

APPENDIX 3: PAPERS PRESENTED by WORKSHOP PARTICIPANTS

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QUALITATIVE RESEARCH...WHAT'S IN A NAME?

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The Oxford English Dictionary (OED) pretty quickly dispatches the category of “quantity” – not enough of a challenge, I guess – but struggles mightily with the definition of “quality.” Let’s hope we have an easier time of it at NSF! The compound term “qualitative analysis,” however, is not quite as hard, since it emerges, from the OED’s rambling historical style, that things became definitionally tidier when “qualitative” was linked to what is now its established “quantitative” flip side. Privileging chemistry, the OED goes on to define qualitative analysis as “identification of the constituents (e.g. elements and ions) present in a substance.” (And yes, I know I’m beginning with the lexical, in strict defiance of our conference instructions! But bear with me...)

Elements, then. In chemistry elements may be one thing – but in the sociological space, the “constituents” with which we researchers operate are first of all signs. A “sign,” you will remember, was the structural linguist Ferdinand de Saussure’s (1965) name for a signified (a concept) and the signifier (a sound pattern, bit of writing, gesture, etc.) that evokes it. But anything can function as a signifier, and become a bearer of meaning, and sociological researchers engage with a variety of substances that do so: bodies; various social practices; natural objects, etc. All sociologists, whether quantitative or qualitative, begin by deploying one body of signs (our social science words/concepts and the theories that are built out of them) that are embedded in and shape our disciplinary practices, and use them to interpret a second level of significant social practice, which sociologists disengage from the analytical material or data under examination. These data are not just “given,” of course: our research practices help create it. So it is the job of the qualitative analyst to confront those data, and to use her or his social science signs – which we often call “conceptual lenses” – to identify the qualitatively separable elements that emerge from those data. Those elements will themselves be organized in significant patterns – whether or not the researcher can see them – in a way that chemical substances are not. For sociologists are studying human actors, who are nothing if not signifying animals, and the modes of action in which they engage.

Note that the OED definition highlights what would be the qualitative dimension present in *all* social science research; I hope this will help keep us from falling into easy, dismissive polarities. The qualitative dimensions I am referring to involve: (1) marking the relevant distinctions among concepts that enable precise descriptions and theories; (2) disengaging the elements that emerge from our observations of the data we’ve assembled and produced. There are two epistemological levels here, and I think that keeping both in mind is important to our collective project because both bear on what makes for good research. If we skip (2), we’ll become solipsistic idealists, conceiving the world as the projection of our paradigms; if we ignore (1) – for example in the fantasy of “grounded theory” – we’ll fall into rank empiricism. Emphasizing both dimensions as empirically interrelated but analytically distinguishable moments of social research may not offer any guarantees, but it’s a start.

Both levels are certainly present in what we call “quantitative research” as well, although they may be relatively underdeveloped depending on how much of the researchers’ energies are directed toward enumerating or counting what turns up. As quantitative methods have gotten fancier and have absorbed a higher proportion of practitioners’ attention, that necessary, even unavoidable qualitative moment in quantitative social research has been unduly neglected – witness the sheer number of articles submit-

ted to journals, even published, that entertain the notion that an entire theory can be tested by entering a single variable into a regression equation! Perhaps one of NSF's goals might be to strengthen the qualitative moment in social science research, period.

So if the lexical route leads us to the "thesis" that *all* social science research is qualitative in important ways, the "antithesis" is the general assumption circulating in the social sciences that the quant/qual monikers can be simply and straightforwardly identified with certain styles of research or research specialties. Perhaps some of the others charged with the task of defining qualitative research will devote themselves to mapping these entrenched disciplinary assumptions. Still, we should always be prepared to revise them. True, formations of knowledge do evolve in a more-or-less specialized fashion, and people are involved in all sorts of social processes that tend to reproduce elective affinities between styles of scholarship and a recognizably qualitative or quantitative methodological orientation. As young scholars who choose to specialize in these sub-disciplinary spaces are trained, for example, they take on board and carry forward particular techniques and old-school epistemological allegiances with which these formations have become associated.

But I want to insist on three big caveats, even if I've no transcendent "synthesis" to offer – perhaps we'll produce this at the conference. First, these affinities can change over time, and even rather rapidly. Historical sociology, which is the part of the discipline that I know best (see Adams, Clemens and Orloff 2003), was almost completely identified with "qualitative work" during its big second-wave explosion of the 1970s and 1980s. Now the third wave includes scholars whom we might classify as neo-institutionalist; culturalist; neo-Marxist; rational-choice; post-structuralist; feminist; world-systems, or post-colonial (to name a few of the more salient theoretical tendencies), and among them they make use of the gamut of qualitative and quantitative methods. Second, and more radically, when one peers closely at the alleged quantitative/qualitative split, its fractal character emerges (see Abbott 2001). Even statisticians break down into Bayesians and non-Bayesians, etc. As we split and scrutinize each separate term, in other words, the two opposed signifiers tend to reemerge within it, perhaps ad infinitum. In any case, we should make time to explore this possibility and discuss its implications for our classification of styles of work.

Third, there may be absolutely nothing *intrinsic* to any mode of research that would forbid its becoming more (or even less!) enumerative, not simply in its findings, but in its analytic practices. Discourse analysis, for example, is generally thought by sociologists to demand qualitative methods. Not only are there already sociologists who think of themselves as "measuring meaning," however – it is also possible that novel quantitative modes of research may be applied to what we now take to be irreducibly textual or impossibly changeable webs of signification. Actually this is already happening – for example, we certainly see some of the prerequisites to mathematicization, such as the incursion of formal theory, making their way into analyses of signification (e.g. Bacharach and Gambetta 2001). Of course it would be far-fetched to imagine cultural analysis as a future branch of economics or mathematics, given that its institutional anchor is so deeply sunk in the humanities. And some of these new approaches are pretty primitive, and may not work out at all. My point is rather that we should never assume that there is a finally-fixed relationship among what are historically-evolving distinctions in qualitative kind, numbers, and styles of knowledge production. Such reifications are the enemy of good science, and we ourselves should take a hand in undermining them.

Perhaps the fruits of our deconstructive and reconstructive labors will even make it into the next edition of the OED.

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URBAN ETHNOGRAPHY*

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Consisting of a range of research strategies, including participant observation, historical research, cultural studies, and content analyses, among others, qualitative methodology differs from quantitative methods that seek to arrive at quantitative indices and generalizations about human society; however, some researchers combine quantitative and qualitative approaches to useful effect. To this end, research attention is paid to documents and public records as well as to human behavior, including in depth observations of how people act and speak. Of particular interest are the local conditions in which subjects live and operate, how they experience their lives, interpret and define one another, and how their lives are different from those of others. A primary goal of qualitative work is to arrive at knowledge and comprehension of the peculiar and essential character of the group of people under study.

A version of this theme is urban ethnography, the close and systematic study of urban life and culture, relying both on first-hand observation and careful interviews with informants, and on available records. Its roots can be traced to the early British social anthropologists. A peculiarly American variant emerged at the beginning of the twentieth century, most notably through the fieldwork of Jane Addams, W.E.B. DuBois, and Robert E. Park, all of whom wrote in the interest of social reform.

Their concern was to inform the wider citizenry of the conditions of the urban poor as well as the nature of racial relations. Concerned particularly with the social challenges of industrialism and urbanization, Park and his students conducted seminal ethnographic work on the city, effectively establishing the premier American school of urban sociology in the early part of the twentieth century. The urban world of the twenty-first century presents new challenges to the ethnographer, who must now deal with the social impact of de- and reindustrialization, increased urbanization, more complex immigration patterns, and the local manifestations of such global economic and cultural processes, including structural poverty.

THE CHICAGO TRADITION

At the University of Chicago, Park and his students produced a series of important and detailed ethnographic case studies of the cultural patterns of the peoples of Chicago. Prominent among these were Anderson (1923), Wirth (1928), Zorbaugh (1929), Thrasher (1927), Shaw (1966), and Drake and Cayton (1945). These studies tended to focus on immigrants, the poor, racial relations, and the various social problems of the day, providing a treasure trove of local knowledge about the city, particularly its neighborhoods, creating a mosaic of social scientific work, and establishing effectively the field of urban ethnography.

After World War II, a new generation of Chicago ethnographers emerged, most notably Everett C. Hughes, whose most prominent students included Howard S. Becker and Erving Goffman. Jointly, they shaped not only the field of urban ethnography but also American sociology more generally. Important examples of urban ethnography also appeared from other settings, such as Boston (Whyte 1943, Gans 1962), Newburyport, Mass. (W. Lloyd Warner's 2 Yankee Studies Series), and Muncie, Indiana (Lynd and Lynd 1929). But as time passed, these efforts were overshadowed by quantitative methods of sociology.

By the late 1960s and early 1970s, the Chicago School was being reinvigorated by Park's students' students, with Morris Janowitz, Gerald D. Suttles, and Howard S. Becker as prominent new teachers. Short and Strodtbeck's (1965) classic study of gangs in Chicago was followed shortly after by influential works on the urban black ghetto. Though not of Chicago, Liebow (1967) and Hannerz (1968) conducted path-breaking ethnographic analyses on the black ghettos of Washington, DC. And Rainwater (1968) added to this work with his impressive study of a failed housing project in St. Louis.

In the mid-1960s, Suttles took up residence in the 'Addams area' of Chicago for three years as a 'participant-observer.' He analyzed and described the social worlds of four major local ethnic groups—blacks, Italians, Mexicans, and Puerto Ricans—and the ways they shared the social spaces of an area undergoing significant 'urban renewal' at the hands of the local government. The groups sorted themselves out in an 'ordered segmentation' created among themselves in a kind of territorial ballet. Residents distinguished their own values and social rules by knowing by whom they were opposed, and thus conflict was kept at a minimum.

During the late 1960s, William Kornblum took a job in a steel mill in South Chicago for two years and involved himself in the social world of the mill employees. They accepted him and his family in ways that became a profound learning experience for him. Among his chief findings was the surprising degree of comity and goodwill in the workplace in spite of the ethnic competition, much of it achieved through political sharing, which provided a certain meaning to the lives of the workers. Contrary to widely held assumptions, the people were quite conservative politically. Getting to know the workers through Kornblum's rich ethnographic experience makes such political views understandable.

In the early 1970s, Elijah Anderson spent three years studying black street-corner men at a Southside Chicago bar and liquor store. He socialized with them closely, drinking, hanging out, visiting their homes and places of work, and he came to know them very well. Contrary to the view of those who are inclined to see this world as monolithic, there were in fact three groups of men at this place. They called themselves 'regulars,' 'wineheads,' and 'hoodlums,' the latter two being somewhat residual, and subject to labeling or name-calling. The study sought to understand the ways in which these men came together on this street corner to make and remake their local stratification system.

Around this time, Ruth Horowitz moved into a Mexican neighborhood in Chicago, and over three years affiliated herself with a male street gang, the young women who often spent time with them, and upwardly mobile youth, learning about the issues facing such groups at first hand. Her work represented an early document in the sociology of gender, but she also found that as gang members went about their daily lives in both the community and the wider society, they would experience tensions and conflicts between their efforts to pursue the American dream and their commitment to a code of honor that demanded actions with a high risk of compromising these efforts.

ETHNOGRAPHIC FIELDWORK

Like the recent Chicago researchers presented above, urban ethnographers typically involve themselves in a social setting or community with the express purpose of learning about the people residing there. Of particular interest is how residents meet the exigencies of life, group themselves socially, and arrive at their shared understandings of the rules of everyday life—conventions, prescriptions, and proscriptions of life peculiar to their world. The answers to the researcher's questions about solving immediate

problems of living reveal much about the social order, or what Clifford Geertz labels ‘local knowledge.’ In particular, key events and people’s reactions to them can alert the ethnographer to the subtle expectations and norms of the subjects, and so to their culture.

In penetrating such local cultures, the ethnographer must not only engage in intensive fieldwork, cultivating subjects and experiencing their social world, but also keep copious field notes—a journal of the lived experience. In developing questions and hypotheses about the nature of the local setting, ethnographers must also deal with their own world view: their ‘own story’ or set of working conceptions about their own world as well as the world of the subjects. Depending on how the ethnographer treats them, such presuppositions can be problematic or advantageous. The subjectivity inherent in the process of fieldwork is often considered to be a strength, for with it can come profound sensitivity to the core concerns of the people being studied.

In this connection, a useful distinction may be drawn between the ‘participant-observer’ and the ‘observing participant.’ The former may be in an early, tentative process of negotiating a relationship with the group under study, and may be satisfied with this position, while the latter has become close to the subjects, effectively empathizing with them, and, it is hoped, able to articulate their point of view. Both positions have their drawbacks and strengths, requiring the ethnographer to remember constantly the primary goal: to provide a truthful rendition and analysis of the social and cultural world of the subjects. To see the world from their point of view requires learning their vocabulary, their concerns, and even their prejudices. It is from such a position that the ethnographer may be able to raise the most penetrating questions, questions that focus on the subjects’ core issues of social organization. In this respect, the most effective questions blend both the ‘problems’ confronted by the subjects in their everyday lives and the conceptual ‘problem’ – the answers to which would presumably advance the field theoretically. The ethnographer’s formal response to such questions, once formulated, can be considered a hypothesis, which in turn may serve as the tentative organizing principle for the ethnographic representation and analysis to follow. Here, the critical task is to advance the hypothesis toward a tenable proposition, or a plausible argument.

The ethnographer’s accumulated field notes will likely include either positive or negative cases, requiring revision of hypotheses to take the case into account. Through this style of analytic induction, the goal is always to develop an accurate account of the world of the subjects, while at times knowingly generating ever more penetrating questions. Such questions, by provocation and stimulation, trial and error, help to advance the ethnographer’s case to surer ground. In this sense, the questions can be, and often are, more important than the ‘answers.’

In the effort to apprehend, understand, and ultimately represent the social setting, the researcher becomes a kind of vessel, a virtual agent of the subjects themselves, serving as a communication link to the uninformed. Such a task is not accomplished easily. Not only does it require a certain amount of empathy in addition to impressive conceptual and observational skills, but the audience, including other social scientists and the ‘lay public’ to whom the setting is represented, may have such strong presuppositions that no amount of evidence will be convincing. This is one of the inherent difficulties and challenges of doing and presenting worthwhile ethnographic work, particularly in socially, politically, or racially charged environments.

THE CHALLENGE FOR URBAN ETHNOGRAPHY IN THE TWENTY-FIRST CENTURY

In recent years, deindustrialization, reindustrialization, increased urbanization, immigration, and economic globalization have made urban areas increasingly complex, both geographically and ethnically. Boundaries, including national ones, are, at the start of the twenty-first century, less important as a barrier to the movement of people, goods, capital, and culture. Los Angeles, for instance, with its ethnic diversity, sprawl, and lack of a single center may be an anticipation of the shape of future cities. So may 'edge cities' (Garreau 1991), such as the Valley Forge-King of Prussia area northwest of Philadelphia, Pennsylvania—urban centers that develop near, but not within, existing cities. And New York, as a global hub with an increasingly international population, epitomizes the tensions between 'local' and 'global' ways of life.

To understand the new global immigration, for instance, ethnographers must now come to appreciate and learn more about the lives of British Sikhs of California who travel back and forth between New York and extended families in India, as well as Bombay elites and Punjabi farmers arriving in the United States at the same time, and low- and high-caste Indians in Chicago sharing utter confusion toward suburbanites. Also important is the manner in which such 'new' people impact established ethnic and racial populations. The black street vendor's story is important, as are the stories of the New York Haitian taxi driver, who finds his own identity by actively distancing himself from the African American, and who visits periodically his cousins who reside in suburban Paris (see Duneier 2000). Of no less importance is the social situation of the Taiwanese middle-class immigrants to Philadelphia who assimilate to 'get along,' but who are strongly ambivalent about 'losing' their Chinese heritage (Tsai 1998).

Moreover, the connections between urban poverty and culture become more acute and ever more complicated in these new environments. Park, DuBois, Addams, and other pioneers addressed the effects of industrialization, urbanization, and immigration early in the twentieth century. Modern ethnographers must come to terms with both positive and negative human consequences of deindustrialization and cybernation of industry in the context of the new global realities, noting particularly the implications for living standards in the local urban environment often beset by ethnic competition. These socioeconomic forces have brought about increasing structural poverty, in which many people are unable to develop the human and social capital necessary to rise from destitution (see Wilson 1987, Anderson 1990). The process of reindustrialization in the areas of light industry, cybernetics, and service must be studied, with its attendant issues of hard and soft skills. 'Brown racism' (Washington 1990) must also be addressed, with its sources and its implications for local urban life and culture. The social world of illegal immigrants from China and from Mexico must be rendered, as well as that of the former peasant from the Ukraine who now makes his living brokering rental properties in New York. In many respects, Thomas and Znaniecki's (1918) early studies of Polish peasants in Europe and America anticipated the kind of ethnographic work on immigrant 'flows' being done at the start of the twenty-first century.

The shifts in social theory that accompany the growing complexity of the empirical world create new lenses with which to see, and therefore present new challenges for conducting a faithful ethnography. Increasingly, 'local' social processes are influenced by supra-local forces that must be studied to illuminate the connections among race, class, power, deindustrialization, and pluralism. To be effective, ethnography, then, must be holistic.

Ethnographers themselves, their audiences, and other consumers are also becoming increasingly diverse. The articulate voices of African-American, Native American, Asian- American, Latino, and gay and lesbian ethnographers as well as local residents who have become anthropologists and sociologists are being heard. Such diversity raises obvious questions about the politics of representation. Increasingly, as never before, ethnographers want to render their own stories, their own realities and local knowledge, and in doing so, make competitive claims on intellectual turf. In these circumstances, some stories get heard, others are silenced, and some interested parties want only the most flattering stories of their 'own' represented.

These are some of the more pressing challenges for urban ethnography today, and they are well worth the effort. As these challenges are met, urban ethnography will become more complex, meaningful, and it is hoped, effectual. David Riesman once likened worthwhile ethnography to a conversation between classes. In this sense, each ethnographic case study can be viewed as an important part of a dialogue for understanding between and among those of diverse backgrounds, a dialogue that becomes steadily more urgent.

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IMPROVING QUALITATIVE RESEARCH PROPOSAL EVALUATION

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Questions Motivating this Essay: These are the questions we were asked to consider as we panelists wrote our essays.

1. What exactly do we want the NSF to advance?
2. How can NSF help strengthen the scientific basis of qualitative research?
3. What might be considered “best practices” in qualitative research, and what are promising new directions and developments?

This essay tackles two basic problems I think are reflected in these questions (and in the second question in particular). What I mean to say is that I think that we may be posing the wrong questions. To me, our main problem is not that we need to strengthen the scientific basis of qualitative research. Rather, our problem is that in our discipline, qualitative methods have a reputation for being insufficiently “scientific.” A related problem is this: if the discipline has difficulty understanding and valuing qualitative research, we assume this problem is the concern and responsibility of qualitative researchers alone. I will explain how it is I came to address these problems.

I served on the National Science Foundation (NSF) Advisory Panel for Sociology during the academic years 2001-02, and 2002-03, and in those two years the panel reviewed hundreds of proposals. Quantitative researchers largely outnumber qualitative researchers in the discipline of sociology. Therefore, they represent the majority of the gatekeepers to the funds that qualitative methodologists need in order to do their work. Surely, it is difficult for quantitative researchers to “see” what the qualitative researcher will do, given that the research plan can neither be explained as a proposed set of “variables” that will be catalogued, a “model” that will be tested, nor an “algorithm” through which the data will be fed. I reflected on these facts, and began this essay to answer the following questions:

1. What does a good qualitative research proposal look like? How may qualitative researchers best propose their projects, at the planning stage, so that quantitative gatekeepers can best understand and evaluate their projects?
2. Exactly what criteria should be used to evaluate qualitative research proposals?

Upon reflection, however, I felt that it was quite problematic and erroneous to propose that the answers to these questions turn on what work the qualitative researcher had to do, i.e., plan and propose their projects in clearer terms, and translate their work so that the uninitiated quantitative researcher can understand the “language” of qualitative methods. Qualitative research is undervalued because we define our discipline in a certain way – as scientific, controlled, and (still) largely following the positivist tradition. I realized that panelists tended to disfavor certain kinds of proposals: those where the qualitative researcher was clear about “letting the data speak for themselves,” perhaps an indication, for some, of a lack of control over data and settings in ways that seem patently unscientific. I also noted that disfavor

found those PIs who proposed fieldwork in international settings, particularly if that research was comparative. With regard to biases against international work, my best guess was that panelists (who had not done international field work themselves) could not envision doing such work with the “limited” time or funds that scholars proposed to use would be sufficient; remained suspicious of what the quality of the work would be, even if the scholar proposing the work had written and published books that resulted from prior research in international settings; and generally felt that if someone had done research in one area of the world they may not be able to translate those skills to study another country or region. (This last was especially true if there were language differences between the researcher’s former field location and the proposed location.) Thus, other questions come to mind:

1. What can be done to have quantitative researchers understand the nature of qualitative work such that some of the suspicions about qualitative research can be alleviated?
2. What is the source of the disbelief about the feasibility of international fieldwork, and how can that be lessened?
3. What can be done to encourage the support of new scholars in qualitative methods (for whom a track record may not be so well established, and therefore in whom panelists may be less willing to invest, particularly because the proposed outcomes are not so “clear” relative to the work proposed by a young quantitative researcher)?

Here are some of my answers:

FOR THE QUALITATIVE RESEARCHER: HOW TO WRITE A PROPOSAL QUANTITATIVE RESEARCHERS CAN RELATE TO

Researchers who use quantitative methods are in the habit of *controlling* each aspect of their research. For example, they begin research with a theoretically-based judgment about which variables are important to the research question under examination. They oversee a scientific process of data collection that is sanitized of respondent and researcher influence (anonymous survey research comes to mind), or they purchase prepackaged datasets. Whether they collect or purchase these data, they make decisions to “clean” the data of observations to be ignored, and keep only those they wish to include. They predetermine which software and models will promote the “right” output at the data analysis stage, using computer analyses to determine the important relationships among the relevant variables. Finally, it is the job of the quantitative researcher to employ statistical tools that will summarize trends and test hypotheses that describe samples and predict the likelihood of event occurrences.

Qualitative research differs from quantitative research on all these points. Instead, qualitative researchers actively *avoid control*. In fact, if they seek control over their subjects, interview material, or the ethnographic setting under scrutiny, they are surely doing something very wrong. In many cases, they are in the process of theory-building, so may have no idea about which variables are important in a research setting until their analysis is complete. Data collection precisely depends on both the relationship between the respondent and the researcher, and on their unique contribution to the research process. The respondent is often not interchangeable with just any other person, for their particular words, thoughts, and intent are the targets to be captured in the act of data collection. Moreover, the researcher cannot be “invisible” to the process, for it is the researcher herself who serves as the tool that will capture these data, and it is the investigator alone that constructs knowledge by the very act of present-time hand-col-

lecting and -assembling various representations of reality. Perhaps what makes the average reviewer leery of qualitative methods is the acknowledgement of the lack of control (over the data itself and the environment in which data are collected) and requirement of trust (where the researcher must trust the respondent to give an accurate representation of self, the respondent must trust the researcher's promise for truthful accounting of their oral history, and we are asked, as disciplinary peers, to trust that the researcher herself is a faithful instrument of data collection and the most accurate instrument of analysis we can employ on this work). Clearly, these methods are nearly antithetical to the scientific method influenced by a longstanding positivist tradition.

Thus, the qualitative research wishing to appeal to a panel of their disciplinary peers or betters (be it a dissertation committee or grant proposal review panel) has a difficult task ahead. Here are my suggestions on What a Good Qualitative Research Designer Can Do to Write a Proposal that Those Who Speak in Numbers Can Find Appetizing:

1. *Leave Breadcrumbs*: The researcher should allow us to follow him/her into the field by providing a trail that leads us down the path they took (or will take) to gather their fieldnotes and compose their findings. Explain the research setting, why it's important to go there, and how you plan to "get in" and "get out" of the field. Items to be discussed in this vein include: how you gain access to the field or the targeted group of respondents, what types of questions you will pose to respondents or what kinds of observations you will make, how long the trip into the field will be, (for comparative work) what kinds of contrasts you need to see in order to come to conclusions, and at what point you end an ethnography or set of interviews (i.e., how did you come up with that number of days in the field, or number of persons to interview?).
2. *Hand out the Recipe (Even if You Don't Tell The Secret Ingredients)*: Even if you don't know all that will happen before your analysis comes to a close, don't assume the reader of your proposal will be in any position to visualize the finished research product without your candid assistance. Help the reader to see, taste, smell, and feel what being in the field will be like. Don't just say that you'll use a convenience or snowball sample – explain who those people will be, and why their subset of the population is the best sample to use for your project. (In this way, you also help those used to "randomized" samples to understand why these sampling methods can be as valid in the qualitative research world as randomization is in the quantitative world, i.e., if the sampling is done right.) Remember two basic points: you should not assume that potential readers will have prior knowledge of the fieldwork experience, and you must assure the reader that a lack of "control" does not mean the absence of a research method. To the best of your ability, be clear about the methods employed, and the rationale for their application in your project.
3. *Allow Taste Testing*: The qualitative research process is to me akin to cooking without a cookbook. Those with lots of experience can be trusted to make a great meal – but newcomers to the table might not know of the cook's skill. If you as the PI have made your reputation already, provide some samples that describe your previous field experiences – describe not just the findings, but also the analytical process. Use phrases like "post-field analysis of the interviews revealed that..." or "what emerged from the fieldnotes was..." and provide a little detail about how interview analysis is done, or how fieldnotes are sifted so that significant findings do emerge. For newer PIs, you might do the same with the results of your pilot studies of the proposed field research.

FOR THE QUANTITATIVE RESEARCHER: HOW NOT TO EVALUATE QUALITATIVE RESEARCH

As much as I would like to list all the criteria that need be looked for in a qualitative research proposal, I think I would be far from complete in making that list. Still, if I can't give a comprehensive list of "Dos," I can suggest some "Don'ts."

1. *Do not expect these proposals to look like quantitative proposals.* When you don't see evidence of randomized sampling, or a discussion of what the "model" or "hypotheses" are, don't immediately conclude that there's something missing from the study. While quantitative researchers are in the game of accepting or rejecting theories posed as hypotheses, the qualitative researcher, for the most part, has their shoulder bent at the wheel of theory building and rebuilding, and consequently, hypotheses generation and reworking. Theory-building research needs to be evaluated by a different set of criteria.
2. *Do not expect this work to "test" already existing theory – they may be building it instead.* Perhaps one of the best ways to evaluate theory-building research is the degree to which it can open up new ways of thinking in areas where we are deadlocked. In this, remember that quantitative researchers depend on new theories in order to have hypotheses to test and new questions to put on our surveys. We need qualitative scholars to go out and investigate new relationships, and tell us about the contours of underexplored areas, for they do the work that explains new associations among variables that we know are correlated, but we don't know why. Moreover, these researchers often take up questions for which satisfactory answers have not been forthcoming, or in the crux of a body of work that has not had a lot of testing. Qualitative researchers ask questions without knowing all the answers in advance. This is not wrong – it's what they do.
3. *Don't fail to give credit for being interesting.* Sometimes it's the PI's take on the research question that is the real contribution – and we often fail to appreciate the new and innovative, and instead tend to fund the projects that ask familiar questions or use familiar constructs. We have to get out of the way in order to make room for paradigm shifts. Sociology, we need remember, is a discipline, i.e., a way of seeing, not a set of things to see. I liken our discipline to philosophy, but one that has an empirical basis. So give credit for questions that are theoretically interesting or innovative. Panelists can suggest the PI tweak the proposed method if they find it decidedly problematic.
4. *Don't reject a proposal based on a little voice saying to you "How is this person going to do this with so little... (time, money, or other resource)?"* If you are called upon to evaluate qualitative research, please suspend disbelief about whether you think the project is "reasonable" at least until you have decided that the project is interesting, different, and/or innovative enough to make a contribution to the discipline. If you decide it's none of the above, then "reasonableness" should not matter – don't fund it. But if it is all of the above, either give the researcher the benefit of the doubt, or send the proposal back to be revised and resubmitted with further clarification as to the details you're missing. It is a mistake, however, to judge feasibility too harshly if this kind of research is not of the kind you've tried before, especially if the researcher has a strong record of doing the kind of work they're now proposing.

5. *Don't fail to reconsider the international proposal.* In my time as an Advisor to the NSF Panel for Sociology, I noticed a willingness to fund “international” projects of the kind that are national-level data analyses of the political economy of the world system, or projects focused on globalization, especially those that would analyze large data sets to formulate macro-level theories. That is, the project could be “international” as long as the researcher stayed on US soil while they did their work. Conversely, the panelists seemed decidedly less willing to fund international field studies. I am not sure I know why, unless the reasoning falls under the “little voice” rubric mentioned above. (I assume that anthropologists, geographers, and other social science disciplines that may send US scholars out of the country for fieldwork as a matter of course may have less of a problem with this.)
6. *Don't forget to give the new scholar a break.* We should probably encourage the newest trekkers into the field, especially if they show evidence of sufficient training and that they've done their theoretical homework in the area of proposed research.

FOR THE DISCIPLINE: HOW MIGHT WE ADVANCE QUALITATIVE RESEARCH AND METHODS

I think the absolutely most important thing that the NSF can do to promote better qualitative methods projects and proposals, and a better disciplinary-wide understanding of the importance of these methods, is to recommend to all graduate programs that they require qualitative methods as a part of the normal training for all sociologists. To get better qualitative studies, we need more and better qualitative researchers. Moreover, we need more people of all methodological camps to be able to evaluate projects in a knowledgeable and unbiased fashion. Surely, we simply cannot continue to advocate requiring only statistics and higher-level quantitative methods and then assume that a researcher has been fully trained by the time they graduate. While reading qualitative works as a graduate student (e.g., Durkheim) may give one an appreciation of theory, how that theory develops during the course of one's time in the field, and the difficulties of data analysis and writing up qualitative research, all remains a mystery to the average student who took only the required (quantitative) research methods and theory courses. The mystery of qualitative research is never solved for the new scholar if he or she is never asked to go into the field, conduct an interview, or write and analyze fieldnotes even once before they take up academic jobs that tend to further constrict the range of one's own research areas. (As well all know, in order to get tenure, one has to show a developing agenda, which generally leads to even more narrowing of research topics investigated and methods employed.)

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THE PROBLEMS OF ANALYSIS

Howard S. Becker

A distinctive feature of qualitative work is that analysis of data goes on continuously. It starts with the first item of information the researcher takes in (and this often happens before the researcher even knows that there is any research going on), continues throughout the data-gathering process, and of course is what happens in the last phase of the work, as you write up the results. The methods of analysis appropriate to this range of situations are varied. What works best in the beginning is not what works best at the end.

