Conceptions of a Former Program Officer

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From 2000-2004 I served for two years as an NSF Sociology panelist and for two years as NSF Sociology Program Director. During this time I read colleagues' proposals in Sociology, Political Science, Law & Social Sciences, Anthropology, and other social science disciplines, and I read colleagues' reviews of proposals in these areas. Among the several hundred proposals that crossed my screen in those four years, I observed some disciplinary differences in approach (more qualitative methods in anthropology than political science, more modeling in sociology and political science than in anthropology, more cross-disciplinary uses of literatures and methodologies in Law & Social Sciences proposals). Quite often, however, I was struck by similarities among reviewers in identifying strengths and weaknesses in proposals and, often, by the reactions of PIs to those reviews. Below are some of my observations as they bear on the conduct of qualitative research and the goal of identifying standards across disciplines. My comments are organized around three themes: the methodological imperative, the question of theory, and the logic of research design.

The Methodological Imperative

It is a common social science assertion that the research question should dictate the methodology and data used to answer it. My experience, however, is that researchers tend to be wedded to a particular research technique (surveys, ethnography, archival document analysis, interviews, conversation analysis), and that they look around for a research topic on which to ply their chosen methodology: where can I conduct field research, what documents are available for me to examine, what can I count, whom can I listen to, interview, observe? As a result, in sociology, for example, we tend to train "survey researchers," "ethnographers," "conversation analysts," "network analysts," or "comparative-historical" sociologists who toil in different subfields most of their careers and who embrace a relatively fixed set of beliefs about the superiority/inferiority of various methodologies and data sources. A methodology, thus, can become a way of life.

The tendency for social scientists to specialize in particular methodologies leads to the development of disciplinary subcultures that sometimes take on the characteristics of sects—complete with doctrines, founders, believers, converts, heretics, secret codes, initiation rites, revealed wisdoms, sacred rituals: the graduate statistics course sequence, the fieldwork experience, the event history analysis workshop, the archive, grounded theory, significance tests. Methodological specializations produce important and useful expertise, but they sometimes (not always) can create advocates, defenders of the faith, witch hunters, and crusaders for and against particular ways of conducting research. Extreme methodological zealotry (for one's own) and bigotry (against those of others) is relatively rare, more often methodological specializations are marriages of convenience,

comfortable arrangements that researchers relax into and cannot imagine working without. Researchers seldom change sects.

In Sociology there is tolerance, even respect, for a variety of research methods, but there still tends to be career specialization in particular methodologies. This can make it difficult to establish standards for, say, qualitative research proposals across a variety of techniques, such as ethnography or content analysis or in depth interviews. How many cases (observations, settings, articles, interviews) are enough? How do we know if the observations are accurate or typical? What is the validity or comprehensiveness of coding categories? Is generalizability even a goal? Did the interviewer bias the responses? We cannot necessarily presume unanimity among researchers working within and especially across methodological traditions, not to mention disciplinary differences in methodologies (e.g., ethnography practiced by anthropologists compared to sociologists).

Since the development and dissemination of standards for designing and evaluating qualitative proposals is an important goal for social science, it might be most expedient to recognize rather than resist the institutionalization of methodological differences. The mobilization of methodological practitioners by category may be the most organizationally expedient strategy for articulating qualitative standards and training future social scientists. Such a strategy would be to organize gatherings of ethnographers to suggest strategies for strengthening ethnographic research, meetings of archival researchers to recommend best practices for archival research, or workshops for interviewers to develop techniques for constructing and conducting oral histories and interviews. The general utility of such segregated endeavors would depend in part on shared epistemologies and research goal, in particular on agreement about the question of theory and the logic of research design.

The Question of Theory

Although different disciplines sometimes seem to have different answers to the question of theory—what is it, where is its proper place in the research process, what constitutes a satisfactory explanation, plausible accounting, convincing argument—virtually all NSF program panels whose deliberations I observed and whose reviews I read expressed interest in answers to questions of why, when, or under what conditions. The question of theory often was answered differently by reviewers and researchers. Reviewers frequently looked for theory in proposals; researchers seldom gave them what they wanted. I found both quantitative and qualitative research proposals typically failed to offer even the most modest theoretical contribution. Most researchers seemed satisfied to identify an "interesting" phenomenon or process and content to offer a means of documenting it—often phrased as "looking at"—whether by counting it, observing it, or talking to it.

The question of theory—to do or not to do—was especially often debated by qualitative researchers. Although I did not find this to be the case in practice, quantitative projects, data, and researchers generally are presumed to be "testing"

theories and "predicting" outcomes whereas qualitative projects, data, and researchers are presumed to be "generating" theories and "observing" outcomes. Despite these presumed relationships to theory and hypothesis testing, few projects we reviewed (and many that we funded) while I was at NSF were embarked on systematic hypothesis generation or evaluation. A main complaint of reviewers across disciplines about both qualitative and quantitative projects was the proposal's low level of theoretical development and the project's limited potential for conceptual contribution. Interestingly, a main complaint of PIs across disciplines of both quantitative and qualitative projects was that reviewers were asking for theory. PIs often argued that funding for data collection or analysis alone was perfectly justifiable since their topic was so important, and anyway, they argued, theory either was present in the proposal (i.e., simply was invisible to our obviously blind reviewers) or theory would emerge out of the data (i.e., easily would materialize through the magic of empiricism).

