Studies in American Political Development, 17 (Spring 2003), 107–110.

What's Wrong With APD?

Stephen Skowronek, *Yale University*

In his methodological critique of APD research, John Gerring steers clear of the issues that have bogged down debates about methodology in recent years. He does not argue that historically-oriented students of American politics should adopt some allegedly superior method current in other sectors of political science, nor does he demand clarification of an alternative method that might be more pertinent given some allegedly distinctive qualities of the APD project. Instead, he scores the project for its failure to measure up on universal standards of good research design – clear concepts, clear propositions, clear logic. The resulting critique is, on its face, singularly disarming. What self-respecting scholar wants to be unclear?

Not me. Appearances notwithstanding, I strive by my own lights to avoid getting lost in what Gerring calls "the meat-and-gristle of politics"; I prize tools that promise to cut to the heart of the matter; I think in terms of robust concepts, accessible arguments, and consistent applications. Truth to tell, though I try to avoid compromising clarity for any reason, I also strive to conceptualize problems and processes in ways that seem to me to be fresh and that promise to open up new avenues of exploration. No doubt these interests compete in ways that I am not always fully aware of when putting words on paper, but I do not lightly dismiss reports of confusion, and I react to misunderstandings or miscommunications with a keen sense of my own responsibility.

All this said, I have several concerns about Gerring's critique. One, which comes from trying my own hand at criticisms of this sort, is whether there is any real traction to be gained from them. A related but narrower concern is whether the works Gerring reviews are really as unclear as he makes them out to be. Finally, and most importantly, I wonder whether Gerring's critique of APD does not put the cart before the horse. Combining these last two concerns, I would venture that the central problem confronting APD is not a lack of clarity in individual studies, but a deeper uncertainty about the APD project itself, what it is, or should be, today.

The basic proposition that clarity is good and more is better may be irresistible, but I do not think it lays a foundation for very effective criticism. The problem is that the critic needs to have it both ways: beyond the initial appeal for clarity, everything in this critique rests on faith in the transparency of Gerring's own presentation. Yet, as the whole purpose of the exercise is to show just how muddy the waters can get, it is hard to say in the end where the balance of clarity lies. Consider in this regard how easy it is to turn Gerring's charges against his own. No one will deny that APD is primarily a substantive enterprise or that there are parts of contemporary political science that are method-driven in a way that APD is not, but we are asked here to take it on faith that APD's substantive concerns have turned it into an outlier in the discipline, that it exhibits a "willful avoidance of methodological concerns." This claim, if I understand it correctly, seems to me utterly fantastic. I do not think it is any accident that APD has its center of gravity in political science departments and not in history departments. In fact, the criticism I get from historians runs just the other way: that APD is too intent to cut through the gristle to some allegedly deeper truth, that our analyses are too preoccupied with establishing general propositions and illustrating overarching concepts. It does not suffice in this instance simply to say that Gerring's charge of field indifference is never demonstrated; after all, the absence of more serious attention problems of demonstration, in particular to the presentation of comparative evidence for claims asserted, is part and parcel of his critique of work in the field. More surprising still is that the authors Gerring charges with indifference have themselves published articles that address directly Gerring's concerns, that is, directly consider "methodological issues as they affect the study of American political history."[1]

1. For example: Rogers Smith, "Science Non-Science, and Politics: On Turns to History in Political Science," in *The Historic Turn in the Human Sciences,* ed. Terry McDonald (Ann Arbor, University of Michigan Press, 1996), 119–59; Rogers Smith, "If Politics Matters: Implications for a new Institutionalism," *Studies in American Political Development* 6 (1992):1–16; Rogers Smith, "Political Jurisprudence, the New Institutionalism, and the Future of Public Law,"

© 2003 Cambridge University Press ISSN 0898–588X/03 $12.00

https://doi.org/10.1017/S0898588X03000038 Published online by Cambridge University Press

It hardly needs mention that Gerring's dissection of particular texts also rests on leaps of faith. Anyone who has attended graduate school knows how easy it is to make hash of a book, and knowing something of the appetite of graduate students for criticism, my guess is that just about any book will do. In graduate seminars, however, we can at least assume that everyone has just read the texts for themselves. Gerring's dissections call for special indulgence. It is not simply that, short of plowing through the 1,400 pages of text in question here, the reader has to take it on faith that Gerring is providing a true reading of these texts; it is also that Gerring has placed himself in the particularly difficult position of proving that something is missing, of demonstrating that what he is looking for cannot be found in texts under discussion. He does what he can to make that case: he arranges arguments, finds quotes, and reaches rhetorically for alternative interpretations of the author's meaning. In the end, however, all we have are Gerring's presentments and our own knowledge that it is easy to make hash. Having put myself in this box on occasion, I confess that I have never found an easy way out.