At the end of our research we want to consolidate our data so that we can see if the information we have gathered supports the ideas we have about the situation we studied. That sounds simple, but of course it isn't. This is a problem for every sort of research and, at a very general level, the methods used in both qualitative and quantitative research are very similar. That is, the logic is similar. Which is why, for instance, I have always found Paul Lazarsfeld's analyses of property spaces and index formation so useful and have always recommended them to students. And why I found Charles Ragin's set-theoretic methods so congenial. They do things that I always do in my own work, and make them understandable and defensible at a general level.

There are some differences, of course. I think it's true that, in general, qualitative researchers are more likely to be interested in process models, which are more difficult to manipulate in quantitative research. And they are, conversely, less interested in questions of the distribution of properties in a population. Similarly, they are more interested in characteristics that are widely shared in a group—cultural understandings, for instance—and less interested in things that differ among the members (thus providing the variation that is so necessary to quantitative analysis).

The big difficulty in making these analyses at the end has always been the trouble of manipulating large amounts of uncategorized (and difficult to categorize, because its gathering wasn't constrained by considerations of that kind of categorization) data. How do you take field notes or unstructured interviews and turn them into little pieces of data that can be worked with analytically? There are a lot of computer programs that help researcher do this now, and so far as I know they all work pretty well. Everyone has their favorites, I have mine, but in fact they're all OK. I don't think this is an area that is worth spending enormous amounts of time on, because it's been pretty well handled.

One thing you can say about all these programs is that they definitely will not think for you, will not invent new ideas, not make interesting comparisons, not come up with novel categories, etc. They will do what computers do well, which is grunt work. They will save you copying stuff over and over again, they will count things for you, and all those things they can do are worth doing, but they ain't it. (I remember a photo teacher of mine saying to the class, "OK, all of you people can print pretty well now, you make a nice looking print. The thing is there are only about 400,000 people in the US who can do that, so that can't be it, can it?" Which was a dispiriting thing to have to recognize.

The methods that need more investigation and thinking about, at least in my judgment, are the ones that are more characteristic of the earlier stages of research. They are methods, you might say, for making a lot out of a little. That is, you're working in the field, you see something interesting, and you get an idea.

This is typically treated as an unanalyzable experience (an “Ah-hah!”) that there’s really nothing to say about, you just hope it will happen to you.

But these inventions in the field are not unanalyzable. It’s easy to sketch out just how they happen, and I’ve done that for one of the ideas that came to inform our study of medical students in a paper called “How I Learned What a Crock Was” (which is available at <http://home.earthlink.net/~hsbecker/crocks.html>). What I describe there is a series of investigative steps that lasted many weeks or months, depending how you count, and which led to some major ideas that we worked with in the entire study. These steps included testing out analytic ideas in the field by gathering new data related to them; thinking about possible extensions of those ideas to other areas of the research; and looking for connections suggested by them between seemingly disparate parts of the research, disparate topics. I’d like to see a lot more analyses like this. I don’t think this one is so great, but I do think it’s a model for what might be done. Imagine a lot of such analyses, from which we could begin to extract Lazarsfeld-like principles for extending single observations through repeated observations and interviewing.

A preliminary step to such a venture, of course, is to convince people who need convincing that this is a job that can be done and that is worth doing. This is the part of qualitative research that is typically regarded by people who don’t do it as somewhat mystical, not reducible to any kind of principles, etc. It shows up in the kind of things that are often said about admitted masterworks of social research that were done in this style: “Oh, yes, *Asylums* (or *Street Corner Society*) is a great book, but you have to be Goffman or Whyte to do that.” Which I don’t think is true and I think there are rafts of excellent monographs done by us non-geniuses to show otherwise.

A way of doing this might be to start analyzing seriously what makes books like those so good. I’ve done a little bit of that in an earlier paper on “The Epistemology of Qualitative Research” and I’m going to be lazy and refer you to that piece (which is available at <http://home.earthlink.net/~hsbecker/qa.html>). Suffice it to say that I invoke such criteria as accuracy (avoiding indirect indicators of what we talk about when more direct ones are available, even though those are more trouble to accumulate and work with) and knowing about a lot of the things involved in what we are studying, instead of gathering data on a few things intensively and speculating about the rest. There’s a lot of work to be done here and much of it could be done through the intensive analysis of exemplary works. (Something like this occurred with Herbert Blumer’s critical study of Thomas and Znaniecki’s *Polish Peasant in Europe and America*, which was commissioned by the Social Science Research Council in the 1930s.

A DANGER

As I read the other things so far put up on our site, and as I thought about my experiences in such events in the past, it occurred to me that we are all in danger of making a simple and perfectly reasonable mistake, which I have made many times in the past. So I thought I’d warn us.

The mistake is to compare qualitative and quantitative research and to imagine that quantitative research is always carried out just the way it says in the book, that researchers in this mode always plan everything out in advance and then follow that plan rigorously, never improvising or making changes due

to changes in circumstances, etc. The mistake is to imagine that it's only qualitative researchers who behave in this fashion and so that what has to be explained is how what we do could possibly be science even though we don't follow those rules, when in fact (in my experience, maybe I hang out with bad kids, who knows?) nobody does research by following all those rules. If we set it up this way, we are misleading ourselves and anyone we are trying to enlighten about the nature of field research.

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TESTING THEORIES AND EXPLAINING CASES

Andrew Bennett
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Social science encompasses a variety of epistemic goals. The testing of established theories is only one such goal, with others including attaining rich and accurate theory-based explanations of individual historical cases and developing new theories worthy of additional testing. Qualitative methods are particularly powerful in these latter two tasks. They can achieve a rich and verifiable historical explanation of a case by incorporating large numbers of variables and using detailed observations on the processes through which the outcome arose to eliminate some competing historical explanations and increase confidence in others. As for developing new theories, researchers can study “deviant” cases that are anomalous with regard to extant theories, or that are outliers in statistical distributions based on extant theories, to observe inductively from primary sources what factors may account for the unexpected outcome. In contrast, in a statistical study one can only carry out statistical tests on data that someone has already thought to code; the act of actually coding data can lead inductively to new variables in a statistical study, but it is less likely to do so than the intensive study of deviant cases with the dedicated purpose of generating new hypotheses.

While the comparative advantages of qualitative methods in historical explanation and the generation of new theories are important, for purposes of stimulating discussion the present paper focuses on the task of theory testing, a process at which qualitative methods are often assumed to be inferior to statistical methods. Qualitative methods do indeed face a set of epistemological challenges in the task of theory testing. The very qualities that make these methods strong at explaining particular cases - - their ability to take into account many aspects of a particular context or case - - make it difficult to generalize to other cases with different contexts or different configurations of variables. I will argue, however, that qualitative methods can indeed be used for purposes of theory testing. My argument proceeds by three steps: 1) qualitative researchers often have a somewhat different view of “theory testing” from that of statistical researchers, derived from skepticism about broad assumptions of “unit homogeneity;” 2) theoretical and historical explanation are linked: you can’t explain a population if your explanation does not hold for individual cases in that population; 3) qualitative researchers use an informal kind of Bayesian logic to generalize from tests of competing historical explanations of a case to theories that apply across types or populations of cases.

Theory testing is often conceived of as testing which of several competing theories best explains a specified population of cases. This might be termed the “subsumption” model, as the emphasis is often on subsuming as wide a population as possible under as spare a theory as possible. As Charles Ragin has pointed out, however, specifying a relevant population of cases is not unproblematic or independent of theory. Related, the “unit homogeneity assumption,” or the weaker “constant causal effects” assumption that a unit change in an independent variable will have a constant causal effect on the dependent variable for all cases in a population, is always open to challenge for a specified population, and it becomes more dubious as we expand the population in the interest of subsuming more cases.

Thus, an alternative conception of “theory testing” often favored by qualitative researchers puts as much emphasis on specifying relevant populations for each theory as on assessing in some sense the general utility of competing theories for broad populations of cases. In this view, each theory may be accurate

in explaining some cases even if it does not explain all cases within a specified population. Put another way, the relationship being studied may exhibit “equifinality,” that is, there may be more than one path to the same outcome, and the different paths to the outcome may have little or nothing in common. The goal of theory testing, then, is to expand or narrow the scope conditions of contending theories as the evidence demands, and to identify the conditions under which the particular causal mechanisms hypothesized by these theories interact with one another in specified ways. I label this the “mechanism” model of theory testing.¹

The astute reader will note that the mechanism conception has a “narrower” homogeneity assumption—cases of the same “type” or with the same configuration of variables should have the same outcome—but a homogeneity assumption nonetheless. How, then, do these homogeneity assumptions differ, or how is it different to seek the right population for a theory rather than the right theory for a population? The difference is only in the degree of specificity of theory and the contexts in which it applies, not in the nature of theory itself as a source of explanation. As a practical matter, qualitative researchers are usually inclined to question whether the unit homogeneity assumption might be fruitfully narrowed by including at least one or a few contingent variables more than is common in the statistical research on a given problem. Thence the familiar debate, whenever such a move is made, on whether the resulting decrease in parsimony and narrowing of the relevant population is compensated for by the increase in fit that arises from adding variables. As a philosophical matter, the unit homogeneity assumption is in principle always open to challenge from a finer grain of detail or a more narrowly defined population, until there is a level of detail beyond which we cannot observe or the number of cases is reduced to one. As social scientists with an interest in generalization across cases, rather than historians, we are rarely if ever interested in pushing to this level, but we do usefully argue over where in the middle of the spectrum we should be between general regularities covering broad populations and highly contingent theories that apply only to small populations.

There is a range of degrees of generalization here. The findings of a case study may be relevant only to an improved historical explanation of that case. More broadly, they may apply to a type of cases, or a particular configuration of variables, of which the case is a member. More broadly still, if a case study generates a new theory on an un-theorized causal mechanism, this finding may apply to many different types of cases, though this mechanism may play out in different ways in different types or contexts.

This brings us to the relationship between the historical explanation of particular cases, in which each step in a case is explained with reference to some theory, and the testing of theories that apply across cases. Milton Friedman has famously argued that the probability with which a model correctly characterizes or predicts a phenomenon is the only standard for theory testing. In this view, it does not matter if actors behaved according to the mechanisms specified by the model; it only matters that they behave “as if” this were true. Few social scientists, regardless of their methods, currently endorse this radical perspective, but it is worth reflecting on where Friedman’s powerfully-stated argument oversteps. Friedman was certainly correct in arguing that all theories are simplifications of reality and are thus always in some sense wrong or incomplete. Where he was wrong was in extending this claim to suggest that the probabilistic goodness of fit of a theory for a specified population was the only standard of theory choice. If detailed historical evidence on a case indicates that its outcome clearly did not arise through the processes described by a theory, and if this evidence fits an alternative theory much better, the alternative theory clearly offers a superior historical explanation of the case. The alternative theory may or may not offer superior explanations of other cases in the population under consideration, or of the whole population, but this is a separate question.

In this regard the logic of historical explanation functions quite differently from that of statistical correlations. If a theory hypothesizes that one hundred steps should happen in sequence, leading to the outcome of the case, and one step is not as hypothesized, the theory must be modified, perhaps trivially or perhaps fundamentally, if it is to explain the case. It does not matter that the theory got a statistically significant number of steps right, or that it explains other cases very well.

This still leaves us with the question of how to generalize from a superior historical explanation of a case to other cases. Here, following Harry Eckstein, case study researchers have used a logic analogous to the Bayesian approach to theory testing. As Eckstein argued, if a theory fails to fit a case in which it is most likely to be true, then our confidence in the theory and/or our estimation of its scope conditions is greatly reduced, whereas if a theory fits a case which it is least likely to fit, our confidence in the theory and view of its scope conditions increase. If we find that even anarchist movements are hierarchically organized, for example, we may conclude that hierarchy is endemic to social behavior.

Using Bayesian logic more explicitly allows us to improve on Eckstein's formulation. Bayes theorem highlights that in judging cases as most or least likely, we need to consider not only the likelihood that the theory of interest will explain the case, but also whether the case is most or least likely for the alternative hypotheses, a factor that Eckstein neglected. Thus, the strongest evidence for a theory is when it is likely to apply only weakly to a case, the alternative hypotheses are likely to apply strongly to the case and all predict an outcome opposite to that predicted by the initial theory, and the initial theory proves true. In this instance, the outcome cannot readily be attributed to causes other than the initial theory. The most powerful disconfirming evidence is when a theory strongly predicts an outcome, the alternative hypotheses predict the same outcome, and yet the outcome does not occur. In this instance, the theory's failure cannot easily be blamed on the presence of countervailing mechanisms. Such pure examples are rare, but it is still useful to qualify or extend the findings of an individual case depending on how likely a case it was initially considered to be for alternative theories. Also, in the extreme instance when a theory posits that a variable is a necessary or sufficient condition for an outcome, a single case that does not turn out as expected can discredit the theory (barring the possibility of measurement error). In short, even tests of theories in single cases can have far-ranging implications for our confidence in these theories and our view of the contexts in which they apply.

ENDNOTE

¹ For a discussion of the related "unification" (which I have termed subsumption) and "mechanism" models of explanation, see Wesley Salmon, "Scientific Explanation: Causation and Unification," in Salmon, *Causality and Explanation* (Oxford University Press, 1998) pp. 68- 78. Salmon argues that these two modes of explanation are not inconsistent with one another.

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DEFINING QUALITATIVE RESEARCH

Joel Best
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One of the problems with trying to write about naming or defining qualitative research is that this is hardly untracked terrain. When I started grad school in 1967, a fairly complete bibliography on the theory and methods of qualitative sociological research probably could have fit on a single page with plenty of blank space left for notes. In those days, people contrasted the dearth of writing on qualitative research with the huge literature on quantitative research; this gap—plus the responsibility to document one’s methods for readers—justified the appendices (partly methodological, partly confessional) that began appearing in the growing number of ethnographic monographs. Doing field research seems to inspire methodological musings in a large share of researchers, who often devise brand-names for their techniques. Thus, the thick, recently published handbooks devoted to qualitative research, interviewing, and participant observation contain chapters of performance ethnography, autoethnography, ethnographic content analysis, institutional ethnography, and on and on. Sharing tricks has become very common in our trade. I don’t know whether anyone has dared to produce a comprehensive bibliography of writings on the theory and methods of qualitative research, but it would contain many hundreds of items.

Why should we bother to define and name something that already has been assigned so many definitions and labels? Perhaps the best reason is because qualitative sociology has become a very large tent; the label provides shelter for a diverse set of actors, who have very different agendas. At a minimum, the list includes:

- **Theorists:** Theory-building encompasses everything from Grand Theory, to theories of the middle range and grounded theory. Some theorists never leave the armchair, while others’ hands are dirty from working in the field, but theorizing is an important form of qualitative sociology. Thus, even though our camp rarely claims them, it is difficult to explain why, say, Parsons or Merton should not be considered qualitative sociologists.
- **Social Researchers:** This, of course, is the group in which NSF is mostly likely to be interested—the ethnographers, participant observers, and other field researchers, but also sociologists whose research involves the qualitative analysis of documents of various sorts. Typically these folks do some theorizing in trying to make sense of their data, but their contributions are more empirical than theoretical.
- **Philosophers:** Here, I mean to draw a distinction between theorists who try to devise theories that account for social life, and those writing self-conscious analyses of what we can know and how we can know it. Sociologists who start worrying—and writing—about ontology and epistemology tend to focus on criticizing the philosophical flaws in others’ research, rather than actually reporting what they have learned about the world.
- **Activists:** Sociology seems to be more ideologically homogenous than the other social sciences, and doubts about the possibility of a “value-free” stance have encouraged sociologists to proclaim their works’ commitment to justice, equality, and other ideals. In some cases, researchers merely announce their ideological leanings, but in others, the reaffirmation of ideology seems to be the central contribution.

- Artists: Other sociologists have experimented with writing poems, plays, and cultural criticism, with photography, and so on. Again, there is a range from work that is strongly grounded in research (e.g., the ethnographies accompanied by photographs that appear in *Visual Sociology*) to poems whose claim to sociological status seems to depend on our knowledge that the poet is a sociologist.

I'm sure this is not a complete list—I threw it together in haste. But it suggests that there are lots of different people waving the banner of qualitative sociology. Perhaps every piece of sociological work can be viewed from all of these dimensions—as having some theoretical content, some foundation in research, philosophical underpinnings, political implications, and artistic value—but the relative importance of these clearly varies from work to work.

It seems to me that, for NSF to take a greater interest in qualitative sociology, we need to be focusing on projects where research—rather than any of the other concerns—is central. Further, it seems to me that the logic of research must incorporate what strikes me as the key principle of science—that is, its claims must be falsifiable. It must be possible to derive propositions, whether via deduction or induction, that can be subjected to tests that can prove them false. Analysts whose primary allegiances are to philosophy, ideology, or aesthetics are likely to discount the importance of falsifiability in favor of some greater truths, and they can do that, but they shouldn't be allowed to hijack the label of science in the process.

In other words, I am concerned that the proliferation of many sorts of activities under the label of qualitative sociology threatens to confuse our discussions. While I hate to think that we need to devise yet another name for what we're doing, it seems to me that we do need to acknowledge a basic principle: we're committed to developing falsifiable statements about the empirical world.

EVALUATING QUALITATIVE RESEARCH

Kathleen M. Blee
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Qualitative research in sociology can face two, nearly opposite, sources of criticism. On the one hand, the proven analytic and predictive strengths of quantitative research – in sociology and elsewhere – have contributed to a widespread sense that good social science research means a quantitative approach. Quantitative research fits easily within the templates of science with its generally linear work flow: theory → hypotheses → data collection → analysis → conclusion. And this model of research is taught to every graduate and undergraduate student in sociology; hence scholars need to spend little time and few pages to justify its logic or use in research. Against this implicit standard, qualitative research, even at its best, may appear soft, tentative, preliminary, or unfocused.

Qualitative research tends to bump up against the assumed model of quantitative research in various ways; especially its simultaneous data collection and analysis, lack of theory testing or theoretical specification; open-ended research or analytic strategies; and unclear connection between theory and results. Although important recent works have developed and codified qualitative research strategies, many qualitative designs still are not in the sociological canon. Too, qualitative research strategies (to say nothing of techniques) are quite disparate, further undermining the likelihood of creating a simple qualitative research template or checklist against which such work could be effectively and efficiently judged. Thus, even very good qualitative research proposals or papers may have a difficult time finding solid support from reviewers.

On the other hand, at least some qualitative sociological research faces criticism precisely because it does attempt to specify a research strategy. Various ethnographic methods that draw on the immersion strategies of (generally older) anthropological work, for example, emphasize the value of very open-ended research. Elaborate attention to methods, analytic strategies, or theory can be seen as detracting from a researcher's ability to immerse her/himself in the field, develop an insider's understanding, or follow his/her nose in the research process. Such criticisms are more likely to occur in the evaluation of qualitative research *proposals* than in the evaluation of qualitative research *outcomes*. Relatively unstructured qualitative projects often produce innovative and useful methodological approaches but few can (or want to) specify these in advance, especially in the proposal stage. Requirements for detailed methodological or analytic strategies or data description in a proposal are likely to discourage many such researchers from applying for funding.

Given these opposite sets of concerns, how can we envision and evaluate an approach to research that is both scientific and qualitative? I offer four lines of thought as fodder for our discussion.

1. QUALITATIVE RESEARCH SHOULD BE EVALUATED ON ITS OWN MERITS.

It would be ideal (although unlikely) that we could develop a simple template for evaluating qualitative research. But, at least, it is important that qualitative research proposals be judged against their own strengths and weaknesses, not as weaker versions of quantitative research. This may mean expecting data collection and analytic procedures to be more clear for the initial stages of research than for latter

stages; recognizing the need for sufficient labor time for data collection/analysis; rewarding comprehensiveness over efficiency; and considering the development of hypotheses as mid/endpoints rather than as preliminary to research.

We may also want to rethink the relationship between qualitative and quantitative research. Generally, qualitative research is regarded as preparatory to quantitative research, as a stage of developing hypotheses and exploring relationships on a small scale that can be studied more definitively, systematically, and rigorously in subsequent quantitative work. Certainly, this gives qualitative approaches a place in sociology, but more often as maidservant to quantitative studies. Doug McAdam has usefully questioned the universality of this relationship, arguing that “quantitative analysis can be used ... to uncover consistent empirical relationships that can be interrogated more fully using systematic qualitative methods.” One way to proceed, then, might be to encourage a more complex engagement between qualitative and quantitative researchers who now often proceed on the parallel tracks. For example, NSF might try to encourage quantitative researchers to consult qualitative studies for ideas and to specify relationships (or puzzles) from their findings that might be studied with qualitative research, as well as encouraging the reverse for qualitative studies.

2. GOOD QUALITATIVE RESEARCH IS SYSTEMATIC AND DEMONSTRATES RIGOR.

Qualitative research needs to be evaluated on clear criteria, including its rigor and systematic approach. Ad hoc and casual approaches to data collection and analysis should not be counted as qualitative research strategies.

At a minimum (or maybe, ideally), a qualitative research project should have the following elements, although some are more likely in a report of *completed* research than in a *proposal* to conduct research. Qualitative research should begin with a sharp, focused question or puzzle that will be solved in the research, rather than proposing generally to *describe* social phenomena. The study should have an initial sampling and analytic framework, although these are likely to change over the course of data collection and analysis. It should address questions of validity of method and interpretation rather than assuming that these are non-issues in qualitative research and it should construct ongoing means of assessing validity throughout the study. It should have a thoughtful approach to data analysis and not rely on empty – almost magical-seeming – discussions of doing theory-building or data-coding through computerized text retrieval software. It should reflect the systematic and ongoing exploration of alternative explanations; that is, the author’s developing explanations should be continuously checked against possibly competing explanations. And the project should strive for coherence and comprehensiveness in addition to plausibility.

3. QUALITATIVE RESEARCH SHOULD MAKE GOOD USE OF THE ADVANTAGES OF QUALITATIVE APPROACHES.

Qualitative research should consciously employ its strengths. For example, a qualitative research project might be evaluated by how effectively it makes use of flexibility in design and research strategies. To this end, it may be better to expect and reward research in which the results and proposal do not “fit” as tightly as they would in quantitative research, i.e., in which the researcher has traveled some distance from the initial conceptualization of the problem, following the twists and turns of her/his data rather than rigidly adhering to the original formulation. Moreover, qualitative research should highlight — rather than hide — the development of non-parsimonious explanations (including the ability to

“tell a story” about the data), the contextual focus (especially where discrete or categorical data might be misleading), the ability to uncover the “facts” of what informants believe “the facts” to be, and the researcher’s long involvement in a research site or issue.

4. WHEN POSSIBLE, QUALITATIVE RESEARCH PROJECTS SHOULD BE EXPANSIVE.

Quantitative research is generally expansive. It tends to be tightly tied to previous scholarship and self-consciously positioned to build knowledge, not only through dissemination of results but often also by making data publicly available for replication and student instruction. And many quantitative research studies involve multiple investigators, including graduate students.

Qualitative research tends to be less obviously tied to projects of cumulative knowledge-building and qualitative data is less often made available to other researchers and students. Much (certainly not all) qualitative research also is fairly solitary. The result is that students may be less likely to be involved in the nitty-gritty work of qualitative than quantitative data collection and analysis before they begin their own work. And there is relatively little access to qualitative data that could be used for replication or instruction. A final aspect of the evaluation of qualitative research proposals, therefore, might be whether they have developed avenues of expansion, e.g., by making data available or incorporating students into the research agenda.

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ETHNOGRAPHIC PROTOCOL FOR WELFARE, CHILDREN, AND FAMILIES: A THREE CITY STUDY

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I. SAMPLE DESCRIPTION

The ethnographic sample, which was identified in the second half of 1999, is composed of approximately 256 families across the three cities (Boston, Chicago, and San Antonio). The families who participate in the ethnography will mirror those involved in the survey in terms of welfare receipt and family structure. In addition, each family will have a child aged two to four, thus matching the age range of the children in the Embedded Developmental Study (EDS).¹ Within each family unit, the ethnography will focus primarily on parents, a target child aged two to four, the primary care provider for the target child, his or her siblings, and the social networks of the parents. The ethnographic component includes a subsample of families with two-to-four-year-old children with disabilities (as with the other families, some of these families will be receiving welfare and some will not).

II. SAMPLING PLAN

As part of the start-up phase of our project, we developed working relationships with community agencies and representatives in each of the three cities. These consultants are assisting us in recruiting participants for our study.

To achieve the major objectives of this research, a purposive sampling plan is being used in the ethnography. The plan will be executed in several stages. First, in order to inform more systematically other components of the overall project, the ethnography requires samples that are similar in character and drawn from the same geographic areas as the survey and Embedded Developmental Study samples. As such, our ethnographic families will be recruited from several of the same neighborhoods (technically, block groups) as the survey and embedded study respondents.

In the early spring of 1998, Research Triangle Institute Inc. (RTI), our survey contractor, selected block groups at random in each city for inclusion in the sampling frame for the survey. We are choosing two of these block groups for each race-ethnic group as sites from which to recruit families for the ethnography (with the exception of non-Hispanic whites in Chicago and San Antonio, which is discussed below). For example, we are choosing two block groups for African Americans in Boston, two others for Puerto Ricans in Boston, etc. We may at times augment a block group with geographically contiguous areas to form what residents and community advisors think of as a “neighborhood.”

When selecting the two block groups for a particular ethnic group in each city, we are attempting to maximize the differences between them in terms of neighborhood resources. Using neighborhood profiles provided by RTI (such as poverty rate in the block group) and existing assessments of neighborhood social service resources in each city, we are attempting to identify two types of neighborhoods—high-risk and low-to-moderate-risk—as settings for recruiting our ethnically diverse ethnographic samples.

We use three variables to define the level of risk in a neighborhood: (1) the poverty rate, (2) the employment rate, and (3) the density, stability, and integration of formal social service resources and organizations. Our goal in studying families within these neighborhoods is to provide a more in-depth exploration than the survey and Embedded Developmental Study can provide of such issues as the effects of neighborhood context on the employment experiences and parenting practices of adults, and the relative impact of these factors on developmental outcomes for children. Our selection of ethnographic sampling neighborhoods is being done in consultation with RTI, the survey and EDS teams, and our community consultants. We will not choose the block groups randomly from RTI's list because some block groups may be fine for yielding a small number of survey interviews but might not be good sites to explore ethnographically as neighborhoods. For example, in order to study a neighborhood successfully, ethnographers need some entrée through local groups or community leaders; this condition may not exist in all block groups. We expect to choose block groups that have the highest probability of yielding samples with the required race-ethnic, structural, and welfare-status characteristics. For non-Hispanic whites in Chicago and San Antonio, our preliminary studies have convinced us that we cannot find racially homogeneous poor white neighborhoods in these cities. Sampson, Raudenbush, and Earls recently conducted a very large household-based survey of poor neighborhoods in Chicago, noting that "there are no low-SES white neighborhoods" in the city (Sampson, Raudenbush, and Earls, 1997). Therefore, in each of these two cities we propose to study 10 white families, about half of whom are receiving TANF, who live in racially and ethnically heterogeneous, low-to-moderate income neighborhoods. (Even though it is prohibitively expensive to sample a representative group of whites in San Antonio for the main survey, we still think it is worthwhile to include them in the ethnographic component.)

The second stage of our sampling process addresses the circumstances that ethnographers require to approach and recruit families effectively, in ways that help them to become comfortable with the ongoing presence of an ethnographer in their lives. Respondents are often more comfortable if they are recruited directly through agencies or informal organizations with which they are involved. This stage of our sampling plan, therefore, requires strategic involvement with community service agencies in our designated neighborhoods. Once we have decided on the sampling neighborhoods, the senior ethnographers in each city will undertake a rough survey of child-focused community services available to families with at least one child who is two-to-four years of age. These services include churches, childcare or nursery programs, child-focused health care facilities and clinics, early childhood intervention programs, parks and recreation programs, and the Women, Infants and Children nutrition program (WIC). The community service surveys will focus on identifying the race-ethnic, family structure, and socioeconomic characteristics of the services' client population.

Using these data, we will select comparable agencies (such as clinics) across sites for recruiting our ethnically, structurally, and socio-economically diverse ethnographic samples. Our goal is to identify agencies that can provide us with the greatest access to welfare-dependent and non-welfare-dependent populations that meet our sample criteria. The senior ethnographers in collaboration with community consultants will negotiate sample recruitment through the designated agencies.

After the agencies have been selected, ethnographers will approach each agency seeking introductions to one or more families who meet the sample criteria. Under these circumstances, families will know that an institution with which they are already acquainted has participated in bringing them together with the ethnographer. In order to assure variations among our families, no more than two from each racial-ethnic group will be selected through any one agency. Furthermore, the ethnographers will ensure that the two

families selected have no close relationships with each other. This selection procedure will allow us to select a range of families and avoid choosing those in one small network or using one particular service. It will also allow us to approach families through introduction by a known helping organization.

Nevertheless, we are aware that many neighborhoods include families that are relatively isolated and who avoid or do not avail themselves of neighborhood services. Similarly, we are aware that some families take their children out of the neighborhood for services. It is important, therefore, that we include in the ethnography families who are not affiliated in any way with mainstream institutions that provide services to children and their parents. Therefore, we will work with our community consultants to gain introductions to other households that do not appear to have affiliations with neighborhood service providers. Once we make contact with these families we will ask them to introduce us to others who are also not associated with support agencies or providers (taking what we call a “leap frog approach”). In this way, we are attempting to ensure that we draw a mix of families who have varying associations with social service agencies and who do not all share the same social networks.

SAMPLE RETENTION

A major concern of any longitudinal study is respondent attrition. Respondents may leave the study for a variety of reasons—they may grow uninterested, move, or have a family member become ill or die. We will institute strategies to help keep respondents involved. One incentive is that the target family will receive yearly compensation for its participation in the study. We have budgeted \$25 per visit, for up to 10 visits a year for each family, in addition to non-cash gifts totaling \$250 per family. (The type of non-cash compensation, such as food coupons, that families receive will be determined based on what we learn from our field experiences.) Thus, we have allotted \$500 per family in years 1 and 2 of the study. In years 3 and 4, when we will be contacting the families just once every six months, we have budgeted \$100 per family.

DESCRIPTION OF THE ETHNOGRAPHIC TEAM

Two study principal investigators, Linda Burton and William Julius Wilson, are supervising the ethnographic component of the overall study. Burton is coordinating all elements of the ethnography; Wilson is concentrating on the neighborhood aspect and assisting Burton. Four senior ethnographers directly manage ethnographic activities. They select and train ethnographers, administer local budgets, and oversee specific topics of the study. Connie Williams is the senior ethnographer in Boston; Monica McManus is in Chicago; Laura Lein is in San Antonio; and Debra Skinner is senior ethnographer for the disability component. One or two research scientists assist each principal investigator and senior ethnographer. Alan Benjamin is at Penn State working with Burton to coordinate for the ethnographic component. Jim Quane and Gwendolyn Dordick are working with Wilson. The other research scientists are Judy Francis in Boston; Kevin Roy in Chicago; and Jane Henrici in San Antonio. William Lachicotte is working with Debra Skinner on the disability component.

