many will remain committed to the job (Katz, 1982a), or which aspiring boxers will carry through 'a project of *ontological transcendence* . . . to fashion themselves into a new being. . . .' (Wacquant, 1995: 507 and 2000: 234–8)[7] and which will quit trying, but one may well be able to 'retrodict' the experiences and stages an adept will have had to go through, and in that regard, one can explain the personal and social meanings of the new status.

If we consider the form in which ethnographic description best serves causal explanatory objectives, we can appreciate the especially strategic character of a particular variation of temporal data. Sometimes eth-nographers can describe not only how a given phenomenon emerges in social life but also how it erodes or vanishes. If so, the data will afford extra tests for causal explanation. Some processes of personal change, and some forms of collective social transformation, are reversible. Boxers lose heart. Service workers who distinguished themselves by not burning out when many colleagues did will often quit at some later point. Some writers lose the conviction that they have anything more to say. The transition from adolescent to adult is not irreversible, at least not in the sense of securing respected, empowered, and self-confident status. If our theories of the conditions of making a transition are correct, it should be useful to examine whether the transition is reversed when those conditions are removed.

Often it will be impractical for the ethnographer to follow the phenom-enon not only into emergence but also through disappearance. Clearly, the more microscopic the social phenomenon – for example, when a bout of laughter or crying is described and analyzed – the more likely the researcher can sustain a descriptive focus through the life cycle of the phenomenon (see, for example, the videotaped data in Katz, 1999: chapters 2 and 5). But it is worthwhile remaining alert to the possibility that data gathering may

be stretched to encompass the course of changes out of as well as into a distinctive state of being. Because ethnographers often cannot specify the nature of the phenomenon they are looking for before entering the field, they may not plan to build the temporal structure of the data set strategically. But when they find that descriptions are shaping themselves to the full life cycle of a discovered phenomenon, they might appreciate and exploit the distinctive methodological possibilities for testing emergent explanations.

Rich, contextualized descriptions; varied, massive, densely textured data sets

Whether they study in sociology or anthropology, ethnographers tend to develop craft concerns for qualities of description collectively. Some find models in mentors and colleagues when they are students. In the work of the 'Second Chicago School' there is a common sense of what is well-presented data, and that sense is very different from the quality of descriptions produced by the 'first' Chicago school. I think of the work of the students of Herbert Blumer and Everett Hughes as distinguished by several enviable characteristics. Writing had a simplicity and directness that was cultivated in a generation of students through the years that Helen McGill and Everett Hughes edited the *American Journal of Sociology*. Data excerpts feature impression management in face-to-face interaction, details about the practical challenges or the 'work' of producing conduct, and idiosyncratic personal strategies for surmounting commonly encountered problems.

Presumably anthropologists can identify data written in the style of the Rhodes-Livingstone Institute and Manchester schools. The 'extended case method' (or 'social dramas' as developed by Victor Turner, see Kuper, 1973: 184) fit especially well with the study of disputes and conflict resolution. Gluckman's law training may be related to the structure he gave to anthropological data, which is not unlike the structure of knowledge routinely created by lawyers. Whether or not they play roles in courtroom dramas, for lawyers a case is a thing with a long history of personal facts on two or more sides, brought by clients who represent clients who represent clients (a director of a company that is owned by institutional stockholders).

The 'extended case' is one described over time and through distinguishable stages. It shows the interplay of multiple individuals and social groups who, through a sustained collective focus, shape cultural themes into an evolving drama. Within the history of anthropology, data structured into extended cases stood out against the background of structural functional ethnography. Typically without describing particular cases in detail, Radcliffe-Brown had offered synchronic understandings of how

ways of doing various things (or economic, kinship, and religious 'insti-
tutions') fit together in a community. The diachronic focus of the extended
case method requires the anthropologist to act like a sociological field-
worker engaged in a 'case study'. A diffuse and diverse knowledge about
the social setting and history is invoked, but kept as a background
resource for documenting the course of a social phenomenon (an accu-
sation of witchcraft, a property dispute, the response to a failed fishing
expedition or one that was unexpectedly successful). The themes that the
writer brings to the analytic foreground emerge from a search for patterns
across collections of cases.