This leads me to my second concern. I have taught the Bensel book many times, and I have ventured my own criticisms of it, but my criticisms were based on what I thought I understood the book to say, not on its impenetrability. If I thought that the *Yankee Leviathan* were anywhere near as "bewildering" as Gerring makes it out to be, I would hardly have wasted the time and effort. This is not to argue that this book is transparent (what book is?); my claim is that any ambiguity in its logic or the analysis is likely to be exposed more effectively, and productively, by direct engagement with its substantive arguments. At the "heart of the book," at least as I read it, is a "feedback effect" with important implications for the development of American politics: in the first period, the Republican party created a client group in the private sector to help it finance its war program; in the second period, the demands of these newly empowered financiers, and their strategic position within the party, undercut the Republicans' postwar program for southern reconstruction. Gerring mentions this proposition in a blur of others only to dismiss it as "difficult to pin down." I think he makes it a lot more difficult than it is, and in doing so, simply side steps the harder task of showing why it is wrong.

My own book, *Building A New American State,* was published twenty years ago, when no one, least of all me, thought of APD as a "field." I count it a credit to the larger enterprise that those who associate themselves with this field today are producing better political science and better political history. Still, I would venture that no American historian would have thought to conceptualize state-building as a distinctive process of political development, much less set up an analysis of state-building the way this book does. As Gerring says, the book compares three arenas of state-building between 1877 and 1920 – the reform of central administration, the reform of the army, and the reform of economic regulation. What he does not say is that the book also sets out criteria for identifying the emergence of a new American state in developments across these spheres during this period. It identifies "stateness" with the intellectual, procedural, and organizational features of governance as these can be observed in each sphere and compares these features before and after the state-building period for evidence of a state transformed. Now one can argue with the robustness of the criteria set forth for observing state-transformation (Bensel's are certainly more elaborate), but these are hardly obscure elements in the analysis; they are outlined on the first page of the second chapter and followed throughout.

> The early American state can be described much as one would describe any other state. Readily comparable determinants of a state's mode of operations will be used here to assess this great anomaly as a working complex of institutions and personnel. These determinants are: the *organizational orientations* of government, the *procedural routines* that tie institutions together within a given organizational scheme, and the *intellectual talents* employed in government. Identifying the early American state along these operational dimensions will capture the distinctive way it performed the basic tasks of government. The reconstruction of state power between 1877 and 1920 will be observed in qualitative changes along each of these dimensions."[2] (emphasis in original)

I agree with Gerring that without such criteria, the transformation of the American state, the shorthand comparisons to other Western states, even the initial characterization of early America as "a state of courts and parties" would seem haphazard and difficult to follow, but that, of course, is precisely why criteria were set out.

The central finding of *Building A New American State* is that institutional developments in these three disparate spheres of governance followed similar pat-

American Political Science Review 82 (1988):89–108. Karen Orren and Stephen Skowronek, "Beyond the Iconoography of Order: Notes for A New Institutionalism," in *The Dynamics of American Politics: Approaches and Interpretations,* ed. Lawrence Dodd and Calvin Jilson (Boulder: Westview, 1994); Karen Orren and Stephen Skowronek, "Institutions and Intercurrence: Theory Building in the Fullness of Time," *Nomos* 38 (1996); Karen Orren and Stephen Skowronek. "In Search of Political Development," in *The Liberal Tradition in American Politics,* ed. David Ericson and Lousia Green (New York: Routledge, 1999), 29–42.

2. Stephen Skowronek, *Building A New American State, The Expansion of National Administrative Capacities* (New York: Cambridge University Press, 1982), 19.

https://doi.org/10.1017/S0898588X03000038 Published online by Cambridge University Press

terns. Gerring finds the patterns difficult to distinguish; however, had he paid attention to the criteria of state transformation, he might have had an easier time of it. In the late nineteenth century, I found evidence in each sphere of a significant change along the intellectual dimension of state operations – a new governing cadre with different ideas about government, and different skills to bring to the task of governing, made its appearance. But on the procedural and organizational dimensions the old mode of governance held fast in each case. State-building was a "patchwork" in this period because, even as new laws were passed and new institutions formed, the basic authority relations established in the early American state remained intact: "Those holding positions of power in the state of courts and parties . . . deflected all broadside challenges to the established mode of governmental operations, and maintained their prerogatives by keeping innovations subordinate to preexisting political and institutional arrangements."[3] In the early twentieth century, under quite different electoral conditions, the procedural and organizational arrangements of the old mode of governance gave way. An extended scramble for advantage among incumbents then ensued, a scramble which implicated all the constitutional branches in a renegotiation of their prerogatives vis à vis new concentrations of bureaucratic power. I called that state-building as reconstitution:

> state building as reconstitution will be observed across our reform areas between 1900 and 1920. During these years, the doors of power finally opened to the champions of national administrative development, and a new struggle ensued over the redistribution of official prerogatives and the reconstruction of constitutional relationships. . . . The new American state emerged with a powerful administrative arm, but authoritative controls over this power locked in a constitutional stalemate.[4]

