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APD from a Methodological Point of View

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Although in existence for only a few decades, the field of American political development (APD) has been an agent of many changes within the discipline of political science. It has been a leading force in the rediscovery of institutions; it has bolstered a comparative approach to American politics; it has helped to draw attention to previously neglected issues such as race, class, and gender; and it deserves primary responsibility for bringing the temporal dimension of politics into consideration.

APD has not, however, been a locus of methodological reflection. While methodology gains in stature throughout the social sciences, and while debate rages over the vices and virtues of new methods and models, surprisingly little consideration has been given to methodological issues as they affect the study of American political history.[1] Heedless of current academic fashion, APD has retained a stubborn focus on the meat-and-gristle of politics, on the twists and turns of social policy, on the institutional development of the American state, on the causes and consequences of racial division, and on many other topics of historical and contemporary concern.

The willful avoidance of methodological concerns might be looked upon as a virtue. Indeed, the current attractiveness of the politics-and-history subdivision of political science probably derives, in part, from its dogged pursuit of the who-what-where-when-and-why. For those who like politics, APD cannot help but intrigue. And it is absorbing precisely because one is given license to ask big questions, to deal with things that matter, and to discuss real events with texture and detail. In the context of political science's current preoccupation with formal models, narrowly-defined empirical puzzles, microfoundations, and increasingly complex analytic techniques, APD has been a welcome breath of hot air.[2]

Yet, in acknowledging the virtues of substance, we might also consider the costs of APD's free-wheeling approach to questions of method. Does the neglect of methodology matter? Has it hampered the advance of APD as a subfield of political science? Has it hindered the interchange between APD-ers and historians? Does APD have anything to learn from "methodology"?

THE ORIGINS OF APD: A BRIEF, SCHEMATIC TREATMENT

The origins of APD[3] might be traced back to work on realignment theory by V.O. Key, Walter Dean Burnham, E.E. Schattschneider, and others, and to the immense field of study devoted to the failure of socialism and the American *Sonderweg*, within which the oeuvre of Seymour Martin Lipset deserves special mention.[4] As teachers, the influence of Ted Lowi and

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1. To be sure, APD-ers have engaged methodological debates; but they have not engaged in methodological debates *about* APD. When APD has been debated the focus has been substantive – on models and theories used to reconstruct and explain the past – not methodological.

2. James A. Morone, "Get Lost! Find Your Own Wittgenstein!: On Politics, History, and Political Science." *CLIO* 10 (1999–2000).

3. We must define our subject loosely, since APD is not a well-bounded subfield. At minimum, APD refers to work by political scientists who study American political history. It is clearly more than that, but the additional attributes are more difficult to identify and have also changed considerably since the inception of the subfield, as we shall see. Thus, we fall back on an ostensive definition. APD is what people say it is; that is, it includes those who identify with the label and those who are commonly identified with the label. For further reflections on the utility and disutility of APD as a moniker for work on American political history, see John Gerring, "From APD to APH?," *CLIO* 12 (2001–2002):37–39.

4. Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton, 1970); William Nisbet Chambers and Walter Dean Burnham, eds., *The American Party System: Stages of Development* (New York: Oxford University Press, 1967); V. O. Key Jr., "A Theory of Critical Elections," *Journal of Politics* 17 (1955); Theodore Lowi, "Party, Policy, and Constitution in America," in *The American Party System,* 238–76; E. E. Schattschnei-

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J. David Greenstone was profound. Many first-generation APD-ers worked with these renowned scholars and were doubtless influenced by their perspectives – often critical, often historical – on American politics. (Much later, Greenstone's own historical work joined other classics in the subfield.)[5]

However, as a self-conscious field of study, APD owes its provenance and current identity to a raft of studies appearing in the 1970s and early 1980s, including Martin Shefter's work on political parties and bureaucracy (1977, 1978), Ira Katznelson's *Black Men, White Cities* (1973) and *City Trenches* (1981), Stephen Skowronek's *Building a New American State* (1982), Amy Bridges's *A City in the Republic* (1984), and Richard Bensel's *Sectionalism and American Political Development 1880–1980* (1984).[6] Although it is not my purpose to track the intellectual history of APD, it is worth reflecting for a moment on the circumstances of the founding.

Intellectually, these writers may be viewed as conduits for a comparative-historical style of research and a non-American (perhaps even un-American) research agenda, with explicit focus on social class and state formation. The work of Perry Anderson, Reinhard Bendix, Fred Block, Eric Hobsbawm, Samuel Huntington, Barrington Moore, Gianfranco Poggi, Nicos Poulantzas, Theda Skocpol, and Charles Tilly seems to have been particularly influential.[7] Indeed, a course titled "American Political Development" was cotaught at Harvard in the early 1970s by Huntington, Lipset, Shefter, Philip Gourevitch, and James Kurth. Its intent was to examine American politics in a comparative context, to bring the U.S. case into the grand narrative of political development.

Each of the foregoing writers had three main points in common. First, they were engaged in an effort to reconcile Marx with Weber. This meant, among other things, bringing class analysis into harmony with the analysis of the modern state. (Even those who were openly critical of Marx, like Huntington, were concerned with questions raised by the Marxist tradition of scholarship.) Second, they were attracted to – though also suspicious of – the modernization paradigm, a preoccupation that could also be traced back to Marx and Weber. (Here, I presume, is the source of 'development' in APD.) Finally, against the dominant tradition of methodological individualism, this cohort of scholars looked to the grand continental tradition of comparative historical analysis.[8]

Yet, the Bendix-Moore school devoted little attention to the United States. At best, the United States was a single case in a large comparative study. Rarely, did it receive detailed treatment. This slight was a product of the comparative orientation of these scholars, but it was also rooted in the apparent peculiarities of the U.S. case. On the topics of primary

der, *The Semi-Sovereign People* (New York: Holt, Rinehart, and Winston, 1960); J. David Greenstone, *Labor in American Politics* (Chicago: University of Chicago, 1977); John J. S. Laslett, and Seymour Martin Lipset, eds., *Failure of a Dream?: Essays in the History of American Socialism* (Berkeley: University of California Press, 1974); Seymour Martin Lipset, *The First New Nation: The United States in Historical and Comparative Perspective* (New York: Basic Books, 1963); Lipset, *Agrarian Socialism: The Cooperative Commonwealth Federation in Saskatchewan. A Study in Political Sociology* (Garden City, NY: Doubleday and Co., 1968); Lipset, "Radicalism in North America: A Comparative View of the Party Systems in Canada and the United States," *Transactions of the Royal Society of Canada,* vol. 14 (1976); Lipset, "Why No Socialism in the United States?" in *Sources of Contemporary Radicalism,* vol. 1, ed. Seweryn Bialer (Boulder, CO: Westview Press, 1977).

5. David Greenstone, "Political Culture and American Political Development: Liberty, Union, and the Liberal Bipolarity," *Studies in American Political Development* 1 (1986); Greenstone, *The Lincoln Persuasion: Remaking American Liberalism* (Princeton, NJ: Princeton University Press, 1993).

6. Martin Shefter, "Party and Patronage: Germany, England, and Italy," and "Party, Bureaucracy, and Political Change in the United States," in *Political Parties and the State: The American Historical Experience* (Princeton: Princeton University Press, 1994); Ira Katznelson, *Black Men, White Cities: Race, Politics, and Migration in the United States, 1900–30 and Britain, 1948–68* (New York: Oxford University Press, 1973), Katznelson, *City Trenches: Urban Politics and the Patterning of Class in the United States* (Chicago: University of Chicago Press, 1981); Stephen Skowronek, *Building A New American State: The Expansion of National Administrative Capacities 1877–1920* (Cambridge: Cambridge University Press, 1982); Amy Bridges, *A City in the Republic: Antebellum New York and the Origins of Machine Politics* (Ithaca, NY: Cornell University Press, 1984); Richard Bensel, *Sectionalism and American Political Development 1880–1980* (Madison: University of Wisconsin Press, 1984).

7. Perry Anderson, *The Lineages of the Absolutist State* (London: New Left Books, 1974); Reinhard Bendix, *Nation-building and Citizenship: Studies of our Changing Social Order* (New York: Wiley, 1964); Fred Block, "The Ruling Class Does Not Rule: Notes on the Marxist Theory of the State," *Socialist Revolution* 33 (1977):6–28; Eric Hobsbawm, *The Age of Revolution: Europe 1789–1848* (New York: Praeger, 1962); Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968); Barrington Moore Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston: Beacon Press, 1966); Gianfranco Poggi, *The Development of the Modern State: A Sociological Introduction* (Stanford: Stanford University Press, 1978); Nicos Poulantzas, *Political and Social Class* (London: New Left Books, 1975); Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge: Cambridge University Press, 1979); Charles Tilly, *The Vendee* (Cambridge, MA: Harvard University Press, 1964); Charles Tilly, ed., *The Formation of National States in Western Europe* (Princeton, NJ: Princeton University Press, 1975). To this list one might add the various edited volumes devoted to the interrelated topics of crisis, sequence, and political development: Gabriel Almond, Scott C. Flanagan, and Robert Mundt, eds., *Crisis, Choice and Change: Historical Studies of Political Development* (Boston: Little, Brown, 1973); Leonard Binder et al., *Crises and Sequences in Political Development* (Princeton, NJ: Princeton University Press, 1971); Raymond Grew, ed., *Crises of Political Development in Europe and the United States* (Princeton, NJ: Princeton University Press, 1978); Joseph LaPalombara, ed., *Bureaucracy and Political Development* (Princeton, NJ: Princeton University Press, 1963); Joseph LaPolombara and Myron Weiner, eds., *Political Parties and Political Development* (Princeton, NJ: Princeton University Press, 1966).

8. James Mahoney and Dietrich Rueschemeyer, eds., *Comparative Historical Analysis in the Social Sciences* (Cambridge: Cambridge University Press, 2003).

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interest, nation-state, and social class, the U.S. case revealed a signal *lack* of development. Only the economy modernized. Was it worth dwelling on a case that seemed so exceptional, so eventless, in the grand sweep of world history?

Work in the APD genre has remained, to some considerable extent, an inquiry into American exceptionalism. The exceptionalist question, inaugurated by Europeans including Crevecoeur, Tocqueville, Marx, Engels, and Sombart, engaged European questions on American ground. At the same time, Americanists coming to intellectual maturity in the 1970s must have felt that this discussion lacked something in depth and subtlety. Was there not more to the American case than pure, unremitting failure? (How, then, was one to explain progressivism and the New Deal?) And even if failure *was* the dominant motif, how could this failure to modernize politically be squared with such outstanding achievements in the economic realm? Clearly, more work was needed, and development rather than failure seemed a healthy corrective to the general assumption of American exceptionalism. APD's emphasis would be on change, rather than stasis, and this change would generally be conceived of as progress in the direction of a modern polity (defined in Weberian terms).

A second intellectual source for APD work was more obvious. Shefter, Skowronek, and their compatriots engaged the work of American political historians. At the time, we should recall, the field of American political history was thriving. Anyone doubtful of this contention should peruse the roll-call of scholars active in the postwar decades: Lee Benson, Allan Bogue, David Brody, David Donald, Eric Foner, Ronald Formisano, Frank Freidel, Eugene Genovese, Oscar Handlin, Ellis Hawley, Samuel Hays, Richard Hofstadter, James Holt, Michael Holt, Richard Jensen, Morton Keller, Paul Kleppner, Arthur Link, Richard L. McCormick, Richard P. McCormick, Marvin Meyers, James Patterson, Arthur M. Schlesinger, Jr., Charles Sellers, Joel Silbey, Kenneth Stampp, Leonard White, Sean Wilentz, and C. Vann Woodward. (Revolutionary-era historians are excluded from this list since this was not an area of interest to early APD scholars.) It is important to recall that APD was born at a time when elite-level politics, i.e., government, was still a central concern within the historical discipline.

