

Memo for NSF workshop on Qualitative Research, May 2005  
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This memo provides some thoughts (from the perspective of someone engaged in comparative historical-institutional research in political science) on the general issues we were asked to address, namely:<sup>1</sup>

1. What are the standards of rigor in your discipline?
2. How might these standards of rigor be communicated to or applied in other disciplines?
3. What areas or topics are most promising for investigation using qualitative methods?

The executive summary of the workshop on “Scientific Foundations of Qualitative Research” that Michele circulated (Ragin, Nagel, White) provides a very useful summary of the criteria that define rigorous, high-quality qualitative research in my field. I’m not sure that I can improve much on these, so I will use this memo to focus on three issues that are mentioned in the report but that might be fleshed out more fully. All of these have particular salience for the type of work done by political scientists engaged in qualitative, comparative-historical research, but I think they also apply to other disciplines as well. My points A-C below address the first two questions raised in the workshop charge, while point D deals with the third question.

(A) Indicating the type of theoretical contribution expected from the research

The Ragin et al. report rightly pointed out that qualitative research is often conceived and executed with goals in mind that may be quite different from that of other types of work, especially some quantitative statistical work. Not all theoretically informed research is primarily engaged in “theory testing” in a strict sense (certainly not all in the same sense). Some qualitative work is aimed, rather, at other theoretical contributions, such as theory refinement, concept development, or tracing the causal mechanisms that appear to lie behind observed correlations or relationships. As an example, qualitative research in my field, focusing on carefully chosen “critical” cases, has been important in correcting mis-specified theories on the development of certain labor market and welfare state institutions, by showing that prevailing interpretations of very robust statistical correlations essentially had the causal arrows reversed (to put it too crudely, not strong unions → large welfare states and centralized collective bargaining, but rather centralized collective bargaining and large welfare states → strong unions). In order for qualitative research not to be viewed as an “inferior” version of quantitative work (because smaller “n”) – and especially so that reviewers can apply the appropriate criteria -- it is important for qualitative researchers to articulate explicitly what goals have motivated the conception and design of the research.

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<sup>1</sup> I draw at some points in this memo on ideas elaborated in more detail in (Thelen 2002) and, to a lesser extent, (Locke and Thelen 1995).

(B) Articulating the logic of comparison/case selection

In comparative historical work in political science (and I presume this applies more broadly too), and partly for reasons hinted at in (1) above, cases are often selected for reasons other than their “representativeness.” Since the logic of comparison or case selection is typically crucial to the success of qualitative research projects, it is important that the researcher explicitly address this – again, among other reasons, so that the research strategy is transparent for reviewers and so that the appropriate standards are applied. A researcher’s choice of cases is frequently guided by the state of current theorizing on a particular subject – sometimes focusing on “critical cases” that allow for direct assessment of received wisdom on causal mechanisms or linkages; sometimes comparing across cases that are typically grouped together in the literature (or seen as very different); sometimes zeroing in specifically on a case or cases that appear anomalous in light of existing theory and that therefore may be fruitful from the perspective of theory development.

In terms of research that is specifically comparative, one of the well known advantages of qualitative research is that it gives the researcher the advantage of close knowledge of and familiarity with the cases under study. Proximity to the empirical cases has the distinct advantage of increasing the probability that the concepts with which the analyst is working capture what he or she is trying to get at, and, especially, that they capture the same thing across all cases under consideration (see especially Collier 1998; Coppedge 1999; also Locke and Thelen 1995; Mahoney and Rueschemeyer 2002: 6-8). In addition to these widely recognized advantages, in some cases, qualitative comparative historical research can lead to wholly new framings of traditional research questions by comparing cases that research designs formulated at a greater distance would not put together or might even see as “non comparable.” In such cases, however, establishing equivalency is extremely important and a failure to explicitly articulate the logic of comparison is likely to lead reviewers to misunderstand and therefore discount research proposals.

The general point is that qualitative researchers should take care to articulate explicitly the logic governing their selection of cases, whether or not the research has an explicit comparative dimension.

(C) Attending to issues of application/replication and specifying the scope conditions that apply to the claims being advanced.

Issues of replication and falsifiability are often raised in connection with qualitative work, and so it is important for researchers to address explicitly how the claims advanced in the work can be put “at risk” (or alternatively, how other researchers might “check the work” of their colleagues who employ qualitative methods). In doing so, scholars might keep in mind the several different kinds or levels of application/replication that could be brought to bear, which include but are not limited to:

(1) Same cases, new data: Whereas in some quantitative research, a case can consist of a single “data point,” in comparative historical work any given “case” will consist of a multitude of observations. In many ways the strong empirical grounding of claims made in much qualitative research makes putting the findings of that research “at risk” a much more straightforward task than it is for theories that employ highly pliable concepts formulated at great distance from the empirical cases. Other researchers interested in “checking the work” of the comparative-historical scholar can go one of two routes, either “revisit” the same documents and sources as the original researcher, or collect additional observations on the case that could confirm or disconfirm the interpretation or claims being advanced.

(2) Different cases but within the scope conditions stipulated in the original design: Often qualitative research based on close examination of a limited number of cases is designed to apply to a larger number of cases that are seen to be similar in terms of the core causal mechanisms or relationships observed in the smaller sample. In such cases, the theory can be tested against additional observations outside the original cases but within the broader category of phenomena. This requires that the researcher be very explicit on the scope conditions that determine the boundaries of such applications. What is this a case of? To what class of cases can the theory be meaningfully applied? These scope conditions can be defined either with reference to empirical (for example, geographic or temporal) bounds, or with reference to theory, but either way, being explicit about the “reach” of the theoretical propositions set out will be crucial to ensuring that appropriate standards are applied to the evaluation of the work.

