Joe Soss. Forthcoming. "Talking Our Way to Meaningful Explanations: A Practice-Centered View of Interviewing for Interpretive Research. " In D. Yanow and P. Schwartz-Shea, eds. Interpretation and Method. Armonk, NY: ME Sharpe. Chapter 6.

"You're just a number."
--Sarah, client in the Social Security Disability Insurance (SSDI) program

"I felt like a number."
--Alissa, client in the Aid to Families with Dependent Children (AFDC) program

Sarah and Alissa used similar words, but did they mean the same thing? And what, if anything, did their words signify about the ways they understood and oriented themselves toward state welfare agencies? Throughout 1994 and 1995, I interviewed SSDI and AFDC clients, listening to individuals in each program say that agency workers "treat you like a number" and "you feel like a number."[1] The consistency of language was impressive. Had I treated the words as literal reports of emotion (coding them as "respondent did / did not feel like a number"), my analysis would have suggested no difference across program groups. Alternatively, I might have inferred that clients were using a metaphor to say they had been treated as less than human. But this interpretation, reasonable it may seem, would have been based on nothing but my own intuitive reading and would have led to the same mistaken conclusion: equivalence across groups.

The first approach (coding literal language) would have sidestepped the thorny problem of meaning in order to get on with the task of converting words into a form of data more suitable for variable-based analysis. The second approach would have solved the problem by fiat, imposing a fixed meaning based on my own assumptions about shared common sense. As an empirical matter, however, I wanted to know how *participants* understood their welfare relationships—what conceptual frameworks *they* used to make sense of their encounters with government. I wanted to know where such understandings came from and how they led clients to see particular courses of action as permissible, reasonable, and right. These research goals demanded that questions of meaning be placed at the forefront of empirical research, rather than being pushed to the side or settled on the basis of assumptions I had carried into my fieldwork.

Like any approach to generating social science evidence, the in-depth interviews I conducted were imperfect in many ways and inappropriate for some purposes. One advantage of this method, however, was that it permitted me to treat client statements as more than a series of discrete verbal reports to be coded, each in its own right, and then correlated with one another. It allowed me to pursue the meanings of specific statements by locating them within a broader web of narratives, explanations, telling omissions, and nonverbal cues. The open-ended format of my conversations with clients, and the large bodies of text they produced, made it possible to explore how individual comments fit together as parts of a more meaningful whole. Indeed, the parts and the whole, as I gradually came to understand them, could be used as a kind of commentary on one another. Small, seemingly isolated statements hinted at broader conceptions; their patterns of convergence and discord offered a way to develop, assess, and revise an emerging account of latent understandings. At the same time, as my inferences about broader conceptions took shape, they offered a contextual standpoint for making sense of each individual comment and for linking seemingly unconnected remarks.

Through this process of tacking back and forth (Taylor 1979), I came to see that AFDC and SSDI clients meant different things when they said they "felt like a number." SSDI clients often deployed this phrase while describing the pleasant but uninterested demeanor of the bureaucrats they spoke with in impersonal telephone conversations. When telling a story about such an interaction, clients occasionally acted out the lines of the agency worker with a stiffened back and the voice of a robot. In this group, "feeling like a number" was part of a field of metaphors that included being a "needle in a haystack" and a blip "lost in their computers somewhere." It was almost never accompanied by references to feeling degraded, mistreated or vulnerable. Rather, clients used the phrase to express their perception of anonymity as participants in the program as well as the ambivalent feelings that accompanied this sense of status. The number metaphor expressed a small lament that clients were not really known "as themselves" in the program. But it also signaled a highly valued sense of privacy from the state (relief that their cases did not seem to be under close surveillance) and a sense of security about equal treatment under program rules (relief that no one seemed to be singled out on a personal basis). I asked Sarah, "What do you mean when you say you're a number?"

> Well, a lot of people say "I feel like a number." Well, you feel like one of millions getting SSDI. So, that means you don't feel like they're watching over you. They can't watch over every single person….[I feel] like they don't single me out. I don't feel like less of a person. I know if I had questions, I can call them on the hot line. And actually, those people are very nice.

When AFDC clients said they felt "like a number," the same words took on a different meaning. In this group, the phrase routinely appeared in stories that turned on themes of humiliation and impotence. When imitating an agency worker, AFDC clients were less likely to strike a robotic pose than to drop into a deep, commanding voice while pointing at me with an index finger. Their explanations of the number metaphor emphasized that being a mere number meant people could do anything to you. And rather than the image of a "needle in a haystack," they offered analogies emphasizing powerlessness, silencing, and vulnerability. Consider Alissa's remarks:

> It's a big system. "Stand in this line." You feel like cattle or something being prodded. That's how I felt. You go all the way through this line to do this, and then this line to do that. It's like a cattle prod. It's like you're in a big mill. I felt like a number, or like I was in a prison system….It feels like you're in a cattle prod. They're the cowboys, and you're a cow. I feel like a cowboy would have more respect for the animals because he knows that the cattle are his livelihood. But these people are like, "I'm helping you. This is something I'm doing for you. So just be quiet and follow your line."

But why worry so much about the meaning of one phrase? The goal of my research was not to show that a single phrase carried two different meanings, nor was it simply to suggest that AFDC clients felt worse about their experiences than did SSDI clients. As Howard Becker points out (1998, 151), efforts to make sense of "strange talk" provide an entry point for explanation, often taking the researcher "right to the heart of how a complex social activity is organized and carried out." I struggled with the meaning of this phrase because it offered an entry point into the specific ways clients *conceptualized* their welfare relationships. Such conceptions were the pivot point for my explanatory analysis. On one side, I sought to show how clients' understandings emerged from experiences with particular types of policy designs and bureaucratic transactions. On the other, I sought to show how these understandings led clients to see particular choices, attitudes, and actions as sensible and, in some cases, natural and inevitable.

The preceding examples are taken from research I conducted for my doctoral dissertation, later published as *Unwanted Claims* (Soss 2000). This chapter makes use of that study, sometimes in comparison with other research I have pursued, to explore the strengths, limits, and uses of in-depth interviews for interpretive research. *Unwanted Claims* was built around a set of research questions regarding the political lives citizens lead in relation to the U.S. welfare state. Comparing across the AFDC and SSDI programs, I set out to investigate the demands citizens make on government bureaucracies (welfare claiming), the political relationships citizens have with particular types of government agencies and officials (welfare participation), and the consequences these relationships have for citizens' broader political orientations and behaviors. In each area of inquiry, I sought to illuminate individuals' reasons for the choices they made and actions they pursued; I sought to show how these reasons were explicable in light of more basic understandings of identities and circumstances, norms and obligations, the workings of power, the nature of government, and so on; and I sought to show how these understandings, in turn, came to be for the people in my study. In Weber's (1978, 4) well-known terms, the project concerned itself "with the interpretive understanding of social action and thereby with a causal explanation of its course and consequences."

My goal in this chapter is not to add to the number of valuable texts that offer how-to instruction on conducting interviews (Rubin and Rubin 1995; Spradley 1979; Gubrium and Holstein 2002) and interpretive analysis (Manning 1987; Riessman 1993; Feldman 1995a; Yanow 2000). Instead, I aim to provide something that lies midway between general accounts of interpretive social science (Geertz 1973b; Taylor 1979; Weber 1978; Giddens 1976; Dean 1994; Norton 2004) and specific accounts of field research experiences (Liebow 1967; Kondo 1990; MacLeod 1995; Zanca 2000). My goal is to outline a practice-centered view of how interpretive methodologies and interview methods can be brought together in a fruitful manner.

To do so, I start by sketching a grounded view of what interpretive methodology might mean in the context of interview research. Rather than emphasizing philosophical paradigms, I address interpretive methodology as "a concrete practical rationality" deployed in a particular research project (Flyvberg 2001, 29) and outline some ways to think about this logic-in-use in the context of interview research. Next, I explore interview methods, highlighting their distinctive qualities and asking what might make them more or less "in depth." The third section draws these discussions together, offering an assessment of the strengths and limitations of interview methods when researchers take an interpretive approach. Finally, the conclusion offers some reflections on the role of emotion in interpretive interview research.

So, What Makes Interview Research Interpretive?

To understand the uses and limits of interviews for interpretive research, one must first clarify what is meant by "interpretive." The key issues here concern *methodology*, not methods per se. A method such as interviewing can offer a better or worse fit for a given methodology, but the label "interpretive" has less to do with one's techniques than with the logic of one's inquiry. As Kenneth Waltz (1979, 13) explains, "once a methodology is adopted, the choice of methods becomes merely tactical." In the research for *Unwanted Claims*, my interpretive methodology drove me toward methods that emphasized time in the field and flexible, detailed conversations with participants. But the interpretive nature of the project did not flow from the specific ways I chose to access data, nor did it arise from the fact that my evidence was primarily "qualitative" rather than "quantitative." To begin with methods or data in this way would be to start too close to the ground; it would put the proverbial cart before the horse.

It can be equally misleading, however, to begin too far from the ground—too far from what we do when we do research. I think of *Unwanted Claims* as an interpretive study. Yet as I pursue this label in relevant literatures, I often find it difficult to map the abstract descriptions onto my work. Some of the most widely assigned essays on methodology (e.g., Lincoln and Guba 1994) seem to imply that an interpretive project is one that is carried out by a particular type of person (an *interpretivist*) whose worldview is defined by a particular epistemological and ontological paradigm (*interpretivism*). This literature is bracing stuff. Packs of scholars take sides in longstanding philosophical disputes and clash over the bedrock beliefs that drive their research. Reflecting on my own work, however, I find it hard to square such accounts with experience.

