The Evolution of Political Knowledge

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Over the course of the last century political scientists have been motivated by two ends: (1) understanding and explaining political phenomena in a way that is both theoretically and empirically grounded; and (2) analyzing matters of public interest, whether in terms of public policy, fidelity between principle and practice in the organization and conduct of government, or the conditions of freedom, whether of citizens or of states. The discipline has been driven and shaped by what the world yields that is of consequence for understanding the exercise of power and choice in the public arena, as well as by the adaptation, refinement, and employment of analytical tools developed largely in neighboring fields, such as economics, psychology, and sociology. Political science commenced and continues as an “interdisciplinary” and complex field of study, the subject of which is both fundamentally important and ever changing. Its chief aims have been to develop a body of verifiable political knowledge and to enhance the quality of both public and private life. These two dimensions intersect but are not coincident, and the constant tension has been such as to strain and lessen the space created by the intersection.

Many of the key advances in the field of political science have been driven by a concern for the public good. The study of groups is a clear example, stirred early on by the particularly insightful and influential work of Bentley (1908), Herring (1929), and Truman (1951). Other areas of early dedication and lasting interest include the psychological dimensions of political action (Merriam 1924; Lasswell 1930); the social conditions and calculus of voting (Lazarsfeld et al. 1944; Campbell et al. 1960); the character and consequence of public opinion (Key 1961); the practical sources of and the constraints on executive power (Neustadt 1960); and the formalization of rational behavior in coalition formation (Riker 1960). In political theory, such areas include the intention of meaning and
anticipation of audience in the development and exposition of political thought (Skinner 1969), and the fundamental issues of equality and justice in society as explored in the original and systematic thought of John Rawls (1971). These are, of course, only a sample of the many strands of research stimulated by concerns about aspects of political life that have resulted in the creation of core areas of inquiry.

Since the mid-twentieth century and particularly during its last third, however, the political science discipline has devolved into increasingly specialized subfields even as it has fostered and witnessed the development of distinct core areas of inquiry and thought. This has occurred even in the wake of efforts to provide systematic conceptual integrity for the discipline as a substitute for the putative coherence that characterized it during its first half century as a contrapuntal contribution to the fugue (Easton 1957, 1965; Almond 1960, 1965; Eckstein 1966, 1973). The tendency toward subfield specialization can be traced from Charles Merriam’s lament in 1926 that the field of political science was so broad and its practitioners so few as to inhibit the focused and specialized inquiry that development of a discipline required. Consequently, he argued, political scientists were not fully appreciated by either practitioners or by their colleagues in the “higher and brighter world of theoretical social science” (Merriam 1926, 4, 8).

In a perceptive and elegant address given three decades later, V. O. Key observed that the discipline had contributed substantially to the vocation of public service and had compiled an enviable, if rather inchoate, body of knowledge. Key viewed “the enlargement, improvement, and refinement of that body of knowledge” as the most pressing problem facing political scientists (Key 1958, 963). Resolving it, he surmised, would require greater numbers of talented and well-trained researchers and the formulation of “modest general propositions” in a manner where theoretical and empirical work would be mutually instructive. Within two decades and in the midst of growing specialization within the discipline, Warren Miller would urge that subfield parochialism be diminished and unification of the discipline enhanced by pursuing large-scale research projects led by teams of investigators to address complex problems that cut across subfields and that single investigators could not pursue alone (Miller 1981, 9–12). Collective endeavor might temper the parochial excesses encouraged by individual pursuits. At century’s end, Matthew Holden (2000), in his reflections on Merriam’s formative estimation of progress and purpose in political science, would suggest that the “competence of political science” required a closer and more informed and valued relationship between theory, inquiry, and political action.

It was with these and attendant reflections in mind that we accepted the invitation of Robert Jervis, then president-elect of the American Political Science Association (APSA), to serve as co-chairs of the program committee for the 2001
annual meeting of the association. The meeting would be the first of the century, was close to APSA’s centenary, and encouraged a retrospective on the evolution of political knowledge and the context in which it has occurred in a broad range of fields. A principle intention for the meeting was to encourage the organization of forums for discussion of contesting values and views about what should matter most in our collective inventory of issues to study and to provide occasion for debate of the even more deeply contested views on how best to study them. Another was to organize clusters of thematic panels concerned with strategies of political inquiry, policy and the public interest, and the 2000 presidential election.