We may surmise that, for early APD scholars, the utility of this work was great, but so too were its limitations. Historians like Hawley, Keller, and White examined the American federal bureaucracy, administrative reform, and government regulatory behavior, all central topics of APD work.[9] But the detailed historical spadework produced by these scholars and others of this generation did not culminate in generalizable propositions about the American state. American labor historians focused on particular unions or eras of union activity, leaving larger questions about social class and political action unaddressed (or addressed only at an implicit level). American social historians, whose number and prominence increased over the course of the postwar decades, tackled questions of class but tended to investigate the subject through social and cultural lenses. Social history, like the field of history generally, dealt more comfortably with small units of analysis (states, cities, counties, individual unions), and small temporal periods. American political history was small-bore stuff, mired in dates and details that spoke to the circumstances of particular eras. Work by American historians thus failed to engage the macro-level questions animating European social theory and comparative-historical research.

A third intellectual source of APD was political science, the home institution of APD scholars. At the time, the behavioral revolution was in full swing. Indeed, one might consider the 1960s–1970s to be the high point of behavioralism within American political science. Challenges from older paradigms such as interpretivism were in full retreat and its future competitor, rational choice, only beginning to emerge. Behavioralism – a vague word generally understood as referring to a scientific orientation, quantitative methods, and an individualist methodological stance – was hegemonic. APD-ers had mixed feelings about this institutional heritage. Some, like Burnham and fellow realignment scholars, exploited the new methods for historical analysis. But Burnham's other work, and his irrepressible commentary on what realignment *meant* for American politics, was more typical of APD. This growing subfield tended to eschew quantitative methods, engage grand theory, and use institutions rather than individuals as units of analysis, all of which ran counter to the strictures of behavioralism. Nonetheless, it would be a gross error to see APD as simply a reaction against behavioralism.

Indeed, my thesis here is that APD drew from all three of these currents – from European social theory and the comparative-historical tradition, from traditional political history, and from mainstream American political science – while remaining critical of all three. These are the complex, multiple traditions of APD.

Decades have now passed since the founding of APD. In this time, scholars associated with this school

9. Ellis W. Hawley, *The New Deal and the Problem of Monopoly* (Princeton, NJ: Princeton University Press, 1966); Morton Keller, *Affairs of State: Public Life in Late Nineteenth Century America* (Cambridge, MA: Harvard University Press, 1977); Leonard D. White, *The Federalists: A Study in Administrative History* (New York: Macmillan, 1948); White, *The Jeffersonians: A Study in Administrative History, 1801–1829* (New York: Macmillan, 1951); White, *The Jacksonians: A Study in Administrative History, 1829–1861* (New York: Macmillan, 1954); White, The Republican Era: A Study in Administrative History, 1869–1901 (New York: Macmillan, 1958).

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have expanded well beyond their Marxist and Weberian roots. Now, the common categories of interest include not only social class and the state but also race, ethnicity, and gender. Within the Weberian framework, focus has broadened to include all manner of political institutions. Also, greater attention has been given to cultural properties of the American political experience, factors which echo weakly in Weber and not at all in Marx. In some respects, then, APD has departed from its point of origin.

Yet, in other respects it may be fair to say that APD still struggles with the substantive and methodological demands placed upon it by its founding traditions. And if one takes into account the fact that these traditions themselves are evolving, we may look upon APD today much as we might have looked upon it in the early 1980s – with a foot in European social theory and comparative-historical analysis, a foot in American political history, and a third foot in American political science. It is only from within this complex and divided lineage that we can begin to understand APD's current methodological struggles.

METHODOLOGY

Although the topic of methodology is commonly equated with statistical methods and formal models, the term will be employed here in its broader – and, I think, fuller – sense. All writers face choices with respect to concepts, propositions (arguments), and research designs. These are the building blocks of social science. Moreover, the principles affecting these basic choices are consistent across the various divisions of social science – qualitative/quantitative, formal/empirical, and so forth. The assumption of this discussion, therefore, is that methodological self-awareness is as important for APD as it is for the study of international relations or voting behavior. Arguably, the pliability of the evidence employed in qualitative analysis requires *greater* attention to methodological norms, for here one cannot fall back on statistical procedures and standard models to solve questions of conceptualization and research design. Thus, rather than looking upon historical work as somehow exempt from methodology, my perspective is that methodological self-consciousness is *essential* to the task of historical reconstruction. This, at minimum, is the argument of this essay.

Similarly, we ought not think of methodology and empirical research as being at odds with one another. To be sure, there is a certain style of recondite, highly technical, methodology which leads away from an active engagement with the facts and toward an ever more sophisticated understanding of procedure. Yet, theory and practice are not irreconcilably opposed. If they have been at each other's throats over the past decade, this is mainly because we have taken an excessively narrow and restrictive view of methodology. A good methodology is precisely that set of principles that *facilitates* substantive analysis. A good methodology should allow us, in C. Wright Mills' hallowed phrase, to get to work.

This is the intention of the present methodological discussion, which focuses on several methodological problems that seem to characterize a good deal of work in the APD genre, but which are not, in my opinion, intrinsic or ineradicable. By calling attention to these difficulties, and by treating them with greater care and self-consciousness, the field of APD should be able to strengthen its arguments and broaden its relevance to the fields of political science and history. Three intertwined difficulties will concern us here: (1) unwieldy concepts, (2) inadequate specification of arguments, and (3) circularity in causal logic. I will explore these general issues briefly before diving into our primary material.

Concepts are the building blocks of science, and in particular, one might argue, of social science. In order to be useful, social science concepts must be clearly defined and employed in a consistent manner. Beyond this, I have argued elsewhere that social science concepts respond to seven criteria of adequacy: coherence, operationalization, validity, field utility, resonance, parsimony, and analytic utility.[10] Three of these criteria, *resonance, operationalization,* and *analytic utility,* are especially important for the following discussion.

Concepts must resonate with standard usage patterns within natural language and within the language region of interest in order to be useful in social science research. Idiosyncratic terms, as well as idiosyncratic definitions (both of which may be referred to as neologisms), make the task of cumulation more difficult. Such concepts do not make sense. Or perhaps they make fleeting sense (if the writer has carefully defined the terms), but they do not stick, since we are apt to forget their meanings. Operationalization is equally important, for unless a concept can distinguish its own referents from referents belonging to other concepts it will be unable to function empirically. It will be *merely* conceptual. Concepts, finally, must be chosen and defined by reference to the analytic (theoretical) task at hand. If a causal argument is being made one must be sure to isolate the attributes of independent and dependent variables (as well as of various independent variables) so that endogeneity is not introduced, *sub rosa,* into the argument. Other analytic requirements are specific to the theory being proposed, which brings us to our next category of methodological concern.

Propositions, like concepts, must be clear in order to be convincing. We must know precisely what it is that a writer is arguing before we can begin to evaluate its claim to truth. As Durkheim remarked a century ago, "a theory . . . can be checked only if we know

10. John Gerring, *Social Science Methodology: A Criterial Framework* (Cambridge: Cambridge University Press, 2001).

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how to recognize the facts of which it is intended to give an account."[11] Propositional clarity may be understood as a matter of adequate *specification:* (a) What type of argument is being made (descriptive, predictive, and/or causal)? (b) What are the positive and negative outcomes (the factual and the counterfactual, or the range of variation) that the proposition describes, predicts, or explains? (c) What is the set of cases (the population, context, domain, contrast-space, frame, or base-line) the proposition is intended to address? (d) Is the argument internally consistent (does it imply contradictory outcomes)? If a writer has not answered these questions satisfactorily – if, that is, one has not fully specified an argument – then an argument has not been stated in falsifiable form. Specification is the sine qua non of falsifiability.[12]

Non-circularity, like falsifiability, is a shibboleth of social science. Yet, it is not often realized that this single term can be applied to any one of three distinct methodological difficulties. The first, and most usual, is the problem of *differentiation:* can the *X* and *Y* be distinguished from each other (empirically)? The second concerns the issue of *independence:* is the *X* independent of the *Y*? Evidently, one may find a dependent relationship between distinguishable *X*s and *Y*s, which is to say, we may have independent measures for variables that are not empirically independent. Such would be the case with trade (imports and exports as percent of GDP) and corruption (as calculated by various crossnational polls); they can be independently measured (there is no problem of differentiation), but they are not independent of one another (corruption is likely to decrease trade; increased foreign trade is likely to decrease governmental corruption). A third meaning of circularity concerns the relative causal *priority* of different factors. To call a cause circular may mean that the *X* lies extremely close to the *Y* it is intended to explain, as motivations generally lie close to actions ("I did it because I wanted to"). This does not involve problems of differentiation (so long as some measure of motivation can be found that is prior to, and separate from, the action to be explained), or of independence (so long as the action does not, in a functionalist manner, determine the motivation). It is merely a question of how much causal distance separates the independent and dependent variables.[13]

In order to bring these rather abstract methodological points to light we must investigate specific works. My discussion will focus on three classics from the APD canon: Stephen Skowronek's *Building a New American State,* Richard Bensel's *Yankee Leviathan,* and Rogers Smith's *Civic Ideals.*[14] These books were chosen for their powerful arguments and their broad – and well-deserved – influence within the field. While my focus will be primarily methodological, one should not forget the impressive substantive contributions that these works have made to our understanding of American political history. Visions of the American state and of American political culture have been shaped, in no small part, by these seminal APD studies. Thus, although I shall cite certain shortcomings in these works, it should be clear that this critical attention is brought forward in a spirit of admiration, and is lavished on those most worthy of our critical efforts.

Building a New American State

Skowronek's study of American statebuilding in the late-nineteenth and early-twentieth century probably deserves to be considered the founding text of APD. More than any other single work, *Building a New American State* established the state as a central object of historical study and a theoretically-informed style of history-writing as a mode of political science. The book also attracted attention to several periods of American history heretofore neglected by political scientists (and even, arguably, by historians). Lastly, Skowronek broke with the previous generation's fixation on functionalist arguments, opening up a new way of thinking about causal relationships in American politics.

In 1982, when *Building* appeared, the dominant theoretical frameworks in American political science were marxist and weberian and the dominant methodology was behavioralist. There was grand theory and antitheory, but little in between. It was Skowronek's innovation, following the lead of his mentor, Martin Shefter, to scope out a middle ground. Against the functionalist line of thought suggested by Marxist and Weberian models Skowronek emphasized the contingencies of American statebuilding. Against a behavioralist methodology devoted to quantification, Skowronek spun a narrative.

The central task of this book, as I see it, is to sketch the *politics* of American state development. It is a task accomplished with grace and insight. Three case-studies are chosen: administrative reform, army reorganization, and railroad regulation. For each case, Skowronek shows that the exigencies of class struggle and economic modernization were important, but not determinative. To put it bluntly, Marx and Weber were right, but not entirely so. If we wish to under-

11. Emile Durkheim, *The Rules of Sociological Method* (1895; New York: The Free Press, 1964) 34.

12. There are other important elements of falsifiability as well. See Karl Popper, *The Logic of Scientific Discovery* (1934; New York: Harper & Row, 1968).

13. For further discussion, see Gerring, *Social Science Methodology,* chap. 7.

14. Skowronek, *Building a New American State;* Richard Franklin Bensel, *Yankee Leviathan: The Origins of Central State Authority in America, 1859–1877* (Cambridge: Cambridge University Press, 1990); Rogers M. Smith, *Civic Ideals: Conflicting Visions of Citizenship in U.S. History* (New Haven, CT: Yale University Press, 1997).

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stand the particular shape of American institutions, the timing of their development, and the frequent halts and reversals in this ongoing process, we must pay close attention to the interest groups, legislators, political parties, and court systems that created and sustained (and occasionally terminated) these institutions. In short, politics matters.

But Skowronek does not move into the Rankean camp. Another contrast must be drawn with then-standard treatments of federal state development, where excruciating attention to historical detail precluded anything so presumptuous as a theory.[15] Skowronek is working self-consciously toward synthesis, not a mere accumulation of facts. He claims that there is an underlying logic to the events under study. Drawing upon European social theory, mainstream political science, and political history, *Building* exemplifies the complex methodological task undertaken by APD scholars. Let us see how Skowronek manages this three-part balancing act.

