Chapter I_

Policy Issue Substance and the Revitalization of Legislative Studies

WHEN LEGISLATIVE STUDIES EXPLODED in the 1960s and 1970s, many scholars took seriously the idea that policy issue substance was theoretically and empirically a very important consideration. During this earlier moment, scholars such as Theodore Lowi (1964, 1970, 1972), Aage Clausen (1967, 1973), David Mayhew (1966), and a young aspiring PhD candidate (and future Congress member and vice president), Richard Cheney (Clausen and Cheney 1970) were curious about how the types and content of issues under discussion shape political behavior and how lawmaking subsequently produces particular policy outputs. At the outset of this fertile period for legislative studies—which would produce tremendous advances in systematic studies of Congress—work privileging policy issue content seemed poised for prominence, perhaps even predominance, within the subfield. The situation would quickly change, however, for reasons explored in this book. By the mid-1980s the substance-centered line of research was nearly nonexistent, and by the 2000s it seemed to have donned a cloak of invisibility.

The complete disappearance of issue substance from the study of policy-making and Congress by Congress scholars leads to two natural questions. First, what was the reason for this disappearance? And second, does it matter? In other words, has the removal of policy issue substance from our study of Congress hindered progress in our understanding of how the lawmaking and policymaking processes work in the United States?

In attempting to answer the second question—which is the central focus of this book—I demonstrate repeatedly, through both theoretical and empirical exercises, that the removal of policy issue substance from our study of Congress has mattered very much and indeed has been extremely costly.

¹Aage Clausen and Richard Cheney (1970) demonstrated the distinct existence and effects of economic and social welfare policy dimensions in roll call voting, hypothesizing that the economic dimension is influenced more by partisan differences and the social welfare dimension by constituency constraints.

The cost of omitting policy substance takes two primary forms. The first is mischaracterization of the policymaking process. Specifically, I show that we often make incorrect inferences about lawmaking when we neglect policy substance. Sometimes these mistakes in assessing theories of lawmaking and/or policymaking lead us to lump laws together by issue area in our analyses rather than considering them separately. For example, an empirical regularity that we believe to be true (and that therefore might serve as a cornerstone for future theory building) may be contingent on aggregating all policy types together; that "regularity" changes or even disappears once we disaggregate policies by issue type. A concrete example can be found in the literature on congressional polarization, which is built around two empirical findings that are now treated as facts: that polarization has been increasing since the 1970s, and that polarization has followed a U-shaped form across the last 130 years of American history (McCarty, Poole, and Rosenthal 2006). These two empirical findings have driven theoretical and empirical research on polarization, but as I show later in this book, they do not hold across policy issue areas. In reality, the polarization story is a much more complex one after policy issue substance is introduced into the equation.

There are consequences for getting it only partially right, or even wrong. In the case of the polarization literature, introducing a more accurate picture of how polarization behaves across policy issue areas could reshape the theoretical literature on the topic. I show here that domestic politics is almost always highly polarized, but that the pattern changes in the realm of international relations and sovereignty policy. These divergent findings call for a rethinking of polarization. This point is not, of course, confined to the study of elite polarization.² Regardless of whether our efforts are aimed at testing theories or empirically attempting to understand patterns in policymaking, not taking policy issue substance into consideration repeatedly leads us into making incorrect inferences and statements about lawmaking. Simply put, omitting policy substance makes us get it wrong, and getting it wrong is extremely costly for many reasons, given what is at stake. Fifteen years ago, Charles Jones (1995, 1) made this point about the costs of getting lawmaking wrong in his presidential address to the American Political Science Association:

Lawmaking is the core decision-making process of a democratic state. It is the means for defining, promoting, and regulating community life and, accordingly, is spectacularly interesting and highly relevant to our purposes as political scientists.

² "Elite polarization" is the term for professional politicians differentiating or separating themselves along party lines.

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Thus, a critical endeavor for political scientists is to arrive at clear and accurate understandings, characterizations, and explanations of the lawmaking process. The lessons of Jones's address are no less relevant today than when he delivered it. I argue throughout this book that we will never get lawmaking right without seriously bringing policy issue substance into our study of it.

The second cost of omitting policy issue substance as a topic of study is less obvious and also more difficult to prove. I argue that this omission has sequestered congressional studies from other subfields—particularly American political development (APD) and to a lesser extent policy studies—primarily because both are interested directly in what Congress scholars for the most part no longer study: issue-specific policy outputs.³ I also suggest that this sequestering of the Congress subfield is an important reason why it has lost its place as the most theoretically and empirically interesting area of political science. This is not to say that the Congress subfield is not still producing interesting research. Instead, I argue that policy issue substance is the key for the subfield to make another significant advancement in our understanding of how Congress behaves as well as how lawmaking works. Policy issue substance is the key to new progress.