A central commitment, however, was the organization of a series of major panels in select areas of research that have evolved and flourished over the past several decades and that continue to do so, thus following a trajectory incipient in Merriam’s vision, explicit in Key’s advocacy, while challenged in Miller’s and Holden’s pleas. Each of these panels would have a single core paper, accompanied by a series of substantial commentaries. The core papers were to review, analyze, and evaluate the intellectual evolution, the “natural history” so to speak, of major areas of inquiry over the past three decades, in some cases more. In so doing, they examined the major puzzles, conversations, findings, and debates that have characterized research choices and progress in the field, and charted particularly promising azimuths, puzzles, and concerns for future work. We also felt it important to have one of our colleagues (David Laitin) examine, reflect upon, and endeavor to provide a set of propositions that could furnish greater intellectual cohesion and integrity for the discipline than is currently the case, a matter that has been left largely unattended over the past two decades. We also felt it important to have at least one other paper explore an important normative political issue and infuse thought from the perspective and with the techniques of a kindred discipline. Thus, we have included John Roemer’s (2004) paper on democracy and equality in the companion volume to this one.

The papers in this volume are revised versions of those originally presented that address key areas of inquiry in the field of American politics and how developments in the United States have influenced the study of political science. Each paper is followed by a set of commentaries designed to constitute short papers with their own intellectual legs and grounding. Together, each paper and the accompanying commentaries form a lively and important dialogue that will help shape debates, thought, and practices among political scientists, and assist and influence new generations of scholars entering the field.

The scope of core areas of research here encompassed is clearly incomplete. This is a function of choice as well as happenstance. We do not address such important areas as electoral systems and electoral behavior; parties, party systems, and groups, whether organized within or across states; public policy, either processual or substantive; gender and politics; the impact of European
philosophy on American political thought; the courts and judicial behavior; and the economy, wealth, and politics. This is but a sample of missing topics. Nevertheless, the areas here addressed are robust and fundamental ones, have drawn successive generations of practitioners, have been attended by serious debates, and have enjoyed the evolution and become the reservoir of a substantial fund of reliable and, hopefully, socially useful knowledge.

Included in this volume are six core chapters. Initially, David Laitin offers a perspective on the discipline of political science, taken as a whole. Laitin argues that there is a great deal of intellectual coherence in the discipline, but that it remains institutionally chaotic. Among the most striking manifestations of the discipline's "institutional incoherence," in Laitin's view, are the lack of a standard introductory course in political science and the absence of a standard curriculum; the tendency to identify subfields and courses using proper nouns, rather than structuring them around a core set of questions and issues; and the increasing attention being devoted to methodological posturing and debates over ontology and epistemology.

His chapter aims to restructure the discipline's organization in order to better reflect what he sees as its underlying intellectual coherence. To this end, Laitin argues for structuring the field around four substantive areas of inquiry: political theory, comparative politics, political institutions, and international relations. Much of his essay is devoted to explaining how an introductory course in political science can be organized around these areas and why this organization is more intellectually coherent and useful than various alternatives.

One of the most controversial and important features of Laitin's proposal is the omission of American politics as an independent subfield within political science. He calls for folding this area of inquiry into the subfields of comparative politics and political institutions, arguing that it is most fruitful to study the American political system in a comparative framework rather than in isolation and that the study of positive political economy—which has become central to much research on American politics—should be a core concern of research on political institutions. Americanists will undoubtedly have much to say about this proposal. The chapters on the evolution of work on American politics that we discuss later should help lay the foundation for debate on this topic.

Rogers Smith, however, addresses another core issue, having to do with the evolution of research in the field of political theory. He observes that after a period marked by grand theoretical work with little connection to empirical social science, the last decade or so has witnessed a steady rise in empirically engaged research by political theorists. After documenting this trend, Smith seeks to explain it. Crucial in this regard, he argues, are shifts in the underlying political climate, especially in the United States and the study of American politics. The trend toward grand theory in the 1960s and 1970s grew out of the
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intense political conflicts and important political changes that marked those decades, most notably the Vietnam War, the Cold War, the civil rights movement, the women’s movement, student demonstrations, and the deterioration of urban areas. Empirical research and the behavioral revolution that had swept the social sciences seemed unable to address these topics satisfactorily. Moreover, Smith maintains that “in such a climate, . . . it can seem sensible, even necessary, for thinkers with grand aspirations to transform the ways of thinking and acting dominating the politics of their times to avoid direct engagement with these severely polarized issues” (Smith, p. 77 in this volume).

Equally, political theory’s shift in a more empirically engaged direction over the past decade can be viewed as a reflection of underlying political conditions. Contemporary politics—again, particularly in the United States—is less contentious, polarized, and conflict-ridden than during the 1960s and 1970s. There is also much less of a sense that dramatic political changes are on the horizon at the moment. In this situation, there is less room for grand theory to have much impact on political life. Instead, “addressing existing practical problems in empirically informed ways that may perhaps help political debate to inch further in one’s preferred direction often seems like all one can hope to do” (Smith, p. 37 in manuscript). Smith views this turn toward empirically grounded research as a promising development, since political theory is likely to be most useful when brought directly to bear on key substantive problems in the political arena.