There is certainly room in social science for various styles of research: descriptive, policy, evaluation, model building, theory evaluating. I would argue, however, that the National Science Foundation cannot and should not fund all of them. NSF should specialize in the latter—theory development and evaluation with evidence. Research that is not designed to adjudicate among explanations, produce new theoretical understandings, or contribute to the conceptual development of a concept, process, phenomenon, should not be funded by NSF. This means that reviewers, program officers, and colleagues need to press researchers to push their work a step (or two or three) further toward richer theoretical framing and production. Standards for qualitative (and quantitative) research should emphatically respond to the question of theory with a clear answer: YES, projects must make a theoretical as well as an empirical contribution.

The Logic of Research Design

In the thousands of email messages I sent as NSF program officer, I often repeated myself. So, I started saving messages that I found myself composing over and over again. Below is one that I frequently sent in response to a research précis sent by a PI planning to write a full proposal or to a researcher asking if a project idea was appropriate for the Sociology Program:

> Your project sounds very interesting and might be suitable for submission to the Sociology Program. Successful proposals are those that pose an important and interesting research question, that situate the planned project within ongoing and major research in the area, that enter into a dialogue with the relevant literature in Sociology and related fields, that contain a clear and detailed plan for obtaining and analyzing data, and that outline a research design that is falsifiable—one that allows you to be wrong as well as right.

It was that last recommendation—what Karl Popper referred to as "falsifiability," that generated the most questions from my email correspondents. What exactly did I mean by a falsifiable research design and what did one look like?

Except for experimental researchers, the closest most of us come in our research to designing falsifiable projects is to undertake a systematic search for negative evidence—what are the available alternative explanations for what we are trying explain, and what evidence can we find that these alternative explanations are incorrect. When I was studying the dramatic increase in the American Indian population in the U.S. Census (a 72% increase from 1970-1980), I looked for evidence of the usual demographic or technical reasons for such an increase: declining death rates, increasing birthrates, immigration, changes in the measurement of "Indian." When none of these accounted for the growth, I offered my own explanation (increasing Indian ethnic self-identification) and tried to argue why it had occurred (federal policies, cultural politics, ethnic mobilization).

When we push ourselves and our students to devise ways to ask research questions that make them even modestly falsifiable, we have a much better chance of improving the theoretical quality of social science research. Just as it is important to resist qualitative researchers' protestations about polluting research with theoretical expectations in advance of data collection, it is important to stress the feasibility of making a theoretical contribution a goal of all research. Challenging researchers to struggle to imagine a falsifiable research design helps them to embark on one of the most direct routes to theoretically and empirically rigorous research.

Two Promising Ways to Advance Qualitative Research

A few years ago a particularly vexing exchange occurred in the *American Journal of Sociology* among ethnographers Loic Wacquant, Eli Anderson, Mitch Duneier, and Katherine Newman. Wacquant didn't like their politics, perspectives, or presumptions, and they didn't like his politics, pomposity, or presumptuousness. Wacquant's complaints centered mainly on what he saw as a conservative apologia imbedded in the three very well-reviewed and well-received books written by Anderson, Duneier, and Newman, and they defended their work by responding to his criticisms. I sympathize with Wacquant's targets having been savaged myself in the past by a supercilious critic who called me naïve and my work unsophisticated. But what vexed me most about this "dialogue" was that it centered almost exclusively on ideology and very little on data.

As I noted above, there is room in social science for all kinds of research, and I guess, for all kinds of reviews, debates, and criticisms. NSF can't force researchers to focus on the facts. But we can make it possible for them to do so. Neither Anderson nor Duneier nor Newman nor Wacquant had access to any of the data in the others' books beyond what was published. (They might not have asked anyway—only once have I ever been asked for my data which I shared since they were funded by NSF!) But what is important is that all data be available—not only quantitative data, but qualitative data as well. This is, in my opinion, the most pressing issue that faces the development of rigorous qualitative research. It is an area in which NSF could have a major impact in two ways: technical assistance and funding. All NSF funded projects require that data be made available to share with other researchers. This is not currently possible for many PIs since the procedures and resources needed to make it possible to share data with other

researchers are not available. There are many unresolved problems associated with sharing qualitative data: how to guarantee confidentiality, make data sharing feasible, fund the preparation of data for publication.

Science depends on replication, evaluation, reinterpretation of empirical findings as well as arguments about the internal and external validity of data collection regimens, analytical strategies, and interpretative frameworks. Progress and the evolution of knowledge—what Thomas Kuhn referred to as the “structure of scientific revolutions”—depend on both new ideas and the accumulation of empirical findings that pile up and challenge prevailing theories. Social theory can be enlivened as researchers engage in battles of words over bias or politics. I would like to see, however, more arguments about facts. Social knowledge and the credibility of social science cannot advance by debates staged in the absence of evidence. What is needed for informed debates and the advancement of knowledge is evidence that everyone has access to and can evaluate and analyze. Providing technical assistance and funding for qualitative data sharing are important ways that NSF can advance qualitative research.

My second recommendation for strengthening qualitative research is to encourage the design and incorporation of qualitative research methods courses in all graduate programs. Since this involves science education, perhaps support for the development of curricula could be funded in partnership with NSF’s Education and Human Resources Directorate. This workshop and the previous workshop and report on qualitative methods sponsored by NSF’s Social, Behavioral, and Economic Sciences Directorate are steps in that direction. What is needed next is a means of institutionalizing further the pursuit of rigor in qualitative research. Graduate education is one way to achieve this, another might be through the establishment of a center for advanced study in qualitative research methods.