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Analytic Inspiration in Ethnographic Fieldwork

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Debate over the place of methods and analysis in ethnographic fieldwork comes and goes. Some, such as Barney Glaser and Anselm Strauss (1967), have advocated rigorous and systematic coding, the method entailed becoming the analytic process (see Thornberg and Charmaz, Chapter 11, this volume). Earlier, Herbert Blumer (1969), Everett Hughes (1971), and others championed sensitizing concept formation, which amounted to working analytically in close proximity to empirical material and not straying into grand theorizing. More recently, some have questioned the ultimate empirical grounding of ethnographic methods and analysis, the extreme view being that these are literary projects (e.g., see Clifford and Marcus, 1986).

This chapter describes a perspective that places conceptual imagination at the center of the research process, featuring its transformational qualities for both methods of procedure and analysis. In part, the perspective follows in the footsteps of Blumer's, Hughes's, and others' theoretically minimalist proclivities. But it is more attuned to the

epistemological dimensions of ethnographic engagement, continually tracking the reflexivity of the enterprise (see May and Perry, Chapter 8, this volume). The chapter starts by drawing a stereotypic distinction between quantitative and qualitative methods and analytic procedures. The aim is, by way of contrast, to champion the exceptional theme that researchers need to move beyond such divisions and their related methodological strictures. Slavish attention to procedure shackles the imagination. Highlighted instead is a kind of explanatory excitement not usually addressed in methodological discussions, which we call 'analytic inspiration.'

Some may claim analytic inspiration is more evident in qualitative than in quantitative research, a view we do not share. Some have flagged it themselves by other names, such as finding analytic 'hooks' or applying explanatory 'punch.' Some would resist considering it methodological because it has no procedural rules. But it is palpable, describable, and holds the keys to understanding. It can change everything, even while none of

what it changes can be adjusted to readily bring it about.

We present three illustrations of how analytic inspiration develops in ethnographic fieldwork, leaving it to others to illustrate it for other research traditions. We take the liberty of using Harry Wolcott's (1999) apt phrase 'a way of seeing' as a working synonym for analytic inspiration. If Wolcott applied the term specifically to ethnographic understanding, it can refer more generally to imaginings of how the empirical world works in other research contexts. Analytic inspiration is a way of seeing across the board. It brings into view what methods of procedure cannot do on their own.

The first illustration is taken from our reading of Lila Abu-Lughod's (1993) feminist interpretation of Egyptian Bedouin life. Her empirical work is inspired by a storied sense of culture, which 'works against' a widely accepted alternative. The other two illustrations come from our own organizational fieldwork, so they will be more personal. Analytic inspiration in these cases works against formal organizational understandings of everyday life, bringing into view the way organization is socially situated and interactionally constructed.

MOVING BEYOND PROCEDURE

It is a time-honored saying that qualitative researchers analyse their data as they collect it. This may be contrasted with the quantitative proclivity to proceed stepwise; data collection and data analysis, among other activities, are undertaken sequentially. The common view is that, first, one conceptualizes and hypothesizes something about the phenomenon in question, such as defining one's concepts, formulating an argument about an empirical relationship, and hypothesizing how one expects the relationship to appear in the data. The hypothesis is not an educated guess, but results from careful conceptualization and concise definition. (That is the ideal anyway.) When this is complete,

data collection proceeds. This second step does not unravel the concepts, definitions, or hypotheses. Rather, in quantitative research this step is taken to provide empirical evidence for 'testing' hypotheses and, by implication, their conceptualizations. The third step is to consider how the evidence – 'findings' – accords with what was hypothesized.

Qualitative research, in contrast, is not sequential. (At least, that is the claim.) While concepts, definitions, and hypotheses are evident, they are viewed as 'working' matters – conditional until further notice. The common view that qualitative researchers proceed by the seats of their pants without concepts, definitions, or hypotheses is farfetched, a perspective that Blumer (1969) rebuked decades ago. While qualitative researchers also conceptualize, define, and hypothesize, they do so in ongoing relationship with data collection. They entertain particular concepts, but they do so provisionally until data collection suggests something different. The same holds for definitions and hypotheses. Regardless of how this process transpires, there is a cultivated tentativeness about the steps, which is the reason why qualitative researchers habitually refer to *working* concepts, *working* definitions, and *working* hypotheses.

It is possible, however, to combine elements of both traditions in ethnographic research. To the extent procedure is sequential in fieldwork, it approximates the common view of quantitative research. Linda Mitteness and Judith Barker (1994), veterans of many large-scale field projects, suggest that a sequential process may be the only realistic choice when it comes to managing large data collection teams and navigating huge data sets. Ethnographers conceptualize, define, and hypothesize – tentatively or not – as a way of moving ahead with their work. The idea that one can proceed without concepts, from the ground up, and derive understandings of how things operate that way, was not Glaser and Strauss's (1967) sense of the craft, even if their 'grounded theory' approach has been formularized this way

(see Thornberg and Charmaz, Chapter 11, this volume). Allaine Cerwonka and Liisa Malkki's (2007) portrayal of process and temporality in ethnographic fieldwork is closer to practice on this front.¹

As a way of moving beyond such methodological distinctions, we take our point of departure from the need for analytic inspiration, something that would best be continually present during, not just before or after, the research process. Analytic inspiration not only provides insight, tentative or otherwise, but also supplies a roadmap for how to move along in the research. Inspiration also provides empirical excitement. How exciting, indeed, it is to see one's empirical material coalesce in an unexpected or new way, which is palpable in our illustrations. If representation of this coalescence may have rhetorical elements, it is not rhetorical in the research process; it is a constant and eminently useful ingredient of the craft. Research guided purely by procedural rules, sequential or not, misses the point, which is to provide understanding.² Above all, analytic inspiration should not be confined to a separate domain called 'theory.'