The book is most illuminating, in my opinion, when focused on its case-studies – administrative reform (the Civil Service Commission [CSC]), army reorganization (the War department), and railroad regulation (the Interstate Commerce Commission [ICC]). For each of these topics, broken down into two broad periods, 1877–1900 and 1900–1920, Skowronek analyzes the political scene. Who is pushing for political reform (who are the statebuilders)? Who is opposed? What is motivating these actors? What, finally, alters entrenched positions, allowing for periods of reform?

Skowronek employs a process-tracing style of analysis.[16] The *X*s and *Y*s are contiguous in time and space, they are articulate (historical actors register their positions on subjects of interest), their motives are scrutable, and there is a great deal of within-unit variation. Taken together, these four features allow Skowronek to construct a causal narrative without recourse to quantifiable data. This part of the story is fairly clear, and a substantial advance over existing historical work.

A second success is Skowronek's perceptive label for late-nineteenth century America, which he calls a polity of "courts and parties." This is undoubtedly the most well-remembered element of *Building*, the scholarly text-bite that all authors both crave and loath. Like most memorable phrases, this one packs a wallop. It is, in fact, more than just a phrase; it manages to capture the essential features of a polity that had (heretofore) lacked identifiable qualities – or was mired in the Progressive caricature of a trust-laden Gilded Age.[17] The nineteenth-century American state, Skowronek correctly points out, was an enigma. It fought several major wars, and was victorious in all; it maintained and extended its regional hegemony over the Americas; it sponsored a system of welfare benefits that (we now know) surpassed European social programs in munificence and comprehensiveness; it maintained free markets across semi-sovereign political units; it oversaw the most extensive railroad building program in the world.[18] Most of all, and somewhat improbably, it survived. For a century, the United States was the lone mass democracy in the world. Skowronek offers us insight into this paradox. Here was a state apparently lacking in all the standard equipment of stateness which nonetheless managed to fulfill essential functions, and to excel at a few (e.g., schooling, veterans' benefits, and postal delivery). Its hidden coherence lay in the integrating capacity of courts and parties.

Granted, the role of the court system at this early stage of development might be disputed. But the role of political parties is indisputable. Here is a mid-level temporally-rooted generalization rarely found in either standard historical work or in work by political scientists, and a clear departure from marxist and weberian models. It is at this level that *Building*, in common with most APD work, is most successful.

However, other generalizations offered in *Building* are more problematic, or are simply difficult to pin down. Consider the question of periodization. The book is divided into two sections, the first extending from the end of Reconstruction to the turn of the century (1877–1900), and the second from thence to the end of the Progressive era (1900–1920). During the first period, Skowronek describes state-building as a "patchwork" – efforts are spotty, and only intermittently successful. During the second period, by contrast, statebuilders manage to lay the groundwork for a modern (i.e., weberian) state with a relatively autonomous, highly skilled, and specialized bureaucratic staff. This was the period of reconstitution, in Skowronek's vocabulary. Yet, Skowronek is also intent on emphasizing the weight of the past in hindering reform efforts in the Progressive Era (and beyond). Thus, *Building* oscillates back and forth: sometimes the statebuilders seem to be winning their fights; at other times they are evidently losing. We are left to wonder whether the story of American statebuilding is one of change, or continuity. The state building process led to an exchange of governing strengths and weaknesses, Skowronek writes in summation (288). Perhaps this conclusion of ambiguity accurately reflects historical reality. Perhaps American statebuilding efforts do *not* divide neatly into nine-

15. Lewis L. Gould, *Reform and Regulation: American Politics, 1900–1916* (New York: Wiley, 1978), Ari Hoogenboom, *Outlawing the Spoils: A History of the Civil Service Reform Movement* (Urbana: University of Illinois Press, 1968); Keller, *Affairs of State;* White, *The Republican Era.*

16. Alexander L. George and Andrew Bennett, *Case Studies and Theory Development* (Cambridge, MA: MIT Press, forthcoming).

17. Matthew Josephson, *The Robber Barons: The Great American Capitalists, 1861–1901* (New York: Harcourt, Brace & World, 1934).

18. Theda Skocpol, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States* (Cambridge, MA: Belknap Press of Harvard University Press, 1992).

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teenth- and twentieth-century episodes. Yet if this is the take-home message, the book's periodization is fundamentally misleading.

The broader picture of state strength or weakness (coherence/incoherence) is also unclear. At times, Skowronek seems to emphasize the strength and capacity of American administrative machinery. In this view, it is only the sense of the state that Americans are missing – a political-cultural phenomenon at odds with an institutional reality (3). At other times, Skowronek characterizes the American state as "distinguished by incoherence and fragmentation in governmental operations and by the absence of clear lines of authoritative control" (viii). Again, the terms of the argument are unclear.

Evidently, the question of stateness can be evaluated against either spatial or temporal metrics. We have already shown the ambiguities of the temporal contrasts Skowronek draws. It is unclear not only how the two periods under study differ from one another (pre- and post-1900), but also how periods before and after the 1877–1920 era might be evaluated. What are we to make of the revival of courts during the postwar era?[19] What are we to make of the modernization of bureaucratic machinery during the New Deal? The spatial contrast is clearly European, and references to the archetypal European state are sprinkled liberally throughout the text. Yet, we do not find a clear statement of how the American state contrasts with its European cousins. It is this lack of clear and explicitly drawn case-comparisons that make *Building*'s arguments tantalizing, but ultimately equivocal.

Turning to questions of causality, we recall that Skowronek rejects the functionalist logic of his Marxist and Weberian forbears (viii).[20] What, then, does Skowronek propose to substitute for this apparently discredited macrotheoretical framework? If there is a big causal argument in the book it concerns the enduring influence of established institutions over the course of political reform. The following passage is exemplary of Skowronek's past-over-present perspective:

> Short of revolutionary change, state building is most basically an exercise in reconstructing an already established organization of state power. Success hinges on recasting official power relationships within governmental institutions and on altering ongoing relations between state and society. The premise of this book is that states change (or fail to change) through political struggles rooted in and mediated by preestablished institutional arrangements. (ix)

19. "Modern American state building progressed by replacing courts and parties with a national bureaucracy," Skowronek writes near the end of the book (287). Does this mean that courts are less powerful today than in the late-nineteenth century?

20. Even so, in the epilogue and elsewhere in the text Skowronek seems to embrace a functionalist logic (e.g., 130, 288). The phrase 'developmental imperative' is ubiquitous.

This is unobjectionable, but it does not say very much. It is the set-up for a causal argument that never fully appears. Or rather, it is the set-up for a variety of causal arguments that might be inferred from various passages in the book, but which are never clearly and explicitly stated.

In chapter one, Skowronek introduces three factors that condition the state building process: "domestic or international crises, class conflicts, and the evolving complexity of routine social interactions" (10). These, however, "are only the stimuli for institutional development" (12). "The intervention of government officials is the critical factor in the state-building process" (ibid.). Yet, he is not content with this argument either. Indeed, to rest here would be to consign causal explanation to leadership. Later thesis statements are not helpful in clarifying matters. "In the final analysis," he writes, "the new American state was extorted from institutional struggles rooted in the peculiar structure of the old regime and mediated by shifts in electoral politics" (13). In a more extended passage, intended to provide a coda to his discussion of the shift from patchwork to reconstitution eras, he writes:

> A state under continuing pressures for new institutional services and supports, no longer tied to the old rules of governmental order, and moving through a series of dramatic alterations in the strategic universe of official action these were the conditions that delimited America's pivotal state-building break with the past and its turn down the bureaucratic road. In these circumstances, administrative power grew as part of an extended and shifting scramble over the reconstitution of political and institutional power relationships. The foundations of the modern American state were forged in the vicissitudes of this scramble. (169)

Again, the language is opaque, and we are left to wonder about necessary and/or sufficient causes.

The most problematic element, methodologically speaking, is Skowronek's unwillingness to specify clear outcomes. Only in light of a specifiable outcome (with its associated counterfactual) can one assess an author's causal claims. This becomes clearer once we begin to intuit what this outcome, or outcomes, might be. Let us suppose, for example, that the primary outcome of interest is "the incoherence and fragmentation in governmental operations" and "the absence of clear lines of authoritative control" found in the American state. With this as our explanandum we can begin to interrogate possible causal factors in a more or less systematic fashion. One obvious suspect would be the U.S. Constitution, which was designed to prevent clear lines of authority. Parties attempted, but never fully accomplished, the interlinking of legislative and executive power at federal and state levels and the integration of federal and

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state governments.[21] Bagehot's buckle was forever coming loose in this separate-powers system. Another possible argument, suggested by Shefter's earlier work, is that American statebuilding was doomed by the *timing* of democratization.[22] Demands for an efficient governmental apparatus that arose in the late-nineteenth and early-twentieth century were impeded by obstreperous mass parties, which were by this time well-entrenched in American political culture. In this view, if democratization had not occurred until the era of World War I, the United States would have presumably realized a degree of state capacity and coherence to rival European powers. Still another interpretation (for which Shefter's work also provides support) is that American statebuilding was impeded by the absence of an absolutist past. In this view, contemporary state strength (or capacity) is an outcome with distant historical roots.

Naturally, these three causal arguments might be combined in some form; and naturally, there are other arguments which beg to be considered. The point is, only a clear statement of causal argument, complete with positive outcomes and their counterfactuals, allow us to evaluate Skowronek's claims. As it is, we can only second-guess. It should also be pointed out that an argument of this scope would require a more explicit and extensive consideration of comparative (i.e., European) cases. An argument about a particular nation-state (the United States) presumes that nation-state is the primary unit of analysis. Some account of other nation-states is called for, even if all such units are not investigated at the same level of detail.

But perhaps this is unfair to the book, which is focused primarily on smaller, historically specific, outcomes. Why did the ICC fail during the late-nineteenth century, while civil service and military reforms achieved substantial success? Why did civil service reform achieve greater success in the Progressive Era than in the Gilded Age? Why did attempts at military reform ultimately fail (prior to World War II)? At this level of analysis, we have no need (or less need) for cross-national comparisons. This, I think, is the more correct reading of *Building,* and on this level the book is, methodologically speaking, more successful.

But the costs of such a narrow reading are also evident. Much of the appeal of this seminal book lies in the big theoretical and historical issues that it tackles. If we ignore the central issue that appears to inform the narrative (the fragmentation of the American state), focusing instead on specific historical episodes (i.e., the trials and tribulations of the ICC, the CSC, and the Army General Staff), the book loses much of its interest, both for political science and for present-day politics. Indeed, if framed in this manner, Skowronek's work is difficult to distinguish from traditional historical work on these subjects (e.g., by Gould, Hoogenboom, Keller, and White).

21. E. E. Schattschneider, *Party Government* (New York: Rinehart, 1942).

22. Shefter, "Party and Patronage"; "Party, Bureaucracy, and Political Change in the United States." See also Skowronek, *Building A New American State,* 9.

Yankee Leviathan

Richard Bensel's *Yankee Leviathan,* appearing eight years after *Building,* tackles the same general subject during a previous period. Bensel's approach builds on Skowronek, but with important modifications. Restricting himself to a narrower temporal scope – the Civil War and Reconstruction eras (1859–1877) – Bensel undertakes a vast empirical effort. This 459-page book is packed with illuminating historical detail. It is not difficult to see why historians read this book with pleasure and profit. A lifetime of work, including a newly-published volume on the Gilded Age (2000), has established Bensel as one of the foremost experts on late-nineteenth century American politics.[23]

Bensel, like Skowronek, transcends a Rankean presentation of facts. Indeed, his theoretical ambition is difficult to match, even among his peers in political science. What Bensel has in mind is nothing less than the unification of political and economic history during this critical period, in much the same way that Marx tackled the Revolution of 1848 in France. (The parallels do not end there. Both of these nearly concurrent events might be looked upon as pseudo-revolutions, insofar as class relations were called into question but in the event only partially transformed. After a wobbly period, the old order re-equilibrated.)