Most of my work is question-driven.[2] I begin with a question about some political phenomenon and then, if I come to see the question as interesting and consequential, I try to specify a methodology that will help me work with it in a fruitful way.[3] As a result, some of my projects follow a more positivist logic while others are more interpretive. As I describe below, the methodology of *Unwanted Claims* differed from my more positivist studies in decisive ways. But as I have moved between projects, picking up one methodology and setting another aside, it has been the logic of my inquiry that has changed. I have not been transformed into a wholly different type of researcher, nor have I been forced to trade in my core beliefs about the nature of knowledge and reality. For this reason, I find it most helpful to apply the label "interpretive" to the logics of specific pieces of research rather than to researchers themselves or to any philosophical first-principles one might attribute to researchers. The interpretive / positive distinction, in this usage, is a matter of practice rather than identity or worldview. It is a matter of what we assume, require, and do for the sake of a particular inquiry rather than an aspect of who we are or a fixed description of what we believe in general.[4]

This emphasis on "concrete practical rationality" (Flyvberg 2001, 29) also underscores that the distinctly interpretive quality of *Unwanted Claims* does not flow in any direct manner from its emphasis on meaning and interpretation. Few political scientists today assert that meanings are inconsequential for what they study, and fewer still deny the need to interpret their data (Yanow 2003, 9; Adcock 2003b, 16). When I conduct hypothesis-testing, statistical research with survey data from the American National Election Study (ANES), I often rely on the seven-point scales the ANES uses to measure racial stereotypes, such as respondents' assessments of "intelligence" among "blacks" and "whites."[3] When I use these measures to produce studies that I view as somewhat standard positivist fare, I do not suddenly abandon my assumptions that the social world is a meaningful place, that racial classifications are socially constructed, and that I must interpret my evidence about people's beliefs and emotions. To the contrary, I am centrally concerned with the ways people make sense of the social and political world, and I deploy concepts and theories that are fundamentally about meaning making.[6]

Thus, in my own experience, the interpretive / positivist distinction fares poorly as a way to identify discrete and opposing classes of methods, schools of researchers, world-defining paradigms, or beliefs about the relevance of meaning. Yet in practice, researcher's orientations toward meaning and interpretation—the *priorities* we place on them, the *assumptions* we make about them, the *roles* we assign them in our analysis—vary considerably across research projects. It is in such areas of "concrete practical rationality" that we must look in order to see how interviews were used in *Unwanted Claims* to pursue an interpretive approach to explanation. I will suggest three aspects of the project's methodology that strike me as distinctly interpretive and that shaped the ways I deployed my interview methods.

First, I prioritized skepticism about shared meaning; I was willing to forego some research goals in order to place greater empirical pressure on my assumptions that particular words, actions, objects, people, and events had self-evident or widely shared meanings. In my more positivist research with survey data, I often bracket concerns about the constancy of meaning in order to pursue goals such as correlating attitudes with behaviors and generalizing to specific populations. When ANES respondents state their assessments of black and white "intelligence," some may be thinking of the innate potential of two groups of human beings; others may be making a factual statement about scores on IQ tests; others may be thinking about educational attainment, or may be rejecting such "book learning" in favor of "common sense and street smarts." Likewise, respondents may draw the boundary between "black" and "white" in different ways or may doubt that such racial categories are meaningful at all. When analyzing survey data, I care about such possibilities, but I cannot do much about them. My research strategy is not designed to facilitate the interrogation of such differences in understanding (beyond the dimension of difference captured by the stereotype scale). This limitation, in turn, can be traced to a deeper methodological decision not to prioritize an empirical account of such interpretive differences as a prerequisite for valid observation and explanation. In such research, I place a high value on the careful pre-testing of fixed-format interview questions and then proceed with my analysis on the assumption that respondents are interpreting and answering the questions in reasonably comparable ways.

In the research for *Unwanted Claims*, by contrast, I was centrally concerned with the analytic problems and opportunities that might flow from polysemy. I foregrounded my suspicions that clients might interpret a single interview question in different ways and that a single phrase might mean different things when spoken by different people. I placed these possibilities at the center of my research strategy and designed my interviews to dig into them. Accordingly, I valued tailored, mutually negotiated communication over the controlled, consistent questioning I would prize in a more positivist project (qualitative or quantitative).[7] Although I entered the field with expectations about meaning, my methodology made it an empirical question whether clients interpreted the act of claiming benefits or the experience of sitting in a waiting room in one manner or another. Of course, it is not possible to treat the meaning of every word and object as up for grabs in a research project; the researcher has to make choices about where and how to dig for differences. But one of my key assumptions in this project was that I needed to use my interviews self-consciously and systematically—even if selectively—to uncover unexpected differences in interpretation. The potential for polysemy was central to the ways I judged evidence, approached client and program comparisons, and constructed explanations for individuals' choices and actions.

Second, I placed client's understandings and sense-making efforts at the forefront of empirical investigation, and I sought to encounter such understandings and efforts on terms plausible to the participants themselves. Because the conceptual worlds of welfare recipients stood at the heart of my study, much of my fieldwork was devoted to assembling coherent accounts of how clients understood relevant phenomena. The goal was not primarily to get detailed reports of behaviors, attitudes, and beliefs—though these tasks were important to me as well. Rather, it was to understand clients' conceptions of how welfare relationships work (and should work), their ways of organizing their program experiences and drawing lessons from them, their notions of what makes it acceptable to claim welfare benefits, their ways of classifying their fellow welfare participants, their images of what government is and does, and so on. As I elaborate below, such understandings were basic to my analytic approach. They were the background premises for clients' choices and actions; they contained the logics that made a host of obscure, seemingly unrelated narratives explicable.

To pursue such understandings, I placed a high priority on encountering clients' conceptions on terms they found plausible and in a language they found familiar. As Jennifer Hochschild (1981, 21) notes, such priorities direct a researcher toward methods that invite "textured, idiosyncratic responses. The researcher must permit—even induce—people to speak for themselves and must be wary of channeling their thought through his or her own preconceptions about what questions to ask, how answers should be shaped, and what coding categories best subdivide the responses." This description captures important aspects of my approach. But here, it is important to emphasize two points.

First, making it a priority to *encounter* participants' understandings on their own terms is not the same as *accepting* participants' descriptions of their understandings. Interviewees can easily misrepresent or misperceive their own conceptions of the world. Likewise, I found that while some aspects of clients' conceptions were consciously held and easily articulated, others were so natural to participants (so much the water in which they swam) that they could hardly be perceived, let alone expressed. They were buried in what Schutz (1967, 74) calls the "taken for granted" of everyday life. To get at them required interrogating the gaps and silences in clients' accounts, the inconsistencies between narratives and declarations, and the unstated major premises of an assortment of incomplete syllogisms (H. Becker 1998, 147-9). Second, *prioritizing* exposure to language that is meaningful to insiders is not the same as *privileging* insiders' concepts over social scientific concepts. Rather, it involves carefully distinguishing the roles of each within our analyses. As Clifford Geertz (1973b) emphasizes, in an interpretive project, we seek to assemble thick descriptions of participants' conceptual worlds so that we can compel them to speak to the social scientific concepts we care about most.[8]

Third and finally, I treated construction of a coherent account of participants' understandings as a prerequisite for adequate explanation and sought to ascertain the sources and consequences of such understandings. All social scientists build explanation out of a dialogue of theory and evidence. But our demands of the evidence—the types of empirical accounts we deem necessary for explanation—vary across projects.

In some instances, I deem it adequate to show that relevant variables covary in a theoretically telling manner. For example, I might conclude that differences in state welfare policies can be "explained" by differences in the racial composition of welfare recipients without demanding any direct evidence of lawmakers' intentions (Soss et al. 2001). My approach to explanation in *Unwanted Claims* placed a higher priority on people's conceptual worlds. Throughout the study, I asked why reasonable people found it sensible to choose and act as my interviewees did. I sought out clients' reasons for considering this action more appropriate than that one, for feeling as they did about an institution, for drawing particular conclusions from particular experiences. Charles Taylor (1979, 35) captures the basic outline of such an effort to construct the rudiments of interpretive explanation:

> We make sense of action when there is a coherence between the actions of the agent and the meaning of his situation for him. We find his action puzzling until we find such a coherence.… This coherence in no way implies that the action is rational: the meaning of a situation for an agent may be full of confusion and contradiction, but the adequate depiction of this contradiction makes sense of it.

Under the methodology I pursued for *Unwanted Claims*, I assumed such "coherence" was necessary, though not sufficient, for an adequate explanation of the choices and actions under consideration (see Weber 1978). Thus, my efforts to "understand" were not a substitute for efforts to "explain" (cf. Hollis and Smith 1990); I aimed to "make understanding a prerequisite of explanation rather than an alternative to it" (Adcock 2003b, 17). One way to think about this methodology is to view participants' understandings and intentions as conditions that mediate the causal effects of other factors in our analysis (Blumer 1956; Lin 1998). C. Wright Mills (1940, 906, 904, 907) captures something of this logic when he writes that:

> Men discern situations with particular vocabularies, and it is in terms of some delimited vocabulary that they anticipate consequences of conduct.…The vocalized expectation of an act, its "reason," is not only a mediating condition of the act but it is a proximate and controlling condition for which the term cause is not inappropriate.…[Yet] the differing reasons men give for their actions are not themselves without reasons.

