The Evolution of Political Knowledge

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The discipline of political science has been driven and shaped by what the world yields that is consequential for understanding the exercise of power, purpose, and choice in the public arena. It has also been strongly influenced by the adaptation, refinement, and employment of analytical tools developed largely in neighboring disciplines, such as economics, psychology, and sociology. Political science commenced and continues as an “interdisciplinary” and complex field, the subject of which is at once fundamentally important and ever changing. The primary aim of this field is both dual and difficult: to develop a body of verifiable political knowledge and to use that knowledge to enhance the quality of both public and private life. These two aims intersect but are not coincident, and the constant tension between them has been such as to strain and lessen the space created by their intersection.

More specifically, political scientists have been moved by two principal purposes over the course of the past century. First, they have sought to understand and explain political phenomena in a way that is both theoretically and empirically grounded. Second, they have analyzed matters of enduring public interest, whether in terms of public policy and political action, fidelity between principle and practice in the organization and conduct of government, or the conditions of freedom, whether of citizens or of states.

Many of the central advances made in political science have been prompted by a desire to improve both the quality of and our understanding of political life. Nowhere is this tendency more apparent than in the subfields of comparative politics and international relations. The study of war and nuclear strategy (Brodie 1946; Jervis 1989; Osgood 1957; Schelling 1960; Waltz 1959, 1979;
Wright 1965), for example, has been of enormous public relevance. So, too, have been investigations of the sources and effects of democracy (Dahl 1956, 1971; Lipset 1959; Moore 1966; Rokkan 1968; O’Donnell and Schmitter 1986; Przeworski 1991); equality and voting (Duverger 1954; Rae 1971 [1967]; Cox 1997); culture and regimes (Almond and Verba 1963; Pye and Verba 1972; Putnam 1993); foreign policy decision making (Snyder 1961; Jervis 1976); and the political economy of international relations (Gilpin 1975; Keohane 1984; Katzenstein 1978; Krasner 1976, 1978). These are, of course, just a handful of the many important avenues of inquiry that have contributed to both the practice and the study of comparative politics and international relations.

Since the mid-twentieth century and particularly during its last third, however, the political science discipline has become increasingly specialized 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 (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 relationship between theory, inquiry, and political action.
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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 principal 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 doing so, 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 in recent years. Equally, we 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 essay on democracy and equality in this volume.

The following essays are revised versions of those originally presented that address how key areas of inquiry in the fields of comparative politics and international relations have developed and how they have influenced, or may influence, the study of political science. Each essay is followed by a set of commentaries by leading experts. Together, each essay 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.

We do not claim that the scope of core areas of research surveyed here is in any way comprehensive. Nevertheless, the areas we do cover are fundamentally
important, have drawn successive generations of practitioners, have been attended by serious debates, and have enjoyed the evolution and have become the reservoir of a substantial fund of reliable and, hopefully, socially useful knowledge.

Included in this volume are seven 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 field, 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.

Following Laitin’s chapter is a set of essays on the two central areas of inquiry in the field of international relations, international political economy and international security. Stephen D. Krasner addresses the development of research on globalization, a key area of inquiry among scholars of the international political economy. Of late, an increasing number of observers have argued that heightened globalization has produced profound changes in international affairs. The development of new technologies, in their view, has led to sharp increases in both the amount and the complexity of international transactions. Equally, rising international intercourse has eroded, if not eliminated, the political authority of sovereign states.

Krasner challenges these claims, arguing that globalization has produced much less change in the global system than is often alleged. States have not been displaced as the primary actors in the system. They continue to exercise considerable authority over nonstate actors by creating the laws, rules, and institutions that shape the political and economic space in which these actors operate. Moreover, the rise in global flows is due in large measure to the power and preferences of the major powers, particularly the United States. Finally, Krasner takes issue with the argument that globalization has undermined state sovereignty. The “contingent” nature of sovereignty and the various challenges to it in the contemporary world are not new developments stemming from globalization, he maintains, but rather enduring problems that states have faced for centuries.
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Krasner concludes that globalization has not eroded sovereignty; instead, effective sovereignty has been necessary for the rise of globalization.