The heart of this book (and its longest chapter by far, spanning 143 pages) sets forth an extended contrast between the northern (Union) and southern (Confederate) nation-states. Prior to this, Bensel sets forth an extensive definition of central state authority (a term I shall use synonymously with "state"), including the following dimensions: contralization of authority, administrative capacity, citizenship, control of property, creation of client groups, extraction, and the central state in the world system (114). Bensel, unlike Skowronek, is attentive to the problem of concept formation. He then proceeds to analyze the strengths and weaknesses of the two states on this multitiered metric (results summarized on 182). In an attempt to analyze the internal dynamics of each political system, he tests key policy decisions taken by the two legislatures (northern and southern) in order to determine patterns of support and opposition to centralizing initiatives.

His conclusions are somewhat startling in light of the ideologies presumed to be hegemonic in these two sections at the time of the Civil War. The Confederate state, Bensel argues, was "revolutionary."

23. Richard Franklin Bensel, *The Political Economy of American Industrialization, 1877–1900* (Cambridge: Cambridge University Press, 2000).

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> The South erected a state that (1) cannibalized the regional political economy by implementing a state-directed system of war-related production and economic development; (2) proposed measures for the adjustment of class conflict that accompanied pressing demands on the plantation economy and manpower mobilization; and (3) increasingly committed the southern state and nation to rapid socioeconomic change. . . . In contrast, the northern mobilization rested relatively lightly on the robust capitalist markets of the industrializing centers of the East and free-soil agricultural regions of the West. For the most part, the Union drew men and materiel into the war effort through open-market contracts that were quite attractive to producers and potential soldiers at the same time that they severely constrained central state influence over the national economy. (234)

Thus, despite the statist policies preached by Republicans and the antistatist policies preached by Democrats, the two nation-states adopted divergent organizational principles in the brief period of their rival existence. In many respects, Bensel concludes, "the Confederate state possessed more modern characteristics than th[e] northern regime" (236).

Bensel attempts to explain this divergence by setting up a most-similar case comparison. Both states experienced the exigencies of war. Both began with near-identical constitutions, and hence near-identical political structures. Both struggled for survival. And both states responded to this struggle by centralizing power (as Hintze, Tilly, and the European state-building literature suggest). What, then, accounted for differential state development? Bensel's argument rests upon the different political economies and populations found in North and South. The North was technically advanced, blessed with a reasonably effective infrastructure, and populous. Most important of all, it was rich. The Union state could, therefore, maintain the war effort without becoming unduly interventionist. The Confederate war effort, by contrast, "far outstripped the productive capacities of the prewar economy and compelled a much more innovative, almost futuristic mobilization of resources. As a consequence, the southern mobilization was far more state-centered and coordinated than its northern counterpart" (97–98). Resource-poor societies, Bensel seems to be saying, lead to resource-extraction states (when those states are situated within a competitive state system).[24]

This part of the argument is convincing and fits nicely within a broad theoretical rubric, though Bensel does not make these connections as clearly as he might. Yet, *Yankee Leviathan* is about much more than the divergence of state structures during the Civil War. Indeed, the rest of the book treats the American state as its primary unit of analysis. Here we find a bewildering profusion of arguments, among them the following:

> *Descriptive argument:* "The American state emerged from the wreckage of the Civil War;" i.e., there was little state prior to 1860 (ix). After the Civil War the state experienced an "explosive expansion" of central state authority (2).
>
> *Suggested causal factors:* (a) "the enactment and implementation of the political economic agenda of the groups allied within the Republican party" (2); and (b) the "mobilization of the northern political economy for war" (3).
>
> *Descriptive argument:* The northern state was characterized by the "almost complete fusion of [Republican] party and state (x).
>
> *Outcome:* The Civil War
>
> *Suggested causal factor:* "capture of the antebellum state by the Republican party" (10).
>
> *Outcome:* The secession of the South.
>
> *Suggested causal factor:* "increasing penetration of the South by institutions and processes associated with the northern political economy, and the imminent exclusion of most of the South from participation in those political coalitions that were to control the central state" (11).
>
> *Outcome:* The Union war against the South (11).
>
> *Suggested causal factor:* "the need to maintain the newly dominant position of the Republican coalition in the domestic political economy" (11).
>
> *Descriptive argument:* The Republican party-state was compromised in the post-Reconstruction era (3), reaching a stalemate (10) in the form of a *laissez-faire* model of government (15).
>
> *Suggested causal factors:* (a) "Republican factionalism, (b) the return of former Confederates, and (c) the Democratic competition" (3), (d) "The way in which the North chose to finance the Civil War *created,* through its own structure, crucial, unforeseen limitations on the growth of the state in the late nineteenth century" (14).
>
> *Descriptive argument:* After Reconstruction the American state slowly developed a statist sensibility (an identity and interest apart from any class or partisan interest) (3).
>
> *Outcome:* The North and South diverged in relative prosperity in the latter-nineteenth century (7).

24. Perry Anderson's work on European statemaking, which contrasted Western and Eastern models of absolutism, seems relevant here. See Perry Anderson, *The Lineages of the Absolutist State* (London: New Left Books, 1974).

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> *Suggested causal factor:* The North undeveloped (or prevented the development) of the South (7).
>
> *Outcome:* The American state at the turn of the twentieth century (parameters unclear).
>
> *Suggested causal factors:* (a) "successful repression of southern separatism and (b) the pursuit of economic advantage over the South by the industrializing regions of the Northeast and the Great Lakes" (10).

Here we count four descriptive and seven causal arguments. This is a lot of arguing (and we have gone no further than the introduction). Moreover, the specification of each *individual* causal claim remains unclear. Consider the Civil War. Is Bensel seeking to explain why the war occurred (counterfactual: no war at all); why it occurred when it did (counterfactual: not sooner or later); or why it occurred in the way it did (counterfactual: variously operationalizable)? Each of these possibilities calls up a somewhat different set of implicit outcomes and cases, and therefore must be considered as an analytically distinct causal proposition. This is the first sort of specification problem.

Specification becomes even more problematic when one considers the several macro-arguments wending their way through this narrative. Statebuilding, the general subject of the book, is initially explained as a product of economic nationalism:

> Economic nationalism of the industrial North was the original impetus behind American state expansion in the late nineteenth century. It was this drive to unify the national marketplace that eventually broke the back of southern separatism. Social and political reconstruction failed because the installation of federally sponsored loyalist groups in the South implied broader policies of wealth distribution that threatened private property rights and had no natural northern constituency. Economic nationalism, on the other hand, had a vigorous, powerful clientele in northern industrialism. Thus, the defensive reaction to economic nationalism . . . was, in fact, intended to restrain the rampaging forces of northern development. (16–17)

In the next paragraph, however, Bensel proposes an explanation for the comparative *weakness* of the American state in the late nineteenth century (presumably in relation to other states in the Anglo-European world). This goes as follows:

> 1) The Republican class coalition that captured the federal government just prior to the Civil War subsequently produced the central state's confrontation with southern separatism. 2) The major problem facing statebuilders was thus not associated with a robust democracy, but southern separatism. 3) In confronting separatism, the central state moved from violent repression to a state-centered solution (involving Reconstruction), and finally, to a (loosely effected) market integration. 4) In sum, American state formation assumed the form of a northern, industrial program in which incomplete political integration coincided with the creation of national markets and corporate consolidation. (17)

We learn now that there were "at least five different American states in the late-nineteenth century: the self-effacing antebellum state; the two national governments of the Civil War; the highly centralized Reconstruction state; and the market-oriented state that followed the withdrawal of military troops from the South" (17). It is then suggested that these various states are "linked together [caused?] by their focus on one central problem, the persistent demands of southern separatism" (ibid.). What, precisely, is being explained here? Are each of these states the product of one cause (sectionalism)?

Complicating the task of specification still further is the breadth and fuzziness of Bensel's key term, 'political economy.' Consider the following passage:

> An immediate consequence of secession is a change in the scope and content of the political economy of the parent nation. This change alters the strategic considerations that supported the position of formerly dominant classes and thus can undermine the cohesion of a ruling class coalition. For example, the Republican coalition of yeoman agriculture and industrialists would probably have rapidly fractured in a political system that did not include the southern plantation economy. Without the South, the national political economy would have lacked the major reservoir of wealth that the coalition could potentially redistribute to its own members and the base of the party would have narrowed as each sector sought to impose redistributive claims upon the other. Thus the viability of a dominant class coalition changes with the scope of the political economy potentially subject to central state rule. (12)

Political economy is ubiquitous in Bensel's work, and so elusive as to undermine any causal argument with which it is associated. It appears as both cause and effect of virtually every element of the narrative. This problem of distinguishing independent and dependent variables is apparent in the venerable debate over slavery. Was it the ideology of white supremacy or the material interests of whites (as slaveowners and as occupants of a privileged social and political status) that motivated Southerners? Evidently, if both elements are incorporated within the concept of political economy we cannot address this question – or we

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must simply *assume* that interests predominate over values and ideals.

Concepts such as class and state are equally problematic, if somewhat more bounded. At times classes serve as motivating agents of the state (e.g., as motivations for the action of the Union army), and at times as captives of the state. At the conclusion of the war, for example, Bensel describes the creation of a "dependent financial class tied to the success of central state extraction and fiscal policy generally" (14). Yet, in the next sentence, he implies that this class was instrumental in ending Reconstruction and in blunting the influence of Radical Republicans (who appear to be free of any political-economic base). Elsewhere, the state is described frankly as "both agent and product of northern economic development" (416). Thus, while avoiding the crude monocausal logic of many neo-Marxist writers, Bensel's causal arguments are difficult to pin down. As best we can determine, there is some sort of interaction among racial, sectional, economic, class, military, and party imperatives that, in different ways at different times, determines various political outcomes.

To clarify, I am *not* imagining that writers like Skowronek and Bensel would produce a single general model to account for all explananda under discussion in a book, in the manner of many Marxist and rational choice accounts. All other things being equal, parsimony and generality in an explanation are to be preferred; but all other things are rarely equal. The point is simply this: even under conditions of explanatory complexity it should be possible to clarify which causal factors are being invoked with respect to which outcomes. Adequate specification of arguments is possible only when one can differentiate among neighboring concepts. If we are to *explain* the fact of North-South conflict or state formation during the late-nineteenth century we need to clarify whether racial, sectional, economic, military, party-political or some other factors (e.g., a "schism within the American upper class" [425]) was primary, or if it was some combination of these. If none was primary, but all congealed together at a particular point in time, then we have another species of (conjunctural) argument altogether. Adequate specification is the first, and most critical, step towards falsifiability.

Civic Ideals

Political culture, rather than the state, is the central focus of Rogers Smith's *Civic Ideals*.[25] In this book, Smith issues a challenge to the orthodoxy. American political culture, Smith argues, is neither singular (liberal), as Hartz suggested, nor double (liberal and republican), as the latest historical revision would have it, but actually *triple* – liberal, republican, and ascriptive.

Challenging the Hartz thesis, as most readers know, has become a regular pastime among historians and political scientists.[26] Yet the sheer volume of criticism that has stacked up since the 1960s has not amounted to very much – which is to say, the Hartz thesis still stands, scarred but majestic. Indeed, this insistent and often vituperative criticism may have had the unanticipated result of *enhancing* the strength of the liberal-tradition thesis, since anti-Hartz scholars have not offered anything to take its place. (Republicanism was intended to do this, but is usually applied only to the eighteenth- and early-nineteenth centuries. The modern era still belongs to Hartz.) Here is where Smith's contribution is most vital, and most unusual. Unlike most Hartz critics, Smith is not afraid to say what American political culture *is*. Not surprisingly, this book has generated large waves within the broad waters of political history and political science. *Civic Ideals,* though just a few years old, is already established as a major contribution to an old and well-worked subject.

