The Evolution of Political Knowledge

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COMMENTARY
Integrating Approaches to the Study of Congress

MORRIS P. FIORINA

When Program cochair Dick Sisson asked me to serve as a commentator on a theme panel titled “Congress in American Politics,” I anticipated an easy preparation. That state of blissful overconfidence dissipated soon after the arrival of the theme paper. On their second page Polsby and Schickler set aside the literature on congressional elections in favor of other aspects of Congress. Given that at least 90 percent of everything I’ve ever said, written, or even thought about Congress relates somehow to elections and representation, the task to which I’d agreed suddenly became more difficult.

Polsby and Schickler provide a useful survey of the congressional literature organized along temporal lines. They begin with a discussion of a transitional empirical phase that led the field away from an older anglophilic and reform-oriented tradition typified by the responsible party advocates. This transitional phase culminates in what we might call “the Great Generation,” namely Fenno, Polsby, Jones, Matthews, Price, Peabody, Ripley, and a few others. The work of these scholars represents the flowering of the behavioral revolution in the study of Congress. Then congressional scholarship moves from what has been primarily a sociological approach to a rational choice/new institutionalism focus, and most recently renews a long-standing interest in congressional history. Polsby and Schickler relate these changes in intellectual approach and substantive focus both to currents in the discipline at large, and to the impact of changing realities on congressional scholarship. In particular, they emphasize the reforms of the mid-1970s that separate the “textbook congress” of midcentury (Shepsle 1989) from the postreform Congress that is now the object of our studies.

In these remarks I will offer a different, more conceptual take on the congressional literature of the past forty years, one that allows us to bring in the literature on elections and representation as well as the topics treated by Polsby and Schickler, and thus one that gives me something to say. In 1970, give or take a year, Dick Fenno assigned his graduate seminar a review essay by Heinz Eulau and Katherine Hinckley. Eulau and Hinckley (1966) organized their review in terms of “inside” and “outside” models of Congress. Inside model studies situated explanations of congressional behavior, institutions, and procedures within the institution itself—in the expectations, beliefs, and powers of the legislative party leaders, the committee chairs, and the rank-and-file members themselves. Think Donald Matthews (1960) and his widely read exposition of Senate norms
at midcentury. In contrast, the outside model located explanations of congressional behavior, institutions, and procedures outside the institution—in the mass parties, interest groups, and constituency interests. Think Julius Turner (1951) and his numerous tables contrasting the roll-call voting records of different categories of representatives.

It seems to me that the arrival of the Great Generation coincided with a shift from the outside to the inside model of studying Congress. To be sure, the difference is only one of degree—a change in emphasis, not a revolution. Work in the Turner tradition continued, of course; Clausen’s (1973) study is an example. And great generation scholars certainly did not ignore outside factors. Masters (1961) discussed constituency and interest groups in his description of the House committee assignment process, Polsby (1969) himself contrasted the inside strategy of Carl Albert with the outside strategy of Richard Bolling in the 1962 House Majority Leader contest, and Jones’s (1961) study will be noted below. But when we think of the mainstream of congressional studies of the 1960s we think of Fenno’s (1962) and Manley’s (1965) discussions of the internal norms of Appropriations and Ways and Means. Leadership studies (e.g., Ripley 1967, 1969) barely mention elections in passing. And former professor David Price (D-N.C.) writes a 350-page book (Price 1972) on lawmaking that has no index entries for “campaigns,” “campaign finance,” “elections,” or “representation.”