As described in the introduction, my interviews revealed that AFDC and SSDI clients differed strongly in the ways they conceptualized their status in welfare relationships. Pressing forward along the explanatory chain, these understandings became the core of my explanation for why SSDI clients were more willing than AFDC clients to assert themselves when dealing with government agencies. They understood their subject position in a way that made assertiveness seem sensible and effective, while AFDC clients saw themselves as vulnerable and impotent actors who would risk a great deal by speaking up. Such understandings gave AFDC clients good reasons to be reticent. The other side of my task was to press backward along the explanatory chain, seeking out Mills' "reasons for reasons." I asked how clients' understandings came to be—their conditions of possibility, their origins in experience, their development through social and personal efforts to make meaning. Ultimately, my explanation emphasized how distinctive institutional designs structured program experiences differently for AFDC and SSDI clients, how participation under these designs fostered different understandings of status in welfare relationships, and how these understandings made it sensible for AFDC clients to adopt a more reticent posture than their SSDI counterparts.

These are the main qualities that lead me to view *Unwanted Claims* as an interpretive rather than a positivist piece of interview-based research: I prioritized and pursued the analytic challenges and opportunities raised by polysemy; I sought to construct accounts of insiders' understandings and sense-making efforts out of empirical materials encountered on terms plausible to the participants themselves; and I treated such accounts of understanding as a necessary cornerstone of my explanatory analysis—as the root of sensible action whose origins and consequences needed to be traced.

So, What Is an In-Depth Interview and What Makes It In-Depth?

For some interpretive projects, in-depth interviews will not serve the researcher's purposes as well as other methods for accessing data, such as participant observation (Eliasoph 1998; Walsh 2004), focus groups (Gamson 1992; Hunt 1997), or archival research (Schwartz 1987; Doty 1993). So, it is worthwhile to ask what "in-depth interviewing" entails and why we might deploy or forego this method for a particular interpretive project. I take up the first question here, and then turn to the second below.[9]

It is conventional to say that an interview is a conversation pursued for the purpose of gathering information to be used for research purposes (Berg 1998, 57). But what makes an interview "in-depth"? When scholars invoke this adjective, they generally seem to mean "semistructured or unstructured"—formats that provide freedom for probes and follow-up questions as opposed to the structured interviews one might find in a survey or some other study that prioritizes reliability-as-uniformity over flexible, detailed exploration. Fixed-format interviews are, implicitly at least, cast as the "shallow" counterpart in this usage because they forbid researchers from digging in areas that emerge as promising during the course of an interview.[10] By contrast, in the introduction to his classic, *Political Ideology*, Robert Lane (1962, 9, original emphases) describes what made his interviews "in-depth" and what made them such a good match for his interpretive goals:

> The conversations were *discursive*; the responses of the men rambled, followed their own trains of thought, gave scope to anecdote and argument, moral comment and rationalization. This had several advantages: it offered insight into connotative meanings of words and phrases, it permitted one to follow the course of associative thinking (something relied upon for clinical insights); it illuminated the mechanisms of argument and evasion employed in dealing with sensitive political material.…The conversations were [also] *dialectical*, that is, conversational. There was opportunity for extended probing, for pushing further into the personal meaning of clichés and conventional phrases, for testing whether or not the first impression gained was the correct one, for reflecting back the sense of what was said to clarify the men's own thinking.

As these comments suggest, the term "in-depth" is usually invoked to suggest a more "conversational" format. Lane (1962) writes of smoking cigars with his interviewees and of a friendly, comfortable exchange of ideas in a relaxed setting.[11] Kristen Monroe (1996, 19) reports that "it seemed more natural to engage in a conversation with the individuals I interviewed, treating each as I would a new friend rather than as a subject." It is worth unpacking this comparison a bit, taking a moment to consider how in-depth interviews are and are not like everyday conversations.[12]

To be sure, there are some similarities. In an in-depth interview, there is a give and take between individuals: each responds to what the other has said. Because the researcher does not simply move on to the next item on a pre-set list of questions, an in-depth interview can be as unpredictable as any other conversation. It may veer off into topics irrelevant to the research; it may get emotionally difficult for one person or both; it may get tense or boring or develop a running joke; it may breeze along or stumble into an uncomfortable impasse. Like a conversation, an in-depth interview must be navigated as it unfolds, and this navigation depends on what Berg (1998, 80) refers to as full-channel communication. The interview is not just an exchange of words, but also an exchange of physical gestures, silences meant to signal, uses of voice tone and laughter—all of which must be attended to by the researcher. And just like other conversations, in-depth interviews can involve evasion tactics, fronts, lies, emotional manipulation, selfserving frames, and dissemblance. These aspects of the interview are, in their own right, evidence for the researcher. But they are also part of what must be wrestled with and challenged. Like other conversations, in-depth interviews involve dynamics of power, control, and authority (Ng 1996). From the outset, the identities of the researcher and interviewee may imply a status imbalance that cannot be overlooked. In the interview, power and social roles are aspects of the "definition of the situation" that must be mutually negotiated. Accordingly, an in-depth interview can take on a dynamic that feels like an interrogation, an amiable chat between friends, or an instructional session in which interviewees hold privileged knowledge and researchers play the role of the uninitiated student.

On the other hand, an in-depth interview is not *just* like an everyday conversation. To begin with, research interviews can involve people who have only known each other a short time, yet they may address topics that are not normally discussed by relative strangers. Moreover, researchers conduct interviews to acquire specific materials needed for a research project, and this agenda usually leads to departures from the norms of everyday conversation. I sometimes had to abruptly steer the dialogue back to relevant topics (and away from, say, a lengthy digression on a crazy uncle in Ohio). I also had to help my interviewees get comfortable with the idea that it would not be rude, in this context, to hold forth on a topic for fifteen minutes without giving me a turn to talk. In the normal course of events, people have precious few conversations in which they encounter a sympathetic listener who hangs on their every word, encourages them to elaborate for long stretches, and reflects their words back to them in hopes of gaining a more nuanced and complete understanding. Interpretive research, in particular, requires the pursuit of thick descriptions, and this means working hard to encourage elaboration, clarification, reflection, and illustration. In all of this activity, we carry the interview away from normal conversation (Eliasoph 1998).

Thus, the "in-depth" aspects of in-depth interviews make them more conversational than a fixed-format interview but also quite different from everyday conversations. It is also possible to take a wider perspective on what makes interview research more or less "deep." When scholars speak of "in-depth interviewing," they frequently have in mind a series of conversations between a researcher and interviewees. Instead, it may be more fruitful to think of in-depth interviewing as an interconnected, simultaneous set of activities that collectively constitute *a mode of field research*. In field research, the acquisition and analysis of data often occur simultaneously, and what appears to be a single method is often a conjunction of interrelated activities (Emerson et al. 1995).[13] In-depth interviewing, from this perspective, can be viewed as a set of simultaneous activities that support and direct one another in the field: discursive and dialectical conversations with interviewees, transcription activities, coding and analysis of data in hand, analytic memo-writing, purposive selection of next informants, revision of interview protocols, and so on.

To illustrate with just a single activity from this list, consider transcription. Often a slow, painstaking process, transcription is among the least appreciated aspects of interview research. In some projects, it is left aside until the researcher leaves the field or is handed off to a paid assistant. In my experience, however, transcription in the field "deepens" the method. In the research for *Unwanted Claims*, transcribing offered an unparalleled opportunity to note and reflect on interviewees' phrases, the organization of their narratives, the salience of one reported experience relative to another, and so on. As a result, transcription sessions were the occasions when some of my most fruitful insights and conjectures took shape. They also offered a unique opportunity to detect problems in my interview technique, reconsider the phrasing of my questions, and reassess the mix of topics I aimed to cover in interviews. Some transcription sessions were nothing but tedious. Others led to the writing of important analytic memos[14] and to significant changes in the ways I pursued future rounds of interviews.

This description of entwined research activities suggests an even broader way to think about in-depth interviewing: as a kind of evolving dialogue between "fieldwork and framework" (Sanjek 1990; Hopper 2003). Here, "fieldwork" refers to all the locally oriented activities the interviewer pursues in the field, both inside and outside actual conversations with interviewees. "Framework" refers to the broader knowledge of theory, history, and social structure that the author brings to bear on their local, case-specific, and person-specific encounters. As Kim Hopper (2003, 7) notes, fieldwork and framework "relate to one another as context and story, disciplinary backdrop and case-at-hand, history and action." Part of what makes interview research "deep" is immersion in, and pursuit of, this dialogue. While in the field, I shifted between conversations with welfare clients and conversations with the ideas, histories, and empirical claims I found in scholarly writings. Each moved my view of the other to a different place. Successive rounds shifted my standpoint considerably over the course of the project.

Such changes in standpoint are a common theme in the literature on field research, especially in writings on ethnography (Emerson et al. 1995). Sometimes, however, I think this process is discussed in a fashion that is too linear to match my experience. The author enters a strange new setting; the swirl of new social discourse is at first opaque; slowly, the researcher gains some interpretive footing; and at last, after some struggle to master emic understandings, the obscure is made plain. This narrative—I am tempted to call it a Geertzian narrative—captures an important part of what changed over time in my research for *Unwanted Claims*. But its emphasis on progressive enlightenment must be tempered a bit with two observations.

