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Author(s): Sidney Tarrow
Review by: Sidney Tarrow
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BRIDGING THE QUANTITATIVE-QUALITATIVE DIVIDE IN POLITICAL SCIENCE

SIDNEY TARROW Cornell University

In Designing Social Inquiry, Gary King, Bob Keohane and Sidney Verba (KKV) have performed a real service to qualitative researchers. I, for one, will not complain if I never again have to look into the uncomprehending eyes of first-year graduate students when I enjoin them (pace Przeworski and Teune) to "turn proper names into variables." The book is brief and lucidly argued and avoids the weighty, muscle-bound pronouncements that are often studded onto the pages of methodological manuals.

But following KKV's injunction that "a slightly more complicated theory will explain vastly more of the world" (p. 105), I will praise them no more but focus on an important weakness in the book. Their central argument is that the same logic that is "explicated and formalized clearly in discussions of quantitative research methods" underlies—or should—the best qualitative research (p. 4). If this is so, then they really ought to have paid more attention to the relations between quantitative and qualitative approaches and what a rigorous use of the latter can offer quantifiers. But while they offer a good deal of generous (at times patronizing) advice to qualitatively oriented scholars, they say very little about how qualitative approaches can be combined with quantitative research. Especially with the growth of choice-theoretic approaches, whose users often illustrate their theories with stories, there is a need for a set of ground rules on how to make intelligent use of qualitative data.

KKV do not address this issue. Rather, they use the model of quantitative research to advise qualitative researchers on how best to approximate good models of descriptive and causal inference. (Increasing the number of observations is their cardinal operational rule.) But in today's social science world, how many social scientists can be simply labeled "qualitative" or "quantitative"? How often, for example, do we find support for sophisticated game-theoretic models resting on the use of anecdotal reports or on secondary evidence lifted from one or two qualitative sources? More and more frequently in today's social science practice, quantitative and qualitative data are interlarded within the same study. A recent work that KKV warmly praise illustrates both that their distinction between quantitative and qualitative researchers is too schematic and that we need to think more seriously about the interaction of the two kinds of data.

Marinating Putnam

In Robert Putnam's (1993) analysis of Italy's creation of a regional layer of government, Making Democracy Work, countless elite and mass surveys and ingenious quantitative measures of regional performance are arrayed for a 20-year period of regional development.

On top of this, he conducted detailed case studies of the politics of six Italian regions, gaining, in the process, what KKV recommend as "an intimate knowledge of the internal political maneuverings and personalities that have animated regional politics over the last two decades" (p. 5) and Putnam calls "marinating yourself in the data" (Putnam 1993, 190). KKV use Making Democracy Work to praise the virtues of "soaking and poking," in the best Fenno tradition (p. 38).

But Putnam's debt to qualitative approaches is much deeper and more problematic than this; for after spending two decades administering surveys to elites and citizens in the best Michigan mode, he was left with the task of explaining the sources of the vast differences he had found between Italy's north-central and southern regions. To find them, his quantitative evidence offered only indirect evidence; and he turned to history, repairing to the halls of Oxford, where he delved deep into the Italian past to fashion a provocative interpretation of the superior performance of the northern Italian regional governments vis-à-vis the southern ones. This he based on the civic traditions of the (northern) Renaissance city-states, which, according to him, provide "social capital" that is lacking in the traditions of the South (chap. 5). A turn to qualitative history (probably not even in Putnam's mind when he designed the project) was used to interpret cross-sectional, contemporary quantitative findings.

Putnam's procedure in Making Democracy Work pinpoints a problem in melding quantitative and qualitative approaches that KKV's canons of good scientific practice do not help to resolve. For in delving into the qualitative data of history to explain our quantitative findings, by what rules can we choose the period of history that is most relevant to our problem? And what kind of history are we to use; the traditional history of kings and communes or the history of the everyday culture of the little people? And how can the effect of a particular historical period be separated from that of the periods that precede or follow it? In the case of Making Democracy Work, for example, it would have been interesting to know (as Suzanne Berger asked at the 1994 APSA roundtable devoted to the book) by what rules of inference Putnam chose the Renaissance as determining of the North's late twentieth-century Italian civic superiority. Why not look to its sixteenth-century collapse faced by more robust monarchies, its nineteenth-century military conquest of the South, or its 1919–21 generation of fascism (not to mention its 1980s corruption-fed pattern of economic growth)? None of these are exactly "civic" phenomena; by what rules of evidence are they less relevant in "explaining" the northern regions' civic superiority?
over the South than of the Renaissance city-states? Putnam does not tell us; nor do KKV.