These experienced ethnographers are assisted further by a variety of additional researchers. Graduate students in a variety of disciplines will conduct a large part of the participant observation and coding of data. Two postdoctoral students will contribute to research activities, in addition to enriching the project with their own perspectives. Two developmental psychologists—Betsy Manlove and Monica Rodriguez at Penn State—are attached to the ethnographic component to provide us with guidance on child de-

velopment issues. Geographer Stephen Matthews is organizing the geographic information analysis. In addition, a computer programmer, Don Gensimore at the Population Research Institute, is advising the project about all aspects of our information systems. Each ethnographic team also includes a research scientist who speaks fluent Spanish.

Each ethnographer is specializing in one or two of the three domains of the ethnographic study: family, disability, and neighborhood. Each city will have two to three disability ethnographers, two neighborhood ethnographers, and approximately eight to 12 family ethnographers. Although Skinner and Lachicotte are located in Chapel Hill, North Carolina, they are managing the disability ethnographers in coordination with the senior ethnographers in each city. Wilson's team will consult with the neighborhood ethnographers at each site.

Each family ethnographer (including the senior ethnographers) will follow approximately five families over years 1 and 2 of the study. These families include the parent, the target child aged two to four, the primary care provider for this child, his or her siblings, and the parents' social networks. Our goal is to match the ethnographers and families racially. Disability ethnographers will operate in a fashion similar to that of the family ethnographers, but with their assigned population. They will receive special training in working with and evaluating the lifeways of people who are disabled.

The neighborhood-level ethnographers will monitor the resources available to the participating families through public and private agencies, civic groups, and religious organizations. They also will monitor the ways in which the local welfare offices interpret and carry out welfare reform. Their input will help us understand the social context that the families in the study face.

Linda Burton, William Julius Wilson, the senior ethnographers, the research scientists, Manlove, Rodriguez, and Gensimore are in frequent contact with each other via E-mail and telephone and have weekly conference calls to assess the overall progress of the ethnographic study (including issues such as field management, data analysis, etc.).

DATA COLLECTION, TRAINING, AND REPORT-BUILDING STRATEGIES

We are calling this a "modified" ethnography because it differs in some ways from more traditional templates for ethnographic research. We have seen above that our sampling process is more structured than is the case in many ethnographies. In addition, the multi-site, multidisciplinary character of this ethnography leads us to other innovations. One innovation is to build into the core of our ethnographic design a fundamental emphasis on continuing, multidirectional, and multilayered communication. Many of the specifics of that communication strategy are described below.

This design is significant because it enables us continually to strive for focus and comparability across what, necessarily, is a highly idiosyncratic research method (i.e., ethnography). It facilitates ongoing responses to emergent themes and continuing evaluation of our methods. We provide feedback in many directions—for example, from coders to ethnographers, as well as from ethnographers to coders and from ethnographers to the principal investigators of the overall study, as well as from principal investigators to the lead ethnographers.

In addition, our ethnographic vision will result in a highly collaborative analysis—unusual in ethnography and unprecedented in its scope. Coders will talk across coding domains, coders and ethnographers working on the same site will communicate, and coders will communicate with ethnographers from different sites. At each step of analysis, each member of our team will be checking impressions and understandings with other members of the team. This fundamentally affects the reliability of our modified ethnography in comparison with other ethnographies; our goal is that it—along with the survey and EDS—will result in reports that can make unusually strong claims of validity

Ethnographers will combine their training with the process of building rapport with respondents and data collection. In a simple sense, the ethnographic method focuses on “getting to know” people. Thus, as our graduate student ethnographers learn to interview, to observe, and to write notes, they also will be getting to know the families that they are studying.

The senior ethnographers and research scientists will be training the graduate student ethnographers by supervising their data collection. At first, differing types of data will be collected using a variety of techniques. Ethnographers will tape-record and transcribe one interview, will practice writing fieldnotes, and will collect basic demographic information. Obtaining basic demographic information, in addition to providing the means by which to ensure comparability with the other two Three-City Study components, will aid ethnographers in becoming familiar with the social “terrain” of the families they are studying. The variety of early contacts will facilitate rapport building, a key factor in obtaining rich data. Ethnographers will pursue interactions that fit with families’ schedules, interests, and activities. The data collection or topical areas that are addressed will be presented to the site supervisors for comment. Ethnographers will receive intensive, direct feedback on early data collection that will facilitate and improve later data collection.

Several data collection strategies will be employed in the ethnography. As training and rapport building progress, ethnographers will conduct targeted, taped, topical, semi-structured interviews. These interviews will focus on a particular domain, such as childcare. They will be conducted with parents and, where appropriate, with other target family members. The interviews will focus on specific topics that the survey and EDS touch on but, because of design constraints, do not explore in great detail (such as the meaning of time, family routines, cultural understandings and the contexts within which varying developmental outcomes occur for children, and on how families generate income and consume goods). Other topics will overlap significantly with those the survey and the EDS address, but will explore them in different ways, with a focus on context and association. The ethnography also will provide more detailed information about issues the survey and Embedded Developmental Study cannot address fully since it provides the ethnographers with the opportunity to establish increasing rapport with their families and for the families to become comfortable with the ethnographers.

A primary data collection strategy used in the ethnography will be participant observation with the target families, their children, and their social networks. This technique requires long-term and intensive participation in a social setting as a means of acquiring information not obtainable through survey techniques, or prerequisite to their use (Agar, 1980; Jorgensen, 1989). It also may enrich the study with unexpected information. Participant observation is effective in producing “surprises,” particularly those to do with the understanding of culture. Participant observation is particularly important for a study of welfare reform, families, and child well-being, where quantitative information may not be sufficient to understand fully the meaning of such issues as the transition from welfare to work, time, parenting, and developmental outcomes in the day-today lives of families and children. This technique will provide

keen insights into multilevel mechanisms and processes that affect change and continuity in our families' lives. It also will provide behavioral data to complement the normative information acquired through the survey, and will generate information that is essential to the creation of culturally sensitive survey instruments for use in subsequent waves of data collection in the survey and embedded study components of this project.

Just as important, participant observation may be necessary in order to determine how welfare recipients are experiencing the various rules and restrictions of welfare reform. We expect that there will be substantial variation in how local welfare offices and other social service institutions implement the revised state welfare laws. As much as possible, we intend to accompany our families to the welfare office, to private agencies that are assisting them, to job placements, and so forth. We also will observe carefully the economic strategies families use when they are cut off the rolls because of rule violations or time limits or when they leave welfare voluntarily. Within each of our sites the participant observation settings also will include the home and neighborhoods of families and childcare environments. To the degree possible, observations will be conducted in the places where children and their parents spend time outside the household (such as eating out, grocery shopping, going to school, visiting with friends, going to community events).

These data collection strategies will be employed in three distinct stages of research activity that produce distinctive types of data. Each stage is linked to the survey and Embedded Developmental Study component of the project. The stages are as follows:

Stage I Field Readiness: In preparation for our comparative ethnographic study we have engaged in a number of “field readiness” strategies to ensure comparability of such elements as samples, neighborhoods, and data collection strategies across sites, and to facilitate smooth transitions for the ethnographers into their respective neighborhoods and families. We conducted a series of longitudinal focus groups in each site with African-American, non-Hispanic white, Mexican-American, and Puerto Rican welfare-dependent and non-dependent women and their male partners. Preliminary analysis of this focus group data provided insights into possible methodological approaches that are relevant for our study populations, as well as issues that are “contextually relevant” for us to explore, such as the temporal organization of family lives, neighborhood influences on child development, and child health.

Other activities comprising this stage included the selection of neighborhoods, interviews with influential people in the community and with welfare caseworkers, and the development of ethnographic training protocols. Focus groups, these “key informant” interviews, and recruitment events have already contributed data that have provided a useful sense of the background within which the more extensive research of Stage II will take place.

Our ethnographers were trained at a workshop held at The Pennsylvania State University in October 1999. At that meeting, presentations and small-group discussions were held to convey to the research team a picture of the overall study, to develop shared understandings within the research team that will facilitate future collaboration, and to deepen communication channels.

Each ethnographer is being provided with a variety of documentary support that further explains the research themes and methods. These include a “structured discovery” document that sets forth the primary and secondary goals of the ethnography and a “fieldnote procedures” document that explains basic pro-

protocols to follow in writing and coding fieldnotes. The “buckets” or core coding categories included in the latter document represent the core theoretical questions being addressed in the ethnography, and will remind ethnographers of the major themes to be explored through participant observation and interviews.

A set of over 30 “interview guides” provides an additional documentary resource for fieldworkers. Each one explores the theory behind the core coding categories, suggests a variety of ways by which ethnographers may operationalize the theoretical constructs, and suggests potential questions to ask and observations that would be relevant to the theoretical constructs. Our notion is that ethnographers will read these interview guides at home; they are a resource for training ethnographers in what to look for when in the field. Finally, since the interview guides are too unwieldy to take to the field, we have produced a “cribs and grids” document. This last document does not include specific questions in order to preclude the temptation to use it like a questionnaire. Instead, it briefly lists the interview guides’ theoretical constructs (the “crib” sheets) and attaches associated checklists (the “grids”).

Using the same training and fieldwork procedures helps ensure credible and thorough descriptions at each site, and that data are comparable and can be used in cross-site analysis.

Stage II Entry and Ethnographic Data Collection (Years 1 and 2): During this phase, which is the core research period, the ethnographers enter their neighborhoods and begin work with their families. The ethnographers will meet at least once a month with the primary caregiver. Much of the time ethnographers spend in the home will be in the form of participant observation, but occasionally they will conduct semi-structured interviews. These interviews will enable researchers to probe further the topics and themes at the core of the integrated study in greater detail and specificity than informal participant observation allows. Often, such interviews will be tape-recorded.

In tandem with the topics that the survey and embedded study examine, the semi-structured interviews will be based on the interview guides, and they will explore a range of issues. These include the meaning of welfare and understanding of policy changes, perceptions of work and education, health and the use of social services, life histories and personal aspirations, and childcare arrangements. They also look at family routines and household labor; how family finances are arranged; intimate relationships; parenting; cultural and contextual meanings of child outcomes; “adultification” (instances in which a child takes on tasks appropriate for someone older); adolescent experiences; neighborhood resources; and social networks. As the ethnographers move toward completion of the modules, they will increasingly engage in participant observation activities with the families and their children, as well as conduct informal interviews with members of the parents’ social networks. Observations will be directed to the phenomena and relationships posited by the research questions, and will be recorded in extensive fieldnotes prepared for a selected qualitative data management program.

Of all the stages of our research design, this phase is the most serendipitous, and the activities are the most intense. The goal of this stage is to gather data that will generate additional hypotheses, facilitate the refinement of coding schemes, and guide more focused field observations later in this research stages. These data will also be instrumental in further developing constructs, measures, and data collection strategies for the embedded study and survey components of our project. During this phase and also in Stage III, we expect to take findings from the survey and embedded study and investigate them further using in-depth interviews and participant observation. In part, we will seek to make clear the larger

context of the findings, and the meanings and perceptions that family members attach to them. We also will investigate unexpected or puzzling findings from the quantitative survey and EDS analyses to see whether we can help determine whether they are valid and what they mean. And we expect to be able to suggest new questions for the survey that can determine whether the findings from the families we have studied ethnographically can be confirmed in large, representative samples.

Stage III Focused/Longitudinal/Follow-Up Observation (Years 3 and 4): During this phase, research activities become more focused as the research design meshes with the discoveries and insights developed in Stage II and with the findings from the survey and EDS. Our activities will become more specialized, and we will record information in a more centered and systematic fashion. We will collect data to evaluate emergent hypotheses. These years will enable significant longitudinal and comparative data collection, as well. We will study people over a longer period of time, increasing the likelihood of observing a wider variety of significant changes in behaviors and outcomes. It will provide further opportunity to monitor changes in social conditions and in families' behaviors with changes in welfare regulations and implementation. Moreover, it will enable us to assess with greater confidence the long-lasting effects of welfare reform.

The linkages between all components of the proposed projects also become more explicit at this stage. The ethnographic, Embedded Developmental Study, and survey components coalesce to help us understand better the relationships between the processes uncovered by the ethnographic assessments of the day-to-day lives of families and children, and the child and family outcomes identified in the EDS and survey.

Based on the variable relationships we uncover during years 1 and 2 of the study, we will develop follow-up areas to explore with the families in our study at six-month intervals during the last two years of the project. (It is important to note that we will continue to maintain contact with the families via telephone and/or occasional personal visits during intervals between the semiannual visits.) This follow-up research will be designed to make in-depth assessments of "developmental" changes the families and children experience in critical areas, such as parents' adjustment to work, that are identified in the survey, EDS, and Stage II of ethnographic data collection and analyses.

During the final months of this phase of the study, the ethnographic teams will begin their exit from the field. Field exiting strategies will be determined in consultation with the families and community consultants.

DATA MANAGEMENT

The multi-site ethnography employs a rigorous research design that incorporates multiple sources of data, multiple methods of analyses, multiple sites, and multiple investigators. While this design greatly enhances the reliability, validity, and completeness of our data, it also will generate a relatively extensive qualitative data set that requires a highly organized, consistent data management system both within and across research sites.

Drawing on existing protocols for data management (Miles and Huberman, 1994; Levine, 1985) and Linda Burton's extensive experience with managing large longitudinal ethnographic data sets, our team will implement a well-designed data management system that ensures quality control in data collection.

It will facilitate easy storage and retrieval, and analysis of the data within and across sites. Extensive and regular fieldnotes will be tagged into basic categories (called “buckets” or “nodes”) and prepared in a format that can be used by the qualitative data management program, NUD*IST4 (Non-numerical, Unstructured Data: Indexing, Searching, and Theorizing). These data will be encrypted and sent by file transfer protocol to The Pennsylvania State University. At Penn State, a team of coders will read every file that arrives, and build organizational systems to classify the material further. The categories that are built are based on patterns observed in the data. These systems—which NUD*IST4 is designed to enable—allow for focused access to notes by topic, and will help channel analysis (Buston, 1997). All other materials (such as maps, photographs, and audiotapes) produced through the ethnography will be sent securely to Penn State as well, and Penn State will become an archive for all the ethnographic data, allowing appropriate access to project researchers.

Our highest priority is to generate comparable, high quality data within and across sites. To achieve this goal, project ethnographers have received the same training in data collection in the modified ethnographic method we use in this study. Experienced ethnographers provided this training at a three-day workshop with the multi-site ethnography teams, and continue to communicate with team members throughout the study. In addition, to ensure comparable quality in data collection, the experienced ethnographers are on site with graduate student ethnographers, supervising their work through regular team and individual meetings to discuss and evaluate data collection practices. We will augment these efforts in several ways. Special coordinators will monitor the ethnographers specializing in families with a child who is disabled or the ethnographers focusing on neighborhoods as the unit of analysis. These coordinators will work across sites, assisting in training and continuing to read fieldnotes. Our geographic information analysis team and specialists in child development also will consult and coordinate across sites. We are also compiling a list of internal consultants who have professional “helping” experience. They will include people who can advise our ethnographers when difficult situations arise, such as those involving mental illness, family processes, or domestic violence. The internal consultants will be available to any ethnographer who wishes to contact them. A subgroup of these internal consultants will comprise a standing, cross-site “reporting and safety” committee. The purpose of the committee will be to assess potentially dangerous situations and determine whether and to whom to report them (for example, if a researcher suspects a case of child abuse).

Finally, we have established a secure web site and a variety of specialized listservs through Penn State for the entire ethnographic team to receive project and procedural updates, bibliographic information, relevant Internet links, and to facilitate general communication among team members.

DATA ANALYSIS

Data analysis for the ethnographic component of this study will be handled according to a common project protocol developed by the senior ethnographers. Given that our data analysis protocol is necessarily complicated, we provide here a general overview of our strategies.

As noted above, our proposed research will produce multiple forms of quantitative and qualitative data appropriate for addressing specific research questions both within and across our research sites in Boston, Chicago, and San Antonio. It also can generate hypotheses to examine in the focused observation stage of the ethnography and in the Embedded Developmental Study and survey. Thus, we plan to use a number of strategies for analyzing our data. For example, to address research questions that involve our

structured measures (for example, the dimensions of parenting styles among working poor and welfare dependent primary caregivers), we will use the appropriate quantitative analytic procedures such as factor analysis. Our quantitative analyses typically will involve cross-site data, and thus will be conducted at our central ethnography office at Penn State.

Using the grounded theory approach, our qualitative data will be analyzed at multiple levels in order to explore both the experiences of individual families and the similarities and differences between families of different ethnicities and those living in different cities. Our analyses will include case-study, cross-case, and cross-site approaches. Analyses will be conducted (1) within family; (2) within race-ethnic group but across families; (3) within city but across families; and (4) across race-ethnic groups, cities, and families. Analysis of data at the within-case level permits a detailed understanding of the experiences of a particular group, whether it be one family, several families within a particular ethnic group, or several families within a city. Analysis at the cross-case and cross-site level allows for the comparison of experiences across families, ethnic groups, and geographic locations.

Analysis protocols for our qualitative data, such as fieldnotes and memos, require that we conduct within-case and cross-case analyses within each site. The purpose of employing within-case and cross-case analyses in each site is to thoroughly familiarize each ethnographer with his or her data and to identify patterns unique to each case and site before trying to compare patterns across sites. In this type of analysis, we examine data for recurrent themes or concepts. Initial analysis of our qualitative data will involve the generation of graphic displays of the case-study data within each site.

Cross-case analysis within each site will help us to extend the generalizability of findings within cases. If we find similarities in processes or behaviors across cases, stronger support exists for the findings of the within-case analysis. In conducting analysis across cases within sites, we will use the interactive synthesis approach, which involves a combination of variable-oriented and case-oriented perspectives (Huberman and Miles; Fischer and Wertz, 1975). With the variable-centered approach, we examine the relationship between variables, identifying patterns and discovering the correlations between concepts, but without information about individual cases. Conversely, the case-oriented approach allows for an examination of the full history of a case, with pertinent information coded and sorted. We can examine data from several cases for recurrent patterns, with the goal of identifying “clusters” of cases that have similar characteristics.

Generally speaking, in using the interactive synthesis approach our ethnographic teams will write individual case summaries of their families. We will then write cross-case narratives based on themes from individual cases. The cross-case narratives are then narrowed to include “essential personal meanings,” which are then compared with individual cases. Based on the fit between the meanings and individual cases, researchers compose a structure that describes the essential process of an experience or phenomenon, such as the meaning of time.

Building on our within-site analyses, our cross-site analyses of qualitative data will take the variable-oriented approach. In collaboration with all our ethnographic teams, cross-site analysis will be centralized at Penn State.

Ethnographic team members will conduct further analysis concurrently and in collaboration with the Penn State team. These analyses will be on many levels and of many types. They will include analyses of families with disabilities, of neighborhoods, and the contributions of geographic information analysis.

Experienced ethnographers will work on high-quality scholarly publications and will collaborate with researchers from the survey and the EDS on integrative reports. Ethnographers will produce theses and dissertations. Other specialized interests will be addressed, as well.

In *Ethnography Unbound* (1991), sociologist Michael Burawoy describes ethnographic research as a collaborative endeavor between observers (researchers) and participants. Anthropologist Carol Stack notes that multi-site ethnographic research is not only a collaboration between observers and participants but also between the ethnographic teams across sites. We want to underscore the point that all aspects of our multi-site ethnography, particularly our data analysis, are part of a collaborative effort. The senior ethnographers have put particular mechanisms in place to ensure that our teams have close working relationships and are in constant contact with each other during the ongoing data collection and analysis phases of our project.

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ENDNOTE

- ¹ A few children will start this study as young as one year old when their mothers will, in some cases, lose their exemption from the requirement to work.

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QUALITATIVE VERSUS QUANTITATIVE: WHAT MIGHT THIS DISTINCTION MEAN?

David Collier, Jason Seawright, and Henry E. Brady

The founding in 2003 of the APSA Organized Section for Qualitative Methods provides a fitting occasion to reflect on this branch of methodology.¹ Given that the other APSA organized section concerned with methodology² is centrally focused on quantitative methods, the additional issue arises of the relation between the qualitative and quantitative traditions. Adopting a pragmatic approach to choices about concepts (Collier and Adcock 1999), we believe that the task here is not to seek the “true” meaning of the qualitative-quantitative distinction. Rather, the challenge is to use this distinction to focus on similarities and contrasts in research practices that provide insights into *how* to do good research, and into different *ways* of doing good research. We have found it useful to think about the qualitative-quantitative distinction in four ways (see Appendix), focusing on the level of measurement, size of the N, use of statistical tests, and thick versus thin analysis. Each of these approaches is associated with distinctive forms of analytic leverage.

FOUR APPROACHES TO THE QUALITATIVE-QUANTITATIVE DISTINCTION

The first approach concerns the level of measurement.³ Here one finds ambiguity regarding the cut-point between qualitative and quantitative, and also contrasting views of the leverage achieved by different levels of measurement. Some scholars label data as qualitative if it is organized at a nominal level of measurement and as quantitative if it is organized at an ordinal, interval, ratio, or other “higher” level of measurement (Vogt 1999: 230). Alternatively, scholars sometimes place the qualitative-quantitative threshold between ordinal and interval data (Porkess 1991: 179). This latter cut-point is certainly congruent with the intuition of many qualitative researchers that ordinal reasoning is central to their enterprise (Mahoney 1999: 1160- 64). With either cut-point, however, quantitative research is routinely associated with higher levels of measurement.

Higher levels of measurement are frequently viewed as yielding more analytic leverage because they provide more fine-grained descriptive differentiation among cases. However, these higher levels of measurement depend on complex assumptions about logical relationships — for example, about order, units of measurement, and zero points — that are sometimes hard to meet. If these assumptions are not met, this fine-grained differentiation can be illusory, and qualitative categorization based on close knowledge of cases and context may yield far more analytic leverage.

The second approach focuses on the N, i.e., the number of observations regarding the main outcome or phenomenon of concern to the researcher. A paired comparison of Japan and Sweden, or an analysis of six military coups, would routinely be identified with the qualitative tradition. By contrast, an N involving hundreds or thousands of observations clearly falls within the quantitative approach. Although there is no well-established cut-point between qualitative and quantitative in terms of the N, such a cut-point might plausibly be located somewhere between 10 and 20 cases. Differences in the size of the N, in turn, are directly linked to the alternative sources of leverage associated with the third and fourth approaches.

The third approach to the qualitative-quantitative distinction concerns statistical tests. An analysis is routinely considered quantitative if it employs statistical tests in reaching its descriptive and explanatory conclusions. By contrast, qualitative research does not explicitly or directly employ such tests. Although

the use of statistical tests is generally identified with higher levels of measurement, the two do not necessarily go together. Thus, quantitative researchers frequently apply statistical tests to nominal variables. In addition, qualitative researchers often analyze data at higher levels of measurement without utilizing statistical tests. For example, in the area studies tradition, a qualitative country study may make extensive reference to ratio-level economic data.

Statistical tests are a powerful analytic tool that can evaluate the strength of relationships and important aspects of the uncertainty of findings in a way that is more difficult in qualitative research. Yet, as with higher levels of measurement, statistical tests are only meaningful if complex underlying assumptions are met. If the assumptions are not met, alternative sources of analytic leverage employed by qualitative researchers may in fact be more powerful.

Fourth, we distinguish between thick and thin analysis.⁴ Qualitative research routinely utilizes thick analysis, in the sense that researchers place great reliance on a detailed knowledge of cases. Indeed, some scholars consider thick analysis the single most important tool of the qualitative tradition. One type of thick analysis is what Geertz (1973) calls “thick description,” i.e., interpretive work that focuses on the meaning of human behavior to the actors involved. In addition to thick description, many forms of detailed knowledge, if utilized effectively, can greatly strengthen description and causal assessment. By contrast, quantitative researchers routinely rely on thin analysis, in that their knowledge of each case is typically far less complete. However, to the extent that this thin analysis permits them to focus on a much larger N, they benefit from an alternative form of analytic leverage.

SPECIALIZING VERSUS BRIDGING

Much valuable research fits squarely within either the qualitative or quantitative tradition, reflecting a specialization in one tradition or the other. At the same time, other scholars fruitfully bridge these traditions. Research specializing in one tradition or the other is easy to identify. On the qualitative side, such research places central reliance on nominal categories, focuses on relatively few observations, makes little or no use of statistical tests, and places substantial reliance on thick analysis. On the quantitative side, such research is based primarily on interval-level or ratio-level measures, a large N, statistical tests, and a predominant use of thin analysis. Both types of study are common, and both represent a coherent mode of research. Correspondingly, it makes sense, for many purposes, to maintain the overall qualitative-quantitative distinction.

Not only substantive studies, but also research on methodology in its own right, likewise often fits clearly in one tradition or the other. From the standpoint of the new APSA Qualitative Methods Section, it is particularly relevant that one can identify coherent traditions of research on qualitative methods.⁵ For example, work influenced by Giovanni Sartori (1970, 1984) remains a strong intellectual current in political science.⁶ This research places central emphasis on nominal categorization and offers systematic procedures for adjusting concepts as they are adapted to different historical and analytic contexts. Constructivist methods for learning about the constitution of meaning and of concepts now play a major role in the field of international relations (Wendt 1999; Finnemore and Sikkink 2001). In comparative politics, Schaffer’s (1998) book on *Democracy in Translation* is an exemplar of the closely related interpretive tradition of research, and interpretive work is also a well-defined methodological alternative in public policy analysis focused centrally on the United States (e.g., Yanow 2000, 2003). These various lines of research explore the contribution of thick analysis; the idea that adequate description is some-

times a daunting task that merits sustained attention in its own right; and the possibility that the relation between description and explanation may potentially need to be reconceptualized. The strong commitment to continuing these lines of careful work on description, concepts, categories, and interpretation is a foundation of qualitative methods.

At the same time, an adequate discussion of the relation between qualitative and quantitative methods requires careful consideration not only of these polar types, but also of the intermediate alternatives based on bridging. For example, strong leverage may be gained by employing both thick analysis and statistical tests. This kind of “nested analysis”⁷ combines some of the characteristic strengths of both traditions.

An interesting example of bridging is found in new research — partially methodological, partially substantive — on necessary and sufficient causes. With this type of causation, both the explanation and the outcome to be explained are usually framed in terms of nominal variables. Quantitative researchers have often been skeptical about focusing on necessary and sufficient causes, believing that interval-level or ratio-level variables and a probabilistic (as opposed to deterministic) framework are much better suited to causal assessment. However, Goertz (2003) has demonstrated that hypotheses about necessary causation are far more pervasive in social science literature than had previously been recognized, arguing that they require more serious scholarly attention. Relatedly, Braumoeller and Goertz (2000: 846–47) have suggested that if these hypotheses are treated within a standard regression framework, incorrect estimates of causal effects will result. This new work thus offers an important argument for employing a nominal level of measurement.

Whereas this research on necessary and sufficient causes reinforces the idea of using nominal variables, methodological work on this topic by these and other scholars provides an important example of bridging. Specifically, this work has made extensive use of statistical and mathematical reasoning to address the complex question of how to select appropriate cases for testing hypotheses about necessary causation (Ragin 2001; Seawright 2002a, b; Braumoeller and Goertz 2002; Clarke 2002; Goertz and Starr 2003). Thus, statistical tools can help refine qualitative methods.

Other areas of bridging include research based on a larger N, but that in other respects is qualitative; as well as research based on a relatively small N, but that in other respects is quantitative. For example, some non-statistical work in the qualitative comparative-historical tradition employs a relatively large N: Rueschemeyer, Stephens, and Stephens (1992; N=36), Tilly (1994; hundreds of cases), R. Collier (1999; N=27), and Wickham-Crowley (1992; N=26). Comparative-historical analysis has become a well-developed tradition of inquiry,⁸ and the methodological option of qualitative comparison based on a larger N is now institutionalized as a viable alternative for scholars exploring a broad range of substantive questions.

By contrast, some studies that rely on statistical tests employ a smaller N than the comparative-historical studies just noted and introduce a great deal of information about context and cases. Examples are found in quantitative research on U.S. presidential and congressional elections, which routinely employs an N of 11 to 13 (e.g., Lewis-Beck and Rice 1992; J. Campbell 2000; Bartels and Zaller 2001). Other examples are seen in the literature on advanced industrial countries, for example: the study by Hibbs (1987) on the impact of partisan control of government on labor conflict (N=11); and the many articles (see below) that grew out of the research by Lange and Garrett (1985; N=15) on the influence of corporatism

and partisan control on economic growth. This literature on advanced industrial countries has stimulated interesting lines of discussion about the intersection of qualitative and quantitative research. On the qualitative side, Tilly (1984: 79), in his provocative statement on “No Safety in Numbers,” has praised some of this work for taking a major step beyond an earlier phase of what he saw as overly sweeping cross-national comparisons, based on a very large N. In some of this literature on advanced industrial countries he sees instead the emergence of a far more careful, historically grounded analysis of a smaller N — thus in effect combining the virtues of thick analysis and statistical tests. On the quantitative side, the Lange and Garrett article has triggered a long debate on the appropriate statistical tools for dealing with a relatively small N.⁹ Finally, another contribution of Lange and Garrett’s article has been to serve as a model within this literature the practice of including an interaction term in regression analysis, which can overcome a presumed limitation of quantitative research by taking into account contextual effects. In the intervening years, the use of interaction terms in regression has become more common, and Franzese (2003: 21) reports that between 1996 and 2001, such terms have been employed in 25 percent of quantitative articles in major political science journals. In sum, this literature points to diverse avenues for cross-fertilization.

CONCLUSION

We are committed both to specialization and to bridging. With regard to specialization, one of the rationales for forming a Qualitative Methods Section is to provide coherent support for new research on qualitative methods. Such support is needed within political science, and the discipline will benefit from the emergence of a more vigorous research tradition focused on qualitative tools.

At the same time, bridging is valuable. The different components of qualitative and quantitative methods provide distinct forms of analytic leverage, and when they are combined in creative ways, better research can result. Of course, there could be the risk that scholars will strive so hard for methodological eclecticism that the ingenious splicing together of different methodologies might take precedence over methodological coherence and sustained engagement with the subject matter. Both bridging *and* specialization are essential.

APPENDIX: FOUR APPROACHES TO THE QUALITATIVE-QUANTITATIVE DISTINCTION

1. LEVEL OF MEASUREMENT

Cut-point for qualitative vs. quantitative is nominal vs. ordinal scales and above; alternatively, nominal and ordinal scales vs. interval scales and above. Lower levels of measurement require fewer assumptions about underlying logical relationships; higher levels yield sharper differentiation among cases, provided these assumptions are met.