Pierre Bourdieu has produced a collage style in which ethnographic
materials from various sources describe individual cases in boxes that float
alongside the main text but whose details are not otherwise addressed (e.g.
Bourdieu, 1984). The style is reminiscent of the hyper-boxed layouts of
recent news magazine articles. Bourdieu's long-term production of the
glossy, contemporary-styled journal, *Actes de la Recherche en Sciences
Sociales*, must not be coincidental, although the journal was not the origin
but an elaboration of a style he had developed in his earlier monographs.
Bourdieu also favors complex sentence structures – said by some to be
'French' or, what is not quite the same thing, 'Proustian' – that layer descrip-
tion, self-portrayal, philosophical commentary, and multiple caveats about
how to read a given passage, all compressed with a syntax that the reader
must labor breathlessly to keep running, the whole process requires an effort
analogous to the multi-tasking done effortlessly by computers but that,
when done by a human reader, recreates the extraordinary, even frenetic
energy that has previously gone into producing the fieldwork and the text,
with the result that the reader obtains a continually refreshed appreciation,
when he finally arrives at the end of a sentence and can take a momentary
rest, for his tiniest of friends, the period. The point seems to be to demon-
strate a passionately intelligent working style by making the reader either
relive it or drop exhausted in admiration by the wayside as the parade of
evidence and conceptualization moves mercilessly on.

Clifford Geertz's (1973) 'thick description' is helpful in this discussion
because of its celebrity and ambiguity. Although his early monographs were
dense with case data, in his later essays what is 'thick' for Geertz is not what
everyone would recognize as description. 'Thick description' might lead one
to expect a text with numerous cases illustrating fine variations around a
given theme. In this sense, a 'thick description' of the cockfight as an insti-
tution would be built up by presenting evidence on a large number of cock-
fights. The analysis could then make nuanced sense of variations among
them, and would naturally turn to specifying distinctions between cockfights
in one culture and seemingly analogous processes in other cultures such
as cockfights in east Los Angeles and in Mexico City, boxing matches in

Las Vegas, soccer games that sometimes turn to destructive madness in Brazil, ball games played by blacks from urban ghettos at midwestern universities, who brutally attack each other to enhance the pride of suburban white alumni contributors, etc. All of these social forms theatrically display totemic or vicarious battling that is passionately supported by men whose sexual fantasies, regional loyalties, caste emotions, class interests, and gambling stakes are kept under tenuous guard barely outside the arena of the distracting entertainment. What Geertz recommends more immediately suggests thick interpretation, a much less happy phrase.

Without describing single cases in detail, with nothing more than an offhand reference to 'the dozens of cockfights I saw in Bali' (1972: 10), Geertz deconstructs the dramatic meaning of a type of event by finding behind its typical ways the workings of multiple social hierarchies and oppositions, such as sexual, religious, village, economic, aesthetic and caste. He adds to this multiplicity of themes the layers of interpretation that thicken as subjects interpret each other's interpretations. Like a psychoanalytic essay, a case serves to demonstrate the workings of the anthropological subconscious: the author portrays a depth of meaning that the subjects feel, but, lost in their heavy bets on the matters manifestly at stake, could not readily state.

Presumably as an unintended consequence, these differences in styles of presenting data in relation to analysis segregate academic audiences. But ethnographies in any style may be appreciated when they contain 'rich and varied', 'contextualized' or 'context-sensitive', and 'densely textured' data. There are at least three useful dimensions buried in these notions. First, 'richness', 'texture', and 'contextualization' capture qualities in the portrayal of specific instances. They praise descriptions that show in fine detail how people as they act take account of and respond to their immediate social and material landscape. Second, 'varied', 'massive', and 'dense' turn attention to the data set. They are useful for suggesting that the data as an aggregation describe a great multiplicity of scenes, actions, or cases. A crucial third idea also resonates in these phrases: 'densely textured' and 'finely detailed' suggest that the research focus has been narrowed and made consistent, such that the data as a whole are characterized by close variations of similar scenes or lines of action, not widely scattered snapshots of a vast terrain.