SEEING CULTURE AS NARRATIVE

Our first illustration, taken from Abu-Lughod's (1993) discussion of fieldwork in an Egyptian Bedouin settlement, relates to the adage that life comes to us in the form of stories. If it is a common expression, it also has been taken to heart by narrative ethnographers for analytic inspiration. Conceptualization, definition, and hypothesis formation remain in the mix, but analytic inspiration serves as a leitmotif in the research process. It is a strong partner indeed, as Abu-Lughod suggests. That life comes to us in the form of stories made the difference in how she 'unsettled' common themes of Arab life in Bedouin society, especially as they relate to women, patriarchy, and patrilinearity.

To attend narratively (see Esin et al., Chapter 14, this volume) while observing

carefully is to pay concerted attention to the things people say about their inner lives and social worlds, something that will resonate in our second and third illustrations. Ethnographic fieldwork is traditionally participatory and observational, but it also has been something else – concerned with how people themselves account for experience. People say things about their lives, about others, to others, if not about them, about their thoughts, feelings, and actions. They recount their pasts, describe their presents, and muse over their futures. They comment on groups, some as small as families and marriages, some as large as communities and nations, whether already part of their lives, in formation, or imagined in the distant past or foreseeable future.

Much of this talk is story-like, extended commentary that describes, explains, or dismisses what is thought or figured about matters in question. If what is said comes in the form of mere yeses, noes, uh-huhs, nods of the head, or other brevities, these can nonetheless be story-like when embedded in collaboratively designed networks of exchanges. In the extended interactions observable in ethnographic fieldwork, the 'small' stories of mere yeses and uh-huhs located in chains of interactions can carry the same narrative weight as the 'bigger' stories told in life history interviews (see Bamberg, 2012; Gubrium and Holstein, 2009). As Abu-Lughod suggests about her initially ill-fated pursuit of Bedouin life stories, to think of stories as extended accounts of individual lives is to shortchange the social complexity and agency of accounts.

Reframed as culturally constructive (see Winter, Chapter 17, this volume), Abu-Lughod's interviews offer apt illustration of how a narrative approach inspired her view of culture in general and specifically of the place of women in Bedouin society. As she describes her conceptualization of culture, she brings narrative understanding to the forefront, appreciating cultural nuance. Analytic inspiration may be drawn from the opposite as well – the museum view of culture – in which indigenous meaning is 'fixed' in material and symbolic

systems of shared meaning. But Abu-Lughod's aim is to unsettle cultural generalizations marked by expressions such as 'the' culture of 'the' Bedouins, which in her view takes understanding away from the ordinary production of culture evident in storytelling. She puts it this way:

a serious problem with generalization is that by producing the effects of homogeneity, coherence, and timelessness, it contributes to the creation of 'cultures.' In the process of generalizing from experiences and conversations with a number of specific people in a community, the anthropologist may flatten out their differences and homogenize them. ... The appearance of a lack of internal differentiation makes it easier to conceive of groups of people as discrete, bounded entities, like the 'cultures' of 'the Nuer,' 'the Balinese,' or 'the Awlad "Ali Bedouin,"' populated by generic cultural beings who do this or that and believe such-and-such. ... [There] are good reasons to consider such entities dangerous fictions and to argue for what I have called writing *against* culture. (1993: 9)

Explanatory punch is evident in Abu-Lughod's eye-opening extended interviews with women. Of her book *Writing Women's Worlds: Bedouin Stories*, Abu-Lughod explains:

This book is intended to present, in the form of a narrative ethnography made up of these women's stories and conversations, a general critique of ethnographic typification. ... I decided to explore how the wonderfully complex stories of the individuals I had come to know in this community in Egypt might challenge the capacity of anthropological generalizations to render lives, theirs and others', adequately. (1993: xvi)

As Abu-Lughod presents the women's stories, she is a listener, now procedurally poised to particularize and unsettle 'five anthropological themes associated with the study of women in the Arab world: patrilineality, polygyny, reproduction, patrilateral parallel-cousin marriage, and honor and shame' (1993: xvi-xvii). Referring to the book's chapters titled the same way, she adds, 'Rather than the chapter titles explaining the stories, the stories are meant to undo the titles' (1993: xvii). Themes such

as patrilineality are not 'just there,' ready data to be carefully recorded in field notes and later systematically described in ethnographic writing as 'the' kinship system of Bedouin society.