Although there are certainly many reasons why the Congress subfield is not directly engaged with the APD subfield and policy studies, and vice versa, the omission of policy issue substance is probably the most important reason (see Katznelson and Lapinski 2006a). The direct cost of not having Congress scholars engaged with APD and policy studies is, of course, impossible to fully determine. How do we determine what we would know about policymaking if there was synergy between these subfields? Fortunately, this question is one that can be partially set aside: it is axiomatic that having three diverse and talented groups of scholars attempting to better understand policymaking would lead to a much better overall understanding. This question must not, however, be fully avoided. The next section considers not only the contribution of APD and policy studies to congressional studies, but also the potential revitalization of these two subfields by restoring to them the study of policy issue substance in lawmaking.

Before discussing the path by which we might bring policy substance back into the study of Congress and lawmaking—with the goal of providing better inferences and predictions about lawmaking while building synergy between Congress, APD, and policy scholars—I take a closer look at why policy substance is no longer a part of congressional studies, what has been lost as a result, and why we need to bring it back.

³Why have Congress scholars avoided the study of lawmaking, particularly issue-specific lawmaking? Jones (1995, 2) surmises that "the study of laws is bound to be issue-specific and will be marked by the dreaded label case study, with its limited potential for theory."

WHY WE NEED TO RESTORE POLICY ISSUE SUBSTANCE TO CONGRESSIONAL STUDIES

There is, of course, no single explanation for why policy substance is no longer an important consideration in our study of Congress. One explanation points to the rising prominence of deductive theory within congressional studies: behavior work on policy substance dwindled as scholars began to think of it as limited to descriptive objectives (Katznelson and Lapinski 2006a). In a related development, the powerful work of Keith Poole and Howard Rosenthal (1985; 1991; 1997) has demonstrated empirically that roll call voting in the U.S. Congress appears to be largely unidimensional. An even more important finding of Poole and Rosenthal's for the disappearance of policy substance from the study of Congress is that members' preferences measured through roll call voting do not vary much issue by issue. Although Poole and Rosenthal's work on this specific topic is limited, the mountain of empirical findings they have produced on the low dimensionality of the political issue space has encouraged most Congress scholars to conflate low dimensionality with the idea that policy issue substance is inconsequential. Why is it so unfortunate that their forceful results have provided a mistaken empirical foundation to the argument that studying policy issue substance is not necessary to understand the behavior of members of Congress or policymaking in the United States? Because low dimensionality does not imply that members' preferences across issue areas are more or less the same. I insist throughout the book that these two issues, low dimensionality and policy issue substance, should be separated.

It is somewhat paradoxical to argue that the omission of policy substance from our congressional studies has been costly when that omission coincides with a remarkably fertile period in the history of such studies. From roughly the mid-1980s until a pinnacle in the early 2000s, congressional studies produced important and rigorous findings that have fundamentally changed our understanding of Congress. The bulk of this period's work focused on why institutions, rules, and procedures are formed and how they influence collective choice. The theory building in congressional studies during this period was supplemented by careful empirical evaluations, and the majority of the hypotheses derived or constructed made empirical predictions that related to political behavior, not to policy outputs.⁴

The study of Congress through roll call votes has a long and rich history in political science. There is certainly nothing wrong with studying the political behavior of members of Congress, and there is no question that roll call voting records provide an excellent way to do so. However, with so many scholars engaged in this institutionally based work on Congress, the direct study of

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 $^{^4}$ There are some notable exceptions, especially in historically oriented work (see Brady 1988).

policy outputs has been left to others. Moreover, the study of political behavior with no counterbalancing focus on outputs may have been unhealthy for both the study of lawmaking and the subfield of Congress studies.

This began to change after the publication of David Mayhew's *Divided We Govern* (1991). Mayhew demonstrated the importance of moving beyond an orientation toward rules, institutions, and procedures in studying the behavior of members of Congress and focusing instead on explaining what government does. His seminal work brought the study of policy outputs back into congressional studies, spurring the creation of a small cottage industry that assesses the legislative performance of Congress.⁵ This work has collectively increased our understanding of the determinants of lawmaking (see Adler and Lapinski 2006; Binder 1999, 2003; Coleman 1999; McCarty, Poole, and Rosenthal 2006), although it has remained silent on the question of whether policy substance is an important causal factor in lawmaking.

Even though we know more today about the determinants of lawmaking than ever before, much remains unknown. This limited understanding has been attributed by some to the near-invisibility of policy issue substance in our research (Katznelson and Lapinski 2006b; Lapinski 2008; Rohde 1991).⁶ Evidence that the omission of policy substance is hindering rather than helping our understanding of lawmaking can be found in some fine recent empirical assessments of theoretical models of lawmaking in the United States.

The two most prominent models of this type are the "pivotal politics" model of lawmaking (Brady and Volden 1998; Krehbiel 1998) and the "party cartel" model (Cox and McCubbins 2005). The pivotal politics model of lawmaking predicts that policy change occurs when status-quo policies are extreme relative to the preferences of members of Congress. More specifically, a gridlock interval can be constructed by examining key "pivots" in the separation-of-powers system. These pivots are defined by the filibuster in the Senate (the cloture pivot) and the presidential veto. Policy change occurs

⁵There is in fact a body of early work that considered the importance of policy outputs. See, for example, Chamberlain (1946). See also the issues of the American Political Science Review (vol. 14, no. 1, to vol. 41, no. 4, 1919–1949) and Political Science Quarterly (vol. 17, no. 4, to vol. 26, no. 04, 1887–1925) summarize legislative activity in Congress.