First, in interpretive research, strangeness has its benefits too. The newly arrived outsider is often able to notice as unusual—and hence, draw into analysis—the very things that insiders take for granted. The famous French observers, Alexis de Tocqueville and Harriet Martineau, were able to see strangeness in the democracy and society of nineteenth-century America, and this made all the difference for their classic analyses. Similarly, the early days of an interview project provide distinctive opportunities based on unfamiliarity. As I became more comfortable with clients' perspectives, I also developed blind spots; I stopped noticing some things at all and started seeing others as unremarkable. After my fieldwork ended, I was pleasantly surprised by some of the long-forgotten observations buried in my earliest journal entries and fieldnotes. Thus, the interviewer's standpoint changes in a complex way over time: the benefits and liabilities of strangeness fade, while the benefits and liabilities of familiarity rise in proportion.

The narrative of progressive enlightenment in the field also misleads in a second way. It understates the importance of systematic analysis after one exits the field. In my experience, the dialogue between fieldwork and framework did not move steadily toward better understandings. I went through periods when *everything* seemed to make sense in terms of some concept I had recently encountered, or when a comment by a particular client seemed like the Rosetta stone of my analysis. Later, everything seemed to make sense in terms of some newer concept or comment. I felt sure that clients saw an experience in a particular way; then I developed doubts; then I returned to a version of my earlier view. To be sure, I understood much more in the final days of my fieldwork than in the early days. But throughout my field experience, and even at its end, I stood in a particular thicket of trees; I was not standing back to view the forest. After my last interview, I felt confident that I knew the story my dissertation would tell. But this confidence was misplaced. Over the next year, as I sorted, sifted, and integrated the materials in a more systematic way, the story changed.

For an interpretive research project, then, in-depth interviewing offers a dynamic method—one that offers flexibility in the interview itself and shifting standpoints over time. It is centered on discursive and dialectical conversations with interviewees. But more broadly, it is an evolving dialogue between fieldwork and framework, mediated by concrete activities of transcription, memo-writing, purposive reading of literatures, and the like. It entails simultaneous data collection and analysis, but it remains incomplete without more systematic analysis after exiting the field. In the following section, I will consider some key strengths and weaknesses of in-depth interviews for interpretive research. But before moving on, it is worth noting that I have said nothing about what in-depth interviews mean to the interviewee. An adequate account would require a separate chapter; I will only point to some indications that we should care about this question.

Research methods are expressive acts. The ways we approach the people we study convey messages—both in ways we control and intend and in ways we do not. At one of my interviews, for example, I spent most of the day with an AFDC client and her pre-teen son, talking about this and that, shoveling the snow off the front walk together, and trying to get to know each other. In the afternoon, when her son brought in the mail, it included a survey from a researcher studying welfare recipients. (Two researchers in one day: what are the chances?) The son opened the self-administered survey and, slipping into the faux-British accent Americans often use to signify pretentiousness, began to pose the questions to his mother. Joining in the fun, she responded with an overdrawn Black English Vernacular, laying it on thick as she gave answers that exaggerated the worst welfare stereotypes. At the end of the charade, they threw the survey in the trash.

Because I sometimes conduct survey research, I felt compelled to ask why the woman had given me so much of her time but would not give a far smaller amount to this other researcher. Her answer was that I "cared enough" to come to her house and see what a day in her life was like, that I was willing to spend my time making lunch with her and getting to know her son. My investment in building rapport and getting to know a participant in my study were, to her, signifiers of respect and caring. The truth is that I did care. But I don't care any less when I work with fixed-format surveys that allow for a larger sample of respondents. What mattered in this instance had little to do with any real difference between this other researcher and myself (an unknowable quantity, in any event); it had to with what our methods symbolized to the person we approached. Moreover, the methods themselves did not inherently mean one thing or the other. Another woman might have viewed my request for a day's time and a lengthy interview as intrusive and presumptuous. The self-administered survey might have been perceived as showing more respect for her privacy and for the fact that she was busy with the work of survival amid poverty.[15] Perhaps this signals a further element that can make our interviews more or less "in-depth": our efforts to address participants' understandings, not only of the phenomena under study, but also of the research interaction itself. Participants' perceptions of what our methods "say" about the researcher and her or his project can have a major impact on what the researcher "finds" in the field.

In-Depth Interviews for Interpretive Research

How, then, do the topics of the two preceding sections fit together? How should we think about the use of in-depth interviews for interpretive research? Without aiming to provide an exhaustive list, I will draw on my experience with *Unwanted Claims* to suggest several important limits and strengths. In addition to interviews, my study included participant observation in a shelter for homeless families, disability support groups, and welfare agencies. Thus, my point of reference for the discussion that follows is based on a contrast between these two methods in my own research and others'.

At the outset, it is worth noting that the distinction employed here is something of an analytic fiction. In many projects, fieldwork consists of an evolving blend of interactions with participants that cannot be easily disentangled. A researcher may initially seek to participate and observe in ways that avoid disrupting "natural" interactions in any way; later, she may find it helpful to start asking more direct questions of her informants; then she may press on to informal ethnographic interviews, and eventually she may decide to make the interviews more formal and tape them for transcription. It seems fruitless to seek out the exact point at which one method ends and the other begins. Instead, the discussion that follows contrasts stand-alone ideal types: (a) flexible interviews unaccompanied by participant observation and (b) participant observation that seeks to minimize researcher disruption.

The limitations of in-depth interviews are most often discussed in relation to the strengths of mass survey research: the difficulties of using in-depth interview data to assess the reliability of a uniformly asked question, state the frequencies of behaviors and attitudes with precision, estimate parameters in a broader population, apply statistical controls to assess partial correlations, and so on. In an interpretive study, however, these limitations may be so distant from relevant research goals that they do not present the researcher with much reason for concern. Other limitations, though they are often ignored by methods textbooks, come to the fore in a comparison with participant observation and strike a bit closer to home for interpretive work (see H. Becker and Geer 1957).

First, social processes of meaning-making—patterns of conflict and collaboration that produce shared conceptions of reality—are often primary objects of concern in interpretive research (Adcock 2003b, 16). Yet they are singularly difficult to observe with a one-on-one interview. The interview, in a sense, stands outside the stream of interactions we seek to understand and, thus, offers only an indirect basis for accessing them. In the research for *Unwanted Claims*, I asked my interviewees about their efforts to make sense of events with others, and I encountered some relevant details, second hand, through the stories they told. But my interviews did not provide access to social meaning-making processes in anything like the form I encountered in my ethnographic work at the shelter. They did not allow me to observe everyday language actually being exchanged, to account for the rituals and conformity pressures in group negotiations, or to trace changes in framing over the course of a group discussion (see Walsh 2004). If my interviews captured such things at all, they did so only in retrospect and out of context.

Second, interpretive research concerns itself with meaningful social discourse and, as Geertz (1973b) emphasizes, this discourse is not at all restricted to the verbal realm. Researchers can encounter some non-verbal communication within the interview setting, but interviews do not allow the researcher to freely explore the broader flow of social discourse: behaviors in everyday social interactions, the design and negotiation of built spaces, the deployment of community symbols, the production of artwork, the invocation of documents produced in the name of the collective, and so on. As an interviewer, I could not compare word and deed to seek out the basis of their contradiction or congruence. My window on social discourse was a conversational one. Without my additional participant observation work, I would never have heard about, let alone observed, a broad range of meaningful behaviors, objects, and settings.

Third, interpretive research is typically concerned with indexicality—the tendency for a given object, event, phrase or identity to take on different meanings in different contexts. We don't just want to know what something means, pure and simple, or how a person categorizes the world, always and forever. We want to explain how and why the identity of "welfare recipient" is significant in one setting but irrelevant in another, a sign of selfishness in one setting and of self-sacrifice in another. My interviews offered access to such variation, but only up to a point. Clients talked about feeling more ashamed when they first applied for benefits than they felt later; they felt more humiliated when using food stamps in a "normal" grocery than when using them in a grocery located in a really poor neighborhood; and so on. In this manner, I found a number of consistent patterns in clients' reports. But the key word here is *reports*. These were clients' retrospective accounts of shifting meanings and emotional responses. The interviews, taken alone, did not allow me to witness changes in the use and meaning of key constructs across different types of interactions. Context-to-context differences were hard to reach with this method because the method was premised on only one kind of context: a one-on-one conversation with me.

Fourth, interviews provide a particular *kind* of context for accessing participants' understandings. Two features of the context merit special consideration for interpretive research. One, because interviews are one-on-one conversations with a researcher, the researcher's identity and self-presentation are central to the data produced. The welfare recipients I interviewed were not delivering a soliloquy; they were having a conversation with a white man, a Jew, a fellow from the university, that friend of Alissa's, that guy (you know the one) who works over at the shelter…a specific person with a social identity that the interviewee made sense of in a particular way.[16] Two, as noted earlier, in-depth interviews provide individuals with a type of audience they may rarely encounter in everyday life: attentive, encouraging, patient, willing to press on vague answers and eager to clear up confusions. Nina Eliasoph (1998, 19, 151) captures the problem in discussing the differences between her ethnographic observations in *Avoiding Politics* and Robert Lane's portrait of political reasoning in *Political Ideology*:

> The results of his respectful, sympathetic interviews offer striking insights into abstract political beliefs and reasoning, but such intimate, therapeutic relationships between interviewers and their subjects may encourage respondents to speak in uncharacteristically serious ways about issues that they usually treat flippantly, or ironically, or do not discuss at all, or discuss in some contexts only for the purpose of showing that they are smart, or discuss in other contexts only to reassure themselves that the world is all right after all….If a curious, open-minded researcher offers, free, unjudgmental, unhurried contexts for interviewees to reason aloud…most people can *become* thoughtful, reasoning citizens. If given this rare opportunity, almost everyone turns out to have the potential to think about politics.…While Americans are able to reason about politics if given the kind of opportunity that the sympathetic, open-minded interview researchers give them, this opportunity almost never presents itself to most Americans.