Whereas Krasner’s chapter addresses some key aspects of the international political economy, Robert Jervis analyzes the intellectual history of research on international security since World War II. A key theme of Jervis’s essay is that, more than in most other areas of political science, there has been a close relationship between scholarship on security issues and public policy. The issues prompting the interest of policy makers have also attracted the interest of academics; research has had a marked influence on and has been influenced by security policy; and the field of security studies itself has been deeply politicized, due largely to its preoccupation with issues that are both partisan and vital to America’s well-being.

Crucial to both the study of national security and U.S. foreign policy over the past half-century has been the advent of nuclear weapons. The nuclear revolution gave rise to a steady vent of scholarly research addressing how to avoid nuclear war, how to respond if one breaks out, and how to manage the proliferation of nuclear weapons. The Korean and Vietnam Wars stimulated interest in limited wars and how to avoid the escalation of such conflicts. More generally, Jervis argues that not only has engagement with current security problems kept this area of inquiry dynamic and lively, it has also helped produce theories and empirical findings of general and enduring value.

The final four chapters in this volume address central areas of inquiry in the field of comparative politics. Leonard Binder analyzes the evolution of research on the nature of ethnicity and the sources of ethnic conflict. Much of his essay considers the merits of a variety of arguments that are crucial to rational choice approaches to ethnic conflict. Among these arguments is that ethnic identity is constructed, not primordial, and that competition within ethnic groups focuses on alternate ethnic identities, which prompt exponents to adopt strategies that contribute to these identities. Equally important is the claim that ethnic groups are organized in ways that are similar to—and face the same sort of collective-action problems as—other groups in society.

Binder also evaluates the merits of psychological approaches to ethnic politics and ethnic conflict. He argues that these approaches frequently hinge on unfalsifiable propositions about the consciousness of ethnic groups or the individuals that comprise them. He analyzes whether rational choice explanations of ethnic competition and conflict are hampered by the limited weight they attach to culture and the emotional qualities of ethnic conflict. Finally, Binder examines a set of core issues pertaining to the links between nationalism and ethnic movements, the use of nationalism and national identities as a political strategy within countries, and the use of nationalism and ethnic strategies as elements of foreign policy.
The remaining chapters consider key features of democratic governance from a cross-national perspective. G. Bingham Powell Jr. examines the development of comparative research on democratic representation. The crucial feature of a democratic system of government is that policy makers respond to the preferences of the populace. Powell distinguishes two areas of work in the field of comparative politics addressing the links between voters and elected officials in democratic settings. One area—“procedural” representation—encompasses the relationship between voting for political parties in legislative elections and the partisan composition of legislatures arising from these elections. In his survey of such studies, Powell emphasizes that representation is influenced by competition among political parties, the electoral decisions made by voters, election rules, and political geography.

The second area—“substantive” representation—focuses on the links between the preferences of citizens (rather than their votes) and the preferences and behavior of elected officials. Studies of this sort stress the relative congruence between constituents’ preferences and the behavior of their representatives, an issue that lies beyond the scope of research on voting. Powell argues that work on substantive representation has led to some important insights but less of a “standard theory” than research on procedural representation. He concludes that, while each line of research has made key advances, each tends to give short shrift to factors emphasized in the other. In Powell’s view, more work on representation is needed that integrates the procedural and substantive research programs.

John E. Roemer analyzes the relationship between democracy and the equality of economic conditions among individuals. A frequent claim in the field of comparative politics is that democracy promotes justice and equality. Roemer addresses this claim by constructing a model in which: (1) two parties represent different segments of society; (2) a primary concern of society is how much to invest in public education for children; (3) education determines how much children will earn once they enter the workforce, and (4) equality (or the lack thereof) is the product of a long-run process. Parties, in his model, include three factions: one concerned only with winning office, another concerned only with serving the party’s constituents, and a third interested in using “electoral competition as a forum to advertise . . . their constituents’ interests” (Roemer p. 265 in this volume).