2. SIZE OF THE N

Cut-point between small N vs. large N might be somewhere between 10 and 20; yet this does not consistently differentiate qualitative and quantitative research. A small N and a large N are commonly associated with contrasting sources of analytic leverage, which correspond to the third and fourth criteria below.

3. STATISTICAL TESTS

In contrast to much qualitative research, quantitative analysis employs formal tests grounded in statisti-

cal theory. Statistical tests provide explicit, carefully formulated criteria for descriptive and causal analysis; a characteristic strength of quantitative research.

4. Thick vs. Thin Analysis

Central reliance on detailed knowledge of cases vs. more limited knowledge of cases. Detailed knowledge associated with thick analysis is likewise a major source of leverage in research; a characteristic strength of qualitative analysis.

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ENDNOTES

- ¹ We draw here on Chapter 13 in Henry E. Brady and David Collier, eds., *Rethinking Social Inquiry: Diverse Tools, Shared Standards* (Roman & Littlefield and Berkeley Public Policy Press, 2003, forthcoming).
- ² The APSA Organized Section for Political Methodology was officially constituted in 1986.
- ³ The four traditional levels of measurement (nominal, ordinal, interval, and ratio) suffice for present purposes; we recognize that far more complex categorizations are available.
- ⁴ This distinction draws on Coppedge's (1999) discussion of thick versus thin concepts; it is also closely related to Ragin's (1987) discussion of case-oriented versus variable-oriented research.
- ⁵ Well-developed traditions of research on methods are of course found within the quantitative tradition as well.
- ⁶ For example, Levitsky (1998); Gerring (1999, 2001); Kurtz (2000). For a related line of analysis, see Johnson (2002, 2003).
- ⁷ This term is adapted from Coppedge's (2001) "nested induction" and from Lieberman's "nested analysis" (2003).
- ⁸ For a new synthesis and assessment of comparative-historical research, see Mahoney and Rueschmeyer (2003).
- ⁹ Among many articles in this debate, see Jackman (1987), Alvarez, Garrett, and Lange (1991), Beck et al. (1993) Beck and Katz (1995), Kittel (1998), and Beck (2001).

SUGGESTIONS FOR NSF

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In the face of postmodern developments in ethnography during the past two decades, there was widespread talk of this method retreating from the highest standards of social science into nihilism and naval gazing. While some of this no doubt took place, the worries were exaggerated. Departments of sociology today are populated by qualitative researchers who are struggling to make their work live up to the highest ideals of social science. We have not seceded from social science, but have struggled to improve ourselves by making ourselves a part of it.

As so-called qualitative researchers, we walk many lines: bringing theoretical questions to the field versus discovering them while working at the site; political agendas versus naïve *tabula rasa*; fully theorized versus open to issues and empirical events; accumulating many thinner observations versus a few thick ones; using an in depth description to enter into a dialogue with a theory versus telling readers only as much about people and places as they need to know to reconstruct a theory. The exemplars of our discipline, the books that inspire us or our colleagues, embody these and many other practical trade-offs as enduring tensions. The NSF should embrace these dilemmas and discourage reviewers from acting as if there is one best way to respond to them. I am personally inspired by particular works that have made all of these (and many other) choices to differing degrees.

Here are some standards that are among the most important that we have in determining whether qualitative work is living up to the highest ideals of social science: (1) being clear about procedures; (2) being clear about uncertainties; (3) presenting alternative interpretations, rival hypotheses, and evidence which could lead to an alternative interpretation; (4) striving to achieve replicability; (5) achieving conceptual clarity; (6) seeking to be aware of investigator effects; (6) modifying, improving, or developing theory; (7) creating reliable micro level data. Much reasonably convincing work lives up to a few of these ideals, and some of the best work lives up to only some of them. NSF should encourage work to live up to as many of these ideals as possible, but reviewers should be discouraged from being harsh on proposals simply because one of the favorite standards mentioned above (or some other) is not achieved. I would like to see NSF help us get more good qualitative work done by focusing on specific needs:

I wholeheartedly agree with Sudhir Venkatesh that those of us who do intensive fieldwork need time. Create a program that buys off whole semesters for people who commit to being in the field. The only requirement for getting this money should be that there is some track record. It should be a special program for investigators who have a hunch or basic research question that can't necessarily be developed until they have been in the field. This program should recognize that many good books begin with nothing but a basic, unmotivated research question, or some interactions that seem interesting. We have enough good books that have started this way that we shouldn't be afraid to fund more of them. These proposals should not be more than a couple of pages. Likewise, similar funds should exist for graduate students who have demonstrated exceptional training. Of course, NSF could still fund plenty of qualitative research based on more elaborate proposals.

I also agree with Sudhir Venkatesh that we need time to write and organize our thoughts. Create a program to improve the record of scholarly productivity in qualitative work by giving some people a chance to sit in the woods or elsewhere for three months to meditate upon their data and write. These proposals should describe the fieldwork that has been completed and say what an investigator will accomplish. There is no substitute for finding time to organize thoughts and get the material written up. This is especially a problem for people at resource deprived institutions who teach five or more real courses every year, have no research assistants, and no staff to help them with even the most basic things. For every Kathleen Blee who overcomes the odds and produces three brilliant books under such circumstances, there are countless others who give up.

I agree with Jack Katz that we should encourage better data. We spend far too little time thinking about data quality, and this is one way that we are *inferior* to the natural sciences, which spend a vast amount of energy on this topic. Support the creation of new and innovative kinds of data. This should be reliable micro-level data that enables us to talk about things at various the levels at which they occur. This data may or may not be available to our colleagues for analysis, but the ideal is that it would be.

I think that NSF should encourage more comparative ethnography, and a greater dialogue between comparative/historical sociology and ethnographic work. An exemplary recent example is Michele Lamont's *The Dignity of Working Men*. For more methodological exemplars, the ethnographers have a lot to learn from reading the work of their comparative/historical colleagues, (and they could learn a few things from reading our work.) NSF should encourage more ethnographic collaborations between U.S. scholars and scholars who work outside of the U.S. such as David Snow's current comparative homelessness project. NSF should also earmark funds for people who want to add comparative cases to projects that are well under way. There is no logical reason why historical sociology should have developed in a comparative way while ethnographic work did not. The scientific foundation of ethnography will be improved by moving in this direction.

NSF should encourage more revisits to old sights. NSF should specifically encourage scholars to go back and systematically restudy the sites that are the basis of the previous generation's classics. Offer money specifically to sociologists who want to "go back" to do what Burawoy has recently called "focused revisits." As Becker often laments, we do not spend enough time even reading the books produced by a previous generation of fieldworkers, no less restudying them. Who at this conference has recently read (or even read once in full) *Deep South*, *Men Who Manage*, or *French Canada in Transition*, among many others?

One of the things that NSF has been good at has been helping research deal with issues of generalizability and training students in a certain way. The Kennedy School of Ethnography (Wilson and Newman) in which the principal investigator delegates the central part of the research project—participating and observing—to assistants has proven to be an avenue for training large numbers of students who have gone on to write their own books. It also becomes a mechanism for investigators to survey a large sample of respondents and cover more ground than any individual ethnographer can possibly cover. It is somewhere between a survey method and an individual ethnography, as if administering a survey to a large group of people but allowing for open ended questions which traditional surveys are unable to code or cope with. Newman in particular has demonstrated that projects like this can train large numbers of students who go on to do their own fine books. This should not become the norm for NSF, but a few well chosen sites for such funding continue to make sense.

On the issue I was supposed to discuss in this paper, I have just this to say: Some of the best work will always combine quantitative and qualitative data, but (as Howie Becker has long reminded his students) *this must be determined by the research problem itself*. It should not be an ideological commitment. I notice a trend among graduate students who seem to begin with a commitment to combining quantitative and qualitative work. Often they do most of their training in demography or stratification, and then want to add a qualitative element at the end. Much of this work, even when done by senior scholars, turns out to be disappointing because it is hard to be good at any one thing, no less two. So NSF should be wary of such proposals unless the research problem clearly suggests that such work really makes sense.

Like some others in this room, all of my own projects have been turned down for funding by NSF. Like some others in this room, I did them anyway. Those of us who got them done are among the privileged ones. We don't know how many people drop out or abort innovative projects due a lack of funding. I am very grateful to the organizers for inviting us to become part of the process.

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THE WHEN OF THEORY

Gary Alan Fine
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One of the charms of ethnography, part of the influence of field research, is its power as description: “bringing back the goods” on spaces far removed from the places of its audience. Indeed, this spying on otherness was certainly crucial to the justification for the earliest attempts of “anthropology” by missionaries, travelers, and explorers - curiosity matched the urgent desire for political control.

While a cogent descriptive strain continues to exist within field research, as the methodology as been integrated into the heart of social science research, increasing generalizability and theoretical elaboration have displaced elaborate description as the primary goal of the ethnographer.

In my recent essay (Fine 2003), “Towards a Peopled Ethnography: Developing Theory From Group Life,” I made the case that ethnography should properly be grounded in theory. I proposed seven characteristics of what I labeled a peopled ethnography. Specifically I suggested that a peopled ethnography is 1) theoretical, 2) grounded on other ethnographies and research studies, 3) focused on groups in routine interaction, 4) based on multiple research sites, 5) dependent on extensive observation, 6) richly ethnographic and detailed in its representations, and 7) based on a fundamental separation between researcher and researched. Within the divisions of ethnographic research, this approach is set firmly within the “realist tradition” as described by John Van Maanen (1988). While certain elements of confessional ethnography, interpretative ethnographies, and the other approaches that Van Maanen suggests are present, my assumption is that, although all knowledge is constructed in some measure, we can present truth claims that generate sufficient levels of consensus and isomorphism with an obdurate reality as to be useful. We can be cautious naturalists - not forgetting the ways that truth is built, but not ignoring that there can be truth.

For much of this century (see Hallett and Fine 2000) generalizations and theoretical expansion have been part of the mandate of the qualitative sociologist. But what kind of theory? Sociologists have tended to divide the world of theory construction into two hostile camps: inductive theorists and deductive theorists - those who discover theory and those who build theory. In its pure (and exaggerated) form the “inductives” gather data without having a clear or compelling focus. The assumption is that by nestling up to the social scene and the events observed, the savvy scholar will eventually come to recognize a set of regularities that can then be applied to other groups. This approach has been most famously and influentially presented in Barney Glaser and Anselm Strauss’s (1967) *The Discovery of Grounded Theory*. During the period during which data is collected and subsequently analyzed, researchers apply the “constant comparative method” and “theoretical sampling” to determine if the inductive claims from one domain (and at one time) have application elsewhere.

In contrast, others, relying on deductive theory development strategies from experimentation and survey research (at least how these methodologies are formally taught), take a distinctly different approach. This model suggests that the researcher will have a clear idea of the topics and even potential findings of the research prior to entering a field site. The data support or disconfirm the “hypotheses” that researchers have proposed, and that this judgment depends on formal assessments.

The conflict essentially is whether “theory” comes before or after one’s fieldwork. However, the way in which the dichotomy has been proposed is fundamentally misleading. It is not possible to separate deduction and induction in the way that has been suggested, particularly as regard to field research.

Part of the problem is to unpack the concept of the hypothesis. A social scientific hypothesis is nothing more than a verbalized expectation that a researcher believes can be backed by empirical evidence and presented rhetorically to persuade an audience of peers. A hypothesis is not a magical scientific assertion that results from a logical progression of ideas. From where do hypotheses derive? The answer on some basic level is that they derive from lived experience. Put another way, the ideas that become established as hypotheses are plausible because they accord with the life of the researcher, and, as a result, hypotheses are formed through induction, and only then becoming used for deductive purposes.

The situation is equally complex as regards to induction. The ideal of induction is to enter a field site as a stranger without preconceived ideas - the sociologist as Martian. Yet, again as a function of the lived experience of the researcher, each site has a set of expectations associated with its public image. Not only are sites not selected randomly, but experience of the site is connected to the stereotypic imaginings of the researcher. In this sense the model of inductive theory-building can not help but be deductive as well. While the implicit “hypotheses” are altered as a function of field experience, they cannot be eliminated.

My point in this brief commentary is to suggest that the inductive and deductive models of research cannot be disentangled. As “natural citizens” we are continually learning from our situational exposure and from what we have been told by others. We are inductive theorists. But we then use this learning to make assumptions and create expectations of the way in which the world operates. We are deductive theorists. The separation that has been propounded both by qualitative and quantitative researchers is fundamentally at odds with the way that people experience and interpret their worlds.

As citizens we are never interested in the description of social scenes to the exclusion of other concerns. We are interested in how we might pragmatically utilize what we learn. Theoretical analysis is not something that occurs only before entering the field or after one has been in the field, but is a continuing recursive process. Induction leads to deduction, which leads to induction, and on and on and on. Researchers should always be engaged in theory building - before, during, and after the gathering of ethnographic data. Qualitative research has the mission of being deeply and richly theoretical, but so does every other domain of social life.

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COMMONSENSE CRITERIA

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Several commonsense criteria are generally useful for assessing texts based on ethnographic fieldwork. Here are three.

1. Can the reader see the subjects independently of the author?
2. What if anything is novel about the relationships that the researcher established with subjects?
3. Is there sufficient variation in the data to compare the explanations offered with alternative explanations?

By characterizing these questions as commonsense, I mean to suggest that these criteria are logically compelling as well as empirically grounded. They are empirically grounded in the sense that readers use them when assessing texts; readers consider research reports to be better, the more effectively they answer these questions. And they should: this commonsense makes good sense. I am trying here both to articulate the system of social relations among readers, authors and subjects in which readers in fact assess reports of ethnographic fieldwork, and I am arguing for the logic of the principles guiding that reading.¹

1. SEEING THE SUBJECTS AND EMPOWERING THE READER

“Show me the people!” the frustrated reader often wishes to yell at the safely distant author.

There are several ways that texts based on ethnographic fieldwork can make a view of the subjects available to the reader independently of the author’s explanatory language. None of these devices are immune from self-serving or self-protecting manipulation, but each makes it more difficult for authors to fabricate the appearance of a database for their theoretical claims.

1. Quote the subjects and present descriptions of field scenes written as contemporaneously as possible. Readers appreciate quotations of subjects, especially if the subjects do not sound like puppets for the author’s analytical voice but speak in their own contextualized and historically specific vocabularies.
2. Describe the practices of the subjects. What do they do and how do they do it? Sociologists often use the unwittingly self-mocking adjective, “concrete,” to specify description and analysis that are commendable. That the irony in the metaphor routinely goes without notice is itself telling. But the virtue presumably in mind is worth struggling for. All action is shaped in specific practical, obdurate contexts, and through making immediate adjustments that seize resources and get around obstacles. Description is better when, instead of labeling what it is that people do, it shows how they innovate their behavior.
3. Identify the subjects. Where can we find them? Who are they? Name them. Show photos of them. Give us access to videotapes.

In any given study there may be good reasons for not doing one or another of these three, but those reasons should be presented. Why, after all, can't the author identify the subjects, or at least the actual locations of the research? If the data are really first hand, there should be resources for describing the situated practices of the behavior observed. And if the researcher learned from what he or she heard people say, the reader should hear what the researcher heard, or as much as the author can convey. At the very least, the burden of justification should rest on the side of authors not meeting these criteria. All of these demands for evidence were at least implicit in Blumer's critique of *The Polish Peasant*, [Blumer, H. (1939). *An Appraisal of Thomas and Znaniecki's The Polish Peasant in Europe and America*. New York, Social Science Research Council], but for some reason the implications of that critique were turned on their head. Instead of indicating that qualitative research is generically strong because one can see when the analysis floats above and away from the presented data, his critique appears to have been taken as a blow to qualitative research and an encouragement to turn to fixed design, quantitative work.

One can of course argue that the author always remains in control, capable of choosing the quotations that are convenient. But as Blumer's critique demonstrated, and as any ethnographer who presents fieldnote excerpts and interview quotations in text knows, it's really not that easy. More precisely, it is much easier to select confirming cases and ignore disconfirming evidence if cases are referred to in the author's analytical language, as opposed to presented in the text in their original form and context of expression. If it is obvious that bias can enter the author's selection of data, it should be obvious that it is much easier for bias to get into the text if the author need not quote subjects at all. As a matter of the researcher's work, it is much easier to advance an argument if one need not produce support either through quotes, by identifying subjects or by describing practical action in its situated context.

In effect, the best that methodological criteria can do is make it harder to cheat, to posture with grand claims, and to float theory independently of data. In other words, methodology is ultimately about the pragmatics of writing up studies. What is key is the sociology of work of sociological work. The empirical claim is that these three requirements do make it harder to cheat, be grandiose in analysis, or get away with a sloppiness in consulting one's data base that the reader can't detect.

At stake in the decision to show the subjects independently of the author's analysis is raw power. When the author's analysis is so entangled formally with the data that the reader has no opportunity for an independent view of the subjects, the reader is always limited to seeing the subjects through the authors' eyes. The reader thus must always wonder whether any disparity between the author's portrait and what the reader understands about society is due to the reader having in mind people or behavior other than those the author examined. Foucault was a master at this power game. Except for a few examples, his books often put readers in the uncomfortable position of wondering whether history still did not look that way because they were not cool or smart enough to grasp his esoteric ideas, because they were thinking of the wrong historical period or context, or because Foucault was floating in his own self-contained world of fantasy. A masterful guide to the workings of power, Foucault was his own best student.

In much contemporary qualitative research, the failure of authors to provide independent access to subjects leaves readers in a similarly dizzying position, and subject to similarly powerful manipulations. Qualitative research usually means case studies and non-representative samples, and readers never have grounds to assert that a theory might not be valid in application to *some* lives. Readers are not equipped to prove the null hypothesis, yet the rhetoric of qualitative studies often throws that burden onto them.

When the qualitative researcher does not give grounds for generalizing findings to lives beyond those studied, any sense on the part of readers that what they know about society does not fit with what the author is claiming can be dismissed by authors and those sympathetic to the author's theoretical, moral, or political positions on the grounds that the reader is thinking of a different population than is the author. The demand to "show the subjects" is a minimal requirement that the author give the reader a basis for judging that the author's analysis at least applies to the lives of the people the author purportedly studied.

It is all the more remarkable that sociologists often now get away with effectively oppressing readers in the name of research that issues calls to overcome social oppression. What is at stake is whether the reader will be vulnerable to a kind of academic bullying that insists that right-minded readers would imagine a society that fits the author's analysis. "Show me the people!" is a demand to enfranchise the reader, to democratize the dialogue among subject, author and reader by equipping the reader to critique the author based on the author's own evidence, in a phrase, to become an active participant in a three-party interaction among reader, author and subject.

2. DEVELOPING NOVEL RELATIONSHIPS WITH SUBJECTS

A second commonsense criterion for evaluating ethnographic fieldwork is whether the researcher has developed novel relationships with subjects. Progress still depends on Robert Park's call that researchers get their pants dirty rather than on fancy intellectual manipulations of the data that are recorded. Put another way, the significance of contributions rests significantly on innovation in data gathering, and in this respect ethnographic fieldwork and quantitative research are not as far apart as is often assumed. While subtlety of data analysis and elegance of writing usually get the attention, the unromantic hero in both qualitative and quantitative research is the laborer who negotiates relationships that bring new and better data to the analytical table.

It is possible to warrant an ethnographic study as a repeat of a prior study, for example as a way of checking whether history has changed conditions underlying previously established findings. If nothing changed then the same research methods can be used to document the same reality. If conditions have changed, an ethnographer would have to change methods. A finding of no change, and thus a study which uses the same relationships with subjects, is logically as important as one finding change. But it is a hard sell to argue that "we have not had a study of a white ethnic neighborhood in a long time." Without more, the possibility that one will find only that time has past usually will not provide much of a warrant for a study. Since ethnographic research typically must take shape in relation to the substance of what is studied, there will be a need to innovate relationships with subjects wherever there has been social change, or even personnel change, between the original and the revisiting study. Put another way, the claim that a study is a "repeat" or a "revisit" can finesse the central issue, which is how the innovations in field relations this time compare to those done last time.

We disproportionately reward ethnographic fieldwork that innovates relationships with subjects, and we should. The forms are many and I will not try to give an exhaustive list. A few examples should indicate the range and value of ground-level progress in data gathering.

Eli Anderson's writings should be appreciated as a corpus. What underlies the authority in his analyses is his unparalleled, continuous involvement in relationships with people living in low income African-American and mixed class, mixed race communities. That plus the fact that he shows us his subjects, or more of them than anyone else shows. There is no mystery here, except for the mystery of why no-one else has done it. Anderson's work is good largely to the extent that he has more and better fieldnotes on street life in black American cities than anyone else, and to the extent he shows them to us.

Mitch Duneier can "show us the people" like no-one else does, even bringing his subjects to his university classes, because of the care he has taken to develop and sustain relationships with them. Critics/cynics may wonder whether, as he claims, he read his manuscript to his *Sidewalk* subjects as a means of seeking their comments and permission, but if you talk to the vendors he studied you find out he really did, and that they attended to the nuanced descriptions of their lives. The comprehensiveness, detail, and fact checking in his investigation made this an unprecedented study of a place-focused social world.

Sometimes the innovation in data gathering is a matter of developing the relationships that get the funding and then developing the relationships necessary for managing a large research team in challenging contexts. Examples are Katherine Newman's organization of the student placements leading to *No Shame in My Game* and *Rampage*, and David Snow's ongoing coordination of a multi-city, multinational, multi-cultural and multi-lingual study of homelessness.

The production of new forms of data becomes increasingly difficult over time, as the easier methods of data acquisition are exhausted. For funding agencies, it is important to appreciate that quantitative research careers converge increasingly with university teaching obligations while ethnographic research careers increasingly diverge. For quantitative researchers, data sets are now available on office computer screens; survey studies are commonly sub-contracted to non-university organizations. The demands of making progress in ethnographic fieldwork, which usually require first-hand involvement on the ground, increasingly diverge from the routines of academic life, as increasingly extensive personal participation and larger scale and more diverse data gathering is required for making significant progress.

Consider the familiar field of studies of behavior in public. This has been a focus in sociology at least since Simmel. Due to the accessibility of the data and the relatively minimal IRB/human subjects restrictions, studies of public place behavior have been widely and continuously attractive to sociology students. Despite, or more accurately, because of all this attention, progress has been minimal since Goffman and Lofland. The lack of progress is not due to the success of prior work in exhausting the description of the social organization of social life in public places. There is a looming gap in the research coverage, which has been based either on eavesdropping and observational methods for describing what strangers are up to, or based on surveys that link demographic background characteristics to attendance at various sites (beaches, parks, museums). What is missing is work on the dynamic social structure of life in public, what Goffman called the "expedition" but which, were the double entendre less troublesome, might more broadly be termed the "outing."²

What stranger observers can't see and what survey interviewers can't readily get at, is, most generally, the trajectory of which a given moment in public is a part, and then, more specifically, the changing social relations along the phases of an outing. Without describing the origins, stages, turning points, anticipated mini-careers, recurrently revised anticipations of next stages, scene-by-scene changing social demographics, endings and aftermaths of outings, we cannot get at the lived meanings of public space,

much less at how patterns of interaction in public life are systematically related to differences in how outings are socially structured. Scattered evidence indicates that there are systematic differences between, on the one hand, how people interact with others in public and, on the other, whether they are on outings of one of the following sorts:

1. *Solo expeditions*: Going out on one's own to jog, read in a park, eat a meal in a restaurant or take a drink in a café, walk a dog, shop for groceries, run an errand to the dry-cleaners... Interaction with others only briefly occasions pauses or detours from a pre-envisioned course of sequential conduct in a single life's biography.
2. *Pre-tied outings*: Going out to a pre-visualized public site in a previously related set or social unit, for a family picnic, on a date, as part of an established group, team, or class. The set may assemble completely before arriving at the target site or coalesce in part or whole only at the destination itself. Once assembled, the members of the set encounter others primarily in the status of landscape features which, in unpredictable fashion, will sporadically occasion commentary or reflections within the pre-tied group.
3. *Hooking up*: Going out, alone or in a pre-set social unit, to explore the prospect of meeting someone new with whom one might have a continued relationship. Issues of loyalty to the pre-set unit and the ethics of realigning with newly met others may perk up at any time.
4. *Site-bounded sets*: Going out to interact with people not previously met, with no expectation of sustaining a continuing relationship after leaving the site: pick up basketball in a public park, chess games at the beach or in donut shops, games struck up in pool rooms. Las Vegas is a city dedicated to the creation of this form of public life, which also characterizes anonymous sex in public bathrooms. Over a series of such outings, some of the others on site may become pre-known and expectations of regular encounters may be engendered, altering the event so that, at least in part, it takes on shades of a pre-tied outing.³ How can we develop a broad range of data on outings? Approaching strangers in public settings to conduct spontaneous interviews or to set up later interviews is awkward, which is basically why 'outings' have never much been studied. But what if we could get a variety of people to record their outings, making them, in effect, participant observers? Drawing on an ethnic range of ethnography interns, this is what Bob Emerson and I are trying to do at UCLA with summer cohorts of undergraduate students drawn from a variety of colleges. Whatever progress we make will be due less to any theoretical breakthrough than to the success of our outreach recruitment efforts, in-class training sessions, and management abilities in keeping the student interns motivated.

The institutions of academia increasingly offer more attractive self-portraits than this unglamorous view of the research intellectual. But the key action does not occur in the more romantically conceived scenes of our work: through epiphanies at the keyboard, as upshots of sparkling interchanges in a graduate seminar, while musing over a cappuccino, or by being so blessed as to capture the intellectual spirit of a great predecessor. The real progress is made in everyday, mundane actions that build social relationships in novel directions, such that new forms of data are produced.

3. Why “why?”?

Ethnographers and other qualitative researchers often try and occasionally succeed, at least in academic reviews and reward processes, in magically pulling explanations out of data sets that lack variation. Most commonly these days, variations in micro-level data, for example the downward course of events in a set of biographies, are interpreted to argue that macro-level structures, like class stratification, racial oppression, or global economic relations, are responsible, even though no evidence of variation in the macro-level phenomena is presented, much less is evidence of change in macro features linked closely with the individual life phenomena purportedly explained. That causal explanation without variation makes no sense is not news, although loud voices that once emphasized the point, like Robert McIver [MacIver, R. M. (1942). *Social Causation*. Boston, Ginn], are no longer heard. Which leads to the question, how can they get away with it?

The real reasons may be the familiar devils, political sentimentality and collective career strategies advanced by preaching to the choir, and I have no new curses to break these spells. But it may be useful to puncture some of the rhetorical cover for what should be transparent games. That cover comes in several varieties of claims that one can somehow achieve explanation that is not causal in nature. Hence the need to answer the question, Why “why?”

First it is necessary to clarify that not everyone sins. A vast array of ethnographic writing is built on a careful causal logic that fits comfortably with the variation in the ethnographer’s data, but the author eschews explicit causal language for various good reasons. Consider Julius Roth’s *Timetables*, a participant observation study of a tuberculosis hospital that showed how patients, in an effort to get a sense of control over their destiny, grabbed whatever bits they could from medical prognoses to construct a timetable for their recovery, and then used that to structure their interactions with staff, family and their own emotions. There is a double causal argument here. On the positive side, Roth shows the construction of a new force, the timetable, which then is more or less implicitly used by patients to influence the structure of their hospital careers. He explains how timetables arise and how they shape patients’ emotions and interactions with staff. On the negative side, Roth negates explanations of patients’ emotions and conduct, such as medical analyses, that would ignore these social phenomenological timetables.

Roth’s analysis nicely fits his data, even though he argued for no explicit causal theory, other than the familiar “symbolic interaction” claim that locally emergent meanings shape conduct; even though he offered no moral advice, other than suggesting that effective and sensitive management of such hospitals should take account of patient culture. (He also conveyed the always welcome implication that sociological fieldworkers should get more employment.) But there is no incompatibility between reading the study as a causal analysis and reading it in the language Roth wrote it. The explanans and the explanandum both vary locally in his data.

The trouble in ethnographic texts often pops up when local culture, with all its rich variations, is treated as false consciousness, a product of or reaction to macro factors which themselves are never described in varying form in the data. In order to obscure the logical problems with the asymmetry of the data, ideological rhetoric or theory singing flows furious and fast.

Some, perhaps all of the dodges are shared by quantitative and ethnographic methods, but in ethnographic texts they are both more critical and more difficult to get one’s hands on. They are more critical

because ethnographic texts require story telling, and, since the “end of ideology” and the dwindling of the debates among alternative 19th century grand causal theories, the story told increasingly has been a moral tale. Most often that has been a tale of pathos but recently there are positive stories of people making it, at least in the sense of sustaining personal pride, in the face of daunting obstacles.

Common to qualitative and quantitative studies is a weakness in delivering the “falling down” tale of increasing social inequality resulting from people in the middle having social structure kicked out from under them, leading to Dante-esque consequences. Quantitative analysts, when they want to be honest about it, acknowledge that they don’t have biographical data. They can show increasing inequality, but typically the demonstration is through cross-sectional data that describe a given geography’s population at different points in time. These are not the same people. They do not permit inferences that people now in the elite were in the local higher reaches before or, more critically, that people lower down were higher up. In LA, for example, a “falling down” rhetoric has been offered repeatedly without warnings that the lower end of the spectrum is now populated by very different people, different in ethnicity, immigration history and family biography, than those who populated the lower regions in early periods. To my knowledge, no one has compared the recently arrived Latino population in LA as of 1990 with the stratification they or their parents experienced in Guatemala, Mexico, or Nicaragua in 1970. If they did, they would find many instances of people not falling down biographically but jumping up, relative to their position in the stratification order they had been in before migrating. Enough such instances to change the picture? No-one knows, but we are talking about something like 2 million people moving across national borders, and many others moving in and out to and from other U.S. locations, as the area grows to a 10 million population. That isn’t chopped liver.

Ethnographic studies “give flesh” to the “falling down” narrative. They invoke “deindustrialization” and other macro explanations of increased inequality without worrying that they have no data showing variations in these explanatory factors, much less data of macro changes that are linked in time to evidence of increasing social pathologies. A huge ethnographic industry of documenting urban pathologies was tacked onto the “deindustrialization” idea, despite the fact that urban crime rates and other indicators of pathology were already up in the late 1960s, and are now substantially lower than they were in 1990, despite a continued bemoaning of “deindustrialization” as an immiserating process now spurred by “globalization.”

Thus one of the strategies for avoiding providing evidence on the causal claims underlying an ethnography’s narrative is to borrow an unassailably popular idea from sociological pop culture. The problem of the missing historical variation in one’s ethnographic data is then laundered, as it were, by reference to claims made by quantitative researchers; but if one looks, it appears that our more hard headed colleagues don’t have the critical data either. This strategy also works for cross-sectional claims. Thus gang activity is said to increase in the “rustbelt,” where one can indeed show a decline of manufacturing and working class employment. The ethnographic format of a case study serves to make the story tellable without looking beyond the immediate city site, for example to the economically expanding sunbelt, where, it turns out, gang activity has accelerated, by official counts, even more than in the rustbelt [see Katz, J. and C. Jackson-Jacobs (2003). “The criminologists’ gang.” in *Companion to Criminology*, Blackwell, pp. 1-34].