(3) Different cases outside the original scope but employing concepts as originally defined for use inside. It is often the case that qualitative research yields insights that apply far more broadly than for the class of cases for which the research was designed. Recent work on path dependence and policy feedback is an example (e.g., Pierson 1994). Originally designed to explain the resilience of welfare policies in the rich democracies, Pierson identifies causal mechanisms (positive feedback, increasing returns effects) that can be applied more broadly, and the broader application of these concepts yields further insights into the important characteristics of their operation. In such instances, the presence of similar causal mechanisms observed in other cases is not a confirmation of the original (welfare state) theory, but nor is their failure to obtain in some other case a disconfirmation of it. Rather, application of the concepts employed beyond the original case or cases can lead to refinement of those concepts and/or a specification of the general conditions in which such causal mechanisms obtain. In other words, whereas contrary findings in (1) and (2) might be considered disconfirming, contrary findings in (3) would not be strictly speaking disconfirming, though they might well lead to refinement of the concepts and/or theory.

In sum and in general, qualitative researchers should devote explicit attention to questions of scope conditions and appropriate application of concepts employed in their research, so that other researchers can test (and appropriately apply) the claims advanced in the study.

(D) What areas or topics are most promising for investigations using qualitative methods?

In my field, the debates on the “absolute” merits of different approaches and methods may be subsiding and giving way to a more constructive mutual engagement process that taps into the relative strengths of different modes of analysis based on the kinds of empirical puzzles scholars are trying to solve. Thus, for example, a number of authors have suggested that formal (mathematical) models are most fruitfully applied in contexts in which the rules and parameters of interaction are established, stable, and well known (e.g., Bates 1997; Geddes 1995). By contrast to this, the strength of a good deal of qualitative comparative historical work is precisely in the leverage it provides on understanding *configurations of institutions* (Katznelson 1997) and over *longer stretches of time* (Pierson 2004) – including where the parameters themselves are changing. In thinking of the specific areas or topics that seem particularly promising for investigations using qualitative methods (question 3 of workshop charge), I would emphasize two: the study of temporal sequences unfolding over time, and the development of theory on institutional origins and evolution. The first of these is in fact a longstanding strength of comparative historical work in sociology and political science, the second defines a newer research frontier that is currently being pursued by scholars associated with a variety of different methodological and theoretical orientations. I’ll say a few words about each.

*Macro historical processes and political outcomes.* Comparative historical scholarship, both the classics and contemporary scholars, has always attached a great deal of importance to issues of sequencing and timing in the analysis of important macro-historical processes (Shefter 1977; Gerschenkron 1962; Lipset and Rokkan 1968; Ertman 1997). A large literature on “critical junctures” of various sorts has probed the significance (for a variety of political outcomes) of the interaction effects among different processes as they unfold over time, and as they unfold differently (including in a different order) in diverse contexts (e.g., Collier and Collier 1991). The attention to sequencing in historical institutional research is partly motivated by the old truism that in order to establish causality you have to demonstrate not just a correlation between two variables, but also provide some mechanism or theoretical account showing why this linkage exists (Rueschemeyer and Stephens 1997). However, beyond that, the emphasis on timing and sequencing in historical institutional research is also motivated by the insight, borne out in a number of studies (and emphasized especially by Pierson recently) that *when* things happen can itself be an extremely important part of the causal story (Pierson 2000). Time series data can be useful for sorting through some of these issues, but qualitative research methods – which specifically focus on processes unfolding over time and the interaction effects among intersecting processes – are explicitly attuned to them as a matter of design. Related to this, macro historical comparative research is well equipped to uncover the deep long-term causal connections between temporally distant “causes” and “effects,” thus making a distinctive contribution in political science (at least) where explanations are frequently sought in the immediate temporal vicinity of the outcome to be explained (Pierson 2004: esp 96-102). An example is James Mahoney’s explanation for contemporary patterns of development (and underdevelopment) which links these

outcomes to colonial legacies (as opposed to other contemporary causal variables such as market reform, etc.)

*Institutional genesis, reproduction and change.* A good deal of work in political science is organized around the study of how institutions shape political outcomes – with controversy and debate centering less on whether or not institutional factors are significant (most scholars agree they are) than about how best to define institutions and to organize the study of their effects. Many, perhaps most, studies take institutions as “given,” and work forward from there to institutional effects (i.e., institutions as independent variables invoked to explain some other outcome). However, a growing number of scholars have begun to turn their attention to issues of institutional creation, reproduction, and change – and on these questions qualitative methods hold special promise. In terms of *institutional origins*, for example, comparative historical research in my field has been employed to identify “critical junctures” – turning points that established important institutional parameters that subsequently shape what is politically possible, even conceivable – thus illuminating aspects of political life that do not emerge through other sorts of analytic strategies or points of departure. In terms of *institutional stability or continuity*, a number of authors already mentioned above have explored the processes of “positive feedback” that account for the stable reproduction of particular rules or arrangements over time (see also Skocpol 1992; and Mahoney 2000). And finally, qualitative research is especially well suited to addressing questions of *institutional evolution and change* and especially modes of change that are incremental but cumulatively transformative (therefore also unfolding in many cases over long periods of time) (see, among others Thelen 2004; Streeck and Thelen 2005). Qualitative research – with its emphasis on process -- has distinct advantages over alternative non-qualitative methods when dealing with all of these general issues.

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