⁶David Rohde (1991, 357) wrote a little over a decade ago that "the challenge for students of congressional politics is... not to prove that one theoretical view is superior in all situations but instead to specify the conditions under which each view applies to behavior and outcomes of interest."

⁷The "conditional party government" model (Aldrich and Rohde 2000; Aldrich 1995) is another alternative account of lawmaking. This prominent model differs from the party cartel model by critically linking the ability of the majority party to pass policy with the homogeneity of the preferences of the majority party. A homogeneous party is one that can agree on policy change, and thus one that is more likely to be productive in passing legislation.

⁸How the interval is constructed depends on the preferences of the president and members of Congress. See Krehbiel (1998).

precisely when the status quo of an existing policy is extreme relative to the gridlock interval. Like the pivotal politics model, the party cartel model of lawmaking also constructs a gridlock interval, but it differs in that the gridlock interval depends on the preferences of the median members of the majority party and the chamber median. In both models, policy change is determined by the location of status-quo policies relative to the preferences of critical members of Congress.

As mentioned, empirical assessments of these formal models of lawmaking have been less than stellar. For example, in a well-executed test of the pivotal politics, party cartel, and median voter models of lawmaking, Keith Krehbiel, Adam Meirowitz, and Jonathan Woon 2005 summarize their overall findings as "painfully inconclusive." They propose two possible explanations for their lackluster empirical results: either the theoretical models were wrong or the empirical tests were flawed. A lack of attention to policy substance may play a large role in either explanation.

The first possible explanation for why theoretical models of lawmaking do not receive strong empirical affirmation may be rooted in policy content. This would be the case if the mechanisms underlying policymaking vary by policy issue substance (Lapinski 2008; Rohde 1991). The idea here is that different processes explain lawmaking across policy issue domains. The task therefore is to identify how mechanisms vary according to policy substance. To understand how these mechanisms work, we need to get a firm grasp on the empirical regularities of lawmaking across policy issues and time periods. This understanding, in turn, will allow us to determine the strengths and weaknesses of our theoretical models.

The second explanation proposed by Krehbiel and his colleagues (2005) for the poor fit between theory and data, the issue of measurement, is also linked to policy substance. Scholars believe that inconclusive empirical results stem from improperly measuring the induced preferences of members of Congress that are needed to assess existing models. In other words, problematic empirical results are not a result of poor models, but of the poor measures used to test the models. This is the explanation favored by Krehbiel and his colleagues

⁹See Cox and McCubbins (2005) for details.

¹⁰According to Krehbiel and his colleagues (2005, 17), "a series of indirect test results were painfully inconclusive.... While this analysis suggests somewhat more convincingly that gridlock-based theories provide marginal value over median voter theory, their marginal value over the random-normal null model is questionable."

¹¹This idea is not new, and in fact it was first seriously introduced by Theodore Lowi (1964, 688) over forty years ago when he argued that we need to better understand how "a political relationship is determined by the type of policy at stake, so that for every policy there is likely to be a distinctive type of political relationship." Lowi's policy classification system, though suggestive, proved too abstract and difficult to operationalize and thus was never widely adopted within congressional studies.

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for the lackluster results of their study. They believe that the empirical results for their pivotal politics model of lawmaking would improve if the induced preferences of members of Congress were measured by policy issue area. Their belief, in short, is that induced policy preferences vary by policy issue domain. Not accounting for variation by policy substance leads to measurement error and therefore attenuated (weak) results. 12

This book contends that policy issue substance is at the center of the poor results found by Krehbiel and his colleagues. Assessing their idea in more detail, we can examine how the assumptions underlying these two models of lawmaking might interact with policy issue substance. Empirical evaluations of the pivotal politics and party cartel models make specific implicit assumptions to produce testable predictions for legislative accomplishment across time. Many of the nonformal theories of lawmaking discussed earlier also make these assumptions, which have to do with the desire and opportunity to pass legislation.

The first assumption is that all legislative regimes are equally interested in legislating. At least one scholar has made the case that not all legislative regimes are alike. John Coleman (1999) argues that Democrats are more likely than Republicans to enact new legislation when they control government. This type of assumption is not fully explored in most preference-based models of lawmaking. Instead, most scholars assume that lawmakers would like to enact or move policies closer to their ideal points regardless of their ideological positioning.¹³

The second assumption about the opportunity to pass legislation is particularly critical for models of lawmaking that construct gridlock intervals. Empirical assessments of such models require that gridlock intervals, which have been constructed from electoral and roll call data, be highly correlated with the number of movable status quos. The usual assumption is that the distribution of movable status-quo points is uniform across the policy space. A uniform distribution is highly desirable because it ensures that change in

¹²In speculating that their lackluster findings are related to the absence of policy substance from their empirical analyses, Krehbiel and his colleagues (2005) specifically suspect the lack of disaggregating roll calls by policy issue area as the culprit in improper measurement of the induced preferences of members of Congress for their theoretical testing. They write: "Unidimensional theories assume only that preferences exist and are well behaved on a single dimension in any given choice situation. They do not assume, as does the implementation of tests of the sort we conducted, that well-defined preferences of Senators maintain the same orderings and locations on the same primary dimension across all roll calls" (17, emphasis in original). In other words, their intuition is that the ordering of senators varies by policy issue substance.