The shift to a rational-choice perspective led by Fenno (1973) and Mayhew (1974) reflects a larger current in the discipline as Polsby and Schickler suggest, but it also is associated with renewed interest in the outside model of Congress. The late 1960s and early 1970s were a period of political upheaval—the Wallace candidacies, Goldwater and McGovern insurgencies, social movements and the interest-group explosion, the decline of party, the rise of incumbency, and the spread of candidate-centered politics. Scholars noted the decline of congressional norms (e.g., Asher 1973), and the apparent waxing of constituency pressures even in such important and previously insulated arenas as Appropriations. Polsby and Scheckler note that changing realities change the perspectives of congressional scholars. I agree—the preceding changes led to renewed interest in outside influences on Congress. I would add that with that shift in explanatory perspective came an associated shift in intellectual perspective. When Congress is viewed as a cozy club insulated from its environment, discussing it in the language of sociology comes naturally, but when Congress is viewed as a collection of individual entrepreneurs highly exposed to outside pressures, the language of economics is more natural.

From the mid-1970s to the mid-1990s the outside model enjoyed preeminence. Again, we are talking about differences in degree here, but the preeminence of the outside model is reflected in the structure of the Polsby-Schickler
COMMENTARY ON POLSBY AND SCHICKLER

paper: there are a number of people in the audience today who consider themselves congressional scholars, who contributed to the literature during these two decades, and whose names are absent from the paper. The discussion in the paper jumps from the early 1970s Fenno-Mayhew transition to the 1990s—in my terms, from the decline of the inside model to its resurgence.

The resurgence of the inside model in recent years reflects the apparent resurgence of the legislative parties. But I don’t believe that our subfield is just swinging on the pendulum between inside and outside poles. Rather, there is visible progress. Congressional scholars today work against a background of 1980s outside model research, and the contemporary literature is more explicit about the connections between the inside and outside. Thus, Dave Rohde roots the resurgence of legislative parties in the electoral arena—a sorting out of constituencies that left both parties more homogeneous internally and more differentiated from each other. Cox and McCubbins (1993) adopt this argument and add to it the argument that a positively perceived party label has electoral value to all members and thus provides a resource available to legislative party leaders. Very recently Jack Wright (2000) offers an interesting revisionist account of the 1970s reforms themselves, arguing that they were stimulated by the changed electoral realities Northern Democrats faced. A great generation forerunner of works like these is Jones’s (1961) discussion of the House Agriculture Committee, which interweaves inside and outside considerations in the politics of agriculture.

I’d like to see our subfield continue to engage in such integration. In particular, despite Rohde’s discussion of the electoral roots of congressional party resurgence, the current debate about party resurgence could use more explicit attention to outside considerations. My colleague, Keith Krehbiel, has been able to stymie the subfield with a deceptively simple question: “why would the median legislator cede to party leaders the power to pull legislative outcomes away from the median?” Some highly respected colleagues have attempted to respond to Krehbiel’s question, none persuasively, in my view. The problem I think is that the answer does not lie entirely within the institution, where most of Krehbiel’s critics have tried to find it: no inside model will provide a satisfactory resolution of the puzzle.

For example, consider the 1998 House vote to impeach President Clinton. There seemed to be universal agreement that the most preferred position of a number of House moderates was the middling position of censure. There were procedural means to insure that censure was an option (Krehbiel 1998). In the end, however, moderates not only did not utilize such procedures but most voted for impeachment. Was this an instance of Tom DeLay and other Republican leaders threatening the stick and promising the carrot? While that interpretation was popular in the media, I think that electoral pressures were prominent in the
decisions of a number of the moderates who struggled with their decisions. Even if majorities in their geographic constituencies opposed impeachment, the hard-core members of their primary and reelection constituencies strongly favored it. Had the moderates supported censure, they might have provoked disaffection at best, and defection in terms of a primary challenge at worst. Either of these outcomes would have left them wounded and vulnerable to a Democratic challenger in the general election.

The lesson I think is that we need to integrate inside and outside models of party influence. An important avenue for party influence today comes through leadership campaign contributions and the ability of leadership to direct interest group contributions by signaling approval of members and would-be members. No internal stick controlled by the leadership is more painful to a member than defeat, thus limiting the power of internal sticks. No internal carrot has any value if the member loses, thus limiting the value of internal carrots. Rather than attempt to work counter to electoral pressures, then, why would party leaders not attempt to work with them instead? I think they do.

