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Letter from the President Comparative-Historical Analysis: Where Do We Stand?

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Following the intellectual success in the 1960s and 1970s of an earlier generation of comparative-historical analysis, led by scholars such as Gerschenkron, Moore, Bendix, Lipset and Rokkan, Tilly, and Skocpol, this approach has been extended and consolidated by a series of valuable studies published in the 1980s and 1990s. This new work includes ongoing contributions by Tilly and Skocpol, as well as books by Luebbert, Linz and Stepan, Pierson, and many other authors noted below.¹ In its earlier iteration, this literature played a central role in advancing the idea that countries may follow different paths of national political development, and that political and social conflict are often crucial features of these alternative paths. In both the earlier and more recent iterations, these studies have offered new explanations for outcomes of great political and normative importance: contrasting types of national states and of specific state institutions, national political regimes (e.g., authoritarian or democratic), the structure of national political economies, revolutions and rebellions, political parties and types of party systems, and major public policies, including the creation and retrenchment of the welfare state.

At the same time that comparative-historical studies have established themselves as an enduring tradition of research, important critiques have been advanced concerning the scope of comparison and the methodology employed. These critiques need to be evaluated as we assess the evolving role of comparative-historical scholarship, which I am convinced continues to make a central contribution to the field of comparative politics.

Comparative-Historical Analysis

The tradition of comparative-historical analysis can be identified in different ways. At the risk of excluding some important studies, I focus on three defining attributes: 1) a sustained focus on a well-defined set of national cases; 2) a con-

cern with a substantial time frame and with the unfolding of causal processes over time; and 3) the use of systematic comparison to generate and/or evaluate explanations of outcomes at the level of national politics. Within what is typically a small-N framework, these studies employ varying mixes of qualitative and quantitative data, with increasing use of the latter as better quantitative data sets have become available.

While many studies possess all three defining attributes, some may lack one of them, and in this sense they have a "family resemblance" to this tradition. For example, they may construct their historical comparisons within a single national case, yet they are centrally concerned with placing that case in a comparative perspective that draws on this broader literature. Other variants are identified in Skocpol and Somers's well-known typology of approaches to comparison.

Although many comparative studies are certainly not a direct outgrowth of this tradition, the continuing vitality of comparative-historical analysis has encouraged a number of scholars to embark on broad, systematic comparison which they otherwise might not have undertaken. I am also convinced that this body of scholarship is part of the inspiration for the current emphasis on multi-country doctoral dissertations in comparative politics. The comparative-historical tradition has in this sense contributed to resetting the parameters of comparison in our field. Given the recent focus on *deductive* work in many general discussions of theory and

method, it is likewise appropriate to underscore the *inductive* component of these comparisons. In conjunction with bringing a variety of different theoretical and conceptual tools to their research, many scholars in the tradition of comparative-historical analysis are centrally concerned with how the process of comparison itself contributes both to the iterated fine-tuning of concepts, and to the discovery and refinement of new explanations.

Some Critiques

An insightful essay by Ira Katznelson has raised questions about the scope of comparison employed in these studies, and debates initiated by John Goldthorpe and Stanley Lieberman within the field of historical sociology have raised important methodological issues, three of which are addressed below.² Katznelson's concern is that this approach has not succeeded in sustaining its initial creativity and ambition in the subsequent generation of scholarship. Instead, he sees a loss of intellectual momentum, a narrowing of research questions and comparisons, and a failure to command the center of attention in debates on comparative analysis to the degree achieved by the earlier generation of work.