The thematic unsettling of patrilineality is especially evident in the stories told by an old Bedouin woman named Migdim. They suggest that patrilineal decision-making does not so much rule the roost, so to speak, as much as the roost plays an important role in making that happen. If patrilineality is a theme of Arab society, it is one articulated and animated as much by women as it is instituted by men. The analytic inspiration of narrative understanding brings this into focus for Abu-Lughod, unsettling the theme as women's stories are taken into consideration. Listen to how Abu-Lughod describes a story Migdim tells of her 'arranged' marriage to a gathering of younger women relatives:

One of the most vivid I heard from Migdim was the tale of how she had resisted marriages her father had tried to arrange for her. I even heard more than once, nearly word for word, the same tale of how she had ended up marrying Jawwad, the father of her children. I heard it for the first time one evening that winter; she told it for the benefit of her sons' wives, Gateefa and Fayga, and some of her granddaughters.

She explained that the first person whom she was to have married was a paternal first cousin. His relatives came to her household and conducted the negotiations and even went as far as to slaughter some sheep, the practice that seals the marriage agreement. But things did not work out. The time was over fifty years ago, just after the death of her mother.

'He was a first cousin, and I didn't want him. He was old and he lived with us. We ate out of the one bowl. His relatives came and slaughtered a sheep and I started screaming, I started crying. My father had bought a new gun, a cartridge gun. He said, "If you don't shut up I'll send you flying with this gun.'" (1993: 46-7)

As Migdim continues, she describes the strategies she used to escape the marriage. Patrilineality notwithstanding, Migdim recounts a tale of personal artifice and resistance, which transpires in the face of a sealed

marriage agreement. Her father and relatives eventually come to an agreement based on another arrangement ostensibly made between them, not between them and Migdim. The story thematizes Migdim's active participation in the process. The account also is a vivid lesson for her listeners, the episodes of which highlight Migdim – a woman – as a determining force behind events. The telling is an unsettling cultural narrative for the women listening, who stand to share it again with their own daughters and others. If their own tellings do not reflect or produce the same results, the tellings nonetheless open their actions to what is possible in the circumstances.

DISCOVERING SOCIAL WORLDS

The second illustration of analytic inspiration takes us to an urban nursing home called 'Murray Manor.' Here, especially, we emphasize how analytic inspiration and methodology go hand in hand. As the illustration unfolds, the idea that expertly planned and deployed research technique leads to excellent data is unsettled. The illustration shows that analytic inspiration can make a difference in everything, from understanding, to procedure, to results – to the very meaning of 'excellent data.' Accenting what people do with words shows the analytic way forward.

One of the authors (Gubrium) conducted extensive fieldwork at Murray Manor in the 1970s, leading to the publication of the first book-length ethnography of its kind (Gubrium, 1997 [1975]). We will write in the first person in this section, from Gubrium's viewpoint. We will do the same for the third illustration in the section following, from James Holstein's viewpoint on fieldwork in civil commitment hearings (Holstein, 1993).

Because I was trained as a survey researcher, it wouldn't be obvious how my ethnographic fieldwork at Murray Manor came about. Along with other nursing homes in the metropolitan area where the Manor

was located, it was originally one of several research sites where I'd planned to conduct a survey of residents' quality of life. At the time, a person–environment fit model was a popular analytic scaffold. The idea was that the fit between resident needs, on the one hand, and available institutional characteristics and resources, on the other, affected residents' quality of life. My hypothesis was that the better the fit, the better the quality of life. I wrote a federal grant proposal, but it wasn't funded. Disappointed, but undaunted, and using local funds and my own time, I decided to conduct the survey on a smaller scale in fewer nursing homes, considerably reducing the sample size. The Manor was included in the smaller survey.

I want to emphasize that Murray Manor at this point in my thinking was a survey research location, not an ethnographic field site. The difference is important, because the methodologies put into place and, as it turned out, the kind of analytic inspiration available for understanding the research topic – which eventually would be transformed – would dramatically alter my view of data and the utility of the research findings. I eventually would learn that a change in or new analytic inspiration can change everything.

The explanatory advantage of the person–environment fit model seemed obvious at the time. It moved beyond a simple bivariate model, in which the characteristics of institutions (one variable) related to the quality of life (the other variable). The better the nursing home, it was commonly argued, the higher the residents' quality of life. Instead, I was inspired by the more complex person–environment model, in which the fit between personal and institutional characteristics (two variables) related to the quality of life (the third variable). In this model, it was possible, for example, that low resident expectations might not lead some to demand as much in quality as would high resident expectations. As such, homes that were reasonably adequate could provide a high quality of life for some residents. (Never mind the unsavory policy potential of this model.)

My plan was to conduct interviews with diverse residents in two or three different nursing homes, code the personal and residential data for the target variables, and see how they co-related.

Ironically enough, now on my own and unhindered by the commitments of grant funding, I decided to 'hang around' in a facility, as I unwittingly referred to it then, to get a first-hand feel for life in a nursing home. If my gerontological interests kept nursing homes in view, amazingly I'd never spent much time in a nursing home nor knew anyone who lived there. (This can be par for the course among quantitative researchers.) Several facility administrators had originally expressed interest in participating in my proposed survey, but now I wanted to get a sense of life and work in a nursing home to get my bearings, something more intense than a survey proffered. The problem was that there was a great deal of bad press for nursing homes at the time and administrators were wary of that sort of thing. Only one of them welcomed me to 'look around to my heart's content,' and that happened to be the administrator of Murray Manor, my eventual field site.