The understandings I encountered in my research were ones that emerged through these specific types of interactions. My work as a participant observer frequently made the effects clear. Shelter residents, a majority of whom were AFDC recipients, talked to one another *and to me* in ways that departed significantly from the detailed, searching conversations I had in my formal interviews. At the shelter, I interacted with residents as the occupant of an actual membership role in the social setting (as a shelter staff member), not primarily as a researcher. Our conversations were grounded in residents' own efforts to meet their needs or their own desires for sociability, not in the topics and goals of my research project. Even after a small amount of such fieldwork, the particularity of the in-depth interview encounter was readily apparent to me.

Thus, the insights and observations I obtained through in-depth interviews were limited by the unusual context of their fabrication and by the specific social identities I represented to participants. But this is not to say that they were false relative to the "real thing" out in the social world or false relative to what recipients would have told a researcher with a different social identity. The issue is more complicated than that. All research activities yield evidence that is *partial*—"partial" in the sense of being fragmentary and incomplete, "partial" in the sense of being ripped out of a more holistic context, and "partial" in the sense of being prone to some bias or another.[17] The understandings people express in everyday interactions—the ones we observe in participant observation—are molded by conformity pressures, by taboos against certain topics or viewpoints, by strategic efforts to look like something other than a dope or a dupe, and so on (Eliasoph 1998). Relative to this context, a private conversation with an encouraging researcher may offer a space in which individuals find it easier to say certain things they understood but stifled in more public contexts. Likewise, the welfare clients I interviewed might have discussed their experiences differently had they been talking to a Latina researcher who presented herself as a former AFDC recipient. Such a shift would be important, but it would not signify the removal of identity-based biases so much as the substitution of one set for another. The understandings revealed in each context of observation, and in the presence of each researcher, would be partial in their own ways. For interpretive research, what is interesting and important is why such differences emerge, how they reflect the limits and distortions of each investigation, and how comparisons across studies can yield deeper insights and stronger explanations. There is a great deal to be learned from the inconsistencies in what participants reveal in different contexts and to different researchers. I think Ann Lin (2000, 191, 194) puts the matter in proper perspective in her study of prison program implementation:

> Clearly prisoners self-censored in their choices of what to mention to me…but…they may self-censor in the other direction when they talk to male researchers.…The more interesting possibility [than the possibility of deception] is that male and female interviewers, especially in a highly gendered environment like the prison, simply 'cue' different responses.…Interviewing by both sexes allows different themes to emerge, themes that might be absent or less salient if only same-sex interviewing and research were to take place. This means that 'lying' should be less our concern than systematic bias, and bias should be evaluated less for how it can be eliminated, than for how it works and what it tells.…The only solution is for the researcher to know who she is, not only as someone who affects the research site in particular ways, but also as someone who characterizes it in partial and biased ways. This is less wrong than inevitable, and because of that, suggestions that one can be unbiased should be the most troubling. A good research ethic should allow researchers to discuss how their questions and preoccupations—as well as their personal characteristics and the context of their interactions—affected their research. When researchers confront their own bias with honesty and matter-of-factness, rather than with fear and denial, they push forth knowledge in the understanding that all knowledge is imperfect.

So what, then, are the primary benefits of in-depth interviews for interpretive research? First, in-depth interviews can be used to pursue questions that are difficult to locate in documentary sources or everyday interactions and to explore such questions in intricate detail. At the shelter for homeless families, for example, interactions with the welfare agency were a common topic of conversation and a significant aspect of observable social transaction. By contrast, I could have worked at the shelter for years without uncovering clients' conceptions of how power works in the broader U.S. political system. As a result, I could not have drawn inferences about how these core political views were built on clients' experiences with welfare bureaucracies and administrators. Conversations on this topic were hard to stumble across because they rarely took place. Even if they could have been located, they would not have allowed for the detailed probing needed to map out clients' conceptions and explore their mixed sentiments.

For many questions that concern students of politics, relevant interactions cannot be readily observed or are, at best, fleeting. The conceptions of interest may be impossible to infer from observable behaviors and may seldom be discussed in the normal course of affairs. To get at these sorts of understandings, we must ask. We must set the agenda, press for sustained discussion, and challenge vague statements for clarification and elaboration. Such asking, pursued informally, can be incorporated into participant observation research. But it lies in tension with, and can undermine, the goal of observing and participating in transactions *as they would normally occur*. In proposing the injunction "Ask me no (research) questions, I'll tell you no lies," Elliott Liebow (1993, 321-2) explains his participant observation approach as follows:

> I did ask questions, of course, but these were not questions I brought with me from the outside. They were "natural" questions that arose spontaneously and directly out of the social situation. They were the same sorts of questions that everyone else asked. They were situation-specific questions, not research questions.…For participant observation, the value of this injunction [is] that it discourages the researcher from contaminating the situation with questions dragged in from the outside. It allows the different situations under observation to develop according to their own inner logic and according to the needs of participants, not the needs of the researcher. In this way, one comes closer to the ideal of observing

behavior as it would have been had the observer not been present.

My own experience leads me to reject the strong version of this injunction: I feel I was quite able to ask shelter residents a few research-related questions during after-hours conversations without disrupting the broader tenor and logic of shelter interactions. But I think Liebow's advice captures an important difference in methods. Research-driven questioning is riskier business in participant observation research. By contrast, it is a central component of in-depth interviewing—one that makes this method a good fit for interpretive projects in which researchers are concerned with hard-to-locate phenomena.

Second, and perhaps most obvious from the discussion so far, in-depth interviews permit an exceptional degree of flexibility, control, and detail in the pursuit of participants' understandings. In in-depth interviews, we are not hemmed in by the fixed scope, order, and wording of items on a survey questionnaire. With a little care and reassurance, we can push beyond the limits of what is normally appropriate in everyday conversation. By being responsive to informants, we can evade the restrictions imposed by our a priori thinking about which topics are important and what they mean. By being a bit more directive, we can pull the conversation back to issues we need to address, even as we continue to encourage our interviewees to speak in terms that are their own. Throughout the interview, we retain freedom to probe, follow up, challenge, double back, abandon a fruitless line of inquiry, ask if we have understood correctly, or simply express our fascination and ask the interviewee to say more. And crucially, the interview format allows us to record a *verbatim* transcript of the resulting dialogue. Unlike most fieldnotes, such transcripts can precisely capture the ways individuals use words and phrases, organize their narratives, and puzzle through the phenomena under discussion.[18] Thus, both in its process and its product, the in-depth interview allows researchers to access participants' understandings in an unusually flexible and fine-grained manner.

Third, in-depth interviews are invaluable for recovering and analyzing the agency of individuals. In my research for *Unwanted Claims*, I was centrally concerned with welfare clients as political actors who actively interpret and categorize, choose and take action. Welfare scholarship is chock-a-block with theories that cast welfare recipients as victims of structural forces, passive objects of social control, products of socialization who simply "enact" a culture-of-poverty script, rational actors who automatically respond to changes in incentives, and "targets" who unfailingly internalize messages conveyed by policy designs. I wanted to explore the agency of such people and to do so in a way that could supply a counter-point to these well-established stories of constraint. Whether I was examining welfare claiming, welfare participation, or broader forms of political action, my goal was consistent: to explain demand making and quiescence, not just as results of forces acting on welfare recipients, but also as products of clients' own efforts to understand where things stood and how they worked, determine what was appropriate and acceptable, and choose a sensible course of action.

In-depth interviews provided an ideal method for uncovering such agency. In the interviews themselves, clients were positioned as interpreters of their own experiences and tellers of their own tales. They were not only the central focus of their narratives; they were the acting subjects of these narratives. The interviews helped me escape some of the scholarly biases I brought to the field—to recover an image of welfare clients as people who try to read the lay of the land, figure out what will happen next, and take action for good reason. The interviews allowed me to see the people I met as agents acting on their own self-concepts and standards, dreams and aspirations, fears and self-doubts, histories of accomplishment and disappointment.[19]

Fourth, in-depth interviews offer an excellent way to map the conceptual world of participants in ways that illuminate both coherence and inconsistency. It is not a coincidence that Robert Lane's (1962) classic study of political ideology is known for revealing *both* the integrated belief system ("ideational counterpart to a constitution") that supplied his interviewees with a moral compass for political life *and* the "morselized," fragmented ways his interviewees thought about political events. Jennifer Hochschild's (1981) classic interview study, *What's Fair*, offers a similar pairing. It shines a light not only on the consistent, coherent ways Americans think about distributive justice in different spheres of life, but also on the troubling disconnects in this thinking and on the profound ambivalence Americans experience when they try to sort through their conflicting values and feelings.

Interviews offer a superb way to learn how individuals knit their own conceptions together and put them to use. They can be used to uncover logics of integration (widely shared or idiosyncratic) and sources of disintegration. On both sides of this ledger, the value is that we can explore the *substantive* connections that link beliefs and sentiments. The "connective tissue" sought here is not the same as a conclusion that several attitudes are correlated across cases. Rather, it is grounded in the way each subjective element justifies, supports, or derives from another; the way each casts its shadow on the meaning of another; the way, ultimately, the elements function together as parts of a broader whole. Interviews allow researchers to pursue disjuncture and ambivalence by directly digging into the stuff of mixed sentiment. Inner conflicts bubble to the surface as individuals traverse complex issues. Interviewees may start in one direction, then reverse themselves; they may stop in midstream to say they feel torn; they may sputter and blush when asked to reconcile two of their own statements. If the interviewer is attentive, such moments can be seized as valuable openings for interpretive analysis.[20]

Conclusion: On Emotion and the In-Depth Interview

In this chapter, I have tried to parse some of the methodological issues that arise when social scientists use interviews for interpretive research. My aim has been to suggest ways of thinking that might be helpful for contemplating the fit between interview methods and interpretive analytic goals. Throughout this essay, however, I have skirted an issue that could have threaded its way through every section. Depending on one's research question, it can be a crucial part of what makes an interview project "interpretive." It can be a key element of what makes interviews "deep." And it can emerge as a major benefit and challenge of the choice to use in-depth interviews rather than some other method of data collection. I am speaking of emotional engagement.