Several observations can be made regarding the scope of comparison. If one considers, from the perspective of the 1960s, the striking novelty of what, for example, Moore or Bendix achieved – with the juxtaposition of large research questions, broad comparisons, and the impressive marshalling of historical evi-

dence – one would not necessarily expect that subsequent studies which followed the model of these authors could command the same level of attention. In addition, compared with the grand sweep of Moore and Bendix, many recent studies identified with this tradition have undertaken more limited comparisons, focusing on a shorter time-span, on a single world region, and in some instances on just two cases, or even a single national case. In fact, what these more sharply focused works may lose in scope of comparison they routinely gain in depth of insight, as in the books of Barkey, Bergquist, Chaudhry, de Swaan, Ekiert, Gould, Hall, Luebbert, Pierson, Scully, Skowronek, Wickham-Crowley, and Yashar, as well as Paige (*Coffee and Power*) and Skocpol (*Soldiers and Mothers*). At the same time, a number of recent studies sustain a longer historical reach or encompass a broader range of cases, including Downing, Ertman, Evans, Goldstone, Goodwin, Mann, and Silberman, as well as Haas (*Nationalism, Liberalism, and Progress*), Tilly (*Coercion, Capital, and European States*), and books on democratization by Haggard and Kaufman, Linz and Stepan, and Rueschemeyer, Stephens, and Stephens.

The more limited and the more sweeping comparisons are both valuable, and this tradition of research gains from the juxtaposition of the two. Hence, I think Katznelson is too pessimistic about the direction this literature has taken.

With regard to the methodological critiques, the first of these suggests that the political and social processes studied are conceptualized at an excessively macro and aggregated level. This is seen as an obstacle to careful analysis, and especially to the assessment of causation. The idea of “huge comparisons,” which was initially advanced as an endorsement of the analytic scope of these

studies, can also be understood as suggesting a focus on phenomena that are so broadly defined that it is hard to get strong analytic purchase on causal processes. However, many studies in this tradition in fact employ some variant of a “micro-foundations” approach, in that their explanation of macro outcomes focuses in part on the goals and strategic calculations of individual actors or specific clusters of actors. For example, *Liberalism, Fascism, or Social Democracy* by the late Gregory Luebbert (after whom the Comparative Politics Section book and article prizes are named) explains broad outcomes, i.e., national regimes and national political economies in inter-war Europe, in terms of the calculations and coalitional options of specific class actors and political parties. Other examples include Goodwin, and Collier and Collier. I might add that the idea of bridging this presumed divide between a macro and a micro focus is underscored in the introduction to Robert Bates et al.’s forthcoming *Analytic Narratives* volume. These authors, some of whom come out of quite a different tradition from that of comparative-historical work, likewise combine a careful focus on micro-foundations with a strong commitment to explaining broad outcomes. I think more scholars in the comparative-historical tradition should link these two levels of analysis.

A second methodological critique suggests that reliance on J. S. Mill’s methods of agreement and difference as basic tools of causal inference is a serious limitation of comparative-historical studies. These methods do not lend themselves to the analysis of multiple explanatory factors or interaction effects, nor to the incorporation of a probabilistic view of causation or any notion of measurement error. Yet in fact, many works of comparative-historical analysis employ a complex mix of tools for causal inference, including not only

the matching and contrasting of cases entailed in these two methods of Mill, but also ordinal comparison and diverse forms of “within case” analysis, including process tracing.³ Used together, these tools provide a considerably stronger basis for causal inference. No one imagines that the use of such tools can deal with error, or with a probabilistic view of causation, in a way that is equivalent, for example, to regression analysis. They can, however, offer other kinds of insight into causation not provided by regression analysis or other large-N approaches. This suggests that scholars may face an important trade-off in the type of data, and the corresponding tools for causal inference, with which they choose to work.