¹³Most current work on lawmaking does not consider problematic the assumption that all legislatures (or parties) want to pass legislation. Robert Erikson, Michael Mackuen, and James Stimson (2002) show, however, that liberal-tilting major enactments outnumber conservative enactments nine to one (for a discussion of this point, see Mayhew 2006).

predicted legislative accomplishment is not dependent on the location of the pivots being changed. The intuitive idea behind this assumption is that an equal change in the gridlock interval width should have the same effect on lawmaking, regardless of whether the change occurs in a moderate section of the issue space or in a more extreme portion. The problem with this assumption is that the distribution of movable status quos is unlikely to be uniform (or even normal) but is in fact probably quite lumpy across time, particularly across different policy issue areas.

Mayhew's (2005a; 2005b) most recent work fleshes out much of the intuition about why policy substance and the distribution of movable status quos are likely to be related. He argues that events such as wars, economic crises, and assassinations are important factors that drive policy change. Why do such events matter for lawmaking? In his work, Mayhew leverages the research of others, primarily John Kingdon's (1984) writings on "policy windows," which suggest that crises alter the demand and supply of public policies. Another way of expressing this is that crises often delegitimize existing government policies that are directly or, indirectly linked to the specific event. Consequently, a crisis can immediately shift the distribution of movable status-quo policies for linked policies. This type of shift can and usually does happen quickly. The most recent and immediate example is the post–September 11, 2001, 107th Congress. ¹⁴

Mayhew argues that there are long- and short-term effects of crises and other major events on policymaking. In his article on war, he constructs lists of legislation that would not have come into existence if specific wars in American history had not been fought. Crises and war therefore can rapidly change the status-quo locations of policies linked to such events. Mayhew's (2005b, 36) own words best explain this intuition.

To use the terms presented by Keith Krehbiel in *Pivotal Politics*, it is not just elections that are capable of moving status quo policy outside the Capitol Hill "gridlock interval." Events, too, can shake up a preference distribution among the realm of elected officeholders to the point where presidential vetoes, Senate filibusters and the rest cease to be a bar to action in some direction. Let me nail this down with an instance. On December 7, 1941, Pearl Harbor was attacked. On December 8, 1941, Congress and the president opted to abandon the American status quo of not waging war against Japan, and war was declared.

The status-quo policies that are affected by a crisis are not random. Instead, they are linked by policy substance to the event itself. For example, in the case of war, international affairs issues, including defense policies that are

 $^{^{14}}$ Mayhew (2006, 248) makes this point by listing sixteen important enactments for the 107th Congress, nine of which were a part of the government's response to the 9/11 attack.

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classified as international affairs, as well as war-related domestic policies have movable status quos that were not likely to have been movable immediately prior to the event unless a major change in the gridlock interval resulted because of electoral replacement. An event such as war will not necessarily change the perception and location of status-quo policies across all policy types. Thus, policy substance enters the equation. Crises can change the distribution of movable status-quo points for certain types or groups of policy.¹⁵

It has thus been demonstrated that it is quite possible, if not likely, that crises and other significant events by themselves can shift the distributions of movable status quos for different policy issues. Some policies might be quite immune to change, however, because they are not linked to the event. The problem with pooled analysis of lawmaking is that the analysis might not pick up such effects, depending on which and how many policies are affected by such shocks. This example simply calls attention to the fact that different mechanisms might drive lawmaking within substantive issue areas. Empirically, chapter 6 explores in more detail the ideas put forward here about the determinants of legislative performance.

It is also possible to argue that policy issue substance matters for measuring the political preferences of members of Congress. This is a belief that informed the earlier Congress literature on political behavior. For example, if instead of moving distributions of status quos, events actually change the preferences of members, policy substance is equally important because the induced preferences of members are likely to be changed for some policies but not others. There are other good theoretical reasons to expect differences by issue area, depending on the salience of each issue (see, for example, Arnold 1990).

Let's consider a specific example of how members of Congress might have distinct preferences across issue areas. One of the issue categories introduced later in the coding schema employed throughout this book is sovereignty policy, which is concerned with the boundaries of a state's internal authority and the content of the citizenship regime linking the state and a diverse population. With only a few exceptions, scholars in American

Unlike election-induced preference shocks, status quo shocks may occur at any time, including within periods that are demarcated by elections. A cause might be new information that suddenly becomes available and that alters the lawmakers' perceptions about attributes associated with old policies....Only rarely is it possible to predict or observe precisely when such changes occur.