In order to understand the multiple-traditions argument, we must first understand its constituent terms. Liberalism, Smith writes (in accordance with standard usage), is "government by consent, limited by the rule of law protecting individual rights, and a market economy." Republicanism, he defines as "popular sovereignty exercised via institutions not just of formal consent but of mass self-governance [which] generally preach an ethos of civic virtue and economic regulation for the public good" (a definition that will please some, but not all scholars). Ascriptivism, Smith's own construction, refers to the belief that "'true' Americans are 'chosen' by God, history, or nature to possess superior moral and intellectual traits associated with their race, ethnicity, religion, gender, and sexual orientation" (507–8). These three traditions, Smith argues, although often in conflict with one another, have characterized the culture of politics in the United States from the colonial era to the present.[27]

Smith pursues this argument through an intensive study of citizenship laws, citizenship debates within Congress, and judicial decisions relating to citizenship. Beyond that, a wide variety of political factors at both the elite- and mass-level are taken into account insofar as they relate to citizenship debates. Thus, although there is a tight empirical focus, the book is not empirically constrained. Indeed, its generous and in-

25. See also Rogers M. Smith, *Liberalism and American Constitutional Law* (Cambridge, MA: Harvard University Press, 1990); "Beyond Tocqueville, Myrdal, and Hartz: The Multiple Traditions in America," *American Political Science Review* 87 (1993):549–66.

26. Daniel T. Rodgers, "Republicanism: The Career of a Concept," *Journal of American History* 79 (1992):11–38.

27. Intimations of this argument have been discovered in W. E. B. Du Bois, *Dusk of Dawn: An Essay Toward and Autobiography of a Race Concept* (1940; New York: Schocken Books, 1968). See Jacqueline Stevens, "Beyond Tocqueville, Please!" *American Political Science Review* 89 (1995):987–90. However, Smith is the first to present this argument in all its fullness and complexity.

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clusionary narrative often reads more like standard political history. It is a vigorous argument, and one calculated to shock.

Yet, the argument is somewhat less revolutionary than its initial claims suggest. To begin with, Smith sets up the book as an empirical test of two, not three, hypotheses. The first, labeled *Tocquevillian,* may be summarized as follows. Arguments about citizenship in the course of American history privileged liberal and/or republican ideals, were realized in partisan conflicts between "those benefiting from liberal rights . . . and majorities suffering from them ('rich liberals' versus 'poor democrats'), and moved progressively (without reversal) towards ever more democratic and egalitarian policy outcomes" (8). The second hypothesis, labeled *multiple traditions,* is that citizenship debates invoked varying perspectives – liberal, republican, and ascriptive – which were often blended together; that democratizing reforms came not "steadily and almost automatically, but only when economic, political, and military factors create[d] overwhelming pressures for change"; and that the overall pattern was one of fluctuation rather than continual advance, "with the long-term trends being products of contingent politics more than inexorable cultural necessities" (8). Thus framed, it seems fairly clear that Smith will emerge victorious over his opponents. Not all citizenship battles pitted rich liberals against poor democrats; advances toward greater equality were sometimes reversed; and ascriptive arguments were often in evidence.

We may grant that Smith's account offers a healthy corrective to a crude and triumphalist liberalism. But we may also wonder whether he has accurately portrayed the field of current writing on American political culture. Would Joyce Appleby, Herbert Gans, J. David Greenstone, Samuel Huntington, Michael Kammen, Seymour Martin Lipset, James Morone, Herbert McCloskey and John Zaller, and Michael Rogin – or even such earlier writers as Richard Hofstadter – identify liberalism with the Tocquevillian' account?[28] It seems unlikely. Indeed, Philip Gleason, Smith's principal foil on the specific questions of ethnicity and citizenship does not seem to view the matter in such simplistic terms either (see discussion below).[29]

Evidently, it is a half-empty/half-full sort of debate. In common with all such debates, which lack a common metric for understanding comparisons and contrasts, it is resistant to empirical inquiry. Moreover, the issues themselves are multidimensional, and difficult to disentangle. First, were advances linear? Smith himself acknowledges that the general trend over the past two centuries has been positive; he himself chooses to emphasize the zig-zags (though others might easily put a different spin on things). Second, were advances automatic? Surely not, but it is doubtful that even the most naive Hartzians think so. Third, were advances forced by economic, political, or military factors? It is difficult to conceive of *any* important change in government policy coming about without one or all of these factors in evidence; so again, the book's argument has indeterminate ramifications. The contrasting causal explanation here seems to be one of cultural necessity. Surprisingly (in light of the book's packaging, as an exegesis of American political culture), Smith seems to be making an *anti*-cultural argument about the making of citizenship policy. It was economic, political, and military factors, and other contingent factors, *not* the simple force of liberal ideals, that led to the liberalization of citizenship laws. Again, we may appreciate the corrective to a crude and crusading liberalism; but it seems unlikely that any academic writer of recent vintage would present the liberal argument in such simplistic terms.

Moreover, one must wonder about the connection between Smith's first and second cuts at the material. The first, we saw, presented three alternatives: liberalism, liberalism/republicanism, and multiple-traditions. The second, which turns out to be central to the book's actual investigation, presents two alternatives, Tocquevillian and multiple-traditions. It is never entirely clear whether the Tocquevillian hypothesis is intended to encompass the liberal or liberal/republican theses, or whether it is a new hypothesis entirely. The latter seems more defensible. Indeed, this book is probably better understood as an investigation into civic and political *equality,* or *democracy* (broadly understood), than of liberalism and republicanism. The latter bear ambivalently on this subject, according to Smith's own definitions. If liberalism means "government by consent, limited by the rule of law protecting individual rights, and a market economy," does this mean that liberals support, or oppose, more inclusive citizenship laws? It is unclear what the policy implications of liberalism (as defined by Smith)

28. Joyce Appleby, "Recovering America's Historic Diversity: Beyond Exceptionalism," *Journal of American History* 79 (1992); Herbert Gans, *Middle American Individualism* (London: Collier Macmillan, 1988); J. David Greenstone, "Political Culture and American Political Development," and *The Lincoln Persuasion;* Samuel P. Huntington, *American Politics: The Promise of Disharmony* (Cambridge, MA: Harvard University Press, 1981); Michael Kammen, *People of Paradox: An Inquiry Concerning the Origins of American Civilization* (1972; New York: Oxford University Press, 1980); Seymour Martin Lipset, *American Exceptionalism: A Double-Edged Sword* (New York: W. W. Norton, 1996); James Morone, *The Democratic Wish: Popular Participation and the Limits of American Government* (New York: Basic Books, 1990); Herbert McClosky and John Zaller, *The American Ethos* (Cambridge, MA: Harvard University Press, 1984); Michael Paul Rogin, *Ronald Reagan: The Movie, and Other Episodes in Political Demonology* (Berkeley: University of California Press, 1987); Richard Hofstadter, *The American Political Tradition* (New York: Vintage Books, 1948); *The Age of Reform: From Bryan to FDR.* (New York: Alfred A. Knopf, 1955).

29. Philip Gleason, "American Identity and Americanization," in *Concepts of Ethnicity,* ed. W. Petersen, M. Novak, and P. Gleason (Cambridge, MA: Harvard University Press, 1982).

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might be with respect to issues of citizenship. Similarly, one must construct a rather unorthodox definition of republicanism in order to see this notion behind movements for greater civic equality for immigrants, women, and African-Americans. (Certainly, it is not implied by Pocock's work, which Smith claims to be following.) It is never clear how the key concepts of liberalism and republicanism relate to the Tocqueville hypothesis.

We now proceed to a more difficult specification problem. Is Smith presenting a *causal* argument? He refers to Tocquevillian and multiple traditions accounts as 'independent variables' and citizenship laws as 'dependent variables' when introducing his argument (8). The argument, we intuit, is that the behavior of historical actors is determined, to some degree, by the three traditions: that liberal results were spurred by liberal ideals, republican results by republican ideals, and ascriptive results by ascriptive ideals. Or perhaps that liberalism (ideals and institutions) and republicanism (ideals and institutions) led to more inclusive views on citizenship debates, and ascriptivism (ideals and institutions) to less inclusive views. However phrased (and of course, the phrasing is critical), there is considerable circularity. Can liberalism, republicanism, and ascriptivism be determined separately from their effects? Or is it only by their fruits that we can know them?

Of course, circularity is a common problem in many cultural arguments. But not all cultural arguments (so-called) are equally problematic. Indeed, the thesis of Hartz's *The Liberal Tradition* is perhaps better understood as an institutional, rather than cultural, argument.[30] Liberalism, he maintains, conditions political behavior (the cultural argument), but liberalism itself is the product of an institutional feature of American history and social life: the absence of a feudal class structure. (In this respect Hartz's analytic perspective on American history owes more to Marx than either Hartz or his detractors cared to point out.)

Cultural arguments that employ religion as a causal variable are also usually specifiable. Weber's argument about Protestantism as a spur to capitalism, or more recent arguments about Protestantism as an ingredient of good governance, are testable hypotheses.[31] This is not to say that they are *easy* to test; but theories of such grandeur rarely are. Even so, we can differentiate between Protestants and non-Protestants within a population; we can measure the percentage of Protestants between populations; we can even, given adequate survey data, describe the level of religiosity among these populations and sub-populations. Protestantism is defined formally, more or less officially, in a series of texts, which can be analyzed – or even coded – as an independent measure of this culture. Indeed, as a variable in social science, religions are almost as easy to specify as *institutions* (they are, of course, connected to an institution).

Smith's three categories, by contrast, are less satisfactory. We have no way of differentiating liberals, republicans, and ascriptivists *prior* to the result of interest (positions on citizenship issues). Complicating matters further, Smith argues that the three strains are most commonly found as *admixtures*. This introduces such flexibility into the multiple-traditions argument that it becomes essentially unfalsifiable. No persons, events, or institutions in the United States (and perhaps in the contemporary western world) would escape this loose typology.

Because of the vagueness of these traditions – because they are not formal belief-systems (like Protestantism) – we cannot easily locate them in empirical space. Liberalism, republicanism, and ascriptivism, as Smith tells us, are ex post facto constructions for cultural properties that historical actors had no (conscious) awareness of. There are no card-carrying liberals or republicans, and certainly no card-carrying ascriptivists. To be sure, etic categories (categories outside the awareness of the actors under analysis) are commonplace and indispensable to the work of social science. We have no trouble imputing causality to changing voting patterns among suburban whites, or to first-past-the-post electoral systems. Yet, when causal arguments are framed around an ideational phenomenon (e.g., liberalism), we require evidence of an *emic* (immanent) variety. We need to know how people are thinking and feeling, independent of how they are acting (or at least independent of the action that comprises the dependent variable). Otherwise, there is no there there.[32]

Among the three traditions, we also find significant differences in operationalizability. Liberalism seems most satisfactory when cast as an independent variable. It is enshrined in a set of semi-sacred texts (the Declaration, the Constitution, the Federalist Papers), symbols (the American flag, the Capitol, the White House, the Washington Monument, the Jefferson Memorial), rituals (Independence Day, Constitution Day, Washington's birthday, election day, the National Anthem, the Pledge of Allegiance), myths (the

30. Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace & World, 1955).

31. Max Weber, *The Protestant Ethic and the Spirit of Capitalism* (1904–1905; New York: Charles Scribner's, 1958). See also S. N. Eisenstadt, ed., *The Protestant Ethic and Modernization: A Comparative View* (New York: Basic Books, 1969). Seymour Martin Lipset and Gabriel Salman Lenz, "Corruption, Culture, and Markets," in *Culture Matters: How Values Shape Human Progress*, ed. Lawrence E. Harrison and Samuel P. Huntington (New York: Basic Books, 2000); Wayne Sandholtz and William Koetzle, "Accounting for Corruption: Economic Structure, Democracy, and Trade," *International Studies Quarterly* 44 (2002):31–35; Daniel Treisman, "The Causes of Corruption: A Cross-National Study," *Journal of Public Economics* 76 (2000):399–57.

32. This of course is one of the core truths of the interpretivist canon. See, e.g., Paul Rabinow and William M. Sullivan, eds., *Interpretive Social Science: A Reader* (Berkeley: University of California Press, 1979).

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Boston Massacre, Washington crossing the Delaware, Paul Revere's ride, the Shot Heard Round the World), and a pantheon of national heroes (Revere, Franklin, Adams, Washington, Jefferson, Madison). Few would doubt that liberalism is the *official* ideology of the United States. As such, its presence is palpable; we have strong emic evidence. Thus, it seems plausible to argue with Tocqueville, Myrdal, and Hartz that this political culture has shaped the course of American politics.

Republicanism, by contrast, has fewer ideological props, particularly in the post-Revolutionary eras (though much, of course, depends upon how one interprets this nebulous concept). It possesses less structure, less empirical reality, less self-consciousness. It is less of a religion, and consequently is more problematic when asserted as a causal force in American history.