To generalize from the problem of Putnam's book, qualitative researchers have much to learn from the model of quantitative research. But their quantitative cousins who wish to profit from conjoining their findings with qualitative sources need, for the selection of qualitative data and the intersection of the two types, rules just as demanding as the rules put forward by KKV for qualitative research on its own. I shall sketch some useful approaches to bridging the quantitative-qualitative gap from recent examples of comparative and international research.

Tracing Processes To Interpret Decisions

One such rule that KKV cite favorably is the practice of process tracing, in which the researcher looks closely at 'the decision process by which various initial conditions are translated into outcomes' (p. 226, quoting George and McKeown 1985, 35). But even here, KKV interpret the advantages of process tracing narrowly, assimilating it to their favorite goal of increasing the number of theoretically relevant observations (p. 227). As George and McKeown actually conceived it, the goal of process tracing was not to increase the number of discrete decision stages and aggregate them into a larger number of data points but to connect the phases of the policy process and enable the investigator to identify the reasons for the emergence of a particular decision through the dynamic of events (George and McKeown 1985, 34–41). Process tracing is different in kind from observation accumulation and is best employed in conjunction with it—as was the case, for example, in the study of cooperation on economic sanctions by Lisa Martin (1992) that KKV cite so favorably.

Systematic and Nonsystematic Variable Discrimination

KKV give us a second example of the uses of qualitative data but, once again, underestimate its particularity. They argue that the variance between different phenomena "can be conceptualized as arising from two separate elements: systematic and nonsystematic differences," the former more relevant to fashioning generalizations than the latter (p. 56). For example, in the case of conservative voting in Britain, systematic differences include such factors as the properties of the district, while unsystematic differences could include the weather or a flu epidemic at the time of the election. "Had the 1979 British elections occurred during a flu epidemic that swept through working-class houses but tended to spare the rich," they conclude, "our observations might be rather poor measures of underlying Conservative strength" (pp. 56–57).

Right they are, but this piece of folk wisdom hardly exhausts the importance of nonsystematic variables in the interpretation of quantitative data. A good example comes from how the meaning and extension of the strike changed as systems of institutionalized industrial relations developed in the nineteenth century. At its origins, the strike was spontaneous, uninstitutionalized, and often accompanied by whole-community "turnouts." As unions developed and governments recognized workers' rights, the strike broadened to whole sectors of industry, became an institutional accompaniment to industrial relations, and lost its link to community collective action. The systematic result of this change was permanently to affect the patterns of strike activity. Quantitative researchers like Michelle Perrot (1986) documented this change. But had she regarded it only as a case of "nonsystematic variance" and discarded it from her model, as KKV propose, Perrot might well have misinterpreted the changes in the form and incidence of the strike rate. Because she was as good a historian as she was a social scientist, she retained it as a crucial change that transformed the relations between the strike incidence and industrial relations.

To put this more abstractly, distinct historical events often serve as tipping points that explain the interruptions in an interrupted time-series, permanently affecting the relations between the variables (Griffin 1992). Qualitative research that turns up "nonsystematic variables" is often the best way to uncover such tipping points. Quantitative research can then be reorganized around the shifts in variable interaction that such tipping points signal. In other words, the function of qualitative research is not only, as KKV seem to argue, to peel away layers of unsystematic fluff from the hard core of systematic variables but also to assist researchers to understand shifts in the value of the systematic variables.

Framing Qualitative Research within Quantitative Profiles

These two uses of qualitative data pertain largely to aiding quantitative research. But this is not the only way in which social scientists can combine quantitative and qualitative approaches. Another is to focus on the qualitative data, using a systematic quantitative data base as a frame within which the qualitative analysis is carried out. Case studies have been validly criticized as being based on often dramatic but frequently unrepresentative cases. Studies of successful social revolutions often possess characteristics that may also be present in unsuccessful revolutions, rebellions, riots, and ordinary cycles of protest (Tilly 1993, 12–14). In the absence of an adequate sample of revolutionary episodes, no one can ascribe particular characteristics to a particular class of collective action.

The representativity of qualitative research can never be wholly assured until the cases become so numerous that the analysis comes to resemble quantitative research (at which point the qualitative research risks losing its particular properties of depth, richness and process tracing). But framing it within a quantitative data base makes it possible to avoid generalizing on the occasional "great event" and points to less dramatic—but cumulative—historical trends.