Cox and McCubbins (2005) argue that the location of status-quo points is a function of policy in the previous time period and of exogenous shocks.

¹⁵Krehbiel (1998) and Cox and McCubbins (2005) discuss the possible influence of exogenous shocks on the location of status-quo policies. In his chapter on coalition size, Krehbiel (1998, 78) writes:

politics have had little explicit interest in sovereignty policy. In particular, the policy has had no analytical traction for Congress scholars, most of whom would lump issues of sovereignty into the category of domestic politics. There is, however, an emerging literature in American constitutional development that has paid closer attention to the differences between sovereignty policy and other policy issue areas. This literature, relatively unknown to Congress scholars, provides important motivation for students of Congress interested in member behavior or lawmaking to consider sovereignty issues separately. It also provides evidence that members of Congress may have very distinct preferences on sovereignty issues compared to other policy categories.

The American constitutional development literature argues that sovereignty policy is unique in the American context because, beginning in the late nineteenth century and continuing today, the Supreme Court has decided that "Congress [has] plenary power to construct the American state and its membership largely immune from judicial review" in cases concerning federal authority over immigrants, Indian nations, and territorial governance—all key aspects of sovereignty policy (Cleveland 2002, 11; Aleinikoff 2002). In deciding that Congress has plenary power to regulate the entry of aliens, the status of the Indian nations, the acquisition of territory, and the admission of states into the Union, "the courts would not subject congressional choices to any limitations on federal power located elsewhere in the constitution (such as the First Amendment or the prohibition against retroactive legislation)" (Aleinikoff 2002, 16). Given the central role of Congress in the formulation of sovereignty policy, the relative lack of attention to it by congressional scholars is striking. 16

The dominance of Congress in these issue areas is likely to have changed the dynamics of lawmaking in complicated ways. For one, the stakes of lawmaking have probably been higher, given the foreclosure of alternative avenues for modifying or overturning legislation and the difficulty of substantially modifying existing laws in Congress. At the extreme, statehood is effectively locked in after Congress has admitted a territory into the Union. Immigration law, Indian policy, and territorial governance are unlikely to be overturned by the Court, and although the Court continues to review administrative actions in these areas, it has not significantly challenged congressional plenary power. This may have had important effects on legislator behavior, especially if encouraging the judicialization of an issue has been an

¹⁶The judiciary is much more active in the realm of civil rights and liberties. Nevertheless, in some areas of civil rights, and in voting rights in particular, the Court has tended during the last several decades to support Congress's assertion of extraordinary power.

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important strategy for political leaders to avoid fracturing their political coalitions (Graber 1993).

More broadly, the content of sovereignty policy suggests that it is the site of politics that do not map onto the dominant ideological cleavage. Sovereignty policy is more likely to be concerned with the "background conditions" of national life—who are the people, and over what territory does the state govern—than with proximate questions of economic redistribution and regulation. The prospects of changed citizenship and territory are likely to alter the preferences and relative positions of political actors in complex ways. If nothing else, changes to territory and citizenship could heighten tensions among existing political coalitions. New territories, bringing in new issues and voters or an expanded or contracted suffrage, might alter the scope of conflict and create new cleavages that destabilize existing coalitions. As noted by Daniel Tichenor (2002, 8) in regard to the politics of U.S. immigration policy—a key area of sovereignty—the political dynamics "have long been influenced by the making and remaking of distinctive political coalitions on this issue that cut across familiar partisan and ideological lines." Even where the boundaries of membership are not being expanded or restricted, sovereignty policy is often concerned with strongly held beliefs about civic identities. Infringements on free speech, the place of religion in American society, and the civic status and equality of citizens are debates that engage beliefs about what it means to be a citizen of a state, and these beliefs are not likely to correspond neatly with the party structure. They may correspond to sectional, religious, and regional identities, or they may be much more idiosyncratic preferences.

For all of these reasons, members of Congress are likely to hold preferences in sovereignty policy that differ from their preferences in many other issue areas. Of course, this literature does not give us strong expectations about the impact of specific member behavior on sovereignty policy. In fact, it does very much the opposite. How these conflicts and altered dynamics play out is historically contingent and depends on how a specific policy interacts with existing coalitional arrangements and member preferences. Nor should we homogenize sovereignty policy or imply that its different dimensions function in the same way. What is likely, however, is that preferences on sovereignty policy will not map onto the dominant issue cleavages of a given period, and so there is good reason to attempt to measure the preferences of sovereignty policy separately for members. A good example of what Krehbiel, Meirowitz, and Woon (2005) were talking about, sovereignty policy shows us why we should take policy issue substance into account when measuring the preferences of members of Congress. The full costs of aggregating roll call votes to study lawmaking are explored systematically in chapters 3 and 4. Here it is sufficient to point out that this is a potentially large problem to which very little attention has been given.