With nods to the wisdom of appreciating the distinctive values of both qualitative and quantitative research, the ethnographer rhetorically avoids the limitations of everyone’s data for the causal claims made. Another common strategy is extreme naturalism. Here one shows things to be so bad that the

reader cannot imagine they could be worse. The dam breaks and the destruction to life and property is described in detail. Crack is introduced in the ghetto and teenagers pimp their sisters for a hit. There was a time before the dam broke, that is clear; there was a time before crack was introduced, that is clear. Things are so bad in the “after” period portrayed that it seems somehow callous, surely unnecessary, to demand evidence that they were better before. And perhaps they were. My point is simply that this is a qualitative rhetoric for avoiding the usual demands that we show variation on both ends of an explanatory process.

More intriguing are rhetorics that suggest that one can provide an explanation and make a moral point without causal claims. One of these is an attack on “positivism,” or deterministic causal explanation, that appeals to the general distaste among ethnography’s readers for anti-humanistic perspectives. But there are non-deterministic causal explanations. For example there are explanations that specify necessary and sufficient conditions for phenomena while leaving a step in the process to personal creativity (e.g., defining the effects as desirable). A distaste for positivism cannot justify abandoning forms of non-predictive, non-deterministic, causal explanation, as Charles Ragin and Stanley Lieberson have helped us understand. A theory of necessary conditions alone is a useful explanation. It is especially demanding and useful to specify the necessary sequencing of necessary conditions. Retrodiction, or the prediction not of what will happen but of what one will find if one looks into the biography of given types of events, is compatible with causal explanation and non-deterministic perspectives. The attack on “positivism” is a red herring, a dodge that substitutes for direct confrontation with the requirements of the many forms of causal explanation that are consistent with the humanistic sympathies of ethnography.

The claim that one is providing “interpretation” instead of causal explanation is another popular rhetoric for finessing the demands of demonstrating variations in explanandum that are linked to variation in explanans. But it can be shown that all claims to provide significant interpretations rest critically on absent empirical descriptions and implicit causal readings of the descriptions that are presented in texts.

What would follow would be examples of other dodges used in ethnographies that offer non-causal justifications, but I’m going to stop at this point. There’s enough to argue with already. This is also a good time to stop because I would have to get into trickier ground and argue that the distinction between “normative” and “empirical” analysis and explanation is false. Nothing novel would be required: the pragmatists have been here before and shown the way. But substantial additional text would be required to apply the point to examples of current research.

ENDNOTES

- ¹ There are many complexities in extending this argument to the various forms of “qualitative research” that are not based on ethnographic fieldwork. In a way, when historical scholars find new sources, they are changing the relations with subjects in their field; as are analysts of qualitative case studies who compile a more varied and documented corpus of cases than had been available. Conversation analysts often treat data acquisition for granted, analyzing portions of transcriptions from tapings done by others and glossing description of the data acquisition process as if it contained no critical contingencies relevant to their qualitative analysis. Here I just want to flag an appreciation of the complexities of extending this argument beyond ethnographic field studies.
- ² A recent paper by Marjorie DeVault, based on one type of outing, family trips to the zoo gives a glimpse of what has been missed. “Producing Family Time: Practices of Leisure Activity Beyond the Home.” (2000). *Qualitative Sociology* 23(4): 485-503.
- ³ For an elaboration of these types, initial hypotheses of the on-site patterns of interaction they are related to, and the significance of their study for improving understanding of a range of issues, including segregation and integration in public life and the meaning and attractions of “being out in public,” see <http://www.sscnet.ucla.edu/nsfreu/>.

EVALUATING QUALITATIVE RESEARCH: SOME EMPIRICAL FINDINGS AND AN AGENDA

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I approach the questions that were submitted to this panel through somewhat unique lenses. For the past few years, I have been working on an NSF-funded study of how scholars who serve on funding panels in the social sciences and the humanities go about assessing excellence. The analysis draws on panel observations and on 81 interviews conducted with panelists serving on twelve funding panels at the Social Science Research Council, the American Council of Learned Societies, the Woodrow Wilson National Fellowship Foundation, a Society of Fellows at a top research university, and a foundation in the social sciences.¹ These panels are all interdisciplinary, and two of them consider almost exclusively social science proposals. Some of the programs I study are often described by respondents as alternatives to NSF funding because they privilege qualitative, comparative, and context-sensitive research that the respondents perceived as less frequently funded by NSF. Hence, it is useful for our purpose to look at some of the insights that are emerging from the data concerning the evaluation of qualitative research in general.

First, the vast majority of the social scientists and all the sociologists interviewed in my study privilege what we call a “comprehensive” epistemological position that, drawing on Weber’s *Verstehen*, values theoretically informed research (as opposed to “theory testing”), as well as focusing on the uniqueness and complexity of empirical cases. Half of the sociologists, and most of the political scientists, combine this position with a more scientific epistemological standpoint that, inspired by Popper, favors falsification, generalization, theory building, and the use of alternative hypotheses (Mallard, Lamont, and Guetzow 2003). Second, whereas the literature on peer review defines originality in terms of the production of new theories and new findings, less than one in five of our panelists refer to “original theory” when discussing originality and only four percent refer to “original results” – the least popular generic type of all. A third of the 217 mentions of originality made by our respondents concern the use of “new approach,” which refers to the novelty of the questions asked, the perspective used, and the arguments made. Humanists and historians clearly privilege originality in approach, whereas social scientists value most originality in method, but they also have an appreciation for diverse types of originality, stressing also the use of an original approach, an original theory, and the study of an original topic (Guetzow, Lamont, and Mallard 2003). Third, the “scripts of excellence” used by panelists can be broadly captured by the following criteria: Clarity; the Articulation of Theory, Data, and Method; Scholarly Significance; Political and Social Significance; Craftsmanship, Solidity and Rigor; Empirical and Scholarly Soundness; Feasibility and Applicant’s Track-Record. Evaluation also takes into consideration more evanescent qualities, such as whether the proposal evinces “excitement;” whether it offers signs of intelligence, elegance, or cultural capital; and whether the applicant abuses theory and shows moral character, and particularly integrity and risk-taking (Lamont, in progress).

These results suggest that the funding of qualitative research is facilitated when 1) panelists are epistemologically flexible and open to seeing merit in hermeneutic and explanatory research as well as in the use of comprehensive and more positivist approaches; and 2) a broad definition of originality is used, as opposed to a definition inspired by the natural sciences that privileges the production of new theories and new findings. Moreover, the evaluation of qualitative research was not construed as “a problem” in the panels I observed, nor in the interviews: panelists do what they are asked to do: they compare and rank. They generally believed that the cream naturally rises to the top, a belief that is supported by the fact that some proposals get universally high ratings. They are convinced that evaluators recognize quality when they see it and in my book manuscript based on the project, I spend several hundred pages explaining how this social process is accomplished and what institutional supports make their shared belief possible.

These findings lead me to reflect on whether the questions that are put to us today are shaped by the distinctive context in which the evaluation of qualitative research is conducted at NSF. Undoubtedly, the dynamics at work in the case of the anthropology panels studied by Don Brenneis (1999) are very different from those found in the political science panels where comparativists have been attacked by formal theorists who promote a narrow definition of theory.

The situation in sociology is somewhat murkier. The majority of the members of the sociology panels listed on NSF's website (through 1996) specialize in quantitative analysis themselves. While methodological catholicism is the official organizational policy, most qualitative sociologists believe that NSF privileges quantitative research. Explanations for this state of affairs diverge: some single out the fact that leaders in their field who are best positioned to evaluate qualitative work are greatly underrepresented among decision-makers (as opposed to reviewers) and that the criteria of evaluation used at NSF are biased against qualitative research (against induction for instance). Others point to the weakness of the proposals or the lack of systematic training in qualitative research in many graduate programs. Because NSF is after all concerned with science, it is often believed that research programs that are clearly cumulative in orientation are favored (one thinks of expectation state theory in social psychology for instance), while research projects that offer "new approaches" and that cannot be clearly located in strongly institutionalized field of research face greater hurdles (in part because their proposals cannot signal clearly appropriate evaluators).

This knowledge is not even a public secret in the profession. Hence, in the subfields that I am very familiar with, I know that a number of senior researchers never applied for NSF grants. Moreover, the knowledge of NSF's preferences shapes the research agenda that new generations of ambitious graduate students define for themselves. Nevertheless, primarily qualitative areas such as cultural and economic sociology continue to attract young sociologists in droves. Year after year, stars on the job market include promising young scholars who have decided to devote themselves to qualitative work. In recent years, our discipline's most prestigious journal, *American Sociological Review*, has gone to considerable lengths to make more room for qualitative sociology, and the current editors are proud to point to a few strong qualitative papers in each new issue. Because publication in *ASR*, like receiving an NSF grant, is a key status marker in the field, these policy changes will increase the likelihood that the young stars will be granted tenure in top departments in the next few years. Hence the importance of such changes: they have a deep impact on the substantive make-up of our field. They also influence whether intellectual trajectories are shaped by conformity with perceived norms or by creativity and conviction. As such they have a deep influence on the intellectual vitality of our field. This is why we are so privileged to be included in NSF's current effort to revisit the evaluation of qualitative research, and why we should work toward not only reflecting on what defines first-rate qualitative research, but also on what we would want NSF to advance (as suggested by Charles Ragin in his letter of June 10th).

The memos prepared by our colleagues go a long way toward outlining the standards of excellence for qualitative work (I think in particular of David Snow's discussion of theoretical development and Susan Silbey's memo on "Designing Qualitative Research Project"). I have the conviction, and my research suggests, that qualitative researchers who serve regularly as evaluators have a pretty clear notion of what first-rate qualitative research looks like. Therein does not lay the issue. In response to the question of what we would want NSF to advance, at least in the case of sociology, I would advocate promoting among evaluators a broader definition of originality and greater openness to knowledge production that is not oriented toward theory testing, but toward the development of new approaches and perspectives. The crucial and distinct contributions made by comprehensive approaches (in line with Weber's legacy) should also be fully recognized. This would be best accomplished by broadening the ranks of the panels to include more of the best qualitative researchers in cultural sociology, comparative historical sociol-

ogy, economic sociology, and related fields, as well as more quantitative researchers who are themselves methodologically polyvalent. As our discipline has been moving clearly toward greater methodological eclecticism, and is coming to consider the qualitative and quantitative techniques for what they are — simple tools (as opposed to expressions of the differently valued selves of various types of researchers) — I have no doubt that evaluators will come to focus on commonalities, as opposed to differences in what defines first rate research.

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ENDNOTE

- ¹ The specific competitions studied were the International Dissertation Field Research Fellowship program of the Social Science Research Council and the American Council of Learned Societies; the Women's Studies Dissertation Grant Program at the Woodrow Wilson National Fellowship Foundation; and the Fellowship Program in the Humanities of the American Council of Learned Societies.

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THE DISTINCTIVE CONTRIBUTIONS OF QUALITATIVE DATA ANALYSIS

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This essay explores techniques of data analysis that are distinctive to qualitative research. These techniques include strategies for both causal and descriptive inference.

I. CAUSAL INFERENCE

Many qualitative researchers are centrally concerned with using methods that will allow them to generate valid causal inferences. In this sense, qualitative and quantitative researchers share broadly similar research goals. However, qualitative researchers rely on distinctive methods of causal inference, three families of which are discussed here.

A. NECESSARY AND SUFFICIENT CONDITIONS.

Hypotheses about necessary and sufficient causes are commonplace in the social sciences, figuring prominently in the work of scholars representing diverse methodologies and substantive interests. Indeed, Goertz's (2003) First Law holds that for any research area one will find important hypotheses about necessary and sufficient causes.

Yet, mainstream statistical methods are inappropriate for the analysis of necessary or sufficient causes (Dion 1998; Goertz and Starr 2003; Ragin 2000). Statistical researchers might defend themselves by arguing that these kinds of causes: (1) do not exist; (2) are trivial if they do exist; (3) are inherently deterministic; and (4) are not commonly used in social science theory. However, recent methodological writings have seriously called into question all of these points (see Mahoney 2003 for a review).

In contrast to statistical analysts, qualitative researchers can employ powerful methodologies specifically designed to study necessary and sufficient causes. For example, while Mill's methods of agreement and difference can only eliminate necessary or sufficient causes, Ragin's (1987; 2000) innovations grounded in Boolean algebra and more recently fuzzy-set analysis provide a full-blown apparatus for testing hypotheses about these causes. Drawing on innovations such as these, scholars who study necessary and sufficient conditions can: (1) analyze probabilistic patterns of necessary or sufficient causation; (2) explore how different combinations of variables are each jointly sufficient for an outcome; and (3) assess the statistical significant and statistical relevance of necessary and sufficient causes.

Currently, however, most qualitative researchers who study necessary and sufficient conditions do so in an informal and implicit way, without explicitly invoking a formal methodological apparatus. There are at least three reasons why qualitative researchers do not employ these techniques more explicitly and rigorously. (1) Qualitative researchers often do not learn about these methods during graduate school because of the dominance of statistical methods. (2) Qualitative researchers are often skeptical about the benefits of formal methodologies, and they prefer to pursue data analysis with as few constraints as possible. (3) The use of these methods is not associated with substantial professional payoffs in terms of publications or grants, in part because of widespread impressionistic skepticism about these methods.

Accordingly, I see three steps that need to be taken to advance qualitative research through the study of necessary and sufficient causation. (1) The formal methodologies used to analyze these kinds of causes must become a more prominent part of graduate training, which may ultimately require getting statistical researchers to start to pay attention. (2) Scholars who choose to analyze necessary and sufficient causation in a highly informal way must do a better job of justifying the advantages of this informality or give way to the more formal apparatuses. (3) Journal editors and funding agencies must turn a skeptical eye on reviewers who raise concerns about research proposals simply because they seek to analyze necessary or sufficient causation.

B. PROCESS ANALYSIS.

A distinctive advantage of qualitative research is its ability to infer causation by examining the processes that link cause and effect. Whereas quantitative research relies primarily on analyzing the statistical association between variables to infer causation, qualitative research relies more heavily on locating the connections between variables to infer causation – a technique that has been called “process analysis” (Brady and Collier 2004).

Process analysis contributes to causal inference in at least two ways. (1) It enables researchers to identify the intervening variables – or what are sometimes called “mechanisms” – that stand between cause and effect (George and Bennett forthcoming; Hedström and Swedberg 1998; Rueschemeyer and Stephens 1997). In scientific research (e.g., on smoking and cancer), the identification of the intervening steps through which a cause affects an outcome is seen as a crucial element of causal inference. (2) The analysis of overtime processes increases the “N” of a study and enables researchers to test their hypotheses against many more observations. Campbell (1975) suggests that this kind of process analysis is a fundamental basis of inference in small-N research.

Most qualitative researchers pursue process analysis through the use of “narrative,” or systematically structured “stories” about sequences of events (Abbott 1992; Stryker 1996). At this point, the works of most analysts are not very explicit about the exact roles that narrative play in causal inference. Yet, methodologists have proposed at least two innovations for more rigorously pursuing narrative presentation: (1) event structure analysis, in which analysts are forced to state whether a given event is a necessary condition for a subsequent event (Griffin 1993; Heise 1989); and (2) event structure flow charts, in which analysts systematically diagram each linkage of their narrative presentations and compare them across cases (Mahoney 1999).

This discussion suggests that qualitative researchers again face a choice between pursuing their research in a more formal/rigorous way or in a more informal/flexible way. Currently, the latter mode of process analysis prevails throughout the social sciences, though it is viewed skeptically by many quantitative researchers. Further progress toward using and enhancing formal techniques of process/narrative analysis would strengthen qualitative research.

C. TEMPORAL ANALYSIS.

As the preceding discussion suggests, qualitative researchers almost invariably are interested in the unfolding of processes over time. While statistical researchers sometimes share this interest, they nevertheless often pursue cross-sectional research that offers little more than static “snapshots” at a couple points

in time. Moreover, mainstream statistical methodologies often cannot analyze highly complex temporal processes even when these processes seem of elementary importance to adequate explanation. For qualitative researchers, a central concern of causal inference entails not only the degree to which variables are present, but also when they occur within broader sequences. As Pierson (2000a) puts it, the issue is “not just what, but when” (see also Abbott 2001). In this regard, qualitative researchers have been at the forefront of several innovations aimed at systematizing the ways in which time can be incorporated into substantive research: (1) the codification of arguments about critical junctures and path dependence (Collier and Collier 1991; Pierson 2000b; Mahoney 2000); (2) the use of time periods as cases (Haydu 1998); (3) time and narrative (Abbott 1992; Aminzade 1992); (4) time and institutional evolution (Thelen 1999; Thelen 2003); and (5) duration and explanation (Pierson 2003).

II. DESCRIPTIVE INFERENCE

Although descriptive inference receives second billing next to causal inference in contemporary social science, it is still regarded by all social scientists as a fundamental component of research. Statistical researchers have very powerful tools of descriptive inference that enable them to summarize the characteristics of large populations from samples. Yet, qualitative researchers have equally powerful tools of descriptive inference that enable them to develop new concepts, achieve measurement validity, and create new data sets.

A. CONCEPTUAL INNOVATION.

Social science knowledge is built around concepts, and the introduction of new ideas into the field often takes place through the creation of new concepts. Qualitative researchers do not enjoy a monopoly over this creation, but they do seem to have made a disproportionately large contribution in the area – a contribution that is rarely acknowledged.

There are at least three related reasons why so many key social science concepts are generated by qualitative researchers. (1) The in-depth examination of small numbers of actual cases stimulates conceptual innovation in way that is not possible if researchers do not learn very much about particular cases. (2) Qualitative researchers are highly interested in theory generation, which often requires the introduction of new ideas and thus new concepts; and (3) The interplay between evidence and theory in qualitative research stimulates conceptual development.

Unfortunately, the social science disciplines seem to systematically underappreciate the role of qualitative research in building new ideas through conceptual development. This underappreciation could be documented in two ways: (1) the absence of publications in leading journals dedicated to the elaboration of a new concept (or methodologies for developing new concepts); and (2) the absence of external grants that fund research dedicated to conceptual generation or the elaboration of existing categories.

B. MEASUREMENT Validity.

All researchers seek to adequately measure their concepts. However, qualitative researchers have been criticized for lacking an explicit framework for conducting such measurement; indeed, many quantita-

tive researchers view their measurement procedures skeptically. Here, however, I suggest that qualitative research offers some distinctive advantages for achieving measurement validity (see Adcock and Collier 2001; Ragin 2000).

The advantages of qualitative research grow out of this tradition's close examination of individual cases. Qualitative researchers can draw on their deep knowledge of cases when determining variables scores for those cases in at least two ways. (1) They can assess how the meaning of indicators may vary across diverse contexts. This, in turn, facilitates the use of context-specific indicators. (2) They can move back and forth between conceptual definitions, indicators, and scores for actual cases in many rounds of iteration. By contrast, quantitative researchers often use pre-constituted data that do not allow for this possibility.

Because of these advantages, many qualitative researchers believe that they achieve a much higher level of measurement validity than what is found in quantitative research. However, because qualitative researchers have been quite informal in their variable coding, many researchers outside of the tradition are skeptical about this belief.

C. DATA CREATION.

Finally, along with historians, qualitative researchers are responsible for producing many of the secondary sources that are used to construct statistical data sets and that become the informational basis for qualitative comparative analysis. For example, recent quantitative data sets on democracy, international war, and colonialism draw heavily on the rich qualitative literatures on these topics in political science and sociology. In this sense, qualitative research has a crucial (though again, not often appreciated) role in a broader division of academic labor.

The publication of books and monographs on foreign countries are one of the most important ways in which this kind of data creation takes place. Yet, currently, there are at least two threats to the ability of qualitative researchers to continue to produce this kind of work: (1) top university presses are increasingly hesitant to publish books focused on individual cases outside of the U.S.; and (2) the allocation of professional status to scholars with case study expertise appears to have declined substantially in recent years. It seems crucial that funding agencies continue to support international travel by doctoral students working in the qualitative research tradition as a counterbalance to these trends.

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A PLACE FOR Hybrid Methodologies

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Qualitative research has been around for a long time serving as a dependable work horse for modern sociology. Many qualitative studies have joined the short list of modern sociological classics, read year after year by successive generations of sociologists for their rich accounts and penetrating analysis of social behavior. Is it a mere coincidence that within the social sciences the standing of sociology as a discipline was higher when most serious sociological findings relied on inferences drawn from qualitative research? The sociological studies that allied social science disciplines turned to for findings that informed their research have included a heavy representation of the classic studies based on qualitative research broadly defined. Today most serious research findings reported in mainstream sociology journals base their inferences on results that employ powerful and sophisticated quantitative methodology. Advances in computational power, the development of user-friendly statistical programs, and the open access to on-line public use data sets have all contributed to making quantitative research irresistible as the method of choice. Despite recent criticism of quantitative research for impoverishing theory-driven research in sociology (e.g., Sorensen 1998), it is unlikely that the methodological pendulum will swing back to embrace qualitative research as the method of choice in the foreseeable future. For one, the sheer momentum driven by large-scale research funded by the NSF and NIH has decisively shifted the balance to favor continued reliance on quantitative methods for advances in sociological research.

I do not mean to imply a pessimistic outlook for qualitative research. Revitalization of interest in ethnographic field research and other forms of qualitative research is in progress. Not only is this fueled by a vague sense of dissatisfaction with the limitations of quantitative research, but if in-depth understanding of a social context is what research seeks to uncover, there is still no better method than to observe qualitatively social behavior in the natural setting. Modern classics of this tradition of research, including Rothlisberger and Dickson's (1939) study of workers in Western Electric's the Bank Wiring Observation Room, Whyte's (1943) study of the Norton Street Gang of Italian American youths, Shibutani's (1978) study of demoralized Japanese American military unit and others written by sociologists at this workshop, confirm the utility of direct observation in the natural setting for producing reliable and enduring findings that illuminate and explicate the nature of social behavior in close-knit groups. Case studies relying on documentary evidence also have provided important source of sociological analysis. Such studies provide not only the basis for generating grounded theory—as in Shibutani's theory of demoralization—but case studies are also useful to support, confirm and illustrate general propositions. The great advantage of the case study method is that it does not deal with isolated, but with interconnected facts, as Homans' (1950: 19) emphasized: "We must not, in the classical manner, use isolated facts to back up our theory, but related facts." Homans demonstrated in his classic study, *The Human Group*, that it is through the systematic shifting of related facts that one can identify and explicate the workings of social mechanisms, which, Hedstrom and Swedberg (1998) argue persuasively, is still the holy grail of causal analysis. The many strengths of the qualitative case studies approach notwithstanding, several limitations come to mind. The case approach does not satisfy the statistician's interest in the ability to make inferences about the population from which the case has been selected.

In the remainder of this brief paper, I will elaborate and illustrate a hybrid method which I have employed in a field study based in Los Angeles of immigrant labor market experiences (Nee, Sanders and Sernau 1994; Sanders and Nee 1996; Nee and Sanders 2001; Sanders, Nee and Sernau 2002). This study adapted Massey, Goldring and Durand's (1994) ethnographic survey method. The aim of our hybrid research design is to combine features of exploratory and confirmatory research using qualitative and quantitative methods. As a hybrid, it can be criticized for falling between the cracks of qualitative and quantitative research. Our study of immigrant labor markets lacks the depth and reliability of findings based on participant observation research, while its very small sample size stretches the credibility of quantitative researchers accustomed to working with large samples. Despite such shortcomings, the study has some merit. First, it has produced a number of findings that shed light on the nature and structure of the labor market in a major immigrant metropolis. Second, it addresses important debates in the literatures on ethnic stratification, social embeddedness and segmented labor markets.

The underlying idea of our adaptation of the ethnographic survey method was inspired by observation drawn from a high energy physics experiment at Cornell, the CLEO project dedicated to the study of the elusive "bottom quark," an unobservable particle. To detect the presence of the b-quark, physicists built a large-scale accelerator on the Cornell campus, the Wilson Synchrotron, which they used to bombard a cloud chamber with high-energy subatomic particles. In the CLEO collaboration, physicists from 22 universities worked together to confirm the existence of the b-quark by observing the trajectory of the shadow it cast in the cloud chamber.

As a casual observer of the sociology of high energy physics, it seemed to me that one could fruitfully adapt the CLEO research design to the study of nature of immigrant labor markets by using event history analysis as a method to study the trajectory of job transitions by immigrants. The path or trajectory of job changes could be used as data to illuminate the nature of the labor market, the reverse of what the physicists were interested in. In our case the shape of the trajectory of job changes could be used to identify and explicate the nature of the institutional environment: to what extent the labor market is segmented by racial and ethnic boundaries, the mechanism of job searches and their consequences, and the scope of socioeconomic mobility permitted within various segments of the labor market in a heterogeneous city like Los Angeles.

The obvious advantage of the sociologist over the physicist is that unlike subatomic particles, immigrants are reflexive agents and can describe in detail the institutional environment in which labor markets are embedded. In the Los Angeles study funded by the NSF, we selected a small, more or less random, sample of immigrants and following an interview schedule we collected in depth retrospective accounts of how immigrants found each of their jobs, their observation of the workplace, their social relations, family context, household economy, individual income and so on. The interviews were conducted over several meetings, lasting several hours. After the field research was completed, the interviews were transcribed and coded with an early version of Ethnograph. These were in turn coded for quantitative analysis.

The work of integrating qualitative and quantitative analysis for each of the published research reports followed a step-by-step progression of analysis. Let me illustrate this by giving a brief account of how we worked on the first research report, "Job Transition in an Immigrant Metropolis: Ethnic Boundaries and Mixed Economy," (coauthored with Sanders and Sernau) published in 1994. The question we addressed in this first paper is to what extent is the labor market of a large immigrant metropolis seg-

mented by relatively impermeable barriers to mobility? Is segmented labor market theory right to assert that segmentation in advanced capitalist labor markets functions as an insurmountable barrier to mobility (Piore 1979). Are Portes and his collaborators right in claiming that the ethnic economy provides a safe harbor for immigrants enabling them to secure comparable returns to investments in human capital as workers in the primary sector of the labor market (e.g., Portes and Bach 1985)? In this study, we first elaborated the theoretical arguments for a theory of porous ethnic boundaries in a heterogeneous urban economy. Briefly, this was accomplished by integrating the proposition that markets serve as integrative mechanisms (Weber [1922] 1978), Blau's (1977) theorem that population heterogeneity increases the chances of random meeting and cross-cutting ties across ethnic groups, and rational action theory which assumes immigrants, like others, are purposive agents who strive to optimize their chances for success in adapting to the constraints of their institutional environment.

We sorted through the ethnographic text using Ethnograph to produce verbal description of the institutional environments of the urban labor market. Overall, we found immigrants expressed caution and anxiety about work conditions in the immigrant enclave economy, preferred to work in the open formal labor market, and described changing jobs frequently, especially in the early years, as a means to increase their chances of finding better conditions of employment and higher wages in the open, regulated sectors of the urban economy. While the verbal accounts tended to support our hypotheses, it was not at all clear to what extent the event history analysis of actual trajectories of job changes would support our prediction of porous ethnic boundaries mediated by mixed ethnicity labor markets until this analysis was completed by Sanders. To our surprise, all of three hypotheses predicting the relative openness of labor market segments and ethnic boundaries were confirmed.

Although the sample of immigrant workers and entrepreneurs was very small, about 120, the number of job changes analyzed came to about 485, which is a reasonable sample for event history analysis. Moreover, the aim was to illuminate the nature of the institutional environment of labor markets by examining the trajectory of job changes. If even a small number of immigrants traveling through institutional spaces confirm that ethnic boundaries are porous, and job mobility in and out of the ethnic and open economy are frequent and mediated by a large mixed economy, then the hybrid qualitative/quantitative study design succeeded to serve its purpose. Above all the neat fit between the vivid ethnographic text of preferences and labor market experiences and the quantitative analysis produced a mutually reinforcing set of evidence that increased confidence in the reliability of the findings reported.

In sum, hybrid methodology that combine qualitative and quantitative research can contribute to innovative research designs and produce significant findings that advance the cause of theory-driven empirical research.

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FROM: THE RIGHT (SOFT) STUFF: QUALITATIVE METHODS AND THE STUDY OF WELFARE REFORM

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Statistical trends are necessary but not sufficient. To me, statistical trends alone are like a canary in a coal mine—they yield life or death information on the “health” of an environment, but don’t always lead to improvement, causes and corrective actions.

Dennis Lieberman, Director of the Office of
Welfare-to-Work U.S. Department of Labor

In the years to come, researchers and policy makers concerned with the consequences of welfare reform will dwell on studies drawn from administrative records that track the movement of Temporary Assistance for Needy Families (TANF) recipients from public assistance into the labor market and, perhaps, back again. Survey researchers with panel studies will be equally in demand as federal, state, and local officials charged with the responsibility of administering what is left of the welfare system come to grips with the dynamics of their caseloads. This is exactly as it should be, for the “poor support” of the future—whatever its shape may be—can only be fashioned if we can capture the big picture that emerges from the quantitative study of post-Aid to Families with Dependent Children (AFDC) dynamics when many of the nation’s poor women have moved from welfare to work.

Yet as the early returns tell us, the story that emerges from these large-scale studies contains many puzzles. The rolls have dropped precipitously nationwide, but not everywhere (Katz and Carnavale, 1998). TANF recipients often are able to find jobs, but many have trouble keeping them and find themselves back on the rolls in a pattern not unfamiliar to students of the old welfare system. Millions of poor Americans have disappeared from the system altogether: they are not on TANF, but they are not employed. Where in the world are these people? Welfare reform has pushed many women into the low-wage labor market, but we are only starting to understand how this trend has impacted their standard of living or the well-being of their children. Are they better off in terms of material hardship than they were before? Are the benefits of immersion in the world of work for parents—ranging from the psychological satisfaction of joining the American mainstream to the mobility consequences of getting a foot in the door—translating into positive trajectories for their children? Or are kids paying the price for the lift their mothers have experienced because they have been left behind in substandard childcare? And can their mothers stick with the work world if they are worried about what is happening to their kids?

These kinds of questions cannot be resolved through reliance on administrative records. Survey data can help answer some of these questions but without the texture of in-depth or ethnographic data collection. States and localities do not systematically collect data on mothers’ social, psychological, or familial wellbeing. They will not be able to determine what has become of those poor people who have not been able to enroll in the system. They have little sense of how households, as opposed to individuals, reach collective decisions that deputize some members to head into the labor market, others to stay home to watch the kids, and yet others to remain in school. Problems like domestic abuse or low levels of enrollment in children’s health insurance programs cannot be easily understood via panel studies that

ask respondents to rate their lives on a scale of 1 to 10. Though one might argue that welfare reform was oriented toward “work first” and was not an anti poverty program per se, understanding the nature of material hardship is an important goal for any public official who wants to get to the bottom of the poverty problem. Trawling along the bottom of the wage structure, we are likely to learn a thing or two about recidivism as the burdens of raising children collide with the limitations of the low-wage labor market for addressing the needs of poor families.