Overall, the appreciation is not unlike the statistical researcher's satisfaction at seeing the appearance of curves that emerge when they connect clusters of data points on a graph. Earlier, in discussing how fieldwork data can be organized strategically, it was helpful to think of data sets that are either split between contrasting versions of plausibly explanatory or explained phenomena, or that follow instances of transformation over their critical turning points. Here, it will be helpful to think of fieldwork descriptions that

cumulate as dots which, in the close variation of each from the others, allow for the perception of emergent figures or themes.

It is commonly remarked that little is written about how researchers turn from describing fieldwork findings to developing analytically explicit explanations. The problem with excess description is too easily attributed to a lack of theory in ethnography. The problem routinely is the opposite, an excess rather than a deficit of explanatory ideas. Initial hypotheses float around almost all ethnographic projects in the form both of cultural opinion and as implications from prior studies. As one writes fieldnotes and conducts interviews, so much can seem relevant to so many important debates that resulting data, despite their enormous collective volume, become scattered and thin. Better to put theory and explanatory concerns aside in the field in order to describe what seem more or less doings of the same thing, done again and again, in more or less the same place, by different people. Writing fieldnotes is usefully practiced as a craft, distinct and separate from substantive explanatory objectives (Emerson et al., 1995). Then, having compiled a 'massive, densely textured, richly varied' data set, the work of moving from description to explanation will be a matter of exploiting the care that was put into description in the first place.

Perhaps the closest that ethnographers have come to specifying a general approach to mining qualitative data sets for explanatory purposes is 'analytic induction', a methodology which has been used implicitly much more often than it has been made explicit (Katz, in press). Analytic induction is continuous with the common sense practices of much scholarship in the humanities. Researchers do not know quite how to define the thing to be explained at the outset; they search simultaneously for an explanation that will fit all the evidence and for a definition of the problem that, without 'cooking' or hiding the data, makes relevant only the evidence that fits the explanation. As one works toward the double fitting of explanatory factors and the thing to be explained, negative cases give marching orders on a day-to-day basis.

To illustrate how 'varied and dense' data facilitate the development of causal explanation via analytic induction, consider Donald Roy's study of quota restriction in the factory. It took shape as he made detailed records on his and other workers' output variations. Industrial sociology had developed two explanations, neither of which fit what Roy found when he went to work in a machine shop. One used notions of worker resentment to explain quota restriction. But Roy found that workers at times ridiculed each other for *not* meeting quotas. Another theory would explain quota restriction by an analysis of economic self-interest: the more work put out, the more likely the management would revise the piecework system to require more output for the same pay. But the workers in several ways acted inconsistently with economic self-interest. At times, they overtly disparaged

money motivations. Even after arranging their output so that they could idle for hours, they sometimes passed up lucrative opportunities to do easy work on overtime pay scales. And although they would often slow down, such slow periods were disdained as boring and physically fatiguing. The workers would often race each other to produce as rapidly as possible with the fewest errors.

These data were densely detailed. They focused on one area of behavior; they were not scattered around the factory's social life. They were richly varied, showing not only quota restriction but work patterns inconsistent with that characterization. They were thus nicely prepared for analytic induction, which strengthens explanations by modifying them through encounters with negative cases. By shifting his explanatory focus to 'work satisfaction', i.e. by changing his definition of the thing to be explained from quota restriction, profit maximization and other definitions of the problem, Roy could make sense of all the rich variations in his data. Work satisfaction meant a lively experience of self-definition. That might require slowing down or speeding up. As a first order of business, it meant not screwing oneself by triggering a readjustment of the piece rate, a result dreaded for its handing over control of personal identity to management. Satisfaction also meant playing a game, enjoying control over interaction in free time, expressing aggression against management, and being held in esteem by close peers. The key drive was not money or resentment, but an interest in exploiting the work setting so that it gave back vivid news about the self (Roy, 1953: 513).

Roy never stated his social psychological perspective in terms of self-exploration. Instead, across his writings, he would emphasize how, in the variety of settings in which he worked and studied, he was struck by workers' passions for gaming, and the power of gaming to make even the most routine industrial jobs pleasurable.