A NATURAL CONNECTION: CONGRESSIONAL STUDIES, AMERICAN POLITICAL DEVELOPMENT, AND POLICY STUDIES

Bringing policy issue substance back to the study of Congress would certainly improve our overall understanding of lawmaking, but how might the study of Congress and lawmaking benefit scholars of American political development? And what specifically might APD scholars bring to the study of congressional policymaking? Before turning to these questions, we should note that APD, though still young as a subfield, has many accomplishments to its credit already, but very few are related to the study of Congress. Congress studies, in fact, have never been integrated into APD (Katznelson and Lapinski 2006a; Lapinski 2000; Whittington 1999), whose focus has been elsewhere, including the pursuit of more and better work on American political history than has been achieved by much of the history profession (especially during the long period, now coming to a close, when political studies were avoided by younger political historians). APD scholars have also contributed a great deal to the growing interest in historical evidence and dynamics on the part of their political science colleagues who are otherwise inclined to deductive modeling and large-N studies. Notwithstanding these achievements, APD currently faces a central problem: the subfield's distinct purposes have become less clearly defined (Katznelson and Lapinski 2006a).

The APD subfield is at a crossroads partially because of its own success. APD effectively advanced analytical political history when historians of the United States were turning away from political subjects and political scientists were seeking to identify behavioral regularities or distinguish portable models of strategic action without much regard for the singular traits of specific times and places. Perhaps unfortunately, the APD subfield's monopoly on interesting political history has ended. Many historians, especially talented younger scholars, have taken a decided turn back to politics and the state, while a growing (though still small) number of Americanists in political science have learned the lesson of APD that history is integral to good causal scholarship. Paradoxically, by encouraging the return of historians to political history and prompting an attention to historical questions on the part of other students of American politics, the APD subfield now is under pressure to better explain how its own qualities validate its continued contributions at the intersection of history and political science.

While there is, of course, more than one promising direction that might be taken to move the APD subfield forward and bring its special qualities to bear on a wider arc of issues and institutions, this book advances a path that advocates bringing Congress to the center of APD, although this effort cannot succeed without some decisive moves. The best way forward for a serious engagement between Congress scholars and APD scholars should be one based

on a robust and systematic approach to studying the policy issue content of lawmaking. In other words, policy substance can be used to make Congress a constitutive feature of the analysis of the most important APD questions. In order for this to work, the study of lawmaking must draw on policy issue content that makes sense to APD scholars. The lack of a theoretically grounded and empirically useful policy issue classification system has been a major stumbling block for integrating Congress and lawmaking systematically into the study of APD. This book introduces the tools necessary to integrate APD into this work on its own terms.

APD, in turn, could help Congress scholars by continuing to bring important and needed historical perspective to the study of Congress and by helping Congress scholars make conscious connections between their work and the larger overarching themes that are important to the American regime. Specifically, APD scholars have primarily worked in four genres: exploring critical periods; steering critical subjects through key moments in, or even the whole of, American political history; tracing the development of key institutions, in both the medium and long terms; and exploring political speech and ideas. Within these genres, APD scholars have developed a few substantive themes, including liberalism, state building, temporalism, and policy feedback (see Katznelson and Lapinski 2006a). Congress scholars rarely make the connection between these APD themes and ideas, although clear linkages exist. For example, representation, a concept of perennial importance to Congress scholars, has a clear relationship to liberalism, but that connection is ignored within almost all Congress scholarship.

In summary, the themes that define APD as a distinct subfield are rarely, if ever, engaged by Congress scholars, to the detriment of legislative studies. Specifically, Congress scholars, in giving no attention to these themes, find themselves unable to join the conversation about central questions concerning the American regime and unable to build specific historical knowledge about timing and context into their models of policymaking. It is one purpose of this book to show that ideas distinctive to APD studies can be used to gain a better understanding of Congress and policymaking.

Although APD would benefit and assist congressional studies by turning attention to the study of policy substance and lawmaking, it is also true that policy studies reconnected to congressional studies would be similarly beneficial and contribute to our understanding of lawmaking. It is paradoxical that a divide exists between the two subfields at all, since at one time policy studies and political science worked in close tandem (and in fact were essentially indistinguishable). In earlier times, influential crossover work focused on "the mechanics of agenda change, the likelihood of nonincremental policy change, and how the policy-making process varies across issue areas" (Gormley 2007, 297). The divide that now exists between policy studies and political science is manifested in several ways. Policy studies scholars publish in their own

journals, and Congress scholars rarely read or interact with this work.¹⁷ Policy schools, including those that focus on public administration and policy more generally, have concentrated their faculty hiring in the field of economics, and some prefer "practitioners in the field." In short, the alliance between policy studies and political science today is clearly weak, and only a few notable scholars work between the two fields.¹⁸

Much might be gained in policy studies and political science if the two fields were to rejoin forces. For example, policy studies scholars, particularly economists, are very interested in exploring the consequences of public policy choices, while political scientists have spent very little energy on this topic (Gormley 2007, 298). This lack of attention is unfortunate considering that past policy choices clearly have a huge impact on the policymaking process. Because past policy choices are specific to particular policy domains, policy substance, again, might serve as a bridge between policy studies and political science and help both disciplines gain a better understanding of how policymaking works.