Ascriptivism is even shakier. The first difficulty is that many ascriptivists support *inclusive* citizenship laws. Smith explains,

> Ascriptive views can undergird universalistic, egalitarian civic positions, as when religious believers esteem the sacredness of all humanity, indeed all creation, on the ground that everything equally comes from God. Despite their ultimate theological moral ascriptiveness, the fact that such egalitarian views almost always urge civic inclusiveness and treat national citizenships as legitimately alterable political memberships makes them effectively liberal and consensual in regard to citizenship laws. (508)

Smith's solution to this dilemma is to subsume egalitarian ascriptivists under the rubric of liberalism. Here is a frank admission that the outcome of interest, not the purported cause, is driving the author's analysis. Indeed, if we look again at Smith's definition of this third and most critical tradition we find the matter stated forthrightly. "Adherents of what I term inegalitarian ascriptive Americanist traditions," Smith writes, "believe that true Americans are chosen by God, history, or nature to possess superior moral and intellectual traits associated with their race, ethnicity, religion, gender, and sexual orientation" (ibid.). To be sure, Smith suspects that most ascriptivists will hold inegalitarian views on citizenship matters. Yet, he does not give his theory the opportunity to fail, even occasionally, for he has told us that one's position on the citizenship question, *not* one's ascriptive (or nonascriptive) orientation, determine one's categorization.

Yet, even if this category had been defined differently (to include all ascriptivists, regardless of orientation to the policy question of interest), we may still wonder about ascriptivism as a causal explanation. Consider the fact that while liberals presumably have much in common and look upon each other with admiration and respect, ascriptivists are an assorted lot, including groups who manifestly despise each other and have virtually nothing in common (except, of course, for their ascriptivism). It is difficult to assign *causal* status to such a far-flung cultural trope. On the face of things, George Wallace was driven to resist racial integration in Alabama not because he was an ascriptivist, but because he was a racist. With the term *racism* one arrives at a level of conceptualization that is close to lived reality. To be sure, Wallace would not have called himself racist. But there *was* a fairly explicit and official ideology in the South that elevated whites above blacks, and it does not seem farfetched to suppose that this ideology might have motivated Wallace's behavior (in addition to the office-holding aspirations that motivate all politicians). Racism is identifiable and causally probable because this way of thinking was connected to a set of individuals who lived in proximity to one another, participated in a common civic life, shared a common vocabulary – in short, shared a set of ideas and institutions that merit the appellation of a *culture*. Ascriptivism, by contrast, has none of these properties; it is a purely logical, and rather free-floating, construction.[33]

RECOMMENDATIONS

This methodological discussion and critique has focused on three seminal works in the APD canon. In a certain sense, we must now point out, this may have been an unfair point of focus. For seminal work, by definition, goes where few have gone before. Its task within the field of scholarly endeavor is prefigurative, not confirmatory. It is left to later generations to go over this ground again with the benefit of hindsight and the easier, safer, and (we must admit) less stimulating goal of confirming or disconfirming initial hypotheses. Founding texts do not always follow the modes of normal science.

33. Here one stumbles over the question of how to *define* culture (and its near-synonyms, tradition and ideology), on the one hand, and institutions on the other. Smith, in common with many culturalists, refuses to separate the two. Tradition, for him, is "(1) a world view or ideology that defines basic political and economic institutions, the persons eligible to participate in them, and the roles or rights to which they are entitled, and (2) institutions and practices embodying and reproducing such precepts" (507). Traditions, he concludes, are "not *merely* sets of ideas" (507). Institutions, by the same token, refer to any structure "thought to shape the conduct of political actors, . . . including biological, physical, and psychological systems, economic and political arrangements, kinship and civil associations, and ongoing structures of ideas, including religious beliefs and political ideologies" (510). If we take these definitions seriously, we are at pains to determine whether liberalism, republicanism, and ascriptivism should be regarded as traditions, institutions, or both. But we must sympathize with Smith and other culturalists on this point. There is no clear terminological choice for phenomena that combine ideational and nonideational elements; conventionally, they are cultures (or traditions). This should not be regarded as a fundamental flaw in Smith's argument, therefore, although it does give the reader pause.

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Even so, it may be conceded that problems of concept formation, specification, and circularity are common in the field of APD, even among paradigm-fillers. In this sense, Skowronek, Bensel, and Smith are not atypical. (Of course, we have focused on only one work in the extensive oeuvres of these prolific writers.) So the question arises, how might these methodological difficulties be avoided, or at least more forthrightly handled?

In bringing these matters forth I do not mean to suggest that APD is the only subfield guilty of methodological transgressions, or even that it has more methodological deficits than other subfields of political science.[34] Every school, we may safely assume, generates its own pathologies. My goal is simply to draw attention to a particular set of methodological problems that seem to characterize a good deal of work in the APD genre.

I will make four basic recommendations here: first, that descriptive analysis be accorded a more prominent and valued place within political science; second, that different types of causal argument be carefully distinguished from one another; third, that case-selection be undertaken in a more careful and conscientious manner; and finally, that the advantages of longitudinal analysis be more fully exploited.

Description and Causation

Description and causation are different forms of explanation, calling forth different criteria of adequacy. To be sure, virtually any work of history or social science invokes some species of causal argument. At the very least, a historian must explain in an informal way the motivations of various actors. What I mean by descriptive is a work whose primary focus is on what happened – who did what to whom? – and on the process of history, rather than on *why* an event or series of events occurred. Descriptive inference involves the analysis of highly *proximate* causes.[35]

The first obligation of a writer, therefore, is to clarify which sort of claim one is making. Too often, however, APD work elides this distinction, preferring soft causal claims, or causal claims that verge on description (e.g., in Smith's *Civic Ideals*). A clearer distinction between what is causal and what is descriptive would do a great deal to clarify the scope of APD arguments.

It would also allow APD writers to engage more forthrightly in what may be their greatest strength of historical work. Arguably, work in APD and in the genre of traditional history is more consistently successful in descriptive analysis than in the analysis of causation. Smith's argument about the multiple traditions of American political culture, for example, is probably more tenable as a descriptive claim than as a causal claim. (This may also be true of earlier work on the same subject by Tocqueville and Hartz.) Skowronek's characterization of nineteenth-century politics as a state of courts and parties is a brilliant and eminently useful way of thinking of politics in this epoch. Skocpol's discovery of a pre-New Deal welfare state defending the rights of soldiers and mothers is a descriptive claim of great significance, whatever the status of her causal claims.[36] It seems appropriate to point out that my own work on party ideology is explicitly descriptive in focus.[37] Realignment theory, the most well known theory of American political history, is perhaps better approached as a descriptive argument, a means of periodizing certain aspects of American politics, than as a causal theory.[38]

Other work that is historical in focus, though not explicitly APD, might be similarly classed. For example, Richard John's work on the post office is crucial to our understanding of politics in the early republic, even though it is difficult to say precisely how much causal weight (and for what outcomes) should be assigned to this factor.[39] Joel Silbey's periodization of American electoral history into prealignment (1789–1838), alignment/realignment (1838–1893), realignment/dealignment (1893–1948/52), and postalignment (1948/52–present) periods offers an exceptionally parsimonious handle on this vast subject.[40] David Mayhew's work on party organization in the American states and on congressional actions is largely descriptive in nature.[41]

Indeed, many of the enduring questions of American political history involve questions that are largely (though not entirely) descriptive in nature. When did political parties form?[42] When did southern

34. The recent spate of rational-choice critiques is a healthy corrective to this errant conclusion. See, e.g., Jeffrey Friedman (ed.), *The Rational Choice Controversy: Economic Models of Politics Reconsidered* (New Haven, CT: Yale University Press, 1996); Donald P. Green and Ian Shapiro, *Pathologies of Rational Choice Theory: A Critique of Applications in Political Science* (New Haven, CT: Yale University Press, 1994). Other fields could doubtless be subjected to similar deconstruction.

35. On causal and descriptive propositions, and their different criteria of adequacy, see Gerring, *Social Science Methodology,* chaps. 5–7.

36. Skocpol, *Protecting Soldiers and Mothers.* It would take a good deal of time to parse these causal claims, and readers should infer no argument on this point here.

37. John Gerring, *Party Ideologies in America, 1828–1996* (Cambridge: Cambridge University Press, 1998).

38. For an extensive, often critical, discussion of realignment theory see David R. Mayhew, *Electoral Realignments: A Critique of an American Genre* (New Haven, CT: Yale University Press, 2002).

39. Richard John, *Spreading the News: The American Postal System from Franklin to Morse* (Cambridge, MA: Harvard University Press, 1996).

40. Joel H. Silbey, "Beyond Realignment and Realignment Theory," in *The End of Realignment?: Interpreting American Electoral Eras,* ed. Byron E. Shafer (Madison: University of Wisconsin Press, 1991).

41. David R. Mayhew, *Placing Parties in American Politics: Organization, Electoral Settings, and Government Activity in the Twentieth Century* (Princeton, NJ: Princeton University Press, 1986); *America's Congress: Actions in the Public Sphere, James Madison through Newt Gingrich* (New Haven, CT: Yale University Press, 2000).

42. Ronald P. Formisano, "Deferential-Participant Politics: The Early Republic's Political Culture, 1789–1840," *American Journal of Political Science* 68 (1974).

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states begin to restrict negro suffrage?[43] What has been the broad chronological pattern of suffrage rights in American history?[44] How strong were ethnocultural cleavages in American elections in the nineteenth century?[45] How have patterns of inequality changed over the course of American history?[46] How has associational activity evolved over the course of two centuries?[47]

Naturally, each of these questions brings causal questions in its train. Any convincing descriptive analysis invites speculation on causal influences. One would have a difficult time discussing suffrage rights without also addressing the issue of why they were extended, or retracted, at various points in American history. Rightly, authors generally address both sorts of questions. The point is, we need to keep these tasks analytically distinct from one another. A coherent pattern of events suggests, but does not presume, a unified causal explanation. Not all phenomena have causes, in the usual sense of that term. Descriptive analysis is useful, therefore, *regardless* of whether it leads to a parsimonious causal explanation. Conversely, causal explanation *does presume* accurate description. Thus, descriptive analysis is doubly important – both for its own sake (as an ordering device for the infinite detail of the historical record) and as a precursor to causal analysis.

Two facts about descriptive inference should be kept in view. First, description need not be small-bore. While some of the generalizations aired in the foregoing paragraphs are rooted in particular eras, others span the length of American history. Still others, such as the argument over American political culture, contrast American history with histories of other countries. Political science rarely attains a broader scope, or a more synoptic style, than Louis Hartz achieved in the *Liberal Tradition*. Yet, the latter (in common with most work on the subject) is primarily descriptive in orientation. Second, descriptive analysis may be quantitative in method. Different subjects lend themselves to different approaches. It would be difficult to *avoid* quantification in a historical study of voting behavior or income distribution, for example. Thus, there is no reason to equate descriptive work with qualitative arguments. In short, to say that historical work specializes in description is not to say that it neglects social science virtues including parsimony, precision, and breadth. Description does not equal interpretivism, though it certainly *includes* interpretivism.

Regrettably, description carries pejorative connotations. One hears *merely* descriptive – implying that a larger, more important, social science task has been neglected or abandoned. This is a travesty of social science. As I, and others, have argued there is often nothing more useful than reliable descriptive analysis.[48] Thus, we ought to begin by praising APD and traditional American political history for their rich descriptive analyses. Unless and until descriptive inference achieves the academic respectability that it deserves historical work is bound to be marginalized, and APD scholars will be forced to dress up descriptive analysis in causal garb. From this perspective, we might say that Rogers Smith did not jump to his causal argument; rather, he was pushed.

The Specification Problem in Causal Argument

We have noted that the task of specification is often confused when different, albeit closely related, causal outcomes are conflated, or left ambigious. Although causal outcomes are virtually infinite in variety (because one can specify positive and negative outcomes in any number of ways), it is useful nevertheless to identify the various classes of outcomes that typify work in the APD genre. These may be considered under four general headings:

> (1) Questions of *timing:* Why did things happen when they did?
> (2) Questions of *process:* Why did things happen in precisely the way that they did?
> (3) Questions of *intranational variation:* Why did things happen differently in different regions, states, and localities?
> (4) Questions of *international variation:* Why did things happen differently in different countries?