> [I]ndustrial workers . . . have an unflagging interest in games, any old game. Their thoughts slip easily into the activities of favorite teams. Work times are often emotionally charged with wagering on whatever is going on at the time. . . . Betting pools appear on the shop floor with regularity. Here, apparently, is the drama of life for the modern worker. (Roy, 1980: 332)

While careful to avoid rate busting that would earn the enmity of peers, they would push themselves for their own benefit:

> Sometimes in his sweat and frenzy, arms flailing to blend in mechanical motion with his spinning machinery, the operator may be hailed by a sympathetic fellow worker: 'All you need is a broom up your ass and you can sweep the floor, too.' (Roy, 1980: 332; see also Roy, 1959–1960)

Roy's illustrative incident here is luminous to the point of sensational.

Does he err by appealing to readers' non-scientific emotions? Ethnographic data *should* systematically over-dramatize the colorful character of people, their settings and their conduct, for reasons rooted in the logic of empirically grounding and testing causal explanation. To illustrate I would like to draw on 'Pissed Off in LA', a chapter from *How Emotions Work* (Katz, 1999: chapter 1) that attempts to explain the emergence and decline of anger among drivers.

The data are dense in ways that make them at once methodologically useful and colorful. First, they come from multiple sources: participant observation, interviews with drivers recounting their recent experiences of 'road rage', and, especially useful, incidents recalled from the passenger seat. Second, their density comes from showing fine variations on behavior within a specific practical environment, that of driving a car. (That it is a common environment, one that the reader is likely to know well, does not hurt, so that he or she may readily add her/his own examples to the data set.) Third, the events are dense with drama because forceful emotions arise and, usually, decline in short periods of time. Read as a series they become tiresome, but each narrative has its moments of compelling interest. This temporal structure facilitates testing causal theory, making each case, analytically, multiple cases. Conditions that are hypothesized to lead to anger, like feeling 'dumb' from an inability to get recognized by another, should be present not only when anger rises but in reverse when anger subsides, as it regularly does when the offending driver acknowledges his infraction.

Fourth, I detail personal styles of outfitting cars, of communicating with other drivers, and of using stereotypes for cursing. I note the ethnicity, age, gender, class status, and other social characteristics which as a whole indicate that the sample is an extremely diverse set of people. I specify what was going on with the drivers before they were cut off, where they were going, what mood they were in, the fantasies they were entertaining, what they were doing with passengers. All of these 'colorful', arguably frivolous details are there for methodological reasons. They are ways of holding constant and ruling out rival explanations: for example, that only people of a certain age, gender, or ethnicity get pissed off when driving; that one must have been in a rush to be offended when cut off; that one must have had a bad day or have been frustrated in some regard to make such a big deal out of such a minor matter.

Finally, the variations in experience among the people present are noted. Passengers, for example, are often in the same traffic, going the same place, in more or less the same mood as the driver before the incident arises, but while the driver gets mad, they often get scared or find the event hilarious. Perspectives from the passenger seat highlight the embodied experience of driving as a key causal condition. Part of the explanation of 'pissed off'

driving will have to treat seriously the surreal claim that drivers, in driving, actually become merged with their vehicles. Then, when their cars are cut off, *they* are immediately and intimately cut off.

The drama in these data does not provide a good representation of the experience of driving in Los Angeles in a statistical sense. The 'color' of the descriptions emerges as an unintentional but *strategic misrepresentation*, because data gathering and presentation is guided by the methodological goal of creating not a representative sample of driving but a closely detailed, massive, finely varied set of descriptions of anger emerging and declining in a given practical context. The researcher can gather such data by seeking what appear, in a gut reaction, to be novel cases; data gathering is shaped almost casually by a lay sensibility to dramatic quality. It is not necessary or even helpful to think constantly about what scientific evidence requires; the ethnographer concentrates on getting the facts recorded in as precise detail as possible. Whether advertently or not, the ethnographer serves the scientific logic of causal explanation by collecting tightly clustered, finely differentiated cases, each of which can serve to negate and force revision of explanatory ideas. The 'richness' of the data set refers to the resources it contains to develop causal explanation.