Some interviews are cool, professional interactions. They address low-intensity topics in a dispassionate way, and they need not do otherwise. Interpretive interviews, however, often take up topics *because* they are meaningful to participants and focus on what experiences mean to people at a more personal level. The interview setting itself can sometimes feel like an intimate conversation, and its open-ended format increases the odds that emotional issues will arise. Blee and Taylor (2002, 96) go so far as to say, "Intensive interviews are the best method for probing deep emotional issues."

Their claim may be a bit overstated. In my ethnographic work at the family shelter, for example, I participated in many emotionally intense events and had many occasions to reflect on them with shelter residents and staff after hours. On the whole, however, I agree that in-depth interviews are a superb forum for exploring emotional issues. They are unusually well-suited for broaching emotional issues with care and in privacy, exploring them in a nuanced manner, and doing so in a way that is responsive to signals about when to dig deeper or go no further. In my work for *Unwanted Claims*, these features were key advantages of the interview method. They provided an ideal way to explore the feelings of futility, vulnerability, anonymity, humiliation, shame, pride, and frustration that accounted for so many of the clients' decisions and behaviors.

It is not enough, though, to cite emotional engagement as a "strength" of in-depth interviewing and leave it at that. The navigation of emotion was a persistent theme in my interviews and a defining feature of my field experience. It was central to the reflexivity, human connection, and reciprocal agency that lent an interpretive quality to my project. And in this regard, there were not only opportunities but also risks and challenges.

My interview questions sometimes opened emotional floodgates in ways that I—with no training as a therapist and perhaps too much confidence in my ability to absorb emotionally jarring conversation—was poorly equipped to handle. Starr recounted how, after learning of her acceptance to the SSDI program, she had tried to commit suicide; she felt overwhelmed with hopelessness when she saw government's official stamp on her life: "*long-term* disabled." Dizzy, once a proud and popular tavern owner, broke down as he described his anger, depression, and loneliness; he had watched even his most loyal friends drift away as his years of in-house isolation accumulated. Hope told one wrenching story after another about the sexual abuse she endured as a child and the brutal beatings and rapes she suffered as an adult victim of spousal violence. On and on it went, as so many AFDC clients described the desperation of their circumstances, their sadness about the lives they were providing for their children, and their anger at personal humiliations experienced at welfare agencies and grocery stores.

This emotional content, it must be said, was not limited to the sympathetic. Some clients openly expressed hatreds and prejudices that I found repellent. Some engaged in emotional manipulation or described behaviors (for example, toward their children) that I found upsetting. The emotional dynamics of the interviews, like the human beings themselves, varied tremendously. But in one way or another, they consistently presented challenges for a new researcher who was unsure how to "appropriately" respond to the high level of emotional intensity in interview encounters. I am hesitant to give advice on this matter; it feels presumptuous. But for what they are worth, I will share three injunctions that I find helpful to repeat to myself when I conduct this type of research.

Know your limits. Acknowledge that there are limits to what you can provide your interviewees and what you can absorb without doing harm to yourself. When raw emotions poured out, I tried to listen to interviewees in an open and accepting way, and I did my best to be supportive and patient. But I am neither a therapist nor a trained social worker. I did not have the skills or resources to provide what people needed. I could not do much to help them cope with, let alone change, their life circumstances. And amateurish efforts by a well-meaning researcher—misplaced intervention or blundered counseling—could easily have made matters worse. For all these reasons, I consider it essential to respect the limits of what I can offer an interviewee.

The other side of this coin is that each of us is limited in the emotional stress we can absorb. I came home from some interviews wiped out, feeling like I had been kicked in the stomach. I went through stretches of sleepless nights. At times, I experienced a deep sadness about what I had heard, anger at people and institutions I saw as responsible, frustration at my inability to effect change, guilt about my privileges and the time I fiddled away on a doctoral dissertation. Eventually, I learned that part of the field research process was learning how to acknowledge and deal with my own potential for burnout and depression. Deadlines be damned, I sometimes took long breaks between interviews and asked for leave at the shelter. I sometimes changed the order of upcoming interviews because the next in line seemed like it might be particularly difficult. In retrospect, I wish I had done more of this. I wish I had paid closer attention to my own signals and been more willing to take such breaks. It would have been good for me, for the people around me, and for my research project.

The researcher role is a human role. When we do research, we continue to be a person just like any other.[21] Interpretive interviews bring this fact to the fore, making the human connections we experience in everyday life a salient element of the research process. As a counterpoint to the difficult emotional material I am focusing on here, it is worth saying that this is part of what makes interviewing such a joy and such a satisfying way to do research. For all the hard moments described in the preceding paragraphs, my field experiences included far more highs than lows, many more good times than bad.

The point I mean to highlight, however, has to do with how we respond to emotional intensity in an in-depth interview. The decision to conduct an in-depth interview is a decision to share an experience with another person, and there are times when the human element has to take precedence—that is, times when we should view our navigation of the interview through a human lens rather than a research lens. It is okay to turn the tape recorder off; our research can wait. In fact, it will probably survive just fine if we decide not to press forward with a particular line of questioning in a particular interview. And although we should not try to take on the role of therapist, we can and should offer the same support and compassion we would extend if the situation occurred outside a research context. To be sure, just as in the rest of life, we sometimes botch the job. My response to an emotional outpouring was sometimes pitifully clumsy. I stammered a reply that conveyed little aside from the fact that I did not know what to say, and I felt all the worse for it afterward. But perfection cannot be the goal in such situations. The best we can offer, and the least we should offer, is a fallible but genuine effort to engage the people we interview as human "ends in themselves" rather than mere means for achieving our research goals (Cassell 1980; Reinharz 1992).

Emotions can advance rather than threaten good research. In the toughest emotional periods of my field research, the concept of "reflexivity" ceased to be an abstract methodological matter for me. I could see clearly that my questions were affecting my interviewees; their emotions and stories were having deep effects on me; and all of this was affecting my research. For some readers, this statement will seem like nothing remarkable—only a description of interpretive research in practice. But for others, all this talk of emotion will sound alarm bells about the loss of objectivity and the risk of sympathetic capture. These are important concerns, but they are unlikely to get us very far if they are framed in a way that makes emotional indifference a precondition for exacting and trustworthy analysis. The issue is not whether emotion is present or absent; it is how emotions affect our research.[22] Rather than holding our research hostage to amorphous anxieties about the loss of detachment, we should be specific about how and when intense emotions can pose a threat to good research. I think strong emotions pose a problem in three general types of circumstances: (a) if the researcher develops an unchallengeable attachment to a preferred methodological or theoretical approach, (b) if the researcher becomes personally invested in a particular portrayal of participants' understandings or a particular answer to their research question, or (c) if the research experience evokes such intense emotions for the researcher that she or he is personally harmed or cannot confront important tasks needed to complete the research project.

All three circumstances are problematic, but none is a *necessary* outgrowth of research that involves direct, responsive interaction with the intense emotions of interviewees. A reflexive engagement with the people one is studying is not at all the same as allowing one's emotions to direct the answers to research questions. To the contrary, emotional engagement can supply a powerful motivation to get one's explanations "right" and an essential means for accomplishing this goal. Emotions are storehouses of knowledge, compasses for navigating the world, and basic expressions of the meanings we attach to political objects and events. Had I not engaged emotion in an open and responsive way, I would have given up a crucial channel for accessing clients' conceptual worlds and a basic process for developing interpretive explanations. Whatever flaws my eventual analysis contained, it would have been far less trustworthy if I had retreated from emotion and steadfastly sought to protect my study from its intrusion.

Strong emotion was a periodic outcropping in my research experience. A large number of interviews were not particularly emotional at all. They were interesting and fun, or even a little dull in some cases. The point I want to end with, however, is the great potential for in-depth interviews to facilitate access both to participants' emotions and to emotional issues related to one's research topic. Social scientists study people who commit atrocities and people who survive them. We study activists who mobilize around horrifying problems and who feel tremendous passion for what they do. Emotions and emotional issues are central to social and political life, so we need methods to explore them. Interviews are superb for this purpose. They bring emotion to the surface, in ways we intend and ways we do not. It is here that we find some of the most distinctive, most fruitful, and most difficult aspects of using in-depth interviews for interpretive research.

Endnotes

1. Unless otherwise noted, all quotations are drawn from interviews conducted for the research that appears in Soss (2000). All names are pseudonyms chosen by the interviewees themselves.

2. The distinction between method-driven and problem-driven research has become a common reference point in debates about methodological pluralism in American political science (see I. Shapiro 2004). I avoid this terminology here because prevailing usage tends to rely on a somewhat naïve treatment of "problems" that ignores (a) the social and political construction of what constitutes a problem and (b) the crucial roles that normative and empirical theories - and related methods - play in this constructive process (see Levi 2005, Norton 2004b). The term used here, "question-driven," is perhaps a little too capacious, but it has the twin virtues of being less beholden to the "problem vs. method" distinction and more faithful to the flow of my research experience. Most of my empirical research originates in my question-asking. And in fact, these questions are often directed at the way a "problem" has been framed.