I accepted the opportunity and was introduced to members of what I later called 'top staff' – the medical director, the director of nursing, charge nurses on the floors, the dietitian, the social worker, and the activity director. All talked with pride about the quality of care in the home. Top staff introduced me to employees I later called 'floor staff' – registered nurses or RNs, licensed practical nurses or LPNs, and NAs or nurses' aides. Soon enough, members of the floor staff introduced me to the patients and residents. The first floor of the facility was designated as residential care and those who lived there were called 'residents.' The other floors of the Manor were designated for various levels of skilled care and its residents were called 'patients.' This has changed since then; now all care receivers are called "residents" and that's what I'll do here.

So I was all set to hang around, but not mentally prepared to do ethnographic fieldwork.

I was ensconced in what eventually would become my field site, but with old analytic lenses. I figured that the administrator's welcome and the staff's follow-through were points of departure for what eventually would be expanded into a quality-of-life survey. In anticipation of that, I would get to know about the nursing home as a living environment and those who worked there as people. I expected to formulate better survey questions as a result.

An interesting facet of what lay ahead is the gradual change in the ordinary terms I used to refer to aspects of my work. The analytic lesson wasn't apparent at first, and couldn't have been, because I needed a different source of inspiration to recognize it. The terms with which I began, of course, were part of the language of variables, measurement, indicators, and correlates. When the Murray Manor research started to become ethnographic, this gradually turned into the language of social interaction, meaning, and representation. The retrospective lesson in this would be that the working vocabulary and procedural rules we apply in research relate to one's form of analytic inspiration (Gubrium and Holstein, 1997). Terms of reference in research are only as general as the analytic framework in place.

This was evident in the preceding illustration from Abu-Lughod's work. She found herself working against the language of culture commonplace at the time – one bereft of narrativity, member agency, and meaning-making. Instead, she was attracted to a language built from terms such as social construction, difference, contention, and resistance. This altered her method of procedure – from collecting cultural data to witnessing its storied production – and changed the way she chose to represent her empirical material in publications (see Gubrium and Holstein, 2009).

But this is getting ahead of the story. Murray Manor wasn't yet a field site and I didn't refer to it as such. I spoke of it as a 'pilot study' and source of background information for survey research. I wasn't doing fieldwork. I was familiarizing myself with things

before the real research took off. I wasn't yet using ethnographic language to describe my activities, even while I was located in a kind of field and conducting a form of empirical work within it. Systematic participant observation (see Marvasti, Chapter 24, this volume) was far from my mind. Social interaction on the premises and the contexts of meaning-making were, as yet, incidental to my interests and were, consequently, undocumented.

In the months ahead I spent listening to, and speaking with, residents and staff, I don't recall having had a grand conversion to an ethnographic view. If anything, I slowly eased into what initially was only a whiff of fieldwork, done for ancillary purposes. A new analytic framework emerged only as I started to take notice of, and to take field notes about, the particular words and associated meanings that various groups used to refer to caregiving and the quality in life. I couldn't glibly leave my initial terms of reference behind because I needed them in order to relate to an informing person-environment literature. But I did start to catalog ordinary accounts of the quality of life and their situated points of reference.

These started to become proper field notes when I began to think seriously about the everyday connotations of what I had been unwittingly treating as background data. I grew serious about the possibility that there might be different worlds of meanings apparent in what was said about living and dying at the Manor. Still, I hesitated to take this fully on board. My sense was that if my survey-oriented definitions didn't quite fit the residents' definitions, for example, that could be corrected in time. If I found myself saying to myself and others that 'there are different worlds of meaning there' that don't jibe with person-environment fit, I still clung to the model. Seriousness didn't immediately prompt a leap in imagination, only troubled curiosity about empirical complexity.

Here's an example of what I found troubling. One of the ostensible characteristics

of a good nursing home is the quality of the staff, especially the floor or front-line staff. Well-trained and considerate staff members were viewed as important ingredients of the quality of care, and presumably affected the residents' quality of life. The criterion could serve to categorize staff members into good and bad workers, or so I figured at first, and could be used as one indicator of the environmental part of the person-environment fit model. What I began to realize as I gathered preliminary ethnographic data – now in the field – was that good and bad couldn't be figured in terms of fixed criteria such as the background or personal characteristics of the staff. Time and again, I noticed instead that good and bad grew out of resident-staff interactions and was a matter of perspective. If, for some, the bad worker was inefficient and didn't conform to established standards of quality care, the same characteristics could signal good work to a resident who wanted a familiar face to 'stay and sit for a spell.'

Here's another troubling example. I coined a catchy term for the activities involved in keeping the premises neat and orderly and the residents dressed and tidy. This was the immediate responsibility of the floor staff. I called it 'bed-and-body work.' If, to the residents, 'staying a spell' and otherwise being attuned to personal needs signaled good care, bed-and-body work was equally significant. Keeping the premises clean and odor-free, keeping beds made and the surroundings otherwise attractive, keeping residents' skins and clothing free of bodily waste were important ingredients of good care for everyone. According to the top and floor staffs, families, and those residents who could care about it, follow-through on this front surely improved the quality of residential life.