Now this may all be wrong. Maybe the bureaucracy was more significantly empowered during the late nineteenth century than I say; maybe the course of development was less clearly bifurcated than these specifications suggest; maybe the constitutional stalemate in the control of administrative power (which I say is characteristic of the new American state) was broken some time later down the road; maybe there is some better way to capture the enduring significance of the transformation of American government that occurred during these years. Not only am I open to the presentation of counter evidence and alternative conceptions, I remain an avid consumer and promoter of works that have pushed beyond this one. The point is that when Gerring describes this periodization as "fundamentally misleading," he is referring to the absence of standards, to willful indifference to the problem of conceptualizing processes and distinctions, to an analysis of state building that is nothing more than the sum of its parts. In one of his rhetorical asides, he professes an inability to understand how a state might be weakened by a constitutional stalemate even as it is being strengthened by new forms of bureaucratic power. Perhaps, all this just brings us back in the end to the simple truth that clarity is in the eyes of the beholder.

And yet, I think there is more to it than that. Paradoxically, Gerring's demand for greater clarity in individual works seems to blind him to clarity as it accumulates over time through scholarship that reacts to what has come before. In *Building A New American State,* I tied the patterns I observed to the struggles of politicians jockeying for advantage as the institutional arrangements within which they found themselves came under siege and electoral opportunities for pressing the seize shifted. Gerring finds that the premise of the book – "that states change (or fail to change) through political struggles rooted in and mediated by pre-established institutional arrangements" is "unobjectionable" but unimpressive: "it does not say very much." But this evaluation might say a good deal more about the development of political science in the twenty years since the book's appearance (perhaps even something about the book's contribution) than it does about the force of the claim when it was initially ventured. I believe that the critique of pluralist, Marxist, and functionalist formulations of the state-building process launched by this book, and by others at the same time, altered the study of politics (political science at large, not just APD) fundamentally. It served to highlight the "autonomy" of politics and place the state at center of our understanding of larger processes of change.

This leads me to my own assessment of what ails APD. Writing as a friend and fellow contributor to the APD project, Gerring offers a simple diagnosis and a straight-forward remedy: more careful attention to basic tenets of good research design will dispel doubts about APD's status within contemporary political science. I wish it were that easy. As I said, I think that historically oriented political scientists are, as a group, producing better history and better political science than ever before. Practitioners alert to the requisites of survival in contemporary political science departments are hardly indifferent to the ever more stringent standards of advancement being imposed by the discipline, so my guess is that whatever there is to the problem Gerring outlines is likely to take care of itself. More troublesome to me is that even as the quality of work has improved, the "field" has grown less coherent and less distinct. One reason may be that virtually all ongoing agendas in the discipline have opened themselves to more careful consideration of the past. Current trends are not only multiplying the

3. Ibid., 46.
4. Ibid., 16.

https://doi.org/10.1017/S0898588X03000038 Published online by Cambridge University Press

subject matters addressed in historical research, they are also pushing this research closer to the ground, and pulling it toward more specialized agendas in congressional studies, presidential studies, race studies, gender studies, public policy studies, and the like. Study for study, much of this research is excellent and makes important contributions to the theoretical debates that it engages; as the stuff of a "field," it leaves "APD" hollow at the core.

As Gerring notes, APD has been a great success in several respects. The question, it seems to me, is whether in the wake of this success it can retain a distinctive voice. Its critique of pluralism, of Marxism, of functionalism, of "Hartz" has done its work. What previously passed for an agenda is now common wisdom – the state is "back in"; in one way or another, everyone believes that history "matters"; politics is the now centerpiece of the social problem, not a mere epiphenomenon; and American identity and political culture are no longer taken for granted. All this leaves the field at what practitioners themselves might recognize as a critical juncture. Absent more careful consideration of what the APD project is – of what distinctive and collective contribution its practitioners have to make to the understanding of politics today – it stands simply to dissolve away or become absorbed into everything else. Gerring has little to say on this score. He describes APD practitioners as conflicted and uncertain of their legitimacy, but sees nothing in these conflicts but confusion. His account of APD recalls where its practitioners came from but says nothing about the contemporary problems and prospects facing those who want to theorize about politics historically. Gerring's prescription may well improve the average historical study of American politics, but I don't think that it will do much to revive or sustain the field. I would venture that if we accept Gerring's interpretation of the most pressing problem, there soon won't be much of a field left to take to task.

This is what I mean when I say that I think Gerring's critique puts the cart before the horse. Direct and sustained consideration of the current prospects for historical theorizing about politics will undoubtedly pull along many questions about method and may even bring forth some important methodological innovations. But if problems of method take precedence over the pressing question of where we want to take the study of politics theoretically, we are likely to chase ourselves in circles with nothing distinctive to say.

https://doi.org/10.1017/S0898588X03000038 Published online by Cambridge University Press