3. The rough outline of a methodology is usually implicit in the way I've asked my initial question. Yet while it may be present in tacit form, the methodology is not fully specified by the question. Its broad orientation is defined, but its precise form is not predetermined. Thus, the challenge is to refine the inchoate, not to "find the best choice" on some abstract checklist of methodological options.

4. In addition, I think Andrew Abbott (2001) makes two crucial points about this distinction. First, in practice, the labels "interpretive" and "positivist" signify relative differences within particular scholarly contexts. We cannot identify a set of absolute traits (A, B, and C) that are shared by all interpretive works and that are wholly distinct from a set of absolute traits shared by all positivist works (D, E, and F). The terms do not define a set of fixed, non-overlapping "classical categories." Rather, they are labels for methodological *family resemblances* shared by pieces of research that may (a) differ from one another in significant ways and (b) share qualities with research in other categories. Second, as a sociological matter, labels such as "interpretivist" and "positivist" often do signal identities that help scholars make sense of their position within their discipline. Just as social identities provide a compass for negotiating political life (Walsh 2004), "methodological identities" help researchers figure out who stands where in relation to themselves. My disagreement is not with Abbott's social-psychological insight; it is with those who would reify such identities. The fact that scholars find it meaningful to say something like "she's a positivist and he's an interpretivist" does not obligate us to view these labels as if they referred to quasi-religious identities grounded in stable, opposed worldviews. For those who value a methodological pluralism, I think it can be counterproductive to do so. Methodological pluralism does not just mean tolerance for diversity across researchers; it is also means freedom and flexibility for each researcher—recognition and encouragement of diversity across a given scholar's research projects.

5. The stereotype questions are asked as follows: "Now I have some questions about different groups in our society. I'm going to show you a seven-point scale on which the characteristics of the people in a group can be rated. In the first statement a score of 1 means that you think almost all of the people in that group tend to be 'hard-working.' A score of 7 means that almost all of the people in the group are 'lazy.' A score of 4 means that you think that most people in the group are not closer to one end or the other, and of course you may choose any number in between.…" The next set asks if people in each group tend to be unintelligent or tend to be intelligent. "Where would you rate whites [blacks] in general on this scale?"

6. Schema theory offers one example. "The primary function of an activated schema is to affect the interpretation of related information. The way ambiguous information is construed and the default values that are assumed for unavailable information are influenced by a

schema. Through these interpretive processes, schemas will influence evaluations and other judgments about an object" (Smith 1998, 403).

7. As Rubin and Rubin (1995, 11) rightly note, if we assume that the meaning of a term can vary across social contexts and groups, and that such variation may not be transparent to the researcher, then we cannot view uniform question wording or ordering as a precondition for valid and reliable observations. "Asking everyone the same question makes little sense… where the goal is to find out what happened and why [or how participants conceive of things], in rich and individualistic terms."

8. I view this careful division between "emic" (insider) concepts and "etic" (outsider) concepts as a key element of an interpretive approach. It underpins, for example, our hesitation to force participants' conceptions onto analytic dimensions we have constructed in advance (such as a closed-ended survey item). Similarly, in *Unwanted Claims*, I applied the concept of "political action" to the activities that made up welfare participation, and I used clients' conceptions to advance the resulting analysis. But I did not proceed on the assumption that clients would share my political view of their activities. Indeed, the apolitical nature of many clients' understandings proved to be an element of my political analysis.

9. As noted in the introduction, I do not intend for this chapter to serve as an introduction to the nature and practice of in-depth interviewing. For more detailed discussions of interview *method*, readers should consult Rubin and Rubin 1995; Leech et al. 2002; Spradley 1979; Gubrium and Holstein 2002; Holstein and Gubrium 1995; Berg 1998, 57-99; Dexter 1970; Seidman 1991; Kvale 1996; Douglas 1985; McCracken 1988; and Gluck and Patai 1991.

10. The deep / shallow distinction refers, of course, to the interview process itself, not to the quality or nuance of the resulting research product. In-depth interview data can be deployed in a manner that is pallid and obtuse, while a study based on data from fixed-format interviews may offer far richer insights into the phenomenon of interest. Such outcomes depend on the knowledge, skill, and creativity of researchers.

11. On the other hand, I would hardly be the first to note that the working-class men Lane interviewed might have felt less than relaxed in a professor's office at Yale University.

12. Here, I touch on only a few key points drawn from longer discussions in Spradley 1979; Rubin and Rubin 1995; and Berg 1998.

13. In a "grounded theory" approach, for example, the entwining of many different data collection, coding, and analysis activities is referred to collectively as "the constant comparative method" (Glaser and Strauss 1967; Strauss and Corbin 1998).

14. On analytic memos, see Strauss 1987; Emerson et al. 1995. Some of my memos were process-oriented: Why am I getting X response? What does this suggest I should do next? Others addressed the major theoretical and empirical questions driving my project. In some cases, I would write multiple memos on the same topic, trying out different conceptual lenses on the same set of observations to see how each played out. Such memos were useful as starting points for the more systematic analyses I conducted after I completed and transcribed my interviews. Equally important, they were a casual, private activity and, as such, did not carry the same emotional pressures as writing *official dissertation text*. In the months after my fieldwork, I was happy to find that the analytic memos frequently supplied "starter text" for my chapters—often helping me to jump-start a section that had brought on a serious case of self-doubt and writer's block.

15. On balance, my research yielded more positive than negative readings of my interview efforts. People who felt like no one cared about their problems were often grateful to tell their stories to an attentive listener. I attributed this partly to the length of interviews, their setting in clients' homes (or an alternative site chosen by the

client), and their focus on clients' experiences, emotions, and understandings. On the other hand, some of my most vivid memories of the field focus on the occasions when I encountered negative responses. At the start of my fieldwork, I spent four months in the community before conducting my first formal interview. During that time I tried to build social networks, get comfortable with new languages and ideas, develop my interview protocol, and make myself and my research into familiar entities for community members. On one occasion, I went to a community meeting in a low-income neighborhood to introduce myself. When I said I had come with the hope of interviewing people in SSDI and AFDC about their experiences in welfare programs, I received a chilly response. An in-depth interview with a welfare recipient was, for this audience, primarily a tool for taking advantage of the vulnerable and producing sensationalist, stigmatizing accounts of poor families. (It was 1994 and, in the lead up to federal welfare reform in 1996, scornful talk of welfare dependency ran thick in the public discourse.) Standing alone at the front of the room, I was asked to answer for a multitude of sins news reporters and social scientists had committed against people who live in poverty. Could I guarantee that my interviews would produce something different? Wasn't I just passing through on my way to a nice university job, while the people who participated in my study would remain behind long after I was gone? My stumbling answers were nowhere near as good, or as forceful, as the questions I was asked. Miraculously, the conversation seemed to end more positively than it began. A number of people at the meeting that day welcomed me and later provided invaluable assistance. But the initial reaction was an emotionally difficult lesson in the complex politics and ethics of field research—and a powerful demonstration of what interviewing can mean to participants.

16. And here as elsewhere, we cannot assume we know the relevant understandings in advance. I list "white" and "Jewish" together in this sentence, but one person in my study (a black woman) ended a commentary on white privilege by saying, "You probably know what I mean; you're not white either—you're Jewish, right?"

17. My uses of the term "partial" are meant to extend Anne Norton's (2004) playful invocations of this concept. The last assertion in this list—that all methods are "prone to some bias or another"—is meant to convey that the use of any particular method, relative to some other method, will systematically raise our chances of observing and understanding X while lowering our chances of observing and understanding Z. In addition, I would say that our use of *any* method will reflect our historical, social, and political standpoints—biases that we will tend to see as natural and commonsensical perspectives, if we perceive them as perspectives at all.

18. In some interview projects, however, there are good reasons to forego the verbatim records produced by audio or video recording in favor of partial handwritten notes or jottings made after the interview. For discussion, see Rubin and Rubin 1995, 125-8.

19. By contrast, Nina Eliasoph (1998, 18-9) suggests that fixed-format survey interviews are actually geared toward, and serve to construct, "the kind of person who will cooperatively answer a stranger's questions and not demand dialogue." Reminiscing on her days as a survey interviewer, she recalls individuals trying to resist, alter, or subvert her questions in some way. "My job, however, was simply to repeat the questions exactly as written in the question booklet until the respondent succumbed to the interview format."

20. The trick, in a sense, is how to balance our accounts of coherence and contradiction. On one side, the researcher who uses interviews to doggedly pursue coherent understandings risks creating an individual-level "just-so" story that encompasses and explains everything. On the other side, the researcher who uses interviews to single-mindedly pursue ambivalence and disjunction risks a conceptual world so chaotic that it offers no basis for interpretive explanation.

21. Among scholars who read interpretive research, this point is most familiar in its epistemological form—as an assertion that

science is a “gutsy human activity” (Gould 1981) pursued by people who occupy specific cultural, historical, and social vantage points (Harding 1991). By contrast, I raise the point here in its interpersonal (and ethical) sense of how we relate to the people we encounter in our research projects. Here as elsewhere, my concern is research practice rather than epistemology per se.