If administrative records and panel studies cannot tell us everything we might want to know about the impact of welfare reform, what are the complementary sources of information we might use? I argue in this chapter that qualitative research is an essential part of the tool kit and that, particularly when embedded in a survey-based study, it can illuminate some of the unintended consequences and paradoxes of this historic about-face in American social policy. From this vantage point, I argue that the “right soft stuff” can go a long way toward helping us to do the following:

- Understand subjective responses, belief systems, expectations, and the relationship between these aspects of world view and labor market behavior;
- Explore “client” understandings of rules, including the partial information they may have received regarding the intentions or execution of new policies;
- Uncover underlying factors that drive response patterns that are overlooked or cannot easily be measured through fixed-choice questionnaires;
- Explore in greater detail the unintended consequences of policy change;
- Focus special attention on the dynamics shaping the behavior of households or communities that can only be approximated in most survey or administrative record studies that draw their data from individuals. This will be particularly significant in those domains where the interests of some individuals may conflict with others and hard choices have to be made.

The intrinsic value of qualitative research is in its capacity to dig deeper than any survey can go, to excavate the human terrain that lurks behind the numbers. Used properly, qualitative research can pry open that black box and tell us what lies inside. And at the end of the day, when the public and the politicians want to know whether this regime change has been successful, the capacity to illuminate its real consequences—good and bad—with stories that are more than anecdotes, but stand as representatives of patterns we know to be statistically significant, is a powerful means of communicating what the numbers can only suggest.

THE CONTENT TOOL KIT

A wide variety of methodologies come under the broad heading of qualitative methods, each with its own virtues and liabilities. In this section, I discuss some of the best known approaches and sketch out both what can be learned from each and where the limitations typically lie. I consider sequentially potential or actual studies of welfare reform utilizing:

- open-ended questions embedded in survey instruments
- in-depth interviews with subsamples, of survey respondents
- focus groups
- qualitative, longitudinal studies
- participant observation fieldwork

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COMBINING QUALITATIVE AND QUANTITATIVE RESEARCH

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It is tempting to focus on abstract polarities when addressing the gulf between quantitative and qualitative approaches (e.g., the chasm separating “prediction” and “understanding”). However, it is more useful to examine practical aspects of these two approaches when trying to understand the difficulty of reconciling them. The sharpest contrast is between practical aspects of qualitative methodology and *textbook* presentations of quantitative methodology. In this essay I sketch eight basic tenets of “textbook” quantitative research that, to varying degrees, are routinely violated by qualitative researchers (summarized in the Appendix). I then describe a common problem in reconciling the results of qualitative and quantitative research.

1. PROXIMATE GOALS OF RESEARCH

Textbook presentations of social research usually focus on the goal of documenting general patterns characterizing a large population of observations. Typically, the study of general patterns is conducted with a sample of observations drawn from a large population. The researcher draws inferences about general patterns in the larger population based on his or her analysis of the sample.

By contrast, qualitative research focuses not on relationships between variables or on problems of inference and prediction, but on the problem of making sense of cases, selected because they are substantively or theoretically important in some way. For example, a researcher might use qualitative methods to study a small number of school shootings in an in-depth manner, selected because they all occurred in comfortable upper-middle-class suburbs and involved many deaths. To find out how they came about, the researcher would conduct an in-depth study of each shooting. This qualitative study would differ dramatically from a study conducted by a researcher who sampled school shootings from the population of such shootings (assuming this population could be delineated) and then characterized them in terms of generic variables and their relationships (e.g., the correlation between an area’s per capita income and the extent of counter-normative cultural baggage displayed by the shooters).

As these examples show, the key contrast is the researcher’s proximate goal: Does the researcher seek to understand specific cases or to document general patterns characterizing a population? This contrast follows a long-standing division in all of science, not just social science. Georg Henrik von Wright argues in *Explanation and Understanding* that there are two main traditions in the history of ideas regarding the conditions an explanation must satisfy in order to be considered scientifically respectable. One tradition he calls “finalistic” and is anchored in the problem of making facts understandable. The other is called “causal mechanistic” and is anchored in the problem of prediction. The contrast between qualitative research and ‘textbook’ social research closely parallels this fundamental division.

2. CONSTITUTION OF CASES AND POPULATIONS

Textbook presentations of quantitative methodology rarely examine the problem of constituting cases and populations. The usual *case* is the individual survey respondent; the usual population is demarcated

by geographic, temporal, and demographic boundaries (e.g., adults in the U.S. in the year 2003). The key textbook problematic is how to derive a representative sample from the very large population of observations that is presumed to be at the researcher's ready disposal.

By contrast, qualitative researchers see cases as meaningful but complex configurations of events, actions, and structures. Further, they treat cases as singular, whole entities purposefully defined and selected, not as homogeneous observations drawn at random from a pool of equally plausible selections. Most qualitative studies start with the seemingly simple idea that social phenomena may parallel each other sufficiently to permit comparing and contrasting them. The clause, "may parallel each other sufficiently," is a very important part of this formulation. The qualitative researcher's specification of relevant cases at the start of an investigation is really nothing more than a working hypothesis that the cases initially selected are in fact instances of the same thing (e.g., school shootings) and are alike enough to permit comparison. In the course of the research, the investigator may decide otherwise and drop some cases, or even whole categories of cases, because they do not appear to belong with what seem to be the core cases. Usually, this sifting and sorting of cases is carried on in conjunction with concept formation and elaboration. Concepts are revised and refined as the boundary of the set of relevant cases is shifted and clarified. Important theoretical distinctions often emerge from this dialogue of ideas and evidence. The researcher's answers to both "What are my cases?" and "What are these cases of?" may change throughout the course of the research, as the investigator learns more about the phenomenon in question and refines his or her guiding concepts and analytic scheme.

3. N of CASES

One key lesson in every course in quantitative social research is that "having more cases is better." More is better in two main ways. First, researchers must meet a threshold number of cases in order even to apply quantitative methods, usually cited as an N of 30 to 50. Second, the smaller the N , the more the data must satisfy the assumptions of statistical methods, for example, the assumption that variables are normally distributed or the assumption that subgroup variances are roughly equal. However, small N s almost guarantee that such assumptions will not be met in most social research. This textbook bias toward large N s dovetails with the implicit assumption that cases are empirically given, not constructed by the researcher, and that they are naturally abundant. The only problem, in this light, is whether or not the researcher is willing and able to gather data on as many cases as possible, preferable hundreds if not thousands.

By contrast, qualitative research is very often defined by its focus on phenomena that are of interest because they are rare—that is, precisely because the N of cases may be small. The key contrast with textbook social science derives from the simple fact that many phenomena of interest to social scientists and their audiences are culturally significant. To argue that social scientists should study only cases that are generic and abundant or that can be studied only in isolation from their historical and cultural contexts would severely limit both the range and value of social science.

4. THEORY TESTING

Conventional presentations of quantitative methodology place great emphasis on theory testing. In fact, its theory-testing orientation is often presented as what makes social science scientific. Researchers are advised to follow the scientific method and develop their hypotheses in isolation from the analysis of

empirical evidence. It is assumed further that existing theory is sufficiently well-formulated to permit the specification of testable hypotheses and that social scientific knowledge advances primarily through the rejection of theoretical ideas that consistently fail to find empirical support. Of course, the textbook view does not bar from social science those ideas that are generated in a more inductive manner, but any such idea is treated as suspect until it is subjected to explicit tests, using data that are different from those used to generate the idea.

It is without question that theory plays a central role in social research and that in fact almost all social research is heavily dependent on theory in some way. However, it is usually very difficult to apply the theory-testing paradigm in qualitative research. Most qualitative research has a very strong inductive component. Researchers typically focus on a small number of cases and may spend many months or years learning about them, increasing their depth of knowledge. Countless ideas and hypotheses are generated in this process of learning about cases, and even the very definition of the population (What are these cases of?) may change many times in the course of the investigation.

The immediate objective of most qualitative research is to explain the “how” of culturally significant phenomena, for example: How do school shootings happen? Theory plays an important orienting function by providing important leads and guiding concepts for empirical research, but existing theory is rarely well-formulated enough to provide explicit hypotheses in qualitative research. The primary scientific objective of qualitative research is not theory testing, *per se*, but concept formation, elaboration, and refinement. In qualitative research, the bulk of the research effort is often directed toward constituting the cases in the investigation and sharpening the concepts appropriate for the cases selected.

5. CONNECTING CASE ASPECTS

In textbook social science, the analysis of cross-case patterns is the primary means for linking aspects of cases. For example: is there a connection between a person’s educational level and that of his or her parent’s? The textbook method for answering this question is to compute the correlation, across many cases, between respondents’ educational levels and their parents’. This correlation gauges the strength of the connection between these two aspects. If the correlation is very weak, then the conclusion may be that there is no substantial connection.

Computing a correlation across cases, however, is very different from examining one case at a time to determine whether educational attainment is passed on from parent to child, and if so, how it is passed on. This alternate approach to the analysis of the connections between case aspects, which focuses on *how* aspects are connected within each case, is central to qualitative research. The key tasks are (1) establishing whether or not there is a connection between aspects within each case, and (2) assessing the nature of the mechanisms that animate and govern the connections that are identified. In qualitative research, connections between aspects are most often made *within* each case, not *across* cases, and the mechanisms governing a given connection may differ from one case to the next.

6. THE DEPENDENT VARIABLE

One of the most fundamental notions in textbook presentations of quantitative social research is the idea of the *variable*—a trait or aspect that varies from one case to the next—and the associated idea of looking for patterns in how variables are related across cases. For example: Do richer countries experience

less political turmoil than poorer countries? If so, then social scientists might want to claim that variation in political turmoil across countries (the dependent or outcome variable) is explained in part by variation in country wealth (the independent or causal variable). Implicit in these notions about variables is the principle that the phenomena that social scientists wish to explain must *vary* across the cases they study; otherwise, there is nothing to explain. It follows that researchers who study outcomes that differ little from one case to the next have little hope of identifying the causes of those outcomes.

Qualitative researchers, however, often intentionally select cases that do not differ substantially from each other with respect to the outcome under investigation. For example, a researcher who studies several school shootings studies instances of the same outcome—a shooting. From the viewpoint of textbook social research, however, this investigator has committed a grave mistake—selecting cases that vary only slightly, if at all, on the dependent variable. After all, the cases are all instances of shootings, the outcome to be explained. Without a dependent variable that varies, this study seems to lack even the possibility of analysis.

These misgivings about studying instances of 'the same thing' are well reasoned. However, they are based on a misunderstanding of the logic of qualitative research. Rather than using variation in one variable to explain variation in another, qualitative researchers often look for causal conditions that are common across their cases—that is, across all instances of the outcome. In fact, one of the very first tasks in the qualitative analysis of school shootings, after constituting the category and specifying the relevant cases, would be to identify causal conditions that are common across all instances. While using constants to account for constants (i.e., searching for causal commonalities shared by similar instances) is common in qualitative research, it is foreign to techniques that focus on relationships among variables—on causal conditions and outcomes that must vary substantially across cases.

7. CAUSAL ANALYSIS

The main goal of most analyses of social data, according to textbook presentations of the logic of quantitative research, is to assess the relative importance of independent variables as causes of variation in the dependent variable. For example, a researcher might want to know which has the greater impact on the longevity of democratic institutions, their design or their perceived legitimacy. In this view, causal variables compete with each other to explain variation in an outcome variable. A good contender in this competition is an independent variable that is strongly correlated with the dependent variable but has only weak correlations with its competing independent variables.

Qualitative researchers, by contrast, usually look at causes in terms of combinations: How did relevant causes combine in each case to produce the outcome in question? Rather than viewing causes as competitors, qualitative researchers view them as raw ingredients that combine to produce the qualitative outcomes they study. The effect of any particular causal condition depends on the presence and absence of other conditions, and several different conditions may satisfy a general causal requirement (that is, two or more different causes may be equivalent at a more abstract level). After constituting and selecting relevant instances of an outcome and, if possible, defining relevant negative cases as well, the qualitative investigator's task is to address the causal forces behind each instance, with special attention to similarities and differences across cases. Each case is examined in detail, using theoretical concepts, substantive knowledge, and interests as guides, in order to answer the question of "how" the outcome came about in

each positive case and why it did not in the negative cases (assuming they can be confidently identified). A common finding in qualitative research is that different combinations of causes may produce the same outcome.

8. NONCONFORMING CASES

It is well known that the social sciences are still in their infancy and that there is a lot of randomness at work in social phenomena. Further, social phenomena are usually portrayed as inordinately complex: the number of potentially relevant causal conditions for almost any outcome of interest to social scientists is thought to be great. On top of randomness and complexity, there is the additional problem of error. Social scientists make mistakes when they collect data and conduct their analyses, and most of their measures are quite primitive. For these very good reasons, researchers are routinely advised to use probabilistic models that allow for large “error vectors.” To try to account for every case is generally considered a sure path to causal misspecification.

Because *Ns* tend to be relatively small in qualitative research, it is possible for researchers to become familiar with every case. Thus, qualitative researchers often try to account for every case included in a study, no matter how poorly each may conform to “expected” patterns. Most qualitative investigators do not explain all their cases with a single model, even when their models allow for complex patterns of conjunctural causation. More typically, they confront nonconforming cases and account for them by citing factors that are outside their explanatory frameworks. Sometimes these attempts to explain nonconforming cases stimulate important revisions of theories.

PROBLEMS IN RECONCILING THE TWO APPROACHES

While the gulf between case-oriented and variable-oriented research seems great, it is possible to span it. Consider the following scenario: A researcher using qualitative methods studies several instances of an outcome (e.g., a small number of firms that are very successful in investing in and retaining top employees), documents causally relevant commonalities, and then constructs a general, composite argument about how these firms do it. This argument leads to four specific recommendations (which might be labeled X1 to X4) based on the observed commonalities. A second researcher reads the report of this study and decides to evaluate it with a large sample using variable-oriented methods. This researcher collects information on a random sample of firms and finds that as independent variables X1 to X4 do *not* distinguish more successful from less successful firms, using various measures of employee retention. In short, the second researcher shows that there is no statistically significant difference in the retention rates for firms with and without these four aspects, considering these aspects one at a time or in an additive, multivariate equation.

What went wrong? Usually, the researcher using variable-oriented methods will claim that the first researcher’s “sample” was “too small” and “unrepresentative.” Thus, the identification of X1 to X4 took advantage of specific aspects of the selected cases. The first researcher might counterattack by arguing that causally relevant commonalities identified through in-depth study are very difficult to represent as “variables,” and that the second researcher’s crude attempt to operationalize them fell far short. Indeed, the first researcher might argue that it would take in-depth knowledge of each firm included in the variable-oriented study to capture these conditions appropriately and contextually.

These criticisms and counter-criticisms are quite common. However, the incongruity between the two hypothetical studies can be resolved without resorting to derogation. The first researcher in this example—the one using qualitative methods—selected on instances of the outcome and identified four causally relevant conditions shared by the firms in question. In essence, this researcher worked backwards from the outcome to causes and thus identified potential *necessary* conditions for the outcome (Ragin 2000). Are these conditions truly necessary? In part, this is an empirical question. To gain confidence, the researcher should examine as many instances of the outcome as possible, to see if they agree in displaying these four causally relevant conditions (or their causal equivalents). But it is also a question about existing knowledge. Is the argument that these four conditions are necessary consistent with theoretical and substantive knowledge? Do they make sense as necessary conditions? If the researcher's finding is consistent with existing substantive and theoretical knowledge, then the argument that these four conditions are necessary is strengthened.

How should the second researcher—the one using variable-oriented methods—respond to the argument that these four factors are necessary conditions? At a more abstract level, the specification of necessary conditions is relevant primarily to the identification of cases which are *candidates* for an outcome. Cases cannot be candidates for an outcome if they do not meet the necessary conditions. But many cases may meet the necessary conditions for an outcome and still not exhibit the outcome because they lack additional conditions which, when combined with the necessary conditions, establish *sufficiency* for the outcome. In fact, the cases displaying the outcome may be only a small minority of those that meet the necessary conditions. Thus, while there are clear gains from specifying necessary conditions, as in the hypothetical qualitative study, the identification of causally relevant commonalities shared by instances of an outcome does not establish the conditions that are *sufficient* for an outcome. Thus, the variable-oriented researcher's finding that these four conditions do not distinguish low retention firms from high retention firms across a large sample of firms does *not* directly challenge the argument that these conditions are necessary.

The variable-oriented analysis of these four conditions across a large sample of firms is much more directly relevant to their *sufficiency*. To show that these conditions are jointly sufficient for the outcome, it would be important to demonstrate that when these four conditions are combined the outcome follows. In other words, the variable-oriented researcher could evaluate sufficiency by examining the correspondence between the combination of the four causes, on the one hand, and the outcome, on the other, across a large sample of firms. Still, this analysis would be an evaluation of sufficiency, not necessity, and the results of this analysis would not bear in a direct manner on the implicit claim of necessity made by the case-oriented researcher.

This sketch identifies only one of several ways to span case-oriented and variable-oriented inquiry. The general and most important point is that it *is* possible to join these two approaches if researchers are careful to distinguish between necessity and sufficiency and to separate the analysis of these two aspects of causation. More generally, this discussion underscores the distinctiveness of qualitative research, especially the case-oriented study of multiple instances of an outcome. Case analysis to strengthen and deepen within-case analysis.

Appendix

Textbook Quantitative Social Science	Qualitative Methodology
1. The main goal of social research is to document general patterns characterizing a large population of observations.	Qualitative researchers focus on the problem of making sense of a relatively small number of cases, selected because they are substantively or theoretically important in some way.
2. Cases and populations are typically seen as given. The ideal typical case is the survey respondent; the ideal typical population is a national random sample of adults. The key issue is how to derive a representative sample from an abundant supply of given observations	The qualitative researcher's answers to both "What are my cases?" and "What are these cases of?" may change throughout the course of the research, as the investigator learns more about the phenomenon in question and refines his or her guiding concepts and analytic schemes.
3. Researchers are encouraged to enlarge their number of cases whenever possible; more is always better.	Qualitative research is often defined by its focus on phenomena that are of interest because they are rare—that is, precisely because the N of cases is small. Also, the intensive, empirically intimate nature of qualitative research makes large Ns very difficult.
4. It is often presumed that researchers have well-defined theories and well-formulated hypotheses at their disposal from the very outset of their research; theory testing is the centerpiece of social research.	Existing theory is rarely well-formulated enough to provide explicit hypotheses in qualitative research. The primary theoretical objective of qualitative research is not theory testing, but concept formation, elaboration, and refinement, and theory development.
5. Investigators are advised to direct their attention to dependent variables that display a healthy range of variation. Outcomes that do not vary across cases cannot be studied.	Qualitative researchers often intentionally select cases that do not differ from each other with respect to the outcome under investigation (i.e., they are all positive cases). The constitution and analysis of the positive cases is often a necessary preliminary for the constitution and analysis of negative cases.
6. Researchers are instructed to assess the relative importance of competing independent variables in order to understand causation and test theory.	Qualitative researchers usually look at causation in terms of combinations. A common finding in qualitative research is that different combinations of causes may produce the same outcome.
7. Researchers study relationships between variables. They control for the effects of other variables when looking at the link between any two.	Qualitative researchers examine configurations of characteristics, seeing how different aspects fit together in each case.
8. Researchers give priority to cross-case patterns; the idiosyncrasies of individual cases are "averaged out" in cross-case analysis.	Qualitative researchers try to make sense of each case through within-case analysis and use cross-case analysis to strengthen and deepen within-case analysis.

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A FEW THOUGHTS ON COMBINING QUALITATIVE AND QUANTITATIVE METHODS

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Most qualitative researchers have come to their practice by dint of interest in questions of meaning and, equally, a concern even thirst for the richness of detail and nuance of social worlds that is difficult to capture by any other means. But there are myriad ways to meet this challenge. For the purposes of this workshop, I am piqued by the query: What are the most productive, feasible, and innovative ways of combining qualitative and quantitative research methodologies? Conventionally, this question encourages consideration on how qualitative data might enrich survey findings and/or what methodological approach should precede or follow the other (e.g., should interviews be used after a survey to help interpret responses, or . . .?). These are important questions and must be adequately addressed. Yet, it is also the case that there exists a far greater range of possibility for quantitative/qualitative hybridization from which might evolve answers to some of our most vexing methodological questions. These include but are not limited to: (1) How might we design the collection of our data such that we still realize the elusive search for ‘meaning’ and yet can do so in a manner that benefits from the more rapid data collection possibilities that survey methodologies enable; (2) How might some of the best attributes of qualitative work be better captured in survey design; (3) How and why might qualitative elicitation devices be combined with quantitative methods? To focus discussion on the first two points, I will draw on my training as an anthropologist with a particular interest in narrative forms. [By narrative, I mean both the storied talk that characterizes conversation, musings, and social discourse in everyday life as well as more formal definitions pointing to the attributes of this form including plot, narration, the imagistic and affective valence of a narrative vignette, and so on.]

The first question is often raised in multi-disciplinary policy contexts wherein a government body, regulatory agency or like institution seeks out a social scientist because they want to know what the ‘public’ thinks about a particular problem (e.g., how the public values a particular wilderness area or national park). Invariably, urgency prevails because a policy decision is pending; hence there is limited opportunity (and always limited funding) for what anthropologists colloquially call ‘deep hanging out’ in the field. When the ‘data’ needed are behavioural (such as how often a park is used, who uses it, whether or not certain areas have specific worth to specific peoples, etc.), carefully structured data collection such as that known as ‘rapid ethnographic assessment’ can be highly useful (see, for instance, S.Low, 2001). Where the goal is acquiring more elusive material, particularly that involving the kinds of moral or ethical depth that cannot be captured by direct question/answer formats – say musings on the beauty of a stream, the spiritual qualities of an old growth stand or one’s highly personal and not necessarily conscious attachment to an important physical place – the problem is more difficult. Meeting this goal requires that we solve what is essentially an articulation problem, that is, we must design interview of data collection protocols such that they enable study participants to articulate in shorter- than-usual-order the contextually, emotively, and morally rich stories and conversations through which we define ourselves and our attachments to the natural world.

In order to elicit such narratives, I have found three elicitation tools particularly useful (Satterfield, 2001). Each can be used in either interview contexts or via pencil and paper tasks. First, one can devise myriad variations on old style Thematic Apperceptions Tests (TAT) wherein participants are asked to tell a story about an unidentifiable person in a photograph pertinent to the research question at hand (e.g., of a person standing in forested area, following the above example). Providing people with the opportunity

to respond to and/or provide rebuttals to affectively charged pro and anti narratives about, say, logging activities can be equally useful. Such tasks ‘exploit’ any connections in participants’ minds between emotional investment in a point of view and expressions of values or ethics (Lutz, 1988; Stocker and Hegeman, 1996). In this second case, I have found that people are more articulate even expansive when the affective valence of such stimuli is subtle as opposed to strongly adversarial. A third option is to provide interviewees with the story of a policy dilemma in which is embedded a difficult moral conflict. He or she must then resolve the conflict and explain their preferred actions on the basis of their sense of a “just” “fair” or “moral” world. In each case, several hundred pages of written or transcribed responses are produced which can then be coded for their ethical or value content.

The second question above — how might some of the best attributes of qualitative work be better captured in survey design? — typically arises because of demands for statistical representation of a target population within a narrow margin of error. But such demands do not necessarily mean that all conversational detail and richer trains of thought more typical of qualitative work must be lost. A fruitful option is made possible by telephone surveys administered using CADI (Computer Aided Design Instrument) systems. Such systems can be programmed to enable particular sequences of questions, each contingent upon the participants’ answer to the prior question. In so doing, something of a conversation (albeit a limited one) is mimicked and captured by the design. Pathway surveys (Satterfield and Gregory, 1998; Gregory et al., 1997) are particularly useful when the goal is to understand what a groups thinks about (or how they might approach) a complex decision such as those faced by land managers (drawing again from the society/environment context in which I often work). The linked question sets can be used to unmask the situational richness and overall narrative train of thought that takes a study participant from point A (a stated value or objective) to point B (an endpoint management plan for, say, a forested area). Decision pathway surveys work to strengthen this link. Practically, they begin by specifying the decision context. In a forest policy context, the decision frame might involve specifying exactly what is being proposed, whether the action is novel or routine, whether it threatens wildlife habitat, and whether it benefits some communities and not others. Thereafter, all participants are asked an initial question to establish broad distinctions or paths of opinion (e.g., “Do you prefer ‘a’ or ‘b’?”). They are then asked a set of questions meant to tease out the reasons behind their initial response including an examination of the objectives behind their preference (“Is that because you want ‘x,’ ‘y,’ or ‘z’?”) and any concerns (risks) that may explain their reasoning (“In thinking about ‘a,’ do you worry about ____ or ____?”). That is, each response can be followed by a series of related questions about the participants’ reasoning processes; these questions would vary according to the person’s previous responses, thus forming a decision pathway.

In theory, there are as many possible pathways as there are participants but in practice the main point is to use pre-survey interviews and small group discussions to ensure that the survey designer characterizes in detail only those pathways that depict major opinion streams. In one example of this design (Satterfield & Gregory, 1998), thirteen potential decision pathways were available to survey respondents but only five paths attracted most respondents, while others attracted only a few participants, or none at all. Ultimately, pathway studies take standard survey methods to a more subtle level by helping participants think through conflicts or vagueness in their answers or elaborate the conditional limits to the meaning of stated value or action positions.

Let me turn, lastly, to my third question: How and why might qualitative elicitation devices be combined with quantitative methods? While surveys have been conventionally used to monitor public opinion, among other applications, it is frequently the case that one is compelled to combine the quantitative rigor

of surveys with the intrinsically democratic impulse of open public debate. This is in large part because our 'opinions' on many things are ill-formed and thus the answers provided in surveys are unstable – they thicken, take shape, and even undergo profound alteration in the face of debate or controversy. It is for this reason that some of the most novel experiments in the hybridization of methods are taking place at the level of civic governance. Such methods are frequently referred to of late as deliberative processes. Deliberation is a multifaceted term often used in reference to questions of democracy and governance. Deliberative democracy it is argued should not be achieved via the totality of the preference or opinion of individual members but through processes of civic engagement wherein people come together to learn, deliberate, and debate the means for achieving a common goal. In such venues it is not uncommon to present and debate myriad evidence, ethical arguments, technical information, etcetera for an extended period of time. Only after debate has taken place is more quantitative data collected. Typically it is collected in the form of both (a) privately stated individual judgments, and (b) as group judgments wherein those participating must articulate their position collectively. One outcome of these experiments is that they (a) provide survey-like data, (b) provide opportunities for the collection of data that is more ethnographic in quality – material on group discussion content as well as group dynamics, and (c) provide a quantitative summation of the judgments that reflect collective or social processes as findings in their own right and as interesting comparisons to aggregated individual judgments.

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DESIGNING QUALITATIVE RESEARCH PROJECTS

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Qualitative research methods are often mischaracterized by advocates, users, and critics alike because too often the reflexive, iterative, and flexible methods are misunderstood as ‘just making do.’ There is a good pragmatic tradition of “making do,” from Dewey to the present, that describes the necessities as well as virtues of using what situations provide in their immediacy as the grounds of social action. While qualitative research certainly shares some of this pragmatic bricolage, good research, qualitative as well as quantitative, is designed as well as improvised. One of the merits of qualitative research is its particular openness to serendipitous invention; one of its failures, however, has been an unwillingness, or inability, on the part of its practitioners, until recently, to specify how that openness to ‘what situations make available’ can be both systematic and creative.

Over the years I have probably reviewed hundreds of research proposals; too large a number of these claimed that because the researcher was doing a qualitative study, the kinds of data and forms of collection could not be specified in advance. I was always a bit embarrassed by this, feeling let down by my side. It sometimes seemed as if our teaching of qualitative research was creating a mystical religion, a set of our own unexamined fetishes just at the moment we set about to ‘identify others’ taken for granted assumptions and social meanings. In this vein, some years ago I heard a colleague advise a student going out to do field work for the first time “to be like a blank slate,” “just tell me everything you see and hear, write it all down.” The student was completely baffled and clearly at a loss about what to do, to do first, or second, or how to begin. What would constitute telling me all you see and hear. Importantly, the student had read a lot of sociology, and knew a lot about signs and signifiers, latent as well as manifest patterns in social relations. She knew that competent social actors are not blank slates. She felt incompetent but not entirely blank. She had a project, after all.

It seemed from the proposals I read and the conversations I observed that we, qualitative sociologists, believed that we could not specify what we were going to do (i.e. lay out a design and plan of the research), because that would mean that we would have — by that naming — necessarily circumscribed what we would do. Having supposedly controlled a priori what we would do, we would be unable to do something else along the way, as the situations and insights invited. We would have lost the distinctive virtues of qualitative research. Somehow, in this mysticism about qualitative methods, research designs seemed to be understood as enforceable contracts or sets of machine instructions; any deviation from the design was understood to be either impossible, a failure, or a mistake. Qualitative research was celebrated for its flexibility; the temporal coincidence of collection and analysis and thus prior design was, by definition, a threat to qualitative research.

Of course, I have overstated the issue but we were asked to provide fodder for discussion. And, to some extent, this overstatement puts the issue in a bold form. Why should qualitative research be any less well designed (or specified) than quantitative research? When I think about the steps in different methods, it occurs to me that most of what gets put into a research design, let us say for a survey project, could also be put in the design for an ethnography or a project of in-depth-interviewing and narrative analysis. The major differences lie in the fact that qualitative projects will not rely on statistical analyses and therefore do not need to produce probability samples and standardized collection instruments at

the same temporal pace and placement in the research process. As a consequence of temporal pace and sequencing, qualitative projects will be able to adjust the forms of data, modes and sites of collection in response to the ongoing processes of analysis and interpretation. This is certainly so. I suspect, however, that the resistance to detailed research qualitative research designs derives less, however, from emphasis on these key differences than from an overly idealized or reified view of how other forms of research proceed, whether quantitative sociology or chemistry or biology. That is, all research develops (is in the making and rethinking) throughout the stages of design, collection, and analysis. Almost all research produces much that was unanticipated and therefore had to be responded to with adjustments along the way. The central difference lies in the explicit weight of recognition of and preparation for this process of adjustment in most qualitative projects. Nothing precludes a preliminary design that sets the researcher on a path that is understood as a first approximation of the work process.