While Smith addresses how political conditions in the United States (and elsewhere) have influenced political theory, most of this volume traces the development of various core areas of inquiry within the field of American politics. Donald R. Kinder analyzes the intellectual history of the study of mass politics and makes a number of central claims about the nature of the American public. First, the American public is relatively ignorant when it comes to political matters and is not very active politically. The public is also, in Kinder’s words, “ideologically innocent: indifferent to standard ideological concepts and lacking a consistent outlook on matters of public policy” (p. 109 in this volume). Second, the public’s attitudes toward policy are inconsistent over time. Third, public opinion has remarkably little to do with self-interest, and much more to do with how the public feels about the particular social groups involved in contemporary issues. Fourth, mass communication has had a profound impact on public opinion by “framing” issues, determining what topics the public thinks about and deems important, and by influencing by how the public evaluates political alternatives.

Fifth, Kinder argues that the American public’s party identification depends at least somewhat on the actions taken by political parties. Further, the public tends to vote for candidates whose positions on key issues conform to their own positions; but voters also tend to make decisions at the polls based on how well the incumbent has done in improving conditions in the country as a whole rather
than just their own personal condition. Sixth, there are three sets of forces that prompt the electorate to change its voting patterns from one election to the next: a new conflict over a key policy issue; a change in national conditions, particularly economic conditions; and the rise of new candidates, especially those that elicit a strong personal reaction on the public’s part. Seventh, elected representatives tend to be responsive to the views and “mood” of their constituents—although there seems to be some variation in this regard across issue-areas—and representatives seem to understand that unresponsiveness will lead constituents to take retribution at the polls. In the same vein, the mood of constituents is powerfully shaped by the country’s economic performance.

Turning from the study of mass politics to the study of institutions, Nelson W. Polsby and Eric Schickler address key developments in research on the U.S. Congress since 1945. Polsby and Schickler trace three intellectual waves of work on Congress. The first, in what they refer to as the anglophile responsible party tradition, argued that the U.S. legislature was marked by a lack of accountable leadership and responsible parties and that it stacked up poorly in comparison to the more centralized and disciplined British system. The second wave of Congress research was sociologically oriented. Closely tied to the growing interest in political behavior, this wave addressed key issues concerning the organization and evolution of Congress, whether it was more a vehicle for promoting social welfare than a vessel through which special interests exert influence, issues of representation, the role of parties and committees, and congressional voting. Polsby and Schickler also point out that the sweeping reforms enacted by Congress during the 1970s had a profound influence on the study of this institution, laying the basis for a third wave of congressional research. This wave has been characterized by an interest in explaining both the causes and effects of these reforms and how reform of the rules has affected congressional behavior. Further, much recent research on Congress, especially work using rational-choice approaches, has focused on understanding the design of its institutions and the consequences of variations in institutional design. Finally, there has been a resurgence of interest in the history of Congress.

Joel D. Aberbach and Bert A. Rockman analyze the presidency and the bureaucracy, rounding out our coverage of American political institutions. They begin by pointing to the underlying tensions between presidents and the bureaucracy. Crucial in this regard is that while bureaucracies have a general inclination to institutionalize practices, presidents have an inclination to change policies and practices. In the same vein, relations between presidents and the bureaucracy are frequently beset by principal-agent problems: the president delegates responsibility to bureaucrats, but they often have different goals than the chief executive and they sometimes have trouble determining exactly what the president wants accomplished. The result is likely to be a considerable
amount of administrative inefficiency and slack. Moreover, Aberbach and Rockman argue that delegation problems are worse in the United States than in parliamentary systems, since the president and Congress often make competing demands on and seek to control the bureaucracy.

Aberbach and Rockman also analyze how the structure of the American political system influences presidential leadership and the ability of presidents to gain the bureaucracy’s compliance. They examine different management styles that presidents have used and address how the sitting administration’s party affiliation and policy agenda have influenced president-bureaucracy relations. One of Aberbach and Rockman’s key conclusions is that the structure of the American system limits the extent of presidential leadership, but that the goals, preferences, and personal characteristics of the particular individual holding office are crucial as well.

The final chapter in this volume analyzes scholarship on race in American politics. More specifically, Lucius J. Barker and Katherine Tate address how race and race relations have influenced and have been affected by various aspects of the American political system, including its institutional structures, elections and voting behavior, political representation, public opinion, and the development of public policy.