More broadly, I urge an integration of inside and outside models in all aspects of congressional research. Congress is our most representative institution. Members of the House are part of the permanent campaign, and if we consider fundraising to be campaigning, so are senators. Arguably the institution is more exposed to environmental forces now than ever before in its history. Whether we talk about the composition and operation of committees, the construction and adoption of rules, the activities of the leaders, or the policy process in its entirety, the constituents and interests on the outside are never far from the calculations of the members on the inside. They should be equally present in our research.

Notes

1. Since delivering these remarks Eulau has told me in personal conversation that the inside-outside distinction belongs originally to Polsby! A citation to that effect was edited out of Eulau’s essay with Hinckley.

2. Omissions rectified in his later work written after his election to Congress (Price 1992).

3. In my view they are part of what is at least a “near-great” generation. Fiorina (2001) contains a partial listing.

COMMENTARY
Boxing Methods for Legislative Research

KEITH KREHBIEL

When I was asked to participate on one of the panels that has culminated in this volume, I had two responses. First, I said, “This collective endeavor seems big, broad, important, and interesting. It would be an honor. I’ll do it.” Second, I thought, This collective endeavor seems big, broad, important, and interesting. Why did I say that I’ll do it? And what was that part about providing a rational choice perspective?

The APSA panel nevertheless proceeded as planned. It was chaired by Richard Fenno, a one-time-or-another professor of all three discussants: David Mayhew, Morris Fiorina, and me. We were in unanimous agreement that Eric Schickler and Nelson Polsby have done a great service to our field by condensing approximately a half-century of scholarship into a remarkably readable and unthinkably neutral document. It seems quite possible that, fifty years from now, under similar circumstances, another pair of first-rate, bigenerational congressional researchers will produce a similar paper, titled “A Century of Congressional Research.” The new manuscript could cover twice as many years in only negligibly more words if it simply includes “See Polsby and Schickler 2004.”

Writing an effective review essay entails sorting through vast quantities of research, each piece of which can be evaluated from any of several perspectives. The consequence of thorough and useful evaluations, such as Polsby and Schickler’s, is that important works are sorted out and implicitly placed into figurative boxes along with other similar works. This process of parceling—no matter how carefully executed—is always subject to second-guessing. So, for instance, in the course of presenting their characteristically deep and thoughtful comments, Mayhew and Fiorina identified several instances of scholarship that had, in effect, been placed into a pile labeled “Also Boxworthy” (see their comments in this volume). In response to such comments, Schickler and Polsby signaled their willingness to make further low-cost improvements in their essay.1

Similar to some of Fiorina’s and Mayhew’s remarks, my comments, were of the low-cost suggested-reboxing sort, which means that they may have already elicited revisions that render them retrospectively nongermane. Accepting that as an occupational hazard attendant with the iterative but overlapping nature of this enterprise, I reproduce them below.
**Boxing Tips**

As a general matter, Polsby and Schickler’s essay is not vulnerable to major subjective objections. This is not to say that readers will or should agree with all claims in the literature deemed worthy of review. Rather, the point is that the authors have done a fine job of summarizing various works and of taking the high ground when reviewing them. On the other hand, one can question whether their four-box classification scheme is tuned optimally for summarizing what most of us have been doing for most of our lives. To help address this question, a guiding premise is: If the field of legislative studies has evolved into a good example of what political science should be, then a good review essay about what we have done should emphasize methodological approaches at least as much as substantive findings.

My main claim is that the essay could do a somewhat better job of this. The corresponding questions are simply: What boxing scheme did the authors choose to employ? and What adjustments in the scheme might improve it? Polsby and Schickler’s boxing scheme is summarized below.