But, here again, leaving it at that proved to be too simple; it failed to take account of the interactions and sentiments involved. It wasn't bed-and-body work as such that differentiated staff, family, and residents' understanding of quality. Rather, the associated

sense of *for whom* bed-and-body work was undertaken made an important difference. When residents perceived bed-and-body work such as keeping them clean to be a matter of 'just getting it done' as opposed to actually 'caring,' it was viewed negatively. It mattered that all the standard quality-of-care criteria in this area were perceived as being done *for* the residents as opposed to 'just getting it done.'

This perspectival stance was the analytic hook needed to understand the complexity, which eventually led me to think the previously unthinkable: No set of quality criteria worked in all circumstances and from all perspectives. Generalizations (see Maxwell and Chmiel, Chapter 37, this volume) such as this helped to move me beyond thinking of what I was recording as background information and into proper field notes about meaning-making. Taken together, the notes gathered from staff, residents, and family interactions were becoming ethnographic data about diversity in meaning.

The shift to concerted ethnographic fieldwork required a more complex, dynamic form of analytic punch. What I was observing and dutifully recording as field notes needed the kind of analytic inspiration that would bring things together into a transportable argument about the quality of life in human service organizations. It's one thing to refer to empirical material as reflecting 'different worlds of meaning,' it's another matter altogether to start thinking that 'an' organization such as a nursing home could house different social worlds constructed out of the ordinary members' interactions, which could also transform from one occasion to another.

It was as much a turn away from the homogeneity assumption underlying the language and idea of 'an' organization, as it was the plural 'worlds' I was documenting, that made the difference. Working against the concept of 'the' organization ostensibly in place was my way of unsettling the desire to measure the quality of care. Thinking in terms of possible worlds, socially organized together within one facility (or scattered

about the landscape of everyday life, as it otherwise might be), eventually did the analytic trick. The possible social worlds of the nursing home (of any organization really) opened my eyes to an entirely different way of proceeding. It put into bold relief the idea that formal organization was something different from social organization, that one couldn't be readily discerned from the other. The idea that the logic of one was different than the logic of the other framed my ethnography of Murray Manor. I now understand this as a matter of analytic narrativity, in which a new way of storying empirical material changes everything.

DOCUMENTING COLLABORATIVE CONSTRUCTION

Our third illustration highlights the way analytic inspiration can transform one's research question. Here again, we write in the first person, this time in Holstein's voice as he recounts how an altered perspective not only alters the research direction, but in this case also challenges leading views of the labeling process.

Like many sociologists and graduate students in the 1970s, I was fascinated by animated discussions of the labeling theory of deviance (see Kitsuse, 1962). The gist of the labeling argument was that 'residual deviance' such as mental illness was identified and stabilized by societal reaction (Scheff, 1966); mental illness was as much a matter of labeling as it was an intrinsic condition. Some argued that non-psychiatric factors – social contingencies and structural variables such as race, gender, social class – were more important in determining the likelihood of being identified and treated as mentally ill than were psychiatric factors. (See Holstein, 1993, for a synopsis of the controversy.) Involuntary mental hospitalization became central to the debate because it involved formal procedures whereby mental illness was determined and reactions to it were explicitly specified.

When I found myself in a postdoctoral position at UCLA, Robert Emerson pointed me to a courtroom in Los Angeles (which I'll call Metropolitan Court) that handled only mental health-related cases, including involuntary commitment hearings. My first visit to the courtroom revealed a striking display of the process about which I'd read so much. Florid psychiatric conditions were on full display, as were the side-effects of their remedies. So were the social processes of labeling and responding to troubles – both psychiatric and social.

Reading Erving Goffman (1961), Harold Garfinkel (1956, 1967), and Robert Emerson (1969) primed me to see the courtroom as a stage for the ceremonial moral degradation and denunciation to which candidate mental patients were subjected in order to account for and justify their involuntary commitment. Sitting in Metropolitan Court, it was hard not to see 'social forces' operating 'behind the backs' (and beyond the vision) of courtroom actors. I was captivated by two questions: *What* is going on here? *Why* do decisions turn out the way they do? On one hand, the answers seemed obvious: the social contingencies of troubled and disadvantaged persons appeared to account for their involuntary commitment. On the other hand, it wasn't clear how this actually transpired, given the extraordinary range of factors and troubles that seemed to characterize each case.

A new analytic inspiration eventually helped me sort through these matters and clarified my research focus, ultimately changing my fundamental research questions. As I watched court proceedings, it dawned on me that there was an important (perhaps even prior) question that I was not asking as I watched courtroom proceedings: *How* were involuntary commitment proceedings and decisions socially organized? It's not surprising that I should eventually ask this question, given that I was working in the sociology department at UCLA, ethnomethodology's hallowed ground. From the beginning, ethnomethodology has been preoccupied with the *hows* of social organization

(see Heritage, 1984). As such, the inspiration to concentrate on the *hows*, rather than on the *whats* and *whys*, of court proceedings was close at hand.

Examples from my field notes and subsequent analysis reveal the difference this would make. As I began to study Metropolitan Court in earnest, I carefully recorded notes – brief narratives that Emerson et al. (1995) call 'jottings' – about what was going on in the hearings. I also recorded jottings of casual conversations or informal interviews I had with court personnel. At the end of each day, I would clean up my jottings and write analytic memos regarding what I observed. The jottings and memos were fairly substantive at the time, concerned with what I observed and with the larger patterns of labeling going on in the courtroom. These *whats* initially took precedence over the *hows* of the matters in view.