I should say before going much further that there are varieties of qualitative research and my remarks will not appropriately characterize all. For the moment, I am referring primarily to ethnographic fieldwork, participant observation, in-depth open-ended interviewing, and other work involving interpretative qualitative analysis of documents of various sorts. The mode of analysis rather than the type of data more appropriately describes work as qualitative. (The content of documents and interviews can be analyzed quantitatively or qualitatively. Observations can be systematically structured and quantified but much observation is not, nor would be productive.)

The goal of research is to produce results that can be falsifiable and in some way affirmable by rational processes of actors other than the author. Most important is that the researcher provide an account of how the conclusions were reached, why the reader should believe the claims and how one might go about trying to produce a similar account. What makes science morally, and rationally, compelling is that it is a public enterprise. I am not referring to the funding or organizational supports. Rather, science is distinguished by the claim to produce shared understanding through modes that can be rationally and collectively apprehended. In short, we have an obligation not to “hide the ball.” To the extent that we do “hide the ball,” we transform our science into rhetorical performance.

Quickly then, research can be and should be designed. I mean nothing more than to specify a plan of action, a plan that is understood at the outset to be revisable as the situation and understandings develop. Research designs include:

- a description of the conceptual topic and aspect of social relations to be studied;
- a review of what is already known about this and the ways in which that knowledge was produced;
- what is not known and needs to be explored;
- identification of the population or setting about which the researcher will draw conclusions or develop hypotheses (theories), (e.g. who will be observed, interviewed, differentiated by status? gender? organizational location?);
- justification of the focus on these population(s) and setting(s) as likely to be generative for what needs to be explored further from what is already known;
- what forms of data will be collected (e.g. observations, interviews, documents);
- how the data will be put into a form appropriate for manipulation and analysis (e.g. through notes in computer files, visual images, transcribed tape recordings);
- how these data sets will be analyzed and synthesized (e.g. by conceptual coding, by textual or narrative structure); and
- how will the results be reported.

Nothing here precludes flexibility, iteration and adjustment to the situation as it develops. I will conclude with some excerpts from the research design in a recently funded NSF proposal.

FROM: SAFE SCIENCE: GOVERNING GREEN LABORATORIES (SUSAN SILBEY, 2002)

...IV. Research Design and Work Plan

For this project, I will be conducting ethnographic fieldwork in the University to document and analyze the creation of a new EHS system for research laboratories. The fieldwork activities include interviewing, observation, and document collection. It is sometimes supplemented by systematic data collection with standardized instruments for observation and via small surveys. According to Van Maanen, “fieldwork usually means living with and living like those who are studied. In its broadest, most conventional sense, fieldwork demands the full-time involvement of a researcher over a lengthy period of time (typically unspecified) and consists mostly of ongoing interaction with the human targets of study on their home ground” (1988:2). Ethnography is the written product

In this project, I will attempt to represent accurately the current and changed practices in the laboratories, the EHS office, and the administration of the University. While representing the daily routines, decision-making processes, and official policy changes, I will also depict these everyday practices in and through the lenses provided by social scientific research on regulation and laboratory science. I will be guided in my observations and interviews by the questions derived from previous research on regulation, organizational cultures, and scientific laboratories as outlined above. Finally, an ethnography that is sensitive to social structures seems the only way such a study could be done. Because my focus is the intersection of three social phenomena—state regulatory bodies and regulations, university organization, and scientific laboratories, my sites include the university structures, changes in those structures in response to the regulatory mandate, the scientific laboratory and changes in response to regulation...

Formal Committee Meetings. In April 2001, I began observing committee meetings, small group meetings, as well discussions among the University’s counsel and staff as they completed negotiations of the consent decree and initiated the process of designing a new EHS system. Since September, I have been aided by a research assistant who attends meetings during my class hours or large meetings where it is helpful to have more than one observer. The formal meetings include those of a committee for facilities, a committee for research laboratories and a committee of administrators and faculty overseeing the work of these two working committees. One or both of us attend each meeting, sitting silently at the side of the room, taking notes on the proceedings. I sometimes interview members of the committees individually; we receive copies of all documents and have been included in all mailing lists.

Interviews with key informants. I have also been interviewing senior administrators, key faculty, and members of the committees overseeing health, safety, and environmental policies and practices. The purpose of these interviews is to develop a firsthand understanding of the decision-making process and goals of the Environmental Management Systems (EMS), as well as the history of the negotiations that led to the consent decree.

Laboratory visits. Since September 2001, I have been visiting laboratories and interviewing lab directors, PIs, some safety and chemical hygiene officers within labs and on the EHS staff. I have also been

observing the meetings of facilities managers as they discuss the forthcoming changes in the EHS system and their concerns. From these observations and interviews, I have identified the variation that seems to exist in organizational structure, risk, and past performance.

Work to be done: I will continue these activities: observing committee and small group, as well as large public, meetings, and interviewing key informants. In addition, I will be expanding the research sites to include the EPA agents, the EHS management office, and more consistent daily observations in the laboratories and facilities.

One-on-one interviewing. I have completed some in-depth, long interviews. I will be doing more throughout the project. Some interviews will be more formal scheduled sessions, the questions prepared in a semi-structured protocol, i.e., a series of open-ended questions that are designed to allow the respondent to describe, in his/her words, the rationale, goals, problems and policy solutions. The interview will be developed, however, only after more observation in the laboratories, and of the committee work, so that the questions will respond to what I have observed. In addition, interviews will be conducted, informally, with students working in laboratories, while they are working there, and with staff and faculty before and after meetings which include the large committee meetings, small group discussions, and large public meetings organized to solicit feedback from the University community. For the most part, these informal interviews seek informants' interpretations of what is happening in the meetings or laboratory. Usually, I ask questions in the context of an ongoing observation or conversation among the parties I am observing.

EPA administrators and agents. Also, I hope to be able to interview the agents who inspected the University three years ago and the lawyers who negotiated the consent decree. My interviews with the University's attorneys have provided some background for the project, but I hope to gather additional information about the EPA's perspective.

Environmental Management Office. With this grant, I hope to place a research associate in the Environmental Management office for daily participant observation. This office is a new phenomenon in the University and one of the first products of the consent decree. Until the last summer, responsibility for overseeing various forms of safety and health hazards on campus had been distributed among 20 or more different offices (e.g. radioactive materials, biological materials, toxic waste, environmental health, chemical hygiene, lasers, fire prevention, etc.). A person seeking permission to use radioactive materials would call one office and call another to discuss a proposed laser experiment. If the sample to be analyzed included biological materials, a third distinct person needed to be brought in to secure the local campus "license" to use the materials or equipment. No research could proceed without the local permits, but coordination was the individual researcher's responsibility, compliance with the permit was also the individual researcher's responsibility, and if there were any problems, it was the individual researcher's job to find the right staff office and person from whom to get help. This was the system of dispersed responsibility that the EPA considered no system and no accountability for compliance. The first step of reorganization has involved the creation of a central Environmental Health and Safety office with a hierarchical structure and division of labor for the distinct laboratories and investigators on campus. The current slogan used by the EHS to characterize this transformation is "one number, one person." All departments and laboratories on campus will eventually be assigned a dedicated EHS liaison who will collect from among her colleagues in the EHS office the relevant persons and expertise needed

for the particular laboratory and experimental materials and conditions. These changes are just beginning with the move to new offices as the first step, spatially as well as organizationally consolidating the expertise that had been distributed across the campus.

The EHS office is an example of what Guston (2001) calls a boundary organization. The changes in this office are critical for the laboratory scientists because, “for most people, the legal system is both remote and arcane, and popular understandings of law and legality come largely from day to day experience in concrete bureaucratic settings, not from exposure to abstract doctrine (Macaulay 1987; Sarat 1990; Ewick and Silbey 1992, 1998a, Fuller et al, 1997). In mundane organizational encounters, formal structures - [such as an EHS office] - symbolize commitment to legal objectives, while informal norms give content to legal principles” (Edelman and Suchman). Thus it is critical to observe the relationship between the transformations in this office and transactions with the laboratories.

Laboratory Observations. Laboratory observations are key to this project. These will take place across a spectrum in which past practice and need for improvement varies with the authority structure and degree of environmental and health risk in the site. I have approached members of the faculty for permission to “hang around” their laboratories, and for my research assistant to do so as well. Interestingly, every member of the faculty whom I contacted agreed to our research in their laboratory. Although it is important to follow the discussions out of which the EHS system design is emerging, it is even more critical to trace the ways in which the law, the EPA, and the regulatory regime is being interpreted and responded to by actors within the organization. The entire EHS organization is created and mobilized to serve the research ongoing in the laboratories and, thus if we are to bridge previous research on regulation with an analysis of the organizational contexts of compliance, we must spend most of our time at this ground level of EHS practices. The culture of autonomy and freedom that characterizes the university has its *raison d’etre* at this ground and center of the University’s organization. If there is to be compliance, or violation, of federal law, it will be in the laboratories.

Although the variation among laboratories resembles the format for presenting the results of a quantitative data analysis more than a project of participant observation and ethnography, it seems useful to represent the systematic nature of the fieldwork. By conducting interviews and observations in laboratories and facilities that vary along these dimensions, I will be able to distinguish compliance practices in organizations that are within the line authority of the University administration (where staff are directly accountable to supervisors with responsibility for evaluation and termination) from compliance practices in the domains of academic freedom, with mentoring relations between faculty and students, and collegial relations among faculty and department chairs and Deans. By studying the laboratory/facilities practices and the EHS office, I will be able to compare the interpretations of law and regulation (of what constitutes risk and safety, of what may provide minimal versus sustainable improvement) by those directly enacting those practices with those responsible for providing only technical assistance.

Data management. All field notes are typed up using Microsoft Word and kept in files organizationally and by topic. We record in our notebooks a description of what is going on in front of us and our queries about what is happening. These notes are typed up at the end of every day or at most at the end of two days. All tape-recorded interviews are transcribed by a person hired for this purpose. The transcriptions are also kept in Microsoft Word files. These are backed up regularly, and printed out as completed. Because of the number of interviews already conducted, ongoing, and to be continued, management of the transcriptions and files this is a time consuming process. I am planning to hire a person to work 3/4 time on this task alone rather than hire an assortment of people to do the transcribing as I have been doing.

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COMPLEMENTARY ARTICULATION: MATCHING QUALITATIVE DATA AND QUANTITATIVE METHODS

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Writing a generation ago, Neil Smelser and R. Wallerstein (1969) proposed one useful way of combining qualitative and quantitative methods, through what they called “complementary articulation”: bringing to bear different disciplinary perspectives and/or methods of data gathering and analysis to shed light on different aspects of the same reality. Part of Smelser and Wallerstein’s point was to debunk the flawed notion that simply using different disciplinary lenses or methods would automatically yield enhanced understanding. In fact, they argued, it is just as likely to yield mutually unintelligible findings, or findings which do not add cumulatively to understanding, as to yield findings which do enhance understanding this way. My starting point is to reflect on the qualities of qualitative and quantitative data and analysis, and to argue that NSF would do well by creating spaces for research that explores not just how to combine, but how to *match*, these qualities in ways that yield complementary articulation. As the reader will discern, combining these methods this way is a project I am beginning to pursue in earnest – I am an ethnographer by nature and practice – and hence I have more questions and ideas than concrete answers about how to do this. But I do hope my ideas offer fodder for good conversation.

Why do COMPLEMENTARY ARTICULATION?

Before delving into how to do or at least begin to think about complementary articulation, the question raises its head – Why do it? If in fact we have learned about a particular set of processes using qualitative methods, why go to all the extra bother of using quantitative methods to analyze the same data or processes? Here I should also differentiate between different usages of combined quantitative and qualitative data and analysis. A first kind of usage is, one might argue, actually the norm among most qualitative researchers: the use of qualitative data to analyze the underlying processes under study, with the use of quantitative data to frame and situate the case or cases under study within the larger world, both to make sense of their potentially larger meanings and to ward off the charge that one is “telling stories” and not doing social science. This is a way of using quantitative data to gauge the representativeness of a qualitative study. A second usage is to combine qualitative and quantitative data and methods to study the same processes, to come at them from a different angle and attempt to take a picture (or video, if you like, emphasizing process and change over time) of them with a different camera. A third usage is when a quantitatively driven project includes a qualitative component, sometimes thoughtfully incorporated onto the main project, and sometimes stapled on as an afterthought. Here, often the privileging of representativeness as an analytical virtue in the quantitative research design means that the qualitative work is so “nested” within the quantitatively driven questions that the work has little chance to produce emergent insights and re-direct subsequent research towards these themes. Here, the qualitative work indeed serves mainly to provide quotes to illustrate quantitatively derived points.

Ok, so why do it? There are a number of reasons I can think of, and I welcome fellow workshop wayfarers suggestions for others. A first reason is that qualitative analysis almost never simultaneously deploys most or even much of the collected data in its analysis, at least not in the same way as quantita-

tive work does. I do not see that as an Achilles heel of qualitative research that makes it a servant to quantitative research – as in the formulation that qualitative research can “generate hypotheses” that can be rigorously tested by “real” science, in quantitative methods. Such formulations overemphasize the limitations of qualitative research while diminishing or denying those of quantitative research. But there is a valid issue here, especially if representativeness is a valued quality in research, as I believe it should be, in combination with others. If we theorize from the most interesting cases we encounter in our fieldwork, we both have the potential to gain leverage into otherwise hidden processes, and to be led astray by those cases particularities. While the latter need not happen (see Burawoy’s 1998 analysis of reflexive analysis for ways to avoid such a problem), it is a legitimate concern. I was delighted by Mark Turner’s imagination of the quantitative objection to the cognitive scientist who presents his audience with a talking pig – that’s an “N of 1; show me another talking pig!”. Yes, analyzing the talking pig would be fascinating, and if we interviewed it, it would no doubt tell us a great deal, and that work would be valuable. But we would also want to be careful about how we generalized our findings from the talking pig to all pigs. (I will leave this analogy now, before we get carried away by it...). My point here is that to the extent possible, it makes sense to go the trouble of finding methods to analyze as much of one’s ethnographic data as possible, which in practical terms means finding ways to convert some of that qualitative data into numbers that can be coded, and entered into a data base and analyzed on a computer program.

A second reason to do complementary articulation is because most quantitative methods are weak in analyzing how things actually work, in describing process, the strength of most qualitative analysis. Regression analysis, for example, does a splendid job in controlling for effects of a variety of factors, but it is at a loss for telling us how these factors actually affect the dependent variable. For that, quantitative scientists must either speculate, sometimes convincingly, or use qualitative methods to fill in the how.

A final consideration here is that most quantitative data and qualitative data are, in fundamental ways, produced by the same process. Both require what Roberto Franzosi calls “rewrite rules” by which some observed event, thing etc is converted into a datum by the human work of recategorizing it as a “strike” “a coup” “a drop out” etc. In quantitative analysis the things are rewritten as data and then considered to be objective, while in qualitative data the rewriting is often more complex and closer to the final analysis, and hence seen as more subjective. Contingency is more inherently a part of ethnography process, and stays alive as a possibility much further into the data gathering process in more ethnographic projects than in quantitative ones. Hence ethnographic or embedded interview studies are more likely to be able to respond to emergent themes in the research, even to re-orient a significant part of the project towards a new or emerging theme. Among other virtues, this means that qualitative researchers can go into the field with their particular pre-conceived notions – similar perhaps to those one has in designing a large survey, which uses specific questions to get at particular ideas – and be wrong in how things work, but not have to live with that mistake, so to speak, in the same way. Reorienting one’s research can make late insights valuable questions to pursue, not 11th hour revelations to regret. Yet given that rewriting is involved in both processes, it should be within our power to attempt to rewrite some of our qualitatively gathered data in quantitative form. I would like here, however, to underline a fundamental difference in most qualitative and quantitative research, which is that the more qualitative the work is, the more the data are embedded within and indeed depend upon the quality of the relationships the ethnographer/interviewer has with his or her informants.

FORMS AND QUALITIES OF QUALITATIVE DATA

A first point in discussing complementary articulation is that variety of forms and qualities of qualitative data. A first general distinction is between interviews and ethnography (or participant observation). The obvious difference here is that the former gives you what people say that they do, while the latter shows you what they actually do. (I do not automatically assume that watching what people do gives better insights than does talking to them – recall Geertz' query of whether the native is winking, twitching or has something in his eye – and would endorse something like Becker's criteria for judging ethnographic work – is it close up work, can it accommodate contingency, and does it show knowledge of a lot and not a little of the thing under study?) But there is more to be said here. In particular, different kinds of interviews are going to yield different kinds of data and different kinds of potentials for combination with quantitative methods, and for triangulation with ethnography or other qualitative analysis, such as of narratives. Part of the difference in the kinds of data yielded results from the different theory of interview inherent in the interview method (see Gubrium and Holstein, 1997), and from the degree to which the interview itself is embedded within other methods such as ethnography that themselves are embedded within relations between the ethnographer and the informant/s. On one end of this spectrum is the survey and on the other end is an ethnographically embedded group interview. I offer a non-exhaustive list drawn from an ongoing project on second-generation immigrant education and gender. The point in explicitly reviewing the qualities of these various kinds of interviews is that the goal of complementary articulation as I outline it is to match the qualities of the data with the qualities of the methods – to bring to bear on the data tools that can actually analyze that data, be they qualitative or quantitative.

Survey – Surveys embody the “vessel of answers” theory of the informant and interview, and usually assume a minimal relationship between the informant and research/question asker. The idea here is to get the information from its source and usually end the relationship.

Ethnosurvey – A souped up version of a regular survey, developed by Doug Massey and his colleagues, this method is designed to get the information more conversationally, but still with the same theory of interview and informant as a regular survey.

Guided Narrative interview – Informants are asked about a set of themes that emerge during a year or more of preliminary fieldwork. It includes both survey type questions that seek specific information, and larger spaces for telling their story, and specific devices to elicit narratives that can be subjected to particular forms of analysis (one of these, the Thematic Apperception Test, or TAT, I will discuss briefly below.). Multiple theories of the interview are included, ranging from the vessel of answers view of the informant, to the mutual construction of a narrative by the asking and answering of questions, and the attentive listening of the interviewer, especially if an ethnographic relationship also exists.

Ethnographically Grounded interview – Such interviews are done with key informants either at regular intervals or after a particular set of events occurred, usually when the ethnographer and informant were both present or both participated in the event. Such interviews seek to get the informants' understanding of what they are up to, and why, and benefit from shared experience by informant and ethnographer.

Grounded Group Interviews – These interviews involve several key informants discussing key themes or particular events. These are especially useful because they engaged the informants in discussions of issues at hand and thus enabled us to observe how informants negotiate their respective positions, thus coming closer to apprehending social reality than, for example, surveys or retrospective solo narrations.

Life history interviews – In their ideal type form are completely open ended interviews, where the informant tells his or her story, offering his (her) own narrative as he (she) understands it. The benefit here is to get the whole story as a piece, and see how the parts of the narrative have been assembled, using what themes, etc.

COMBINING QUALITATIVE AND QUANTITATIVE DATA AND METHODS

In suggesting some ways that qualitative and quantitative methods can be combined, I will draw on two brief examples from my own on-going work. Both examples involve sequence analysis, in its qualitative and quantitative forms. In my work on educational and work mobility of second-generation Mexican American boys and girls in New York, I noticed a sequence of steps that predicted positive and negative outcomes in both areas. For example, if one went to the zoned high school – instead of to a better, out of zone high school – and if one's primary friendship groups went from being pan-minority to being mainly Mexican, the chances increased that you would drop out of high school and perhaps join a gang, especially for a boy. Meanwhile, I detected a set of processes by which girls both got better information about how to apply to out of zone schools and had different experiences of these schools. The end result was a very different experience of being an ethnic Mexican for girls (and some boys) who were upwardly mobile, and being a racialized Mexican for boys (mainly) who were not upwardly mobile. I arrived at this analysis using all of the qualitative data described above: ethnographic work in and out of schools with girls and boys, standard narrative and repeated grounded interviews with them and their main friends, and others. Yet even in doing this, the available data that can be deployed at once is a small fraction of that collected. I am currently working out a way to formalize this qualitative sequence analysis using quantitative methods which would use most of the cases collected. The guiding idea here is that my qualitative analysis of these series of turning points is an analysis of sequence in much the same way as more formal sequence analysis of the Abbott variety is. Here the comparison would be between the entire sequence, and the measurement between sequences would be of how many changes in one would be necessary to change it into another. My hunch is that such analysis will offer graphic and numerical representation to several sequences of upward and downward mobility I have identified using qualitative analysis from these other data. Concretely, it involves a series of rewrite rules for simplifying complex processes into their constituent pieces, captured in ethnography and different interviews, in a form that can be analyzed in a computer.

The matching in this case is between the kinds of social processes being studied and the quantitative and qualitative methods being used. I noted, using qualitative methods over time that some sequences led to particular outcomes, and can describe this using direct ethnographic observation and interviews, but also re-analyzing this same data in quantitative form. This will give me the capacity to analyze at once the entire sample of more than 100 long interviews, while also making interpretive sense out of the meanings and motivations of the youth under study.

A second application is the use of the TAT to trace out and help explain an unanticipated difference in the men's and women's responses. For those unfamiliar with it, the TAT as used here, and by the Suarez-Orozco's, 1995, is a narrative elicitation device, in which the informant is shown a picture and asked to make up a story with a beginning, middle and end, and tell what the person in the picture is feeling. Contrary to expectations, my findings were that women were more likely to have higher aspirations and have more individualist orientations than men. Moreover, their stories tended to have contingency and also the possibility of controlling one's destiny built into the stories. For example, the

women's TAT narratives tended to have more if-then constructions, while the men tended to see the fates of the people in the pictures as sealed. I plan to adapt what Franzosi, drawing on linguistic and sociological techniques, calls "story grammars". He distills stories into subject-action-object (SAO) form, and finds patterns in accounts of protest events. To analyze the differences between boys and girls TAT stories, I will adapt his technique, formulating new "rewrite rules" to create story grammars that include if-then sequences and other patterns seen in the data. There is greater contingency in the girls' stories, and I want to represent this numerically and graphically. My hunch is that the data will yield story grammars quite similar for most girls and upwardly mobile boys, and for downwardly mobile girls and most boys.

The matching here is that the data under consideration – the stories informants tell about TAT pictures – have qualities that can be profitably analyzed using quantitative and qualitative methods. I can embed these stories within a longer narrative of the informant taken from their interviews, and for some, from ethnographic work with them. And there are a set of internal patterns, story grammars, to these stories that reflect underlying orientations of the boys and girls. For example, the girls greater use of if-then constructions in the stories they tell suggests they feel that they have greater agency in their own lives, while the boys are more likely to see their lives as pulling them along, not under their control.

WHAT SHOULD NSF DO?

There are number of possibilities. One of these is to create a research initiative that explicitly seeks to link the use of quantitative and qualitative research as part of a larger initiative in qualitative methods. By this I do not mean the incidental use of qualitative methods to bolster claims made by research mainly driven by quantitative data and methods. Rather, I mean a serious use of both sets of methods to analyze central processes. This would require in many cases the exploration of how different methods applied to the same larger data set, rewritten into different forms, can help us understand the same reality under study. An ideal way to do this work would be qualitative work that has yielded significant insights about the processes under study, and which has data that is re-writeable in quantitative form. In this case, the theoretical payoff would be methodological and epistemological. In one scenario, the quantitative methods and analysis might not yield new or different findings than the qualitative methods and analysis. The researcher would, however, have been able to deploy very different epistemological approaches and methods, but come up with the same substantive findings. In another scenario, these methods would yield different outcomes that would refine the main substantive findings of the qualitative analysis, thus yielding additional insights. In another scenario, they might yield apparently conflicting results, thus suggesting that further exploration is needed.

A second suggestion would be to fund training that includes serious use of both kinds of methods. In such a program, NSF would fund research projects that trained doctoral students in two ways: first, by pledging them to take (or taking students who have already taken) a series of methods courses that include both quantitative and qualitative methods; second, they would develop their capacities in these areas more in working on a faculty project, or on their own project supervised by faculty members competent in both methods. The range of methods here to be considered need not be limited to the normal tripartite division between ethnography, interviewing, and statistical analysis. It could include ethnographic observation with conversation analysis and quantitative enumeration of specific kinds of interactions, and use of visual data (videos or pictures), for example. It could, and with the emphasis on matching, should be further broken down to make the case for the particular combination of methods that match both the social processes under study and the data used to represent those realities.

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THOUGHTS ON ALTERNATIVE PATHWAYS TO THEORETICAL DEVELOPMENT: THEORY GENERATION, EXTENSION, AND REFINEMENT¹

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My objective in this essay is to broaden how we think and talk about the relationship between qualitative research and theoretical development. The term theoretical development directs attention to the various ways or processes through which theories emerge, change, and grow in scholarly work. Qualitative research – whether it is based on ethnography, in-depth interviews, or historical documents – can contribute to theoretical development as much as quantitative research based on assessing the hypothesized relationship between two or more variables. That it doesn't, or at least is seen as not doing so very convincingly, can be explained in part in terms of two persistent problems/dilemmas. One is that the language of theory testing or, more commonly, hypothesis testing, frames too narrowly the connection between research and theory, so much so in fact that it functions as a kind of hegemonic master frame. Thus, I prefer to think in terms of the relationship between qualitative research and theoretical development (or theory building). Compounding the problem is that much, and perhaps most, qualitative research fails to maximize its potential theoretical yield because its practitioners too often enter the field with only the goals of description and interpretation to guide them, treating theoretical development as a black box or ignoring it altogether. A major reason for this short-circuiting is the absence of a robust vocabulary (one that goes beyond ritualistic reference to “grounded theory”) within qualitative research that not only facilitates theoretical development, but that focuses attention on it as a defining feature of qualitative research. The reasons for this conceptual impoverishment are partly rooted, somewhat ironically, in the two traditional objectives and rationales for engaging in ethnography: to discern, grasp, and understand the world at hand from the standpoint of its members or practitioners so that, in the words of Geertz (1973: 6), one can distinguish between a wink and a twitch; and/or to uncover and describe what one needs to know (the structure of rules) in order to behave and thus, in Goodenough's (1966) words, “pass” as a member of the world studied. Although the traditional pursuit of either an insider's perspective or the rules for passing clearly facilitates understanding of the relevant contexts, it seldom beckons researchers to assess or extend theoretically their resultant findings or claims. Readers may nod and acknowledge the intuitive interest of an ethnographic report inasmuch as it reveals aspects of a social world with which they are unfamiliar. But such acknowledgments are often followed by dismissive statements such as, “Interesting, but so what?” “Where do we go from here?” or “What's the theoretical question here?” (And isn't this how we respond in part to many of the qualitative papers we review and reject?) In other words, although ethnographic reports founded on one or the other of the above objectives may be empirically grounded and informative, they too rarely generate compelling theoretical formulations or illuminate clearly some extant theory. One of the reasons for this failure, as suggested above, is the lack of a robust vocabulary within qualitative circles that facilitates theoretical development and makes it's a priority. In order to move us forward in broadening how we think and talk about the relationship between qualitative research and theoretical development, I sketch three alternative paths to theoretical development: theoretical discovery or generation; theoretical extension, and theoretical refinement.

THEORETICAL DISCOVERY/GENERATION

The best-known and most frequently referenced path to theoretical development in the context of qualitative research is “discovery” via Glaser and Strauss’s “grounded theory” (1967). The basic idea is that case or substantive-specific (e.g., conversion, dying, medical education) analytic understandings are discovered or, more accurately, generated, through detailed examination of fieldnotes of one’s observations and/or interviews (see Charmaz 2001, for discussion of analytic procedures). In turn, contingent on the availability of time, resources, and interest, the findings may be assessed and revised in a constant comparative process by looking for similar yet different instances of the phenomena initially observed with the objective of generating a more abstract, formal theory (e.g., status passage with respect to conversion and dying, or socialization with respect to conversion and medical education). A good example of theoretical generation via these means is Lofland and Stark’s (1965) theory of conversion grounded in Lofland’s ethnographic case study of a small religious “cult.”

Although there are a number of prominent examples of “grounded” theoretical generation within qualitative research, they are not plentiful – partly because too few researchers refine their analyses in a way suggestive of a substantive-specific theory, or, if they do, they seldom proceed to the next step of fine-tuning and elaboration. Additionally, it is my sense that relatively few ethnographers actually engage in theoretical discovery in the systematic manner Glaser and Strauss had in mind, and in a fashion Charmaz (2001) has clarified. Nonetheless, the idea of grounded theory is invoked routinely as a kind of ritualistic account or rationale for conducting the research described, with the result that the research is not very compelling and the logic of grounded theory as a means to theoretical development is diluted. These troubling tendencies notwithstanding, there are a sufficient number of compelling works that are based on or reflect the logic of grounded theory to suggest its utility as a qualitative path for theoretical development.

THEORETICAL EXTENSION

A second such pathway is “theoretical extension.” In this process, one does not generate or develop new theory *per se*, but extends pre-existing theoretical or conceptual formulations to other groups or aggregations, to other bounded contexts or places, or to other sociocultural domains. As such, theoretical extension focuses on broadening the relevance of a particular concept or theoretical system to a range of empirical contexts other than those in which they were first developed or intended to be used. Its logic is anchored in Simmel’s (1950) formal sociology, which posits the identification and elaboration of trans-situational social processes and types as sociology’s objective. Thus, attention shifts from the search for factual novelties and the peculiarity of actual events to the search for patterns across a multiplicity of situations or contexts (Zerubavel, 1980).

Numerous examples of extension can be found in the ethnographic literature. Illustrative is Morrill’s (1995) ethnography of conflict among top managers in private American corporations, wherein he extended early anthropological work on “vengeance” to corporate boardrooms. Although the contexts in which Morrill conducted his fieldwork were very different from those in which the initial fieldwork and theoretical formulations on vengeance occurred, Morrill identified similar interpersonal and intergroup dynamics that organized meetings and other formal gatherings among high-level corporate managers. Moreover, Morrill found that some of the same social conditions that anthropologists argue facilitate vengeance in horticultural and preliterate societies – namely, relatively equal status groups with strong

internal solidarity and weak external ties, plus a lack of authoritative third-parties who can legislate settlements – appear in corporate contexts where vengeance occurs. Thus, although the specific contents of the actions observed are different, their social forms are quite similar. The extension of theoretical perspectives and concepts across vast cultural and social distances may be especially intriguing, but one need not travel so far, theoretically or geographically, to engage in theoretical extension. Jimerson (1996), for example, extends Coleman’s version of rational choice theory to understanding the creation and manipulation of norms relevant to the regulation of pick-up basketball games among players. Ponticelli (1999), likewise, extends theories of religious conversion to understand how lesbians are “converted” from their homosexual lifestyles to “straight” orientations.