THE ORGANIZATION OF THE BOOK

The purpose of this book is to reintroduce a substance-oriented research program based on policy issues for studying Congress from multiple vantage points. In doing so, my aim is to make serious progress on systematically understanding Lowi's provocative claim that "policy determines politics," which, while important, has never been satisfactorily understood, either empirically or theoretically. The bulk of this book tackles this question empirically, though there is, of course, plenty of theoretical discussion on how to best construct a proper categorization schema as well as how to construct appropriate measures of political preferences and legislative productivity.

In advancing a substance-oriented approach to studying policymaking and lawmaking in Congress, this book introduces a kit bag of important new tools and ideas to use in determining how policy issue substance matters for lawmaking, including: new data, such as an immense data set on U.S. lawmaking between 1877 to 1994; new and massive measures of political

 $^{^{17}}$ Whether policy studies scholars keep up with work within the Congress subfield is unclear (see Gormley 2007).

¹⁸The most visible scholars working within both fields are Frank Baumgartner and Bryan Jones (1993, 2004), whose work clearly falls within the policy studies tradition and who have produced the most powerful analytical framework and empirical work on the subject. Nevertheless, their work, which focuses primarily on how issues become a part of the political agenda, nearly stands alone as important work that informs both political scientists and policy studies scholars.

¹⁹Keith Krehbiel's (1991) informational theory is a significant exception. An important assumption of informational theory is that politicians are uncertain about policy outcomes.

preferences broken down by policy issue areas for U.S. lawmakers spanning the period 1877 to 2010; and fresh approaches to analyzing these new data sets. The book is predicated on the idea that new and improved data are needed to make additional progress in understanding lawmaking.

The book is organized into seven chapters. Chapter 2, "Bringing Policy Issue Substance Back In," has two purposes. The chapter's title draws a parallel to the famous edited volume Bringing the State Back In (Evans, Rueschemeyer, and Skocpol 1985), which many scholars believe spurred the creation of American political development as a distinct subfield of political science. Similarly, policy issue substance could play a revitalizing role for multiple subfields in American politics, particularly congressional studies and American political development. The second chapter builds on some of the ideas that have been briefly introduced here in chapter 1, namely, why policy issue substance disappeared from the study of Congress and what is needed to revive this approach. I begin chapter 2 by arguing that three factors combined to lessen interest in the study of policy substance: the rise of deductive theory within congressional studies, which led scholars to believe that this earlier behaviorbased work was limited to descriptive objectives; the findings of Poole and Rosenthal (1985, 1991, 1997), who demonstrated empirically that roll call voting in the U.S. Congress appears to be largely unidimensional and not policy-specific; and policy classification schemas that were not theoretically grounded and suffered from being period-bound.

The second and primary purpose of chapter 2, however, is to introduce and explain a new coding schema to parse policy. This coding schema is without question the most important part of the book. By returning to and advancing the key intuition that the content of policymaking matters, I seek to overcome the specification and measurement problems that have plagued past researchers. Picking up where others have stopped, I identify and clear the most important theoretical and empirical roadblock that has stood in the way of the substantive policy research program: the absence of a sufficiently compelling, analytically directed, and theoretically supported method for coding the content of congressional roll calls and public laws and, in consequence, the lack of a data set across American history that records both the behavior of members and the legislative output they generate organized by such a classification approach. The schema introduced here differs considerably from past policy classification systems in that its categories are heavily motivated by theoretical work within the American political development subfield. I explain the schema in detail, including its theoretical justifications, and include a brief descriptive analysis of the roll call and lawmaking data used throughout the later chapters.

Chapter 3, "Political Polarization and Issues: A New Perspective," details the ways in which policy issue substance matters for studying political preferences and is the first of four empirical chapters in the book. Fully exploring how

policy issue substance matters for studying political polarization in Congress, the chapter begins by introducing a new large data set that comprises the estimated induced preferences of members of the House of Representatives and U.S. senators by policy issue area over a very long time horizon—1877 to 2010. Overall, the data set includes 28,196 ideal point estimates for House members as well as 6,652 estimates of political preferences for senators.

The second section of the chapter explores the literature on elite polarization in Congress by policy issue area and studies polarization across a 124-year period (1877 to 2010) by the policy issue areas defined as "tier 1" (the most basic four types of state policy adjudicated by modern democratic legislatures). Here I demonstrate empirically that issue content is extremely important for understanding political polarization and that many of the empirical "facts" about polarization depend on not disaggregating policy by issue areas. The research presented suggests that we have much to learn about how and why polarization varies so much by issue content.

Chapter 4, "The Case Studies: Policy Issue Substance and the Political Behavior of Members of Congress," details the ways in which policy issue substance matters for studying political preferences at the micro level. The first section of chapter 4 reconsiders Poole and Rosenthal's analysis of the 95th Congress (1977–78). In their analysis of the 95th Congress, which happened during President Jimmy Carter's first term, they ask: do different issues give different scales? They find evidence they interpret to mean that issue scales do not vary. I present evidence to suggest that this is not correct: it is possible to agree wholeheartedly with the low-dimensionality findings of Poole and Rosenthal, but at the same time, as I show, members of Congress have distinct preferences across policy issue areas. Measuring the preferences of members of Congress correctly is absolutely vital for empirical testing of theories and hypotheses as well as for inductive-based work on lawmaking. I argue that this is not possible to do without including policy issue substance in the picture.