Thus, with respect to the Civil War, we might ask (1) why it occurred in 1861 (and not sooner or later), (2) why it was pursued in the manner that it was, (3) why the North and South developed different forms of national authority, or (4) why it occurred at all. Specification in causal argument involves the clarification of which, among these various alternatives, the author is concerned to explain. Naturally, if *several* of these causal questions are under consideration then these must be carefully disaggregated in the analysis. Naturally, each of these gross categories offers many suboptions. This typology, nonetheless, may serve as a useful point of departure for those developing causal arguments about the past.

43. J. Morgan Kousser, *The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880–1910* (New Haven, CT: Yale University Press, 1974).

44. Alexander Keyssar, *The Right to Vote: The Contested History of Democracy in the United States* (New York: Basic Books, 2000).

45. Richard Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888–1896* (Chicago: University of Chicago Press, 1971).

46. Jeffrey G. Williamson, *American Inequality: A Macroeconomic History* (New York: Academic Press, 1980).

47. Robert Putnam, *Bowling Alone: The Collapse and Revival of American Community* (New York: Simon & Schuster, 2000); Theda Skocpol, and Morris Fiorina, eds., *Civic Engagement in American Democracy* (Washington, DC: Brookings, 2000).

48. Gerring, *Social Science Methodology,* chap. 6; Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton, NJ: Princeton University Press, 1994), chap. 2.

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Of these four types of causal argument, we should note that there is a particularly broad chasm separating the first three (pertaining to *intra*country variation) from the fourth (pertaining to *inter*country variation). Most APD scholars can be found on both sides of this divide. Skowronek and Bensel, we have noted, are not merely interested in isolated episodes of American statebuilding, but also in the American state, writ large. Smith is not merely interested in ways in which citizenship is being regulated from period to period, but also in what can be said about citizenship regulation *in general* in the United States, and the political culture that these policies embody.

There is nothing wrong, and much that is good, with this grand ambition. It is what makes APD an exciting location in an increasingly calcified discipline. Yet, too often these divergent aims result in work that is insufficiently specified or insufficiently substantiated by the evidence at hand.

The Problem of Case-Selection

What does the intracountry/intercountry distinction in outcomes tell us about case selection?[49] We should begin with a misconception: namely, that APD's primary difficulty is its paucity of cases (the so-called small-*N* problem). This is true, of course, if the unit of analysis is understood to be the nation-state. Historical analysis, almost by definition, is unlikely to incorporate more than one or two country-cases. But it would be absurd to equate nation-states with cases (*N*). Indeed, social science cases are *rarely* composed of nation-states. APD, in common with traditional historical work, often draws on a multitude of within-country cases. Thus, Skowronek and Bensel incorporate the actions of many individuals, social groups, and political institutions. Collectively, these comprise hundreds of cases. Smith, who employs a more formal research design, draws on hundreds of legal decisions; each is legitimately considered as a separate case. So APD is not (usually) a small-*N* venture.

The relevant question concerns the *analytic utility* of these cases, that is, do they offer good evidence for the causal assertions pursued in these books? Here, one may distinguish among two types of cases, those that replicate the unit of analysis of the proposition under investigation (across-unit analysis) and those that use variation *within* that unit of analysis (within-unit analysis). Thus, if we are investigating why a president took a particular action, the primary unit of analysis is the individual, our across-unit analysis is provided by other individuals (who are contrasted with the president), and our within-unit analysis is provided by the president's own thoughts and behavior examined through time. If we are investigating why the United States went to war, the primary unit of analysis is the nation-state, our across-unit analysis is provided by other countries, and our within-unit evidence by variation within the United States (e.g., within Congress, among citizens, and so forth). Ideally, one combines within-unit and across-unit evidence in a given study.[50]

Work in the genre of traditional political history generally employs both kinds of evidence. To be sure, historians rarely mention other countries; however, we must remember that across and within may be defined only with respect to a particular proposition. Because the scope of historical propositions are generally more confined – to questions of timing, process, and spatial difference (as outlined above) – the appropriate across-unit analysis might involve comparing individuals, classes or sections, rather than nation-states. Ironically, historical work may fit comfortably with social-scientific norms, despite the fact that historians are rarely self-conscious in their choice of cases.

APD, by contrast, often takes on a much wider scope of argument. At the same time, its scope of empirical investigation is generally rooted in particular times and places. This introduces a bias in favor of within-unit analysis. Consider work by Skowronek and Bensel on statebuilding. Both writers wish to say something about why the American state developed, or failed to develop. The primary unit of analysis for this sort of proposition is the nation-state. Yet, the evidence of these studies is limited, by and large, to a single country. In this context, *N* equals 1, and we have a problem of case-selection. To be sure, there is within-country variation to exploit. But it is questionable whether or not this sort of evidence is as useful as cross-national evidence would be. At the very least, it would seem that cross-country evidence would be helpful in supplementing comparisons drawn upon spatial and temporal contrasts within the United States.[51]

Similarly, the formidable array of evidence gathered and deployed in *Civic Ideals* speaks most directly to questions of timing and process: when, why, and how did inclusive or restrictive citizenship proposals become law? It has much less to say about the big picture (the overall status of American citizenship regu-

49. A case refers to any observation, or set of observations, that provides independent evidence for a proposition. Cases should be comparable to other cases in the sample, or such incomparabilities must be controlled, in order for valid comparisons to take place. A case need not be quantifiable, although at a certain point it may become necessary to count cases, if only to keep track of them. See Gerring, *Social Science Methodology,* chap. 9.

50. See John Gerring, "What is a Case Study and What is it Good For?" (unpublished manuscript, Dept. of Political Science, Boston University).

51. Bensel is conscious of this problem, referring to his study as "one case of revolutionary state formation" (10). Yet, it is unclear what the other cases might be, and certainly no explicit analysis is carried forth. Earlier, Bensel calls our attention to the Soviet Union, India, and Canada, which also evidenced significant separatist pressure. Does this mean that Bensel's generalizations are intended to apply to nation-states with significant (how much?) separatist pressure? If so, what is the variation (within this class of nation-states) that Bensel is seeking to explain?

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lation or of American political culture), a question hinging on national-level comparisons.[52] Nonetheless, this is a book that focuses resolutely on the big picture.

Philip Gleason's vision of American political culture forms the foil – much more directly than Hartz – for Smith's argument. To be an American, writes Gleason,

> a person did not have to be of any particular national, linguistic, religious, or ethnic background. All he had to do was to commit himself to the political ideology centered on the abstract ideals of liberty, equality, and republicanism. Thus the universalist ideological character of American nationalist meant that it was open to anyone who willed to become an American.[53]

How should one investigate the truth of this proposition? Smith's approach is to construct within-country cases. It is perhaps already apparent to readers that there are many exceptions to Gleason's thesis – that is, cases of would-be Americans who were excluded from the political community by virtue of their ascriptive characteristics (religion, ethnicity, race, sex, et al.). Does this mean that Gleason is wrong? One quickly encounters the half-empty/half-full quagmire that is characteristic of this topic. Those of a more centrist political bent like Gleason focus on cases of inclusion; those of a more critical bent accentuate cases of exclusion and exploitation. The argument has raged since the 1960s. Indeed, this debate bears striking resemblance to older debates over whether conflict or consensus characterizes American history, or whether the United States is (really) democratic.[54]

All of these questions are matters of degree, and matters of degree cannot be settled unless we have a baseline, a metric, upon which to judge variation. Limited to within-country evidence, we are at sea in a vast tumult of facts and easily redefined concepts. But with other country-cases at our disposal we have comparative reference-points that can settle (or at least provide ground for settling) such longstanding issues. It is simply not meaningful to say that the United States is liberal or illiberal unless one has a set of countries in mind that are more, or less, liberal. Indeed, Gleason's perspective seems eminently defensible if judged against the backdrop of European countries, particularly those like Germany, which have defined citizenship in explicitly racial terms. It is not my objective to pose a solution to this vast question; rather, I should like to point the way to a method by which such a solution might be obtained. The initial step, it seems to me, is to exploit variation across units (defined by reference to an author's principal thesis), not simply within the primary unit of interest.

More broadly, APD's methodological difficulties can be traced to its overweening ambition. None of our three authors, in common with most of their APD brethren (myself included), seems able to resist the big picture. We wish to say something about the United States, even though our primary evidence relates to intra-country variation. By contrast, scholars in most other subfields of American politics – e.g., Congress, the presidency, the judiciary, political behavior, parties, and so forth – are usually less ambitious. If an author intends to explain only variation in congressional behavior over time, or variation among different committees at the same point in time, one is not obliged to study the Bundestag. Of course, *implicit* in much work focused on American politics is the presumption that one is elucidating general truths – applicable everywhere and always. In this respect, traditional Americanists are on equally shaky methodological ground.

For the theoretically ambitious, there is no easy way around the across-unit/within-unit dilemma. Sometimes, relevant crossnational reference-points can be integrated easily through secondary literature on the topic (i.e., without explicit study by the author). In other circumstances, the writer may undertake a brief study of these comparative cases on her own (in a comparative chapter of an otherwise U.S.-centered

52. What does the multiple-traditions thesis mean, in the absence of crossnational analysis? Can we examine a topic simply by reference to a definition, or to standard understandings? This does not seem like a very sound way of going about business. To be sure, if it is asserted that event *X* never occurred, and we can find evidence of its occurrence, we shall not need to resort to an extensive examination of other cases. But most academic disputes and, a fortiori, most disputes over American political culture, are not this simple. They hinge on delicate questions of definition and classification, e.g., what is liberalism, and is *X* properly understood as an example of it? These are precisely the sort of questions of judgment that require comparative cases.

53. Gleason, "American Identity and Americanization," 62–63 (qtd. in Smith, *Civil Ideals*, 14–15). Following this passage – and, indeed, throughout the rest of the essay – Gleason qualifies the decisive quality of the universalist American ideology:

> About eight out of ten white Americans were actually of British derivation in 1790, and there was a latent predisposition toward an ethnically defined concept of nationality. Indeed, universalism had its limits from the beginning, because it did not include either blacks or Indians, and in time other racial and cultural groups were regarded as falling outside the range of American nationality. Yet such exclusiveness ran contrary to the logic of the defining principles, and the official commitment to those principles has worked historically to overcome exclusions and to make the practical boundaries of American identity more congruent with its theoretical universalism. (ibid.)

Thus, the passage quoted by Smith offers a somewhat misleading depiction of Gleason's argument. Universalism, Gleason argues, was a strong influence on questions of immigration and ethnicity, but it was by no means the only influence.

54. Bernard Sternsher, *Consensus, Conflict, and American Historians* (Bloomington, IN: Indiana University Press, 1975); Benjamin Ginsberg, *The Consequences of Consent: Elections, Citizen Control and Popular Acquiescence* (Reading, MA: Addison-Wesley, 1982).

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book). Much depends on the complexity of the subject and the ease with which we can generalize about comparative cases.

The general point here is fairly simple, however. Since arguments about general (non-temporally bounded) country-wide outcomes – the welfare state, the state (in general), the absence of socialism, racism, sectionalism – rest on crossnational comparisons, the centrality of such an argument within a study should be directly proportional to the amount of time and space that is devoted to crossnational analysis. If only limited attention is paid to crossnational cases (as is true in most APD work), then writers should lean gently on these macro-arguments, perhaps consigning them to a concluding chapter (where narratives traditionally adopt a more speculative tone). Within-country analysis is relevant to country-wide conclusions, but is rarely (if ever) sufficient to ground such arguments.