A third methodological critique of comparative-historical research concerns its extensive reliance on secondary sources, and especially the filtering of information and the particular interpretations of events built into such sources. It is indeed true that scholars engaged in research on many contemporary, as opposed to historical, topics may have more control over the data available to them. In addition, it is certainly admirable when scholars doing comparative-historical analysis can utilize primary sources (e.g., Yashar, and Skocpol’s *Soldiers and Mothers*), although currently this approach may have more adherents in historical sociology than in political science. In fact, the practice of basing comparative studies on secondary sources is widespread in political science and is hardly a distinctive issue in comparative-historical research. Further, given the availability of a massive monographic literature on major historical features of politics and political economy in country after country, it would be a great loss if scholars did not build synthetic research on this potentially invaluable “data base.” In doing so, more schol-

ars need to follow the practice that is essential in analyzing any kind of data: being explicit about rival interpretations of the data (in this case, including the interpretations contained in the secondary sources) and candid about the implications of these interpretations for the overall conclusions of the study.⁴

The use of secondary sources also has a distinctive strength, in that it lends itself to the replication of research.⁵ Thus, again, there appears to be a trade-off in terms of alternative methodological priorities. In works of comparative-historical analysis based on secondary sources, the bibliography in effect identifies a data set that can be accessed by any scholar to reevaluate the findings of the study. Whereas the political science discipline has had to establish elaborate norms and procedures for making available the data sets employed in published quantitative research, with comparative-historical studies the secondary sources identified in the footnotes and bibliography typically constitute a body of data that is routinely available through libraries.

These methodological critiques thus raise three issues that practitioners of comparative-historical analysis need to consider carefully. Two of them, concerning the macro-micro linkage and the need to move beyond Mill's categorical methods, are in fact already being addressed by some scholars in this tradition. In addition, existing practices have compensating strengths, as with the alternative kinds of insight into causal relations that derive from small-N research and the relative ease of re-analyzing secondary sources. Hence, one must think in terms of trade-offs, and these trade-offs require close examination.

Conclusion

In conclusion, I would like to underscore two other points. First, there

is an ongoing need for synthetic studies that integrate the findings of books like those discussed above, and especially for efforts at synthesis that cut across the different substantive topics addressed in this literature. Such synthetic work could give us greater insight into what is and is not being accomplished by undertaking comparisons of this scope. Second, and relatedly, the theoretical underpinnings of this tradition of research require more attention. For example, along the lines of current work by Paul Pierson, the idea of "path dependence," as applied to political analysis, needs to be developed into a more fully articulated set of arguments about the historical and institutional foundations of political change.⁶ Such refinements should call attention to an advantage of this approach: the longer time frame within which causal processes are examined creates an opportunity to move beyond taking actors and preferences as given, and to consider instead how they are constituted. The effort to strengthen these analytic underpinnings will help to bring into sharper focus the distinctive contribution of the comparative-historical tradition.

¹ A bibliography of comparative-historical studies and methodological commentaries relevant to this letter is available at:

<<http://www.polisci.berkeley.edu:9000/faculty/dcollier.html>>

² Ira Katznelson, "Structure and Configuration in Comparative Politics," in Mark I. Lichbach and Alan S. Zuckerman, eds., *Comparative Politics: Rationality, Culture, and Structure* (Cambridge: Cambridge University Press, 1997). The debates in historical sociology are found in the *British Journal of Sociology*, Vols. 42 and 45, June 1991 and March 1994; *Comparative Social Research*, Vol. 16,

1997; and *Social Forces*, Vols. 70 and 72, December 1991 and June 1994.

³ See the introduction to Collier and Collier, *Shaping the Political Arena*, as well as James Mahoney, "Nominal, Ordinal, and Narrative Appraisal in Macro-Causal Analysis" (*American Journal of Sociology*, forthcoming).

⁴ On dealing with these rival interpretations, see Ian Lustick, "History, Historiography, and Political Science," *American Political Science Review* 90:3 (September 1996), 605-618.

⁵ For a valuable debate on replication, see this *Newsletter* 7:1 (Winter 1996).

⁶ Paul Pierson, "Increasing Returns, Path Dependence, and the Study of Politics," Jean Monnet Chair Papers No. 44, European University Institute, Florence, Italy, 1997. See also Kathleen Thelen, "Historical Institutionalism in Comparative Politics," *Annual Review of Political Science*, 1999 (Palo Alto: Annual Reviews, Inc., forthcoming).