Early on, I came across an intriguing aspect of the hearings that District Attorneys (DAs) – whose job it was to seek involuntary commitment – called 'letting them hang themselves.' Several times in brief conversations, DAs indicated that their job was relatively straightforward. They said that candidate patients would reveal symptoms of mental disorder and interactional dysfunction if they were simply allowed to speak without constraint. Candidate patients would say something incriminating if they were allowed to speak their own minds. According to one DA, this amounted to 'getting them up there [on the witness stand] and just let them talk.' The implication was that if candidate patients were allowed to talk freely, they would almost invariably 'hang themselves,' or 'do themselves in.' As one DA stated, 'You let them talk and they hospitalize themselves.' The operational sentiment was candidate patients did this on their own; this was apparent in their actions if given a chance to reveal itself.

There did seem to be quite a few instances of candidate patients 'doing themselves in,' but was it as simple as that? Drawing from my field notes and a related analytic memo, I

can reconstruct how I initially viewed one particular case involving a candidate patient I called 'PG,' a white female, perhaps 25–35 years old, with a long history of psychiatric treatment. My notes indicate that the DA began to cross-examine PG with a series of questions that appeared to explore PG's 'reality orientation' (Do you know where we are today? Do you know today's date?). Eventually, PG said that if she were released, she would go to see people who would help her 'recharge,' as she put it. The DA asked her to elaborate, and PG soon made an apparently delusional claim that she received rejuvenating 'power from the life force.' Soon thereafter, in summarizing his case to the judge, the DA argued that PG was 'delusional' and she 'lacked the ability to carry out the most basic tasks of everyday life.' He explained that PG was unable to focus on the important matters at hand even though she knew it was urgent for her to be on her best behavior. The hearing ended with the judge declaring that PG was 'gravely disabled' and 'unable to provide for her own upkeep due to her severe delusions and inability to focus properly on the important matters at hand.'

One of my analytic memos reads that 'PG seemed to hang herself.' My summary jottings indicated that the DA patiently allowed PG to talk about mundane matters until PG's delusions emerged. Other notes indicated that 'PG was under a lot of stress.' She was 'out of her element.' She didn't seem completely in touch with what was going on. The notes indicated that this may have been due to the side-effects of medication. I also noted that everyone else in the hearing was a professional (and male) and they looked the various parts. PG was dressed in institutional pajamas. She had been brought directly from (the State Hospital) to the hearing and wasn't given the opportunity to make herself 'presentable.' My notes read, 'See Garfinkel, Goffman on degradation.' These were some of my *what* questions.

Summary jottings also suggested that PG really didn't know her lawyer (a public defender) and 'was not adequately prepped'

for her testimony. Additional notes indicated that she did not have access to the full range of legal safeguards or resources that might have been used to prevent her commitment. The notes suggested that while PG was delusional, multiple 'social contingencies' were at work, indicating that psychiatric factors were not the only determinant in the hearing outcome. These were *why* concerns.

Clearly, in tracing *what* was going on in this hearing, I was sensitized to the non-psychiatric (*why*) factors that could have influenced the hearing's outcome. The concerns of prior labeling studies were apparent in the ways I was prepared to account for this and other hearing outcomes. PG had, indeed, contributed to her own 'hanging,' and it was easy to speculate about the myriad social contingencies that were working against her. There was a great deal going on here, sociologically, but the complexity of the proceedings made a rigorous empirical explanation difficult since many possibly influential variables (e.g., social class) were not proximally apparent. In other instances, key variables seemed to operate in multiple ways.

My inability to get a grip on this opened the door to new analytic inspiration, changing the focus from *what* and *why* questions to how the moment-to-moment activities and realities of the court were interactionally organized. This would sharpen and narrow the research focus to what would be immediately visible. As simple as this shift sounds, its procedural and explanatory implications were profound. The concrete upshot of the change was apparent in the very way I conceived of and recorded happenings in the field. In order to grasp how interactional matters transpired, I began to pay much closer attention to social interaction, the turn-by-turn dynamics of courtroom talk. This was not a doctrinaire shift to a conversation analytic agenda, but it did involve greater appreciation of the sequential environment of courtroom talk.

Jottings and summary field notes were insufficient for this type of analysis. Instead, I began to produce close-to-verbatim 'do-it-yourself'

transcripts of the commitment hearings (see Gubrium and Holstein, 2009; West, 1996). The procedural shift is evident from a before-and-after glance at my field notes. Jottings and detailed summaries were replaced by imperfect utterance-by-utterance records of courtroom talk. The initial drafts of my notes contained no summary, commentary, or analysis (although I would try to add summary comments afterwards). They were merely transcripts to be closely scrutinized and analysed later for their socially organized and socially organizing components.

Consider, for example, the following transcript and subsequent analysis inspired by the question of *how* candidate patients ended up 'hanging themselves.' This is a slightly revised version of the actual do-it-yourself transcript I captured in my notes. It was chosen because it parallels the case described above and clearly illustrates some of the ways in which the shift from *what* and *why* to *how* questions affects the ethnographic enterprise, in this case shaping what actually was put down on paper and the related sense of what constituted relevant field data. Formerly descriptive notes of happenings and personal characteristics (*whats*) turned into displays of collaborative construction (*hows*) of the matters formerly being documented.