Barker and Tate’s overarching argument is that mainstream work in the field of American politics is guided by pluralist theory, an approach that is beset by serious problems. Particularly important from their standpoint is that key socio-economic divisions continue to characterize the United States, many of which stem from race, racism, and ethnic splits. Barker and Tate maintain that problems of race and ethnicity have marked the United States since its founding. Not only do they argue that “it is difficult to accurately explain the nature and functioning of American politics without fully taking race and racism into account,” they also contend that racism poses a fundamental threat to American democracy (Barker and Tate, p. 265 in this volume). Barker and Tate make a plea for further research that will not only enhance our understanding of the politics of race, but that directly engages core policy issues that are central to improving race relations and ameliorating racism.

These chapters distill an enormous body of work and show the choices and paths of development in its creation. In each area covered in this volume, research has been shaped and advanced by an interest in responding to, understanding, and improving political conditions. Each in its own way has been concerned with core theoretical issues in the operation and performance of democracy, particularly with accountability and representation in democratic systems. Further, the five chapters following Laitin’s essay speak to many of the issues he raises, serving to continue the vigorous debate that his paper inspired when it was initially presented. We anticipate that the debates reflected in each of the other core papers will continue to be inspired as well. We now leave that continued debate in the hands of the readers.
Notes

1. While emphasis has varied, compelling evidence is found in many presidential addresses to the American Political Science Association (APSA), landmark essays, and APSA reports. See, for example, Shaw (1907) and Merriam (1926). The rise to maturity of the behavioral persuasion is masterfully reviewed and its impact evaluated in Dahl (1961a). Alternative views and concerned critiques are represented by David Easton (1969) in his presidential address and by Wolin (1968). These two objectives are also evident in early and successive “identity” reports sponsored by the association. See, for example, the Report of the Committee on Policy (1930), which included special reports on major areas by Charles A. Beard, Charles E. Merriam, William W. Willoughby, Pitman B. Potter, and Russell M. Story; and the summary of the Social Science Research Council Committees on Government and Public Administration in McLean (1945).

2. While much attention in the early years of the discipline was given to problems of urbanization, city government, and creating buffers between politics and administration, the expansion of corporations and then unions encouraged the study of groups. The rise of the Soviet state, the collapse of the Weimar Republic, and the rise of Nazi Germany encouraged the study of totalitarian and other authoritarian regimes; the postwar “discovery” of the Third World induced study of modernization and political development; and the proven capability of states to wage nuclear war sparked the creation and study of strategies of deterrence. The search for perspectives and tools from other disciplines commenced in the early years of the discipline. See, for example, Merriam (1926). The changed world order after World War II encouraged advocacy and adaptation of perspectives and research techniques from a multiplicity of disciplines, advocated by an earlier generation of Americanists, but now a new reality in the world added its voice. We have become, with refinements appropriate to the task and with productive effect, net importers of conceptual and analytical tools from the other social sciences as well as from mathematics.

3. The concern with the rise of the “fictive or artificial person,” that is the modern corporation, was of increasing interest to political scientists immediately after World War I and is cleanly captured and a main focus in Garfield’s (1924) presidential address.

4. This is reflected in the dramatic growth in the number of organized sections in our association as well as in the expansion of scholarly journals in the field since World War II. There were but two disciplinary journals before 1940, the *American Political Science Review* (1906) and the *Journal of Politics* (1938). The expansion in the number of professional journals has been particularly pronounced in the fields of comparative and international politics, occurring first in the immediate postwar period and then again a quarter century later.

5. For Easton the effort was driven by a commitment to integrate fundamental concerns in political philosophy with advances in behavioral political science and with nonpolitical determinants of political action. For Almond and Eckstein, both comparativists, the former influenced by structural-functional theory in sociology and anthropology and the latter concerned with the cultural embeddedness of authority relations, the purpose was to establish analytical categories and taxonomies that would facilitate comparison of whole systems. Interestingly, few scholars of American politics engaged in such endeavor. Appeals for discipline integrity, however, were strongly and articulately made by distinguished
Americanists in their presidential addresses to the association. See especially Truman (1965) and Warren E. Miller (1981), the latter seeing disciplinary integrity deriving from inspired and well-formulated comparative work.

6. See also Merriam’s (1921) thoughtful prologue.

7. This formulation is different from the important contributions made in earlier surveys of the discipline in that the papers here are more broad-gauged in scope and more evolutionary in conception and design than these complementary works. The reviews and analyses included in the volume are also stretched and enriched by the accompanying commentaries. Important complementary works include Finifter (1983, 1993); Katznelson and Milner (2002); Weisberg (1986); Polsby (1998, 1999); and the important essays included in Greenstein and Polsby (1975).

8. Given the comprehensiveness of its scope and its radical point of departure and destination, we have included Laitin’s essay in both this and our companion volume. See Mansfield and Sisson (2004).