Three broad comments are noteworthy. First, while substantive emphases have come and gone, they have not done so in a cleanly demarcated, period-specific fashion. Furthermore, with the exception of Movement 1, it is never the case that the substantive focus of the movement determines the label. Second, the epicenters are not as much hotbeds of present activity as they are training camps for future contributors who carried on in a manner compatible with their mentors’ substantive interests and with their graduate institutions’ methodological thrusts. Third, and following immediately from the previous observation, the distinguishing characteristic of Polsby and Schickler’s boxes seem fundamentally—albeit tacitly—to be methodological. Because methods more so than people and places have defined and determined the paths of development of legislative studies over the past fifty years, a different boxing scheme seems

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<th>Movement</th>
<th>Epicenter</th>
<th>Methods</th>
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<tr>
<td>1</td>
<td>Responsible Party Government</td>
<td>Harvard</td>
</tr>
<tr>
<td>2</td>
<td>Sociologically oriented behavior</td>
<td>Wisconsin</td>
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<td>3</td>
<td>Rational choice / Reform Institutionalism History</td>
<td>Rochester</td>
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possible if not preferable. This suggestion is reinforced by movement-specific observations on the Polsby-Schickler scheme.

**Historical Approaches?**

A unique box for the movement labeled “historical approaches” seems unnecessary. One reason is that Polsby and Schickler identify neither an epicenter nor a father figure for historical approaches, as they do for other approaches. Such omissions are understandable: political historians were not the inventors of either the study of history or the study of politics. Another reason is that the methods employed in the works cited are predominantly inductive and descriptive, which reinforces the claim that historical approaches are not methodologically unique. Later in their essay, Polsby and Schickler equate political history with the study of “events,” but whom among us would argue seriously that political historians have a monopoly on the study of events? Similarly, there is no clear (or, at best, only an arbitrary) definition of what constitutes history and what constitutes ordinary empirical research. All of us study events, and all such events share the property of having occurred in the past. In this self-evident way, all empirical researchers are historians and vice versa.

The implication is not that works of so-called political history are not box-worthy but rather they do not require a box of their own.

**Responsible Party Government?**

With works of political history repacked in accordance with their methodological characteristics, three movements remain in the half-century of congressional research. Of these, Movement 1, while significant in many other respects, is not significant as an example of social science as we have come to construe social science. Rather, it is a good example of the opposite (or perhaps complement) of what many of us strive for. In research of this type, descriptions tended to be upstaged by prescriptions, and, from this relatively focused perspective, such works are not essential ones for inclusion in history of the social-science evolution of legislative studies.

**Induction and Deduction**

Of the two movements that remain—induction and deduction—a curve-fitting inductivist might predict that Movement 2, induction, will be the next victim of this Stalinesque purge. The prediction will not be borne out, however, because the surviving boxes and the methods they represent are much more appropriately viewed as complements than as competitors. Polsby and Schickler’s essay makes
clear the fact that many works from each methodological persuasion have contributed to the current understanding of substantive issues in legislative studies.

Boxes within Boxes

Finally, what of the a, b, and c within Schickler and Polsby’s box 3, which they call “rational choice” and I prefer to call “deductive approaches”? Here, too, nothing is fundamentally wrong with the boxes within the box, but there is nevertheless some redundancy. First, as argued above, “History” again adds nothing; anything Polsby and Schickler would put in this subcompartment has an equally comfortable place in the induction or deduction box. Similarly, “Reform” was and is an instance of institutional change and therefore is subsumed by 3b. Indeed, Polsby and Schickler’s own phrase for this box is its “new focus on the design and effects of institutions.” The study of reform is entirely about the design and effects of institutions.

Conclusion

The unifying feature of legislative studies in the last half-century has been a common substantive interest in political behavior and collective choice within legislative organizations. Alongside the common substantive focus have been two distinct but complementary methodological approaches: inductive and deductive. Polsby and Schickler have done an excellent job of selecting and summarizing works that illustrate this nontrivial instance of a maturing social science. That they might have done so with fewer boxes is hardly a scathing criticism. The more parsimonious scheme advocated here does, however, help to emphasize a key methodological reason for the many substantive success stories in the field. Specifically, the time trend of inductive and deductive studies of legislatures reflects steadily increasing expectations and performance in terms of explicitness of both theory and methods. Huitt’s work, for example, is outstanding because of its explicitness about organizational determinants of behavior. The same can be said of Fenno’s Power of the Purse (1966) and Congressmen in Committees (1973). Mayhew and Fiorina continued in this rich, evolutionary tradition by elevating their respective axioms, boldly and proudly, as if they were targets. Anyone could, and many did, take shots at these pathbreaking studies. But, to our collective benefit, no one was confused about the targets toward which critics shot.