What sort of cross-country evidence must we have in order to address the sorts of country-level generalizations that APD scholars are attracted to? With respect to stateness, the subject of Skowronek's and Bensel's work, we may learn a great deal from revenue, expenditure, and employment patterns. Recently, Vito Tanzi and Ludger Schuknecht compiled a dataset allowing us to compare various dimensions of stateness across fifteen or so OECD countries from 1870 to the mid-1990s (the *N* varies according to year and statistic).[55] From this study, we discover that general government expenditures (as a share of GDP) in the United States were somewhat lower than other industrial nations in most periods, but reached truly exceptional levels only in recent decades.[56] Government employment (as share of total employment) has never been that far off the OECD average, and in some periods exceeded that average.[57] Defense expenditure (as share of GNP) was far below average prior to World War II, and far above average afterwards.[58] Education expenditure (as share of GDP) has been near the OECD average during all periods. Health expenditure (as share of GDP) has followed OECD norms during most periods.[59] Other categories of social spending have lagged significantly behind, in most periods.[60]

This sort of information seems relevant to many of the descriptive and causal claims advanced in *Building a New American State* and *Yankee Leviathan*. Here one has a basis to judge whether, when, and to what extent the U.S. state was bigger or smaller than its Anglo-European cousins. Of course, Tanzi and Schuknecht's study is open to questions of data reliability, and does not extend back prior to 1870. Nor can all questions of stateness be addressed with quantitative indicators. Nonetheless, one's portrait of the American state is considerably enhanced by the foregoing descriptive statistics.

Data problems notwithstanding, APD scholars who wish to discuss country-level outcomes are well advised to try to integrate cross-country evidence into their within-country studies. This is probably the best way to avoid unsubstantiated, or simply ambiguous, claims about the American case. It is also the best way to assure that APD studies cumulate with studies in other academic fields and subfields.

The Importance of Secular Time

As we have already noted, APD work usually focuses on events and institutions with long histories. No favored topic in the APD canon (e.g., race relations, social policies, labor movements, political parties, bureaucracies, political culture) is narrowly bracketed in time. Moreover, because of APD's quest for contemporary and theoretical relevance, writers are apt to define these subjects in ways that presage present-day circumstances (i.e., in encompassing, rather than time-bound, ways). Unlike historians, who are usually keen to emphasize the *differentness* of the past, APDers are more apt to emphasize its similarities. This is fully appropriate to a generalizing science, and constitutes an important distinction between the disciplines of history and political science.

Yet, most APD studies examine only a single era, or several adjoining eras.[61] Skowronek, for example, focuses on the Gilded Age and the Progressive Era (1877–1920), Bensel on the Civil War and Reconstruction eras (1859–1877), and Smith – somewhat more broadly – on the nineteenth century (1798–1912). This introduces several sorts of methodological problems. First, it limits the number of

55. Vito Tanzi and Ludger Schuknecht, *Public Spending in the 20th Century: A Global Perspective* (Cambridge: Cambridge University Press, 2000).

56. Ibid., 6–7.

57. Ibid., 26.

58. Ibid., 28.

59. Ibid., 38.

60. Ibid., chap. 2.

61. Exceptions are notable, and perhaps define a current trend towards longer temporal horizons. See, Richard Franklin Bensel, *Sectionalism and American Political Development 1880–1980* (Madison: University of Wisconsin Press, 1984); Louis Fisher, *Constitutional Conflicts between Congress and the President* (Princeton, NJ: Princeton University, 1985); Gerald Gamm and Robert D. Putnam, *Civic Associations in American History* (forthcoming); Gerring, *Party Ideologies in America;* John Mark Hansen, "Taxation and the Political Economy of the Tariff" *International Organization* 44 (1990):527–51; Richard Harris and Daniel Tichenor, *A Question of Representational Bias: The Development of American Interest Group Politics* (Cambridge: Cambridge University Press, forthcoming; Keyssar, *The Right to Vote;* David E. Kyvig, *Explicit and Authentic Acts: Amending the U.S. Constitution, 1776–1995* (Lawrence: University Press of Kansas, 1995); Mayhew, *America's Congress;* Keith T. Poole and Howard Reiter, *Politics Within Parties: Factionalism in the Presidential Nominating Process, 1831–2000* (forthcoming); Eric Schickler, *Disjointed Pluralism: Institutional Innovation and the Development of the U.S. Congress* (Princeton, NJ: Princeton University Press, 2001); Peter Trubowitz and Nicole Mellow, "Bipartisanship in American Politics: Politics by Other Means," presented at the annual meetings of the American Political Science Association, Bos-

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temporal cases at one's disposal. Second, it makes it difficult to adequately bound a subject in empirical space. Third, it makes it extremely difficult to distinguish short-term and long-term trends. We are at pains to integrate temporally bounded case-studies that employ different methods and different vocabularies.

All these difficulties are apparent, in varying degrees, in the statebuilding literature. Skowronek indicates that the statebuilding project took off during the Progressive Era. Bensel, on the other hand, seems to argue that this critical period occurred several decades earlier. And we must entertain the hypothesis (implicit in several of Skowronek's statements and in work by many other writers) that the *really* critical period of statebuilding did not arrive until the 1930s, or later.[62] Evidently, there is no way we can evaluate the notion of a critical period without some direct comparisons among these periods, and without also looking at the periods of relative stasis that connected them. Change presupposes a baseline; yet, the segmented character of APD work on this subject deprives us of a clear view of the *longue duree.*[63]

This is also a common complaint with regard to traditional historical work, which is equally prone to speculation on critical eras, crucial events, turning-points, and crises. Insofar as APD wishes to transcend the era-driven perspective of traditional history it is obliged to come to terms with longer time-frames. It is remarkable, for example, that with so many in-depth studies of statebuilding efforts in particular eras no comprehensive history of this subject has yet been attempted. In this respect, we have not moved substantially beyond the epoch-by-epoch approach pioneered by Leonard White.

Given that most subjects in American political science are addressed from the narrow temporal perspective of the present, it does not help matters if historical work is also period-bound. What APD scholars ought to strive for is the *unification* of presentist and historicist perspectives. This should be the primary research agenda of APD work on the federal bureaucracy, political parties, interest groups, social movements, the media, Congress, the presidency, the judiciary, public opinion, and other topics. Only in this fashion will we be able to bring greater coherence to our understanding of American politics, past *and* present. Only in this fashion will we be able to construct viable periodizations for these subjects.

Finally, to the extent that APD studies expand their temporal horizons we should note that such studies will be pushed toward a more quantitative mode of analysis. It is the same pressure induced by the addition of multiple country-cases, discussed above. If one increases the *N,* a numerical metric by which to standardize comparisons becomes virtually indispensable, even for purely descriptive purposes. How can we compare bureaucratic structure and behavior over two hundred years without measuring something? This may mean the development of soft measures, such as those derived from the coder's own judgments of a particular case. Or it might involve the creative use of secondary indicators (measurable factors which are thought to co-vary with the concept of interest). In any case, it is likely that the immense challenge posed by secular time will push APD scholars in the same direction that it pushed social historians of the *Annales* school several generations ago: toward numbers.

CONFLICTING IMPERATIVES

This essay began with a discussion of APD's diverse heritage, a heritage that includes European social theory and comparative-historical methods, traditional political history, and behavioralist political science. I would like to suggest, by way of conclusion, that many of the methodological difficulties discussed in this essay may be understood as the product of APD's conflicted intellectual ambitions and its problematic place within the discipline of political science.

First, APD is caught between divergent demands for depth and breadth. In common with traditional political history, work in the APD genre aims to present the fullness and richness of historical events. It aims for completeness in explanation (descriptive and/or causal). At the same time, it stretches for generalizations that are broad in scope, even by the contemporary standards of political science. This is a difficult circle to square. The demands of depth and those of breadth are often in conflict with one another. It is usually possible to explain 10 percent of the variation among 100 cases, or 90 percent of the variation among 10 cases. It is extremely difficult to explain 90 percent of the variation among 100 cases. This is an irreducible feature of social science. (I am dubious as to whether 'sequential analysis,' 'configurative analysis,' or 'holistic analysis' can overcome this dilemma.)[64]

Second, and relatedly, APD suffers a legitimation

ton, MA (August–September 2002); Wallis, "American Government Finance in the Long Run."

62. Sidney M. Milkis, *The President and the Parties: The Transformation of the American Party System Since the New Deal* (New York: Oxford University Press, 1993). Bartholemew Sparrow, *From the Outside In: World War II and the American State* (Princeton, NJ: Princeton University Press, 1996).

63. Efforts toward a more comprehensive history of the American state can be found in John Joseph Wallis, "American Government Finance in the Long Run: 1790–1990," *Journal of Economic Perspectives* 14 (2000):61–82.

64. See Ira Katznelson, "Structure and Configuration in Comparative Politics," in *Comparative Politics: Rationality, Culture, and Structure,* ed. Mark Irving Lichbach and Alan S. Zuckerman (Cambridge: Cambridge University Press, 1997); Charles C. Ragin, *The Comparative Method: Moving Beyond Qualitative and Quantitative Strategies* (Berkeley: University of California, 1987); Theda Skocpol

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problem. Although rarely articulated in a formal way, we may surmise that political scientists justify their work on grounds of contemporary or theoretical relevance. Thus, a work on congressional policymaking in the 1990s might stake its claims to relevance by pointing out that what Congress does has important repercussions for American citizens. A formal-modeling approach to the same subject would claim theoretical relevance (and perhaps policy relevance as well). Work in the APD genre, however, is at pains to establish *either* sort of relevance contemporary or theoretical.

By contrast, within the historical discipline it is axiomatic that the past matters. From this it is inferred either that the past should be studied for its own sake or that this sort of disinterested study will eventually result in a clarification of present-day dilemmas (and hence will be relevant in the everyday sense of the term). Historians have no need to explain to their colleagues why they bother to study the Progressive era. APD scholars are not so fortunate. Does the Progressive Era have important things to tell us about (a) American politics today or (b) politics in general? APD-ers would probably answer in the affirmative. Yet, neither question is self-evident – at least not to many in the discipline of political science.

APD work is the complex product of many contradictory impulses. It seeks description as well as causal explanation, depth as well as breadth, the exploration of the distant past as well as of the present. It attempts to write the history of American politics as situated within the Anglo-European world.

I have argued that this set of substantive and theoretical goals is the heritage of a three-cornered disciplinary agenda drawn from European social theory, political history, and American political science. I have also tried to show how these disparate ambitions have contributed to a certain looseness in concept formation, proposition formation, and research design. All the same, we should acknowledge that this tension is part of what makes APD vital and interesting as a site of social science research. What APD has to offer to the discipline of political science is not simply the past – although that might be presumed to be sufficient – but also the present. A richer and more informed present.

More specifically, I have argued that APD ought to expand its reach both *spatially* (by incorporating additional country-cases) and *temporally* (by incorporating longer periods of time). At the current moment, given the profusion of case-studies focused on particular subjects and particular eras in U.S. history, it strikes me that we have more to learn from history that is synthetic in nature than from history that breaks up the past into small chunks. Lumping may be more revelatory than splitting, at least for now.[65]

Whether historical work within political science expands its theoretical and empirical scope, contracts it, or moves in both directions at once, the abiding rule of case-selection remains clear: Cases should be adequate to the proposition under investigation. If a proposition is narrowly focused, then within-country analysis and perhaps even period-specific analysis may be sufficient. If it is broader, then it is incumbent upon writers to incorporate cases from other countries and other time-periods. Theory and evidence usually work best when defined by the same unit of analysis. Indeed, by this measure we have observed that traditional political history is more scientifically respectable than work in the APD genre, where theoretical ambitions often surpass the reach of cases under study.

and Margaret Somers, "The Uses of Comparative History in Macrosocial Inquiry," *Comparative Studies in Society and History* 22 (1980).

65. Granted, synthesis cannot proceed without the empirical groundwork provided by case-studies. A return to grand history within political science, therefore, presumes the survival of a traditional style of political history-writing that covers the ground more closely. Traditionally, this case-study work has been provided by historians. Should this vein of history-writing expire, however – an eventuality that seems increasingly likely – it will have to be resuscitated by political scientists. This, in turn, will necessitate the revival of closely-focused, nose-to-the-grindstone efforts. There is no sense in turning to grand synthesis before some sort of empirical groundwork has been laid. Thus, the direction of political history within political science depends very much on the state of political history within history.

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