Scholars working in the "collective action event"
history tradition have used this double strategy with success. For example, in his 1993 study of over seven hundred revolutionary years in over five hundred years of European history, Charles Tilly assembled data that could have allowed him to engage in a large-N study of the correlates and causes of revolution. Tilly knows how to handle large time-series data sets as well as anybody. But he did not believe that the concept of revolution had the monolithic quality that other social scientists had assigned to it (1993, chap. 1). So he resisted the temptation for quantification, using his data base, instead, to frame a series of regional time-series narratives that depended as much on his knowledge of European history as on the data themselves. When a problem cried out for systematic quantitative analysis (e.g., when it came to periodizing nationalism), Tilly (1994) was happy to exploit the quantitative potential of the data. But the quantitative work, he observed mainly as a frame for qualitative analysis of representative regional and temporal revolutionary episodes and series of episodes.

Putting Qualitative Flesh on Quantitative Bones

These examples are possibly exotic to the traditions of much of American social science practice. But an American sociologist, Doug McAdam, has shown how social science can be enriched by combining quantitative and qualitative approaches to the same data base. McAdam’s 1988 study of Mississippi Freedom Summer participants was based on a treasure-trove of quantifiable data—the original questionnaires of the prospective Freedom Summer volunteers. While some of these young people eventually stayed home, others went south to register voters, teach in “freedom schools” and risk the dangers of Ku Klux Klan violence. Two decades later, both the volunteers and the no-shows could be interviewed by a researcher with the energy and the imagination to go beyond the use of canned data banks.

McAdam’s main analytic strategy was to carry out a paired comparison between the questionnaires of the participants and the stay-at-homes and to interview a sample of the former in their current lives. This systematic comparison formed the analytical spine of the study and of a series of technical papers. But except for a table or two in each chapter, the texture of Freedom Summer is overwhelmingly qualitative. McAdam draws on his interviews with former participants, as well as on secondary analysis of other people’s work, to get inside the Freedom Summer experience and to highlight the effects that participation had on their careers and ideologies and their lives since 1964. With this combination of quantitative and qualitative approaches, he was able to tease a convincing picture of the effects of Freedom Summer activism from his data.

As I write this, I imagine KKV exclaiming, “But this is precisely the direction we would like to see qualitative research moving—toward expanding the number of observations and respecting hypotheses to allow them to be tested on different units!” (see chap. 6). But would they argue, as I am, that it is the combination of quantitative and qualitative methods trained on the same problem (not a move toward the logic of quantitative analysis alone) that is desirable? Two more ways of combining these two logics illustrate my intent.

Sequencing Quantitative and Qualitative Research

The growth industry of qualitative case studies that followed the 1980–81 Solidarity movement in Poland largely took as given the idea that Polish intellectuals had the most important responsibility for the birth and ideology of this popular movement. There was scattered evidence for this propulsive role of the intellectuals; but since most of the books that appeared after the events were written by them or by their foreign friends, an observer bias might have been operating to inflate their importance in the movement vis-à-vis the working class that was at the heart of collective action in 1980–81 and whose voice was less articulate.

Solid quantitative evidence came to the rescue. In a sharp attack on the “intellectualist” interpretation and backed by quantitative evidence from the strike demands of the workers themselves, Roman Laba showed that their demands were overwhelmingly oriented toward trade union issues and showed little or no effect of the proselytizing that Polish intellectuals had supposedly been doing among the workers of the Baltic coast since 1970 (1991, chap. 8). This finding dovetailed with Laba’s own qualitative analysis of the development of the workers’ movement in the 1970s and downplayed the role of the Warsaw intellectuals who had been at the heart of a series of books by their foreign friends.

The response of those who had been responsible for the intellectualist interpretation of Solidarity was predictably violent. But there were also more measured responses that shed new light on the issue. For example, prodded by Laba’s empirical evidence of worker self-socialization, Jan Kubik returned to the issue with both a sharper analytical focus and better qualitative evidence than the earlier intellectualist theorists had employed, criticizing Laba’s conceptualization of class and reinterpreting the creation of Solidarity as “a multistranded and complicated social entity . . . created by the contributions of various people” whose role and importance he proceeded to demonstrate (1994, 230–38). Moral: a sequence of contributions using different kinds of evidence led to a clearer and more nuanced understanding of the role of different social formations in the world’s first successful confrontation with state socialism.