22. I find it implausible to treat researcher emotion per se as inimical to good social science. Presumably, we all feel *something* for the people and issues we spend our lives studying. And presumably, one does not need to feel neutral about genocidal killing in order to study the Rwandan genocide in a manner that merits esteem from social scientists. When we try to police the boundaries of social science by dismissing any study in which the researcher’s emotions are evident, we risk equating the good social scientist with an automaton or sociopath.

References

Abbott, Andrew. 2001. *Chaos of Disciplines.* Chicago, IL: University of Chicago Press.

Adcock, Robert. 2003. “What Might It Mean to Be an ‘Interpretivist’?” *Qualitative Methods.* 1(2): 16-18.

Becker, Howard S.. 1998. *Tricks of the Trade: How to Think About Your Research While You're Doing It* Chicago: University of Chicago Press.

Becker, Howard S. and Blanche Geer. 1957. “Participant Observation and Interviewing: A Comparison.” *Human Organization.* 16(3): 28-32.

Berg, Bruce. 1998. *Qualitative Research Methods for the Social Sciences.* Boston: Allyn and Bacon.

Blee, Kathleen M. and Verta Taylor. 2002. “Semi-Structured Interviewing in Social Movement Research.” In B. Klandermans and S. Staggenborg, eds. *Methods of Social Movement Research.* Minneapolis, MN: University of Minnesota Press. pp.92-117.

Blumer, Herbert. 1956. “Sociological Analysis and the ‘Variable’.” *American Sociological Review.* 21(December): 683-90.

Cassell, Joan. 1980. “Ethical Principles for Conducting Fieldwork.” *American Anthropologist* 82: 28-41.

Dean, Mitchell. 1994. *Critical and Effective Histories: Foucault’s Methods and Historical Sociology*. New York: Routledge.

Dexter, Lewis Anthony. 1970. *Elite and Specialized Interviewing*. Evanston, IL: Northwestern University Press.

Doty, Roxanne. 1993. “Foreign Policy as Social Construction: A Post-Positivist Analysis of U.S. Counterinsurgency in the Philippines.” *International Studies Quarterly*. 37(3): 297-320.

Douglas, Jack. 1985. *Creative Interviewing.* Beverly Hills: Sage Publications.

Eliasoph, Nina. 1998. *Avoiding Politics: How Americans Produce Apathy in Everyday Life.* New York: Cambridge University Press.

Emerson, Robert, Rachel Fretz and Linda Shaw. 1995. *Writing Ethnographic Fieldnotes.* Chicago, IL: University of Chicago Press.

Feldman, Martha. 1995. *Strategies for Interpreting Qualitative Data.* Thousand Oaks, CA: Sage Publications.
Gamson, William. 1992. *Talking Politics.* Cambridge: Cambridge University Press. pp.1-58.
Geertz, Clifford. 1973. "Thick Description: Toward an Interpretive Theory of Culture." *The Interpretation of Cultures.* New York: basic Books. Pp.3-32.
Giddens, Anthony. 1976. *New Rules of Sociological Method: A Positive Critique of Interpretative Sociologies*. London: Hutchinson.
Glaser, Barney and Anselm Strauss. 1967. *The Discovery of Grounded Theory*. Chicago: Aldine Publishing.
Gluck, Sherna Berger and Daphne Patai. 1991. *Women's Words: The Feminist Practice of Oral History*. New York: Routledge.
Gould, Stephen Jay. 1981. *The Mismeasure of Man*. New York: Norton.
Guba, Egon G. and Yvonne S. Lincoln. 1994. "Competing Paradigms in Qualitative Research." In N. Denzin and Y. Lincoln, eds. *Handbook of Qualitative Research.* Thousand Oaks, CA: Sage Publications. pp.105-17.
Gubrium, Jaber F. and James A. Holstein. 2002. *Handbook of Interview Research: Context and Methods*. Thousand Oaks, CA: Sage Publications.
Harding, Sandra. 1991. *Whose Science? Whose Knowledge? Thinking from Women's Lives.* Ithaca, NY: Cornell University Press.
Harkess and Warren. 1993. "The Social Relations of Intensive Interviewing: Constellations of Strangeness and Science." *Sociological Methods & Research* 21(February): 317-339.
Hochschild, Jennifer L. 1981. *What's Fair? American Beliefs about Distributive Justice*. Cambridge: Harvard University Press.
Hollis, Martin and Steve Smith. 1990. *Explaining and Understanding International Relations.* Oxford: Clarendon Press.
Hopper, Kim. 2003. *Reckoning with Homelessness.* Ithaca, NY: Cornell University Press.
Hunt, Darnell M.. 1997. *Screening the Los Angeles "Riots": Race, Seeing, and Resistance.* New York, NY: Cambridge University Press.
Kondo, Dorinne K. 1990. *Crafting Selves: Power, Gender, and Discourses of Identity in a Japanese Workplace.* Chicago, IL: University of Chicago Press.
Kvale, Steinar. 1996. *InterViews: An Introduction to Qualitative research Interviewing.* Thousand Oaks, CA: Sage Publications.
Lane, Robert E.. 1962. *Political Ideology*. New York, NY: The Free Press.
Leech, Beth et al. 2002. "Symposium: Interview Methods in Political Science." *PS: Political Science & Politics.* 35(4): 663-88.
Levi, Margaret. 2004. "An Analytic Narrative Approach to Puzzles and Problems." In I. Shapiro, R.M. Smith, and T.E. Masoud, eds. *Problems and Methods in the Study of Politics.* New York, NY: Cambridge University Press. pp.201-26.
Liebow, Elliott. 1967. *Tally's Corner.* Boston: Little, Brown.
Liebow, Elliott. 1993. *Tell Them Who I Am: The Lives of Homeless Women.* New York: The Free Press.
Lin, Ann Chih. 1998. "Bridging Positivist and Interpretivist Approaches to Qualitative Methods." *Policy Studies Journal* 26(1): 162-80.
MacLeod, Jay. 1995. "On the Making of Ain't No Makin' It." *Ain't No Makin' It: Aspirations and Attainment in a Low-Income Neighborhood.* Boulder, CO: Westview Press.
Manning, Peter. 1987. *Semiotics and Fieldwork.* Newbury Park: Sage Publications.
McCracken, Grant. 1988. *The Long Interview*. Newbury Park, CA: Sage Publications.
Mills, C. Wright. 1940. "Situated Actions and Vocabularies of Motive." *American Sociological Review*. 5(December): 904-13.
Monroe, Kristen Renwick. 1996. *The Heart of Altruism*. Princeton: Princeton University Press.
Ng, Sik Hung. 1996. "Power: An Essay in Honour of Henri Tajfel." In W.P. Robinson, ed. *Social Groups & Identities: Developing the Legacy of Henri Tajfel.* Oxford, UK: Butterworth -Heinemann 191-214.
Norton, Anne. 2004. "Political Science as a Vocation." In I. Shapiro, R.M. Smith, and T.E. Masoud, eds. *Problems and Methods in the Study of Politics.* New York, NY: Cambridge University Press. pp.67-82.
Norton, Anne. 2004. *95 Theses on Politics, Culture, and Method.* New Haven, CT: Yale University Press.

Reinharz, Shulamit. 1992. *Feminist Methods in Social Research.* New York: Oxford University Press.

Riessman, Catherine Kohler. 1993. *Narrative Analysis.* Thousand Oaks, CA: Sage Publications.

Rubin, Herbert J. and Riene S. Rubin. 1995. *Qualitative Interviewing: The Art of Hearing Data* . Thousand Oaks, CA: Sage Publications.

Sanjek, Roger. 1990. "On Ethnographic Validity." In R. Sanjek, ed. *Fieldnotes.* Ithaca, NY: Cornell University Press.

Schutz, Alfred. 1967. *The Phenomenology of the Social World.* Evanston, IL: Northwestern University Press.

Schwartz, Barry. 1987. *George Washington: The Making of an American Symbol*. Ithaca, NY: Cornell University Press.

Seidman, I.E.. 1991. *Interviewing as Qualitative Research.* New York: Teachers College Press.

Shapiro, Ian. 2002. "Problems, Methods, and Theories in the Study of Politics, Or: What's Wrong with Political Science and What to Do about It." / Political Theory / 30: 596-619.

Soss, Joe. 2000. *Unwanted Claims: The Politics of Participation in the U.S. Welfare System.* Ann Arbor, MI: University of Michigan Press.

Strauss, Anselm and Juliet Corbin. 1998. *Basics of Qualitative Research: Techniques and Procedures for Developing Grounded Theory*. 2nd ed. Thousand Oaks, CA: Sage Publications.

Taylor, Charles. 1971. "Interpretation and the Sciences of Man." in P. Rabinow and W.M. Sullivan, eds. *Interpretive Social Science: A Reader.* Berkeley, CA: Universtiy of California Press, pp. 25-71.

Walsh, Katherine Cramer. 2004. *Talking about Politics.* Chicago, IL: University of Chicago Press.

Weber, Max. 1978. *Economy and Society: An Outline of Interpretive Sociology*. Berkeley, CA: University of California Press.

Yanow, Dvora. 2000. *Conducting Interpretive Policy Analysis.* Thousand Oaks, CA: Sage Publications.

Yanow, Dvora. 2003. "Interpretive Empirical Political Science: What Makes This Not a Subfield of Qualitative Methods." *Qualitative Methods.* 1(2): 9-13.

Zanca, Russell. 2000. "Intruder in Uzbekistan: Walking the Line between Community Needs and Anthropological Desiderata." In H.G. De Soto and N. Dudwick, eds. *Fieldwork Dilemmas.* Madison, WI: University of Wisconsin Press. pp.153-71.