The last section of chapter 4 presents five case studies of lawmaking, from five different Congresses. I selected these cases, which focus on issues of notable lawmaking within the tier 1 issue category of sovereignty, to maximize temporal diversity: each is from a different historical period in American political development. The case studies show in a more fine-grained manner the impact of policy preferences across time and issues.

In chapter 5, "Legislative Accomplishment and Policy Issue Substance," I introduce a new measure of legislative accomplishment. At the center of this book lies an ambitious empirical effort to better understand how policy substance is important for lawmaking. To understand lawmaking requires that we move beyond studying political behavior in Congress alone and beyond a complete empirical reliance on roll call votes. Roll calls are invaluable for studying the behavior of members and certain components of lawmaking. But we also need appropriate direct measures of legislative outputs.

Legislative behavior and legislative outputs must be studied in tandem to gain a proper understanding of the lawmaking process in the United States. Unfortunately, the lack of measures that capture legislative accomplishment, especially across a long time horizon (including the period prior to World War II), is perhaps the primary reason why we know less than we should about lawmaking in the United States (Brady and Cooper 1981). In chapter 5, I explain the value of studying more important or notable legislation (Krehbiel 1998; Cameron 2000; Clinton and Lapinski 2006). Although the idea of studying important lawmaking across time is not controversial, constructing an appropriate measure is not a trivial exercise. This chapter conceptualizes and constructs a comprehensive lawmaking data set that provides measures of legislative accomplishment at the aggregate level as well as by specific policy issue areas for a 118-year period. This data set will ultimately facilitate the study of lawmaking by giving us the data necessary to significantly improve our empirical understanding of lawmaking and policymaking in the United States.

The first section of chapter 5 describes the type of data we need to better test current theories of congressional lawmaking and provide an empirical spine to some important questions that are central to APD. I present the case for using direct measures of legislative accomplishment based on actual lawmaking data rather than roll call—based measures, which are indirect measures of lawmaking, even if they are the measures most commonly used by Congress scholars.

The second section of chapter 5 provides a detailed account of the conceptualization and estimation of an individual law–level estimate of legislative significance or notability for each of the 37,766 public statutes enacted between 1877 and 1994 (see Clinton and Lapinski 2006). I provide empirical evidence demonstrating that the data have strong face validity.

The third section provides the hinge between the individual significance estimates and the construction of a new measure of legislative accomplishment. Here I explain the construction of Congress-by-Congress measures of legislative accomplishment, including measures broken down by the policycoding schema.

Chapter 6, "Explaining Lawmaking in the United States, 1877–1994," turns to lawmaking and shows that legislative productivity varies considerably by policy issue area. Specifically, the chapter shows that the key determinants of legislative productivity differ by policy substance, and it provides empirical evidence that questions the benefits of pooling legislation when such aggregation often obscures empirical findings related to understanding the mechanisms of lawmaking.

Through simple correlation analysis as well as multivariate analysis, chapter 6 explores empirically how the measures of legislative performance constructed in chapter 5 behave. The idea is to show variations in policy

outputs by issue area and explore the determinants that explain overall legislative performance versus performance in particular policy areas.

The empirical core of this chapter—the multivariate analysis—aims to determine whether pooling policies (using an overall aggregate measure of all legislation) is potentially inappropriate. In other words, when the determinants of lawmaking are analyzed, are important relationships masked or dampened when policy types are pooled instead of disaggregated? This multivariate analysis draws on data using two different thresholds of significance. The highest threshold uses the top 500 enactments—a measure of which is nearly equivalent to the list of landmark enactments produced by David Mayhew in *Divided We Govern* (1991), except that the measure used here covers an additional thirty-five Congresses (over seventy years). The second threshold uses the top 3,500 enactments. This threshold captures landmarks for very important legislation. The multivariate analysis does not include every possible explanation and related covariate in the model because, with fifty-nine Congresses (the unit of measure) in the 118-year period, doing so would lead quickly to a saturated specification and no degree of freedom.

Instead, by presenting a parsimonious specification that includes the most common and important covariates found in the literature, I am able to show repeatedly through this analysis how policy substance matters by comparing the results from the pooled and nonpooled dependent variables. This chapter also draws on the issue-specific measures of polarization introduced in chapter 3 to show how we can better understand legislative productivity if we correctly measure elite political polarization.

In conclusion, chapter 7, "At the Crossroads: Policy Issue Substance, Congress, and American Political Development," builds on the analysis in the previous chapters. I make a final case that policy issue substance is critical for understanding contemporary and historical lawmaking. The conclusion also returns to the importance of studying policy content to any substantial progress we may hope to achieve in congressional studies and American political development.