Continuing to the present, in the 1980s and through the 1990s, the intellectual children and grandchildren of Riker and Fenno have worked to further raise standards of explicitness to include not only axioms (e.g., Mayhew) and goals (e.g., Fenno) but also and especially the hypothesized game form (institutions)
and the resulting equilibria (behavior). While doing so, more and more excellent work has come online that is equally explicit about—and develops tests for—refutable empirical propositions. Regardless of how it is packaged, the body of work constitutes a solid foundation on which to build.

Notes

1. For instance, to many of the comments about omitted-but-boxworthy works, it was suggested in rejoinder that rounding out the citations would entail not much more than consulting the senior author’s vita.

2. One explanation for this conclusion is that Polsby and Schickler may not accept the premise, and that is fine. Naturally, then, this comment will be more interesting to those of us who do accept the premise.

3. Important exceptions are noted: e.g., V. O. Key and Arthur Maass at Harvard, Ralph Huitt at Wisconsin, Richard Fenno (arguably, William Riker should be added) at Rochester.

4. Actually, it is more like a Boxer Rebellion.

5. The same can be said with respect to another widely used distinction (including its use by Polsby and Schickler): that between “behavior” and “institutions.” While there are differences in central tendencies between so-called behavioralists and so-called institutionalists, a close look at almost any paper that falls even solidly into one box or another reveals numerous institutional constraints on behavior in the former case, and no shortage of behavior in the latter case. Even the Congress of die-hard brick-and-mortar institutionalists has people in it, and its people invariably take actions, i.e., behave. Granted, “sociological-oriented” political scientists (for example, Fenno in *The Power of the Purse*) were more likely to use the word “organization” than the word “institution.” Similarly, “economically oriented” political scientists ran a few laps with social-choice theory—which is *abehavioral*. But, without exception, the best of research within inductive and deductive boxes alike is jointly behavioral and institutional.

6. The distinction is methodologically important. Deduction simply means reasoning from assumptions or postulates. It is not essential that behavioral postulates embody rationality, and, even if and when they do, there are any number of reasons the implications derived will fail, such as getting the institutional features wrong. The advantage of the deductive method (i.e., modeling) is that it raises the standards for explicitness and logic and thereby positions researchers better for learning from both strengths and weaknesses of often quite diverse theories.

7. So, too, is “sociologically oriented behavior,” because such studies almost invariably are sensitive to “context,” “organizational forces,” or, in more recent terms, “institutions.”
I would make two friendly amendments to this account by Polsby and Schickler. The first points to an additional “line of intellectual influence” in the congressional field, the second to, if not exactly that, an additional line of intellectual development. The first refers back chiefly to the 1950s and 1960s, the second to the 1980s and 1990s. My own intellectual vantage point is at the back of both suggestions.

For Polsby and Schickler, it was the pioneering generation (or perhaps double generation) of Huitt through Fenno and the other “boys of Congress” that broke the congressional field out of its old responsible-parties mold in the 1950s and 1960s, set a new behavioral style based in interview work and sociology or anthropology, contributed an unsurpassed oeuvre on Congress, and influenced virtually everybody else writing or teaching on the subject. I do not see why anyone would want to quarrel with any of these claims.