Lisa Sellers (LS), an apparently poor black woman, perhaps 25–35 years old, illustrates how what the DAs called 'letting them hang themselves' was collaboratively accomplished, not just personally emergent (see Holstein, 1993). The do-it-yourself transcript of the DA's cross-examination in this case includes a series of 14 direct questions (not shown here) to which Sellers responded with brief answers (What's your name? Where are we right now? Where do you live? What day of the week is it?). This series comprised 14 straightforward question–answer pairs. There were no notable pauses at the end of questions and answers (i.e., possible speakership transition points), nor were there any intrusions or interruptions of one party by the

other. At the end of this sequence, the DA began to pursue a different questioning tack:

1. DA: How do you like summer out here, Lisa?
2. LS: It's OK.
3. DA: How long have you lived here?
4. LS: Since I moved from Houston
5. ((Silence)) [Note: if unspecified, time is one to three seconds]
6. LS: About three years ago
7. DA: Tell me about why you came here.
8. LS: I just came
9. ((Silence))
10. LS: You know, I wanted to see the stars, Hollywood.
11. ((Silence))
12. DA: Uh huh
13. LS: I didn't have no money.
14. ((Silence))
15. LS: I'd like to get a good place to live.
16. ((Silence 5 seconds))
17. DA: Go on. ((spoken simultaneously with onset of the next utterance))
18. LS: There was some nice things I brought
19. ((Silence))
20. DA: Uh huh
21. LS: Brought them from the rocketship.
22. DA: Oh really?
23. LS: They was just some things I had.
24. DA: From the rocketship?
25. LS: Right.
26. DA: Were you on it?
27. LS: Yeah.
28. DA: Tell me about this rocketship, Lisa.

The sequence culminates in Sellers' seemingly delusional rocketship reference, with the DA avidly following up.

The detailed transcript and central question of *how* Sellers came to 'hang herself' yielded a significantly different analysis from that of PG's hearing above. Differently inspired, one can make the case that Sellers did not simply or inevitably blurt out the apparently 'delusional' rocketship reference as evidence of some troubled inner state or mental incompetence. Rather, I was able to view how the rocketship utterance came into play as a matter of conversational collaboration and Sellers' related interactional *competence* (see Holstein, 1993).

In examining how this exchange was organized, note that the DA significantly changed the question and answer pattern that had emerged as the normative expectation for the interrogation. After the previous series of questions that were answerable with short, factual replies, in line 1, the DA now asked an open-ended question. In his next turn (line 3), he returned to a more straightforward question, but when Sellers produced a candidate answer (line 4), the DA declined to take the next turn at talk. A silence emerged following line 4, where a question from the DA had previously been forthcoming. The gap in talk was eventually terminated (line 6) by Sellers' elaboration of her prior utterance.

In line 7, the DA solicited further talk, but this time it was not in the form of a question. Instead, it was a very general prompt for Sellers to provide more information. The adequacy of a response to this kind of request, however, is more indeterminate than for a direct question. In a sense, the DA put himself in the position to decide when his request for information was adequately fulfilled. The adequacy and completeness of Sellers' response thus depended, in part, on how the DA acknowledged it.

At line 9, the DA did not respond to Sellers candidate answer at the first possible opportunity. When silence developed, Sellers elaborated her previous answer (line 10). The DA did not respond to this utterance either, and another noteworthy silence ensued. Such silences signal conversational difficulties, troubles that implicate the prior speaker, who typically attempts remedial action. Sellers did just that by reclaiming speakership and embellishing a prior utterance on several occasions (lines 6, 10, 15, and 17). In each instance, she filled silences with her own talk, all competently accomplished.

Several times, then, in the course of this conversation, the DA's refusal to take a turn at talk provoked Sellers to continue her own turns. At line 12, the DA encouraged this practice by offering a minimal acknowledgement (Uh huh), which implied that an extended turn at talk was in progress but was not yet

complete. He used this brief turn to subtly prompt Sellers to continue, which she did (lines 13 and 15). Her responses, however, met only with silence. At line 17, the DA explicitly encouraged Sellers to 'Go on,' which she did by changing the line of talk to focus on 'some nice things (she) brought' (line 18). The DA again declined speakership (line 19), then offered a minimal prompt (line 20), to which Sellers finally replied with 'Brought them from the rocketship' (line 21). This utterance elicited a strong display of interest from the DA ('Oh really?' – line 22), who then actively resumed questioning Sellers about the rocketship.

The DA's 'Oh really?' was a compelling display of interest. In the difficult conversational environment that had emerged, it provided a landmark toward which Sellers might orient her talk. Put differently, it signaled that the prior utterance was noteworthy, even newsworthy. Responding to this, Sellers launched a new, more successful line of talk, 'success' being defined in terms of the ability to re-establish and sustain a viable and dynamic question-answer sequence. In vernacular terms, the rocketship statement and its aftermath helped Sellers keep up her end of the conversation. But it also helped her 'do herself in.' In a sense, Lisa Sellers engaged in practices commonly followed in similar conversational circumstances. She *used* the rocketship reference to deal with conversational difficulties and elaborated it to sustain a thriving line of talk. She competently fulfilled her conversational responsibilities, but, in the process, displayed her mental incompetence. Only close examination of the sequential context of conversation makes this evident.