THEORETICAL REFINEMENT

A third pathway to theoretical development is “theoretical refinement.” This refers to the modification of existing theoretical perspectives through extension or through the close inspection of a particular proposition with new case material. Refinement overlaps with analytic induction or negative case analysis inasmuch as both constitute qualitative procedures for modifying extant or emerging theory on the basis of new evidence. Again, this can occur independent of theoretical extension or in conjunction with it. Burawoy’s “extended case method” (Burawoy et al. 1998), or at least its application, can be construed as a prototypical example of theoretical refinement via extension. The objective is not the discovery of grounded theory, but the elaboration of existing theory (Burawoy 1998: 16). If theoretical extension and refinement are conceived as overlapping Venn diagrams, then Burawoy’s extended case method falls into the area of overlap. But not all refinement is predicated to the same degree on extension for the simple reason that extant theories vary in their scope conditions and range of theorized applicability, with some theories being of broader generality than others.

A case in point is Stryker’s role-identity theory (1980), which is intended to apply to all situations where alternative lines of action are available to actors. The theory’s orienting premise is that identity and roles are linked isomorphically, with its fundamental proposition hypothesizing that the choice of behaviors associated with particular roles will depend on the relative location of the identities associated with those roles in what Stryker terms an identity salience hierarchy” (1980, pp. 60-61). While Stryker’s theory constitutes a theorized refinement of the interpretive variant of symbolic interactionism, Snow and Anderson’s (1993) study of identity work among the homeless provides an ethnographically based refinement of Stryker’s theory. Inasmuch as identities are internalized positions or role designations, we might expect the homeless to be imprisoned within personal and social identities of little self-worth. This is not what Snow and Anderson found, however, as their field work revealed a great deal of variation in the avowal of identities among the homeless, thus suggesting that under some conditions social structure does not “bestow” identities upon role incumbents, but rather alternative, contrary identities are asserted. Other examples of theoretical refinement include Cahill’s (1999) research on secondary socialization which suggests that variation in repertoires of emotional capital can affect socialization processes to some occupations just as variation in human and cultural capital, and Snow and Phillip’s (1980) modifications – based on Snow’s ethnographic case study of recruitment and conversion to a Japanese Buddhist movement – to the previously mentioned Lofland/Stark theory of conversion.

IMPLICATIONS

I have suggested that the language that ethnographers use to engage theory has been somewhat impoverished and largely limited to the invocation of grounded theory or discovery as a rationale for research. Qualitative analytic procedures such as analytic induction, negative case analysis, and the constant comparison technique constitute research strategies for developing emerging or extant theory, but do not encompass the different avenues to theoretical development. It is in response to these limitations that I have suggested three alternative paths for theoretical development. Although the examples given illustrate these different paths, those provided for extension and refinement were not conceived or written with this language in mind. Had such a language or repertoire of alternatives been in place, I believe that the theoretical utility of qualitative research would be less ambiguous today and more qualitative research would get published in mainstream journals and funded by granting agencies such as NSF. Thus, I believe it is imperative to create discourses that articulate and clarify ethnographically relevant rationales and processes for theoretical development. I do not presume that discovery, extension, and refinement are the only pathways or languages that might be used, but these are clearly discernible in some of the qualitative literature and thus provide us with a point of departure.

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ENDNOTE

- ¹ Consensus does not exist about the nature of theory, but definitions of theory encompass one or more of these four basic elements: (1) a set of logically interrelated propositions; (2) an openness to subjecting propositions to empirical assessment and falsification; (3) a focus on making empirical events meaningful via conceptualization, and (4) a discourse that facilitates explanation of empirical events.

THE STUDY OF MENTAL EVENTS IN COGNITIVE SCIENCE AND SOCIAL SCIENCE

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The fundamental object of study in cognitive science is mental events, in

- lone individuals. E. g., How does stress affect decision-making? What is the relationship between memory and dreaming?
- two people interacting with each other. E. g., What mental mechanisms enable human children to imitate so adeptly and parents to provide such tractable target behaviors for imitation? How do two people communicate?
- an entire community. E. g., How can communities provide cultural mental templates that guide behavior throughout the community? What mental operations subtend large systems of coordination?
- suites of communities in a line of descent. E. g., How are cultural mental templates passed down and changed through generations?

In the social sciences, mental events are also a fundamental object of study. Nonmental facts (the location of coal, the date of the potato blight in Ireland) have significance in social science only to the extent that they bear on mental events. The distribution of oil in the earth's crust has significance in economics and political science only because the geological facts of the matter are enmeshed in a mental world of belief, desire, need, demand, value, utility, pricing, judgment, decision, competition, cooperation, conflict, and persuasion. The study of oil without mental events is natural science, not social science. Mental events accordingly provide defining problems of the social sciences. What are our basic human mental abilities and how are they used in judgment, decision, action, reason, choice, persuasion, expression? What are the mental powers that make human beings, alone among species, capable of such impressive ranges of cultural and social diversity? Do voters know what they need to know? What is the nature of learning, and how should we teach? How do people choose? How can we design effective incentives? How do individuals embedded in societies develop conceptions of personal identity, of group and national identity, of leaders, of factions? How do those conceptions lead the individual to action? How has coevolution worked over roughly the last 50 thousand years of cognitively modern humanity, over roughly the last 150 thousand years of anatomically modern humanity, and over roughly the last 2 million years of the genus *Homo*, after *Australopithecus*? When is judgment reliable? Can negotiation work? How do cognitive conceptual resources depend on social and cultural location? How do products of cognitive and conceptual systems come to be entrenched as publicly shared knowledge and method? How do addressees learn about governance constraints? What constitutes that learning? How does that learning, and the consequent behavior, differ if the addressees are themselves organizations or at least complexly coordinated individuals in a social context?

METHODOLOGICAL IMPEDIMENTS ON THE ROAD TOWARD COGNITIVE SOCIAL SCIENCE

Given this convergence of cognitive science and the social sciences at their intellectual cores, under the general umbrella of the nature of thought and meaning, it would be natural to conclude that they must converge as disciplines. Remarkably, they have not yet done so. There are promising forays, notably in

behavioral and experimental economics, neuroeconomics, and the study of narrative, counterfactuals, and analogy in social scientific argumentation.¹ But cognitive social science remains a frontier of research, not yet a worked field. It needs concentrated and sustained institutional incubation.

Foundations dedicated to this incubation face a difficulty stemming from an asymmetry in the methodological training of the scientists. Cognitive scientists are often familiar at least in passing with the spirit and rationales of quantitative methods in the social sciences, and at times deploy them with full sophistication. But social scientists are less frequently conversant with methods of cognitive science, for obvious reasons: a training in the methods of cognitive science has not yet become a canonical part of the professional formation of the graduate student of economics, political science, sociology, or anthropology. One daunting challenge for the NSF as we go forward with cognitive social science will be to locate groups of reviewers among mainstream social scientists who can participate in judging proposals in cognitive social science, given this asymmetry of methodological training.

SOME METHODOLOGICAL FEATURES OF COGNITIVE SCIENCE

Methods of cognitive social science often diverge sharply from quantitative methods of social science. Here are a few features to serve as topics for discussion:

1. The importance of a single datum. In the study of mental events, a single datum will often establish a crucial problem and drive the subsequent analysis. A single datum shows that the particular behavior was possible for a human being, and that fact legitimates the research. V. S. Ramachandran, a neurobiologist, often asks members of his audience to imagine that he has begun a lecture by asserting that he can make a pig talk. What do you do? You ask for a demonstration. He brings in the pig, waves his magic wand, and the pig talks. As a matter of the utmost scientific importance, you want to know how this happened. Certainly you do not say, “Well, that’s only an N of 1; show me another talking pig!” Cognitive science, in its attitude toward a single datum, is often closer to the hard sciences than to the social sciences. In the hard sciences, which attract so much emulation and envy, unusual events often command the most attention, on the principle that they are the most likely to reveal general processes. Physicists who noticed that the orbit of Mercury did not quite follow Newtonian theory did not ignore it as an exotic event. On the contrary, it became the central event, calling for new theories and extraordinary new experiments. Similarly, work in the cognitive scientific study of grammar, for example, might focus on a single datum to show that a certain pattern of grammatical combination is available to speakers of a particular language. The quantitative status of that datum with respect to a full data set would be beside the point. Cognitive scientific models often treat usual cases of behavior as products of general processes at work in minimal particular conditions, processes that are often easier to see in the unusual cases.

2. Limited reliance on statistical analysis. One of the principal purposes of statistical analysis is to tease causal relationships out of messy data—“messy” in the sense that complex behavior has made it difficult to pick out simple underlying causal patterns. Statistical analysis rehabilitates the messy data. While statistical methods are available in cognitive science—for example, one can investigate how much of the “variance” in levels of trust displayed in a particular rational choice experiment can be “accounted for” by factors such as the level of the posterior pituitary hormone oxytocin—the purpose of research in cognitive science is often not causal analysis, narrowly considered, and quantitative methods are often unhelpful. For example, a cognitive scientific analysis meant to account for the existence of a grammatical pattern might seek to show that a speaker of the language commands such-and such grammati-

cal constructions and such-and-such integration principles, which can operate jointly to produce the grammatical patterns that prompted the investigation. Such an analysis is not causal in a narrow sense. (E.g., it would be ridiculous to look around for factors in the environment that make it grammatical to say *I loaded the wagon full* and *I loaded the wagon with hay* and *I loaded the wagon full with hay* and *She choked him unconscious* and *She choked him down* but not **She choked him unconscious down*. The outcomes are invariant regardless of situation, and situational factors provide no explanation.) And the application of statistical methods to the grammatical data, while certain to reveal something, would not yield a model of the grammatical operation. (E.g., there is no “variance” to be accounted for in this case across speakers in the community, and although different language communities can have different grammatical constructions, the cognitive scientist is looking for a level of explanation that goes far beyond correlating grammatical judgments with membership in language communities. Similarly, a cognitive scientist of child development might detect a developmental sequence and seek to account for it by giving a model of the sequence. One method of studying child language acquisition for example is the “diary” method, in which one records everything the child says during certain times (e.g. all day Tuesday) over many weeks. One then analyzes the pattern of acquisition of new patterns.

3. *Accounting for variance from the inside rather than the outside.* Cognitive science often regards human mental operations as highly supple, creative, non-algorithmic, stochastic. There is an openness in cognitive science to accounting for variance that derives from the workings of the mental operation itself. Holding the external independent variables fixed can still produce great variety, because the way we think has creativity built into it as a matter of phylogenetic inheritance.

A PARTICULAR CASE: HOW SHALL WE DESIGN THE STUDY OF COMPRESSION SCHEMES IN LEARNING?

At the Center for Advanced Study in the Behavioral Sciences, we are designing an initiative to advance the basic science of compression schemes in learning. Consider a general vision of the research problem:

Learning requires plasticity. Plasticity enables a system to create compressions over events that can then be carried forward and expanded to guide future performance. A genotype is a compression scheme over ancestry. The human immune system compresses over ontogenetic events to guide future defense. Among mammals, mother-offspring simulation of predator-prey interaction develops compressions that guide the offspring’s performance in future do-or-die confrontations. At the conceptual level, there are several robust operations of compression: categorization, framing, the partitioning of sensory fields into elements, metaphor, blending. Cultural artifacts can carry highly useful compressions: A compass rose is a tight compression over ranges of otherwise ungraspable relational structure. And compressions come in many forms. Some compressions are highly abstract (e.g. $ax^2 + bxy + cy^2 + dx + ey + f = 0$ for conic sections), but others are highly specific (e.g. a souvenir map of a city, with drawings of important buildings). Some compression science is highly formal and theoretical (e.g., complexity theory), while some is highly intuitive and applied (e.g. explorations of how a coach can provide a martial artist with an effective grasp of a particular judo move). There is a considerable existing body of compression science in the study of biological systems, cognitive systems, simple species, human beings, expert performance, developmental clines, machine and digital systems, and human-digital interfaces, some of it focused on processes of “decompression” and “expansion” of compressions. To make a new and better compression typically requires “decompressing” it and then “recompressing” it in a different way. This process is evident in the way children learn numbers: whole numbers, fractions, positive and negative integers, irrationals. The science of compression runs through molecular and cellular biology; genetics; develop-

mental biology; neuroscience; cognitive science; psychology; anthropology; linguistics; visual representation and gesture; sociology; political science; economics; institutions, organizations, and law; ritual and religion; the deployment of technology in support of learning; and the history and future of attempts to compress the learning process.

Compression through plasticity is essential in evolutionary biology, adaptation, ontogenetic development, and the functioning of institutions, organizations, cultures, and societies. Compression science includes the analysis of decompression and expansion. The basic science of “compression schemes in learning” runs across the nano, micro, meso, and macro levels. It will be highly influential for applied initiatives in technology, industry, education, and organizations. Compression is recognized in several disciplines for its fundamental importance, but a cross-disciplinary science of compression—crucial as it would be for the purpose of advancing the science of learning—does not yet exist. CASBS proposes to serve as the lead institution in incubating this area of science, and so to become an NSF science of learning center. One of the first questions we must take up is how to design an array of research programs into compression schemes in learning. Clearly, such an initiative will be a fertile meeting ground for the sustained blending of cognitive science and social science. Equally clearly, quantitative research will serve as only one of the methodological instruments deployed in this initiative, and much of the cognitive social scientific research will rely on qualitative methods. One of our first efforts at CASBS will be to organize two workshops for the central participants in our “science of learning” project, one on designing research into compression schemes, another on assessing and evaluating the research.

ENDNOTE

- ¹ See Turner, Mark. 2001. *Cognitive Dimensions of Social Science: The Way We Think About Politics, Economics, Law, and Society*. New York: Oxford University Press.

A NOTE ON SCIENCE AND QUALITATIVE RESEARCH

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In the movie *A Bronx Tale*, a neighborhood teenager confronts the local organized crime kingpin. The meeting is the chance of a lifetime for this Italian aspirant. He asks earnestly and with rapacious eyes, “Is it better to be loved than feared?” From the Don’s answer, we find out that love may be desired, but fear is preferred. The question is instructive for those us—of various stripes and persuasions—who fight for space in that small craft called “qualitative sociology.” We are like that Italian teenager. We grope for a secure place in a field that looks upon us as the ugly stepchild, useful only for menial disciplinary labors: e.g., committee work, signatories on large grant applications requiring “multi-method” commitments, and ‘special op/s’ storytellers sent to recover disaffected undergraduates who find little joy in our leading journals.¹ One need look no further than some my own writings to see that qualitative research has become uninspired. (If you doubt this, pick up an qualitative AJS article or ethnographic monograph; likely, the author will open with “what this paper will show.” Timid, feckless and feculent. Why read on?) With the exception of Howard Becker and Herb Gans, the rest of us rarely write with confidence and irreverence. Few of us know how to weave narrative and, like our quantitative counterparts, we rest on formulaic hermeneutics that challenge neither our readers nor us. We do not write with love for the messiness which surrounds, but with fear that the controlling voices of our discipline will strike us, yet again, with the all too- familiar quips: “How is this *generalizable*? Yes, but it is *representative*? What’s your ‘*n*’?” By failing to lead our discipline, we have relinquished the power to shape societal knowledge to those with big ‘*N*’s.

It may seem curious to address these issues in this particular convening, but I meet many people thoroughly un-excited about contemporary qualitative research. Their complaints? Interviewers dominate the field; conversation analysts, sociolinguists and ethnomethodologists still do not know how to deal with power, race, and class. Ethnographers too often hire students to conduct their fieldwork; and, sociologists have turned to journalism, rather than sociology’s own public (and public intellectual) roots. There is no need to be fashionable, especially for its own sake. I agree with the dictum, attributed to one of our attendees, “Don’t study what’s interesting, but what interests you.” But, catering to the mainstream of our discipline has dissolved the vitality of many qualitative research traditions.

There are, of course, serious strands of qualitative research that have taken experimental analytic and narrative routes—e.g., marriages of psychoanalysis and fieldwork; feminist ethnography, and cultural studies. However, most of these trends began outside of sociology—in literature, anthropology, linguistics—and were imported into our disciplines. I doubt any of these disciplines would convene a panel on “scientific foundations.” Thankfully, NSF’s sociology division has organized such a meeting, so my comments are directed at some of the consequences of adopting science as an orienting principle.

Michael Burawoy, in an article in *Sociological Theory*, addresses the core challenge of combining qualitative research and the logic of science. Qualitative research has not adequately understood what it means to be scientific, or what other logics are available. (His article proffers “reflexive science” as an alternative.) In my mind, sociology’s problematic relationship with the scientific method stems from the postwar period, a recent history we may call the “scientific turn.” Sociology was once a practice that took pride in grappling with social contradiction. After the war, its qualitative adherents answered to

the clarion call of Fordism: explain, predict, and control on larger and larger levels—I recently met an “ethnographer:” who has a four hundred person sample, which made me cower and feel inferior that I only had 164 informants. Explain away the variance, isolate the predictors, control for bias and do not concern yourself with the outliers—which are really those quirky actors who make explanation impossible, but whose lives could yield an entire monograph. And, do all this while intruding minimally into the lives of subjects. We have set aside our dungarees for white lab-coats. We now prize techniques that help remove us, as fieldworkers, from social situations. Who has time to observe? Indeed, *why* observe when an interview will do the trick—and an interview that can be administered by someone else.

To those who say that qualitative sociology has advanced considerably in the last few decades from a scientific embrace, I offer no real rejoinder: Indeed, I agree. I personally have benefited from the scientific turn as much as anyone. For example, I write with economists and for economics journals, I permit ethnography to inform statistical analyses, and I routinely address policymakers and government officials. I embrace multi-methodological strategies and am overjoyed that my colleagues now employ voice recognition and data analysis software successfully. (I recently discovered a computer specialist who told me that, with blackberry technology, my informants could wire me the data from my fieldsite. Virtual ethnography!)

Yet, I have paid a hefty price for my own scientific turn. It is a struggle to tell a story—what journal would accept it anyway? I rarely wonder at the mystery of the sense-making actor. When thinking about my relationship with informants, I find myself concerned mostly with eliminating bias, rather than embracing the inter-subjective aspect of fieldwork. I find it attractive to send my students to do the interview—thereby replacing the possibility for extended, open-ended conversation, drink, and dinner. My own ethnographic writing feels stale, and, quite frankly, I rarely find solace in the writing of my colleagues—unless, of course, I pick up something written fifty years ago.

Many of us conducting qualitative research are in too great a rush to be scientific. In the interest of maintaining rigor, we feel compelled to take steps to remove bias from our work, even if this means naively trumpeting our fieldwork as having gained full access and acceptance by the group—or worse, failing to address fieldwork as a constitutive factor altogether. We increase sample sizes, translate studies into policy recommendations, shorten the time for data gathering, and conduct multi-site ethnography. None of this is necessary evil and some of it is downright laudable. But, alas, none of this is necessarily scientific. It simply adopts the outward features of principled thought aimed at facilitating abstraction. In other words, it adopts the perceived cloak of science, and so, it is *scientistic*, not scientific.

As Burawoy reminds us, a scientific study would entail examination of those elements at the core of qualitative research: (1) the engagement of the researcher and informants (2) how meaning is ascertained viz. research design (3) how the temporal aspects of the object of study are identified (4) determining how much context (and of what kind) must be provided. These questions are not necessarily absent in current qualitative research. However, there is increasing pressure for research to be relevant, timely, yield policy-oriented results, etc., and so, these issues are elided or given little emphasis.

There is nothing wrong with using standardized research protocols—e.g., interviews, research assistants *cum* ethnographers— particularly when large numbers of people must be interviewed in different geographic locations. But, such comparative and large-sample qualitative studies usually end up being little more than open-ended survey research. There is rarely an effort to document, with care and detail,

the engagement of the fieldworker and informant. There is no attempt to stay with people long enough to understand movement and change. Diving informants' aspirations and belief structure is no longer a painstaking process where verbal opinions and statements must be balanced via assessments of practice and inter/action, all the while situated in particular (and *singular*) institutional contexts—instead, a set of interview questions on attitudes will do the trick.

When social problems are the object of study, another ugliness appears. Data are in the service of formulating and promoting more public policy. This holds whether the informant sample is three or three hundred. It is nice to see sociology embracing its reformist, socially engaged roots, but the rush to have practical applicability has resulted in the near absence of criticism and silence in regards to the practices of institutions enacting public policy (e.g., foundations, government agencies). Qualitative studies have always been instrumental in promoting public *discourse*, not necessarily public policy. This conflation can be detrimental for research, particularly for our graduate students who themselves feel compelled to address public policy debates.²

WHAT CAN NSF do?

This convening is a step. Below, are several others.

1. It is critical that we understand the importance of time in qualitative research designs, particularly ethnographic work. Time is the component that permits an ethnographic sensibility to flourish: the researcher's capacity to frame the study (key questions, unit of analysis, etc.) and organize a design framework for it is integral to the fieldwork process. So too is the freedom to observe the social actor's behavior, weighing practice against verbal exposition. Let us resist the pressure to shorten qualitative research frameworks and promote scholarship that theorizes the temporal component of the object studied and the methods employed.
2. Some years ago, a quantitative researcher asked me to join a large research project— $N=3000$. She said, "We need an ethnographer because we [quantitative folks] can find out about structure; we need you to tell us about process." This absurd, theoretically un-informed wisdom must be addressed and thrown aside. Our discipline too often understands social structure as aggregate data—usually of the demographic sort. Thus, ethnographers are brought in when the 'micro' and processual need illumination; which, on most studies, means that the PIs really don't feel that their data is adequate. There are many kinds of social structure: qualitative research is particularly useful for uncovering social structures that shape social action and meaning—structures of aspiration, possibility, moral reasoning, and so on. No amount of demographic data will take us down this road. It is necessary to understand more clearly the different forms of social structure that can be ascertained via our methodological choices.
3. Qualitative research, though having grown formulaic, may be differentiated by the location of writing in the overall practice. For ethnography, anyway, analysis is achieved through the writing, not in advance of it. The "introduction-literature review-data/methods-findings-conclusion" format has not only rendered much of qualitative exposition bland and insipid, but it has eliminated the possibility for the social to emerge through explication. For those of us in mid-career, the problem is multi-faceted: many of us do not know how to write, are fearful that building narrative signals diminished intellectual capacity, and have no time for anything but rigid outlines. But, this does not mean our

students must suffer. A useful allocation of NSF resources is to explore the linkages of qualitative research in various disciplines, in both humanistic and social sciences. We must reward students and our junior colleagues with the time and resources to visit alternate ways of rendering social knowledge. This will only help the pursuit of science, not harm it or water it down.

ENDNOTES

- ¹ Qualitative sociology is a far too imprecise term to be of use beyond polemic. In this paper, I use the term to reference those who conduct fieldwork.
- ² This tendency is evident in book reviews, wherein ethnographers are criticized for not suggesting “solutions,” as if shifting and/or enhancing our understanding of a social problem were insufficient.

ADVANCING THE SCIENTIFIC BASIS OF QUALITATIVE RESEARCH

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A key component of the *scientific basis of qualitative research* is the rigor with which analyses are carried out: doing analysis thoroughly and carefully, so that it has demonstrable reliability and validity (Weitzman, 1999, July). As will no doubt also be argued by some of my colleagues, traditional notions of reliability and validity, as developed for quantitative research, require adaptation for application to qualitative research. I will focus my remarks primarily on the ways in which computers and associated technology can contribute to rigorous qualitative research, and on needs for further development in this field. I won't argue that computers are the answer to the problem of rigor in qualitative research, but I do want to argue that they can be an enormous help, and that, perhaps, they can make possible rigorous (and thus more scientifically sound) approaches to analysis that we otherwise could not or would not undertake.

Some of the very advantages of qualitative research can also turn out to be weaknesses. It is a strength that the researcher can look at the text of what people had to say, and use his or her intelligence flexibly to consider multiple possible interpretations of what it all means. This also means there are multiple conclusions a researcher might arrive at, not all of them necessarily of equal validity. Miles and Huberman (1994) argue that verification of conclusions is a critical step; they sum up the situation thus: "Qualitative analyses can be evocative, illuminating, masterful-and wrong. The story, well told as it is, does not fit the data. Reasonable colleagues double-checking the case come up with quite different findings. The interpretations of case informants do not match those of the researchers" (p. 247). This calls for methods that account for such pitfalls.

Complicating the story is one of the key features of qualitative data: they're messy, and usually voluminous. We wind up with huge piles of texts: transcripts, field notes, documents, questionnaires, and so on, and have to sort our way through them. Whether what we're doing is looking for what we think are identifiable phenomena that we can cluster together into categories or themes, or some more emergent, holistic sense of the data, we need to be able to organize the data in some way. We need to be able to find our way through it, whether by chronology, narrative structure, topic, case type, theme, or by some other kind of relationship between one piece of text and another.

We may need to be able to pull together all the pieces of text that have to do with a topic. We may need to be able to see each utterance in its original context to know what it means. Or, we may need to be able to find support for a proposition, or find the data that contradict it. When working with the often enormous piles of text generated in qualitative research, being careful, diligent, and thorough can be a tremendous challenge, both because of the volume of the data, and the complexity of the thought required to analyze it. Computers can be a big help.

Approaches to the problem of rigor in qualitative research are varied, but typically involve methods that allow the researcher in some way, after having been deeply immersed in the data, to pull back a bit and see things from a broader perspective. Such methods might involve "triangulating"—checking for convergence among different sources of information, different investigators, or different methods of data collection (Creswell, 1994)—or systematically going back through the data in a variety of ways, check-

ing to find out if there are data that argue against the conclusion you are reaching. Miles and Huberman (1994) offer a list of tactics for verifying conclusions that include: Checking for representativeness; checking for researcher effects; triangulation across sources and methods; checking the meaning of outliers; looking for negative evidence; making if-then tests; ruling out spurious relations; replicating a finding; checking out rival explanations; and getting feedback from informants. They also offer a wide variety of methods for building matrices and other kinds of displays that can assist the analyst in seeing larger patterns, both within and between cases, and performing the kinds of checks referred to above consistently and on broad scales. But these tasks can be extraordinarily labor intensive—even well-funded projects can be hard-pressed to carry them out consistently.

So where do computers come in, and in what ways are advances in the field needed? There are the obvious ways computers already help. Software for qualitative data analysis (QDA) allows the analyst to systematically index and organize the data, and then to reliably and flexibly retrieve the data in many different ways (for a fuller discussion of the varieties of types of software, see: Weitzman & Miles, 1995; Weitzman, 1999, 2000). For example, it can facilitate finding all the data *the analyst has previously identified* as indicating a particular theme or conceptual category, and it can facilitate parsing these data into subgroups based on demographic or other categorical or quantitative variables. It can also find all the cases where a theme was not present, or where combinations of themes are present, and so on. With the use of Boolean operators, the analyst can construct queries of arbitrary complexity, and execute them nearly instantly. The speed and consistency with which QDA software can carry out such operations already make it far more feasible to regularly carry out the kinds of analyses referred to above.

The question for the future is how we can advance the scientific soundness of our research, and the portion of that I'm addressing is how computers can contribute to that advancement. New programs, such as Qualrus (the first attempt to bring artificial intelligence to the problem) are appearing, and new versions of existing programs, such as Atlas.ti, NVivo, and fs/QCA, are adding features that facilitate seeing patterns and relationships and checking for consistency in new ways. For example, Atlas.ti (in the newest V.5) and NVivo (in the newest V.2) both have added a variety of new or expanded tools for seeing broad patterns of relationships among concepts across cases, and for exchanging tables of such relationships back and forth with quantitative programs such as SPSS (useful not only for quantitative analysis—procedures such as Crosstabs can be of great value within a qualitative paradigm). fs/QCA takes Ragin's qualitative comparative analysis method and extends it from dichotomous variables to continuous variables (Ragin, 2000). With the exception of Ragin's work, the field has barely made a start at considering new methods of analysis that might be facilitated by such tools.

What sort of work needs to be supported? On the software side, there is a need for further exploration of the kinds of relationships and patterns that can be explored and represented via software tools, along the lines discussed above. Further advancement of the abilities of QDA software to integrate qualitative and quantitative data in new and flexible ways would be welcome. Most software developers have focused on producing tools that do what researchers are already asking for, and this needs to be taken even further. In addition, more thought given to new things computers could do, that people have not yet asked for, is needed.

Atlas.ti in its latest release (June, 2004) includes extensive tools for development of XML versions of datasets and analyses for publishing on the WWW. This opens up possibilities for the sharing and review of both data and analyses in ways that have been called for but rarely been done up to now. This is

a first step in a direction that should be vigorously pursued. There is a need for the development of software tools to facilitate the work of social scientists who conduct narrative analysis (what's needed here are tools that can better represent and manipulate narrative structure). There's also a need for the further development of software tools to support the rigorous analysis of media such as audio and video. Programs like Atlas.ti, HyperResearch, interClipper, C-I-SAID, and Transana currently support such media, but the special challenges of working with such media (for example, there's no equivalent to searching for a word, and watching/listening can't be done at the same speed as visually skimming text while still catching meaning) still need much attention.

Research is also needed on the impact of current software programs on the outcomes of research. In small studies with earlier software, Fielding and Lee had found some differences. More extensive systematic research comparing outcomes with different tools is needed.

To address the needs of researchers who begin their analysis while still in the field collecting data, a practice that can contribute significantly to scientific rigor (see Becker's paper) we need to address the problem of speed. Currently, researchers are stuck with either analyzing audio (with such challenges as are mentioned above) or taking the time to transcribe. Technological advancements that would be welcome would include taking voice recognition software to the next step so that interviews could be automatically transcribed (currently, only a person who trains the software can have their speech transcribed). Further, current trade-offs in recording technology choices (between tape and various digital technologies) could be addressed with the development of high-fidelity, reliable, rugged digital audio recording equipment with fast upload to computers.

On the analysis side, we need support for the exploration of analytic techniques that take full advantage of the things that software can do, such as the improving facilities for finding and displaying patterns of co-occurrences of codes, text strings and case-variables. Work that explores using such tools in novel ways to satisfy *qualitative* notions of validity and reliability would be welcome.

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ENDNOTES

- ¹ There are many complexities in extending this argument to the various forms of “qualitative research” that are not based on ethnographic fieldwork. In a way, when historical scholars find new sources, they are changing the relations with subjects in their field; as are analysts of qualitative case studies who compile a more varied and documented corpus of cases than had been available. Conversation analysts often treat data acquisition for granted, analyzing portions of transcriptions from tapings done by others and glossing description of the data acquisition process as if it contained no critical contingencies relevant to their qualitative analysis. Here I just want to flag an appreciation of the complexities of extending this argument beyond ethnographic field studies.
- ² A recent paper by Marjorie DeVault, based on one type of outing, family trips to the zoo gives a glimpse of what has been missed. “Producing Family Time: Practices of Leisure Activity Beyond the Home.” (2000). *Qualitative Sociology* 23(4): 485-503.
- ³ For an elaboration of these types, initial hypotheses of the on-site patterns of interaction they are related to, and the significance of their study for improving understanding of a range of issues, including segregation and integration in public life and the meaning and attractions of “being out in public,” see <http://www.sscnet.ucla.edu/nsfreu/>.