Triangulation

I have left for last the research strategy that I think best embodies the strategy of combining quantitative and qualitative methods—the triangulation of different methods on the same problem. Triangulation is particularly appropriate in cases in which quantitative data are partial and qualitative investigation is obstructed by political conditions. For example, Val-
erie Bunce used both case methodology and quantitative analysis to examine the policy effects of leadership rotation in Western and socialist systems. In *Do New Leaders Make a Difference?*, she wrote, "I decided against selecting one of these approaches to the neglect of the other [the better] to test the impact of succession on public policy by employing both methodologies" (1981, 39).

Triangulation is also appropriate in specifying hypotheses in different ways. Consider the classical Tocquevillian insight that regimes are most susceptible to a political opportunity structure that is partially open. The hypothesis takes shape in two complementary ways: (1) that liberalizing regimes are more susceptible to opposition than either illiberal or liberal ones; and (2) that within the same constellation of political units, opposition is greatest at intermediate levels of political opportunity. Since there is no particular advantage in testing one version of the hypothesis over the other, testing both is optimal (as can be seen in the recent social movement study, Kriesi et al. 1995).

My final example of triangulation comes, with apologies, from my own research on collective action and social movements in Italy. In the course of a qualitative reconstruction of a left-wing Catholic "base community" that was active in a peripheral district of Florence in 1968, I found evidence that linked this movement discursively to the larger cycle of student and worker protest going on in Italy at the same time (Tarrow 1988). Between 1965 and 1968, its members had been politically passive, focusing mainly on neighborhood and educational issues. But as the worker and student movements exploded around it in 1968, their actions became more confrontational, organized around the themes of autonomy and internal democracy that were animating the larger worker and student movements around them.

Researchers convinced of their ability to understand political behavior by interpreting "discourse" might have been satisfied with these observations; but I was not. If nothing else, Florence was only one case among potential thousands. And in today's global society, finding thematic similarity among different movements is no proof of direct diffusion, since many movements around the world select from the same stock of images and frames without the least connection among them (Tarrow 1994, chap. 11).

As it happened, quantitative analysis came to the rescue to triangulate on the same problem. For a larger study, I had collected a large sample of national collective action events for a period that bridged the 1968 Florentine episode. And as it also happened, two Italian researchers had collected reliable data on the total number of religious "base communities" like the Florentine one throughout the country (Scibbba and Pace 1976). By reoperationizing the hypothesis cross-sectionally, I was able to show a reasonably high positive correlation (R = .426) between the presence of Catholic base communities in various cities and the magnitude of general collective action in each city (Tarrow 1989, 200). A longitudinal, local, and qualitative case study triangulated with the results of cross-sectional, national, and quantitative correlations to turn my intuitive hunch that Italy in the 1960s underwent an integrated cycle of protest into a more strongly supported hypothesis.

KKV are not among those social scientists who believe that quantification is the answer to all the problems of social science research. But their single-minded focus on the logic of quantitative research (and of a certain kind of quantitative research) leaves underspecified the particular contributions that qualitative approaches make to scientific research, especially when combined with quantitative research. As quantitatively trained researchers shift to choice-theoretic models backed up by illustrative examples (often containing variables with different implicit metrics), the role of qualitative research grows more important. We are no longer at the stage when public choice theorists can get away with demonstrating a theorem with an imaginary aphorism. We need to develop rules for a more systematic use of qualitative evidence in scientific research. Merely wishing that it would behave as a slightly less crisp version of quantitative research will not solve the problem.

This is no plea for the veneration of historical uniqueness and no argument for the precedence of "interpretation" over inference. (For an excellent analysis of the first problem, see KKV pp. 42-3 and of the second, pp. 36-41.) My argument, rather, is that a single-minded adherence to either quantitative or qualitative approaches straitjackets scientific progress. Whenever possible, we should use qualitative data to interpret quantitative findings, to get inside the processes underlying decision outcomes, and to investigate the reasons for the tipping points in historical time-series. We should also try to use different kinds of evidence together and in sequence and look for ways of triangulating different measures on the same research problem.

KKV have given us a spirited, lucid, and well-balanced primer for training our students in the essential unity of social science work. Faced by the clouds of philosophical relativism and empirical nominalism that have recently blown onto the field of social science, we should be grateful to them. But their theoretical effort is marred by the narrowness of their empirical specification of qualitative research and by their lack of attention to the qualitative needs of quantitative social scientists. I am convinced that had a final chapter on combining quantitative and qualitative approaches been written by these authors, its spirit would not have been wildly at variance with what I have argued here. As it is, someone else will have to undertake that effort.

Notes

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