Still, for me edging into the congressional field in the 1960s, something additional was going on, or at least I think it was. “Science” was in the air—both in political science in general and in certain works about Congress in particular. By science I do not mean anything mysterious. It was an ambitious thrust toward trying to illuminate a great deal of complicated, jangling reality by bringing to bear simple theories or simple measures. That was to be the discipline’s escape from its regrettable past habits of fuzziness, description, and normativism. The proposed theories or measures varied, yet I remember that certain of them had a reductive, “it’s really nothing but X” quality familiar in the physical sciences yet also in pre-Socratic philosophy where a reputation could be made through asserting that the universe is really made out of, say, water.

In works centering on Congress, “group theory” was the most prominent offering in this vein in the 1950s. From books by Bertram M. Gross (1953), Earl Latham (1952), and others we learned, or at least I think I remember learning, that congressional politics stripped of its confusing detail was “nothing but” a struggle among interest groups. Earlier, that insight had been advanced in a lastingly impressive work on Congress by E. E. Schattschneider (1935). But group theory was not the only simplifying move. For Roland Young (1958), Congress needed to be seen as a “system,” another buzz-phrase of the time. Also, I recall being attracted by William Riker’s (1962) claim that “minimal winning majorities” were the main clue to Congress or any legislature. Besides theories there
were simplifying measures, which sometimes owed partly to V. O. Key Jr. as in the influential volume using such measures to analyze congressional roll calls by Key’s student, Julius Turner (1951).

That is the early intellectual environment I remember, or at least part of it. To be a congressional scholar meant to press ahead with a simplifying measure or a simplifying theory, one or the other.

As for the 1980s and 1990s, Polsby and Schickler point to three themes that have emerged as particularly significant in the congressional scholarship in recent times—congressional reform, the design and effects of legislative institutions, and changes in congressional institutions over time. These are all on the mark, but I see something else. Another recent emphasis is the study of variation in policy results over time, measured or at least thought about more or less systematically, with the causes of that variation located significantly on Capitol Hill. That is a mouthful but I hope it comes across. A trademark of this kind of work is biennium-by-biennium, or sometimes year-by-year, measures of policy results. Is work like this congressional scholarship? Well, why not? It assigns a large role to congressional processes. Certainly there has been a lot of it—to cite a few examples, works by Terry M. Moe on NLRB regulation (1985), B. Dan Wood and Richard W. Waterman on regulatory policy (1991), Paul E. Peterson on budgeting (1989), D. Roderick Kiewiet and Mathew D. McCubbins on appropriations (1991), John Mark Hansen on trade policy (1990), Sharyn O’Halloran on trade policy (1994), and a range of writers including Mark A. Peterson (1990), George C. Edwards III (1989), Sarah A. Binder (1999), Keith Krehbiel (1998), and myself (1991) on lawmaking in general.

Scholarship of this sort is increasingly voluminous, it cuts across a variety of methods and theories, and, so far as I can tell it is largely new. Congressional scholars of the 1950s and 1960s did not dwell on policy results (although Richard F. Fenno’s *Power of the Purse* [1966] plainly did and is a major exception). Also, systematic study of variation over time seldom drew that generation’s talents. Findings and generalizations were often presented as more or less timeless.

If these perceptions are correct, what explains the more recent shift toward studying policy variation over time? I can think of two answers. For one thing, the subject of policy results seems to have gained standing as a subject worth studying in its own right. Perhaps the discipline shook off an old preconception that if you knew the interest groups and you knew the parties you could tell the policies; nothing more needed to be studied. Perhaps the Washington, D.C., universe itself has changed. At any rate, in 1984 it was possibly a transition when Nelson W. Polsby and John W. Kingdon, two leading congressional scholars, wrote books asking directly how and why policy gets made. In those two cases the trademark apparatus of biennium-by-biennium data sets did not appear, but it soon became prominent.
The second answer has to do with explanatory styles. In the 1970s and 1980s, econometric analysis using time series intruded into political science in a brash way, bringing with it techniques as well as a bundle of assumptions about how to frame empirical work. Those techniques and assumptions have gotten a work-out, if often in diluted form, in the scholarly tendency I discuss here.