The central result stemming from Roemer’s analysis is that whether democracy fosters equality in educational investment depends on which faction dominates the ideological character of domestic politics. If the third faction (which is made up of party activists, who care less about winning the election than about proposing a policy on educational spending that corresponds closely to the faction’s median constituent) dominates intraparty bargaining, then convergence in public spending on education will result. Alternatively, if such bargaining is dominated by the first faction (which is made up of individuals whose sole goal
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is to win office), then convergence will not occur. Roemer concludes by assessing various reasons that lie outside his model why we actually do not observe more convergence in educational investment.

Various observers have linked democracy to economic growth, as well as to economic equality. Adam Przeworski, however, argues that most research addressing the effects of regime type on growth has been flawed because it fails to take into account the factors influencing the emergence and endurance of regimes. What conditions promote democratic transitions? Przeworski maintains that it is difficult to say. He surveys a wide variety of economic, cultural, historical, and political factors, none of which seem to have a systematic bearing on the collapse of dictatorships.

In contrast, it is much easier to identify the conditions under which democracies endure. Foremost among them is the level of economic development. Przeworski (p. 308 in this volume) argues that the survival of a democracy grows increasingly likely as its per capita income rises. Furthermore, “no democracy ever, including the period before World War II, fell in a country with a per capita income higher than that of Argentina in 1975, $6,055.” In addition, improving the quality and quantity of education, promoting economic growth, fostering a more uniform distribution of income throughout society, heightened party competition in the national legislature, regular turnover in the executive branch, and a parliamentary (rather than a presidential) system of government help democracies to persist. The effects of these factors, however, are far less striking than that of per capita income.

Finally, Przeworski turns to the impact of democracy on economic growth. After accounting for the factors that give rise to democratic transitions and the survival of democracies, he finds no evidence that regime type influences either the share of investment in a country’s national income or the growth of its capital stock. Further, he finds that while democracies are better at making technological progress and utilize their labor force more effectively, dictatorships are better at exploiting their capital stock. The upshot is that economic growth is not greater in democracies than in other countries.

These chapters distill an enormous body of work and chart 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 six chapters and their accompanying commentaries 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 and advanced 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 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 “discovery” of the Third World after World War II induced study of modernization and political development; and the proven capability of states to wage nuclear war sparked the creation and testing 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 (1921, 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 of major concern to scholars in comparative and international politics who also elaborated additional dimensions and techniques. For the origins of this change see, for example, Lowenstein (1944), Deutsch (1953), the Report on the Interuniversity Seminar on Comparative Politics in Macridis and Cox (1953), Neumann (1957), Rustow (1957), and Jervis (1976). 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.

2. While emphasis has varied, compelling evidence is found in many presidential addresses to the American Political Science Association (APSA). See, for example, Shaw and, with particular regard to comparative and international politics, Bryce (1909) and Lowell (1910). The themes are subsequently found in numerous addresses, illustrative and important among which are Merriam (1926), Key (1958), Friedrich (1963), Deutsch (1971), Lijphart (1997), Holden (2000), Keohane (2001), and Jervis (2002b).

3. This is reflected in the dramatic growth in the number of organized sections in APSA 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* (1908) and the *Journal of Politics* (1938). The expansion in the number of professional journals has been particularly pronounced in the broad fields of comparative and international politics, occurring first in the immediate postwar period and then a quartercentury later. Journals devoted to particular international regions developed during this period as well, reflecting a parallel development in area studies.

4. 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, 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 endeavors. 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 Miller (1981).

5. The thoughtful and provocative presentations from one such panel have recently been published as a symposium entitled “Shaking Things Up?: Thoughts about the Future
of Political Science,” and includes contributions by Kristen Renwick Monroe, Russell Hardin, Robert Jervis, Elinor Ostrom, Susanne Hoeber Rudolph, Marion Smiley, and Rogers M. Smith. While each contributor has a separate piece, in the references to this volume the symposium is cited as Kristin Renwick Monroe et al.

6. This formulation is different from the important contributions made in complementary 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 considerably 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).

7. 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).

8. One of the most controversial and important features of Laitin’s proposal is his call for folding the study of American politics into the subfields of comparative politics and political institutions. Americanists will undoubtedly have much to say about this proposal, and the chapters on the evolution of work on American politics in our companion volume should help lay the foundation for debate on this topic. See Mansfield and Sisson (2004).