To summarize, in my initial observations of Metropolitan Court, I typically looked past conversational structure (see Toerien, Chapter 22, this volume) and dynamics, which were heard but not noticed. This was the case both procedurally – in the way I took field notes – and conceptually – in the way I formulated summaries of the proceedings with little mention of the interactional dynamics themselves. Initially, the field included constructs or variables not actually evident in

the hearing talk but arguably operating at some other level to shape hearing outcomes. But this field did not – as a practical, procedural, or conceptual matter – include the turn-by-turn conversational practices and structures comprising the hearings themselves. New analytic inspiration transformed the field at least partially into the sequential environment of conversational turn-taking and adjacency pairs. The analytic mandate now was to describe in close detail and explain how the recognizable, orderly, observable interactional regularities of the courtroom proceedings were collaboratively accomplished, in situ, not analytically imported.

This transformation of perspectives resembles Abu-Lughod's shift in focus from merely describing culture (*writ large*) to analysing its narrative production. Hers was also a shift in emphasis to *how* questions, inspiring her to imagine culture in the local telling of stories. Exploring *how* questions clearly yields different sorts of reports and analyses than those emerging when questions of *what* or *why* focus research attention. Sources of inspiration are key to what can be seen, heard, described, and reported.

INSPIRATION AND METHOD

We hope these illustrations have shown how new ways of seeing can be analytically inspiring and bring punch to ethnographic fieldwork. As we noted, while there is no rule of thumb for inspiration – it is in the nature of the beast – it is palpable and describable. Inspiration is not procedural in that regard, because it is not derived methodically. Rather, it is closer to imagination; it is a leap in perspective that produces a new way of seeing things otherwise on display before our very own eyes.

Yes, the punch of analytic inspiration is rhetorical. It persuades as it inspires. But what it persuades us of is not derived from rhetorical tropes, but rather from the persuasiveness of insightful understanding, something centered in what comes into view in analytically satisfying ways. Like jokes told without an

apparent punch line, empirical material and analysis without punch fall flat. We come away saying, 'Yes, I heard it, but what was that about?'

In her ethnographic fieldwork, Abu-Lughod sought cultural understanding. What opened her eyes to what she had been viewing was imagining herself observing cultural construction. The same was the case for Gubrium's pilot survey of the quality of life in a nursing home. Seeing the quality of life as a matter of perspective and social sentiments was inspirational in transforming a study of assessment into documenting sectors of meaning. Holstein's analytic impatience with labeling theory raised critical questions about the empirical status of labels, providing a route to seeing labels in the courtroom as a matter of collaboratively doing things with words, not simply being a victim of them.

If analytic inspiration is not straightforwardly procedural, neither is it simply empirical. None of the three ethnographers whose work we illustrated could have been closer to what they were studying. Abu-Lughod lived in the settlement where she conducted her observations. Gubrium spent months in various locations in the nursing home he observed. Holstein was a daily eyewitness to court proceedings. Their respective viewings were intense and extensive. While concertedly empirical, it was new ways of seeing that made a difference. What developed from the ground up for them was embedded in new imaginings, not simply discovered in data.

We stated earlier that analytic inspiration changes everything. A new way of seeing makes a difference on several levels. The very nature of what is being observed can change, the method of data collection is altered, the relevance of empirical observations is transformed, and the manner of reporting findings is altered. If analytic inspiration changes research practice, this is not to say that being methodical in data collection, systematic in thinking about empirical material, and accurate in reporting the results no longer matter. Analytic inspiration is not license for procedural recklessness. The aim

still is systematic, empirically centered understanding. The key question is: Which way of seeing things provides an inspiring way of viewing those things? This is not a matter of doing away with methods, but making analytic inspiration an integral part of them.

NOTES

1. Glaser and Strauss's (1967) original idea of grounded theory, presented in their book *The Discovery of Grounded Theory*, was a reaction to what at the time was called 'grand theory,' especially the emphasis on the verification of theory. While not dismissing verification, Glaser and Strauss argued for a more balanced view of the place of theory in social research. They underscored the need to view theory as a form of abduction, in which theory formation goes hand in hand with data collection, which Cerwonka and Malkki (2007) describe as 'tacking' back and forth between the two in practice. It was not a particular kind of theory that Glaser and Strauss had in mind, but rather a perspective on how theory of any kind should develop and be used in social research.
2. While Glaser and Strauss's (1967) perspective on the place of theory in social research rewarranted the value of qualitative research at a time when quantification was dominant, the perspective was linked with a recipe-like view of analysis, especially coding, which served to formalize 'discovery' and work against analytic inspiration.

FURTHER READING

- Cerwonka, Allaine and Malkki, Liisa H. (2007) *Improvising Theory: Process and Temporality in Ethnographic Fieldwork*. Chicago: University of Chicago Press.
- Gubrium, Jaber F. and Holstein, James A. (2009) *Analyzing Narrative Reality*. Thousand Oaks, CA: Sage.

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