Acknowledgements

Notes

Editor's note: the second part of this article will appear in the next issue of *Ethnography*.

1 I sidestep the anthropologists' debates on the problematic status of writing on culture, which to some has meant that 'hanging out', extensive partici-pant observation, and the production of reports about 'life among the so-and-so' no longer makes much sense. For sociologists, the focus has not been on what is useful for writing about culture, much less about 'cultures'

as distinct unities of meaning, but on people and the methodological culture that is useful for describing people in order to explain their social lives. 'Culture' may have lost its moorings in any particular place around which a participant observer might 'hang', but it is something else to claim that people now shape their conduct without reference to the interaction situation they are in, specifically the practical resources and obstacles they find at hand for defining how they will be perceived by and thus responded to by other people who may be around or have access to them. If there is increased sensitivity to *global and virtual culture*, there is also, in a simultaneous and balancing trend in social thought, increased attention to how human *action is embodied* (Bourdieu, 2000). There is no necessary conflict between these perspectives. 'The attention to "reading" cultural products and public representations . . . does not displace but complements the characteristically anthropological emphasis on daily routines and lived experience' (Gupta and Ferguson, 1997: 5).

2   Experiments purport to attribute causation by, in effect, describing behavior over 'before' and 'after' times, but they also require control groups, i.e. a cross-sectional comparison. And if done in labs or other artificial situations, they will require further cross-sectional comparisons to overcome doubts about generalizability.

3   See the discussion in Pollner and Emerson (2001), especially the relevant distinction between early, breaching forms of ethnomethodological studies, and later studies in which the researcher attempts to become a member. For a useful demonstration of how one can pursue ethnomethodological questions with ethnographic data, see Goode (1994).

4   Part of the organization's solution was to create a special staff that worked on appellate and class action cases, essentially a staff that only represented clients they never saw. Other devices are routinely used by lawyers who greet, interact with, and part from a steady stream of clients every day. One is to end service interactions by negotiating the appearance of non-endings. A client will be told some version of 'your case has substantial merit, and we could sue on your behalf, but before we can, the judge would require that we first try x and y, so gather these specific papers, make these specific calls, and if that does not work out to resolve the problem, come back'. The door, eternally left open, closes on client after client. No one systematically tracks whether clients return. When they do not, staff lawyers can comfortably take for granted that everything worked out to the client's satisfaction.

5   Although Malkki does not pursue the lead, it should be noted that the bifurcation of the data set facilitates causal analysis by highlighting negative cases of counter-group variations within each population. Examples would be nationalistic town Hutus or cosmopolitan camp Hutus. The camp environment institutionalized the tragic past, giving refugees relatively little to do with regard to their identities but to exploit their commonalities;

while the town environment invited diverse 'strategies of invisibility', pro-
viding attractive temptations to downplay Hutu identity in mating, resi-
dential location, and occupational engagement. But opportunities are
nowhere distributed with perfect equality, nor are constraints anywhere
experienced with precise similarity. By investigating the biographical and
social interaction processes leading to exceptional, counter-group experi-
ences, the researcher could find clues to the contingencies of nationalistic
(or 'sedentary') ethnic identification.

6 For a study of the social organization of qualitative differences among
swimmers that takes advantage of their rapid transformations, see Cham-
bliss (1989: 1): 'careers in swimming are relatively short; one can achieve
tremendous success in a brief period of time. . . . This allows the researcher
to conduct true longitudinal research in a few short years'.

7 Fighters conceive of boxing not as a springboard for aggression and an exer-
cise in violence but as a skilled bodily trade, a competitive performance craft
requiring sophisticated technical know-how and an abiding moral commit-
ment that will enable them not only to improve their material lot but also,
and more urgently, to construct a *publicly recognized, heroic self*. Boxing is
the vehicle for a project of *ontological transcendence* whereby those who
embrace it seek literally to fashion themselves into a new being so as to
escape the common determinations that bear upon them and the social
insignificance to which these determinations condemn them. (Wacquant,
1995: 507)

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