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Edited by E. Scott Adler and John S. Lapinski: The Macropolitics of Congress

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Introduction

Defining the Macropolitics of Congress

John S. Lapinski and E. Scott Adler

In June 2001, the Republican Party was surprised to learn that Senator James Jeffords (Vt.) was leaving the party, resulting in a swing of control of the Senate to the Democrats and transforming a unified Republican government into one with divided control of Congress. This was the first time that such a switch occurred *during* a congressional session and offered scholars the rare opportunity to study its effect in legislative midstream. The event consumed the national media for days and provided political scientists with an exceptional opportunity to test an important and perplexing theoretical question—what effect does the switch between unified and divided government have on policy outputs?

Fortuitously, the Jeffords switch happened simultaneously with a conference on macro-level research on Congress that was considering, among other issues, the long-range effects of unified and divided government.¹ As most conference participants were students of Congress, our initial collective inclination might have been to think of this event in terms of related micro-oriented work.² Questions that could arise would include, how would the Jeffords switch alter congressional committee portfolios? How would Jeffords's act of dropping Republican affiliation affect his own voting? How would this change impact the overall work and agenda of Senate committees? In short, questions and subsequent analyses would center on the ways this switch might influence micro-level behavior.

The conference, however, was not about the microbehavior of lawmakers—at least not directly. Instead its theme was “the macropolitics of Congress,” focusing specifically on the macro-level *outcomes* produced by Congress, oftentimes in conjunction with the president and the courts. This distinction influenced the questions conferees focused on with regard to the Jeffords switch. Our discussion concentrated on how this event would affect aggregate policy outputs. Would it change the agenda or content of policymaking in the 107th Congress? Specifically, how could the switch of one member be “outcome consequential” in a supermajoritarian institution that technically requires, by its own rules, 60 senators to transform policy ideas into public statutes? These types of questions, among many others, were asked and discussed by conference

attendees. Of course, no one suspected at the time that the tragic events of September 11 would introduce a huge shock to normal politics, removing the partisan divisions that emerged that summer and eviscerating almost any possibility to analyze in a meaningful way this transformation from unified to divided government. The bipartisanship that followed the terrorist attacks was short lived. Extreme partisanship and unified control of government returned with the midterm election of 2002.

Political scientists, sociologists, economists, (some) historians and journalists alike are very much interested in how control of government institutions (e.g., divided government) impacts policymaking. It almost goes without saying that for many political scientists the motivation for studying politics is to understand how governments perform under differing political conditions. This is perhaps why the debate over divided government has had such a long life, as it is considered by many to be perhaps the most important factor in explaining policy gridlock, which is a key indicator of system performance. Understanding the causes (Fiorina 1992; Jacobson 1990) as well as the consequences of divided government (Mayhew 1991; Binder 1999, 2003; Coleman 1999; Edwards, Barrett, and Peake 1997; Howell et al. 2000) is therefore a topic that political scientists have deemed worthy of study.³

If the debate over divided government is placed in a larger context, it leads to consideration of how our political institutions, specifically the separation-of-powers system, operate. This timeless debate about institutional design and the performance of our political system has existed since the Founding. It was of vital concern to early political scientists, gaining prominence in Woodrow Wilson's tour de force, *Congressional Government* (1885). His book painstakingly argued that the institutional arrangements of the U.S. separation of powers were pathologically flawed.⁴ Like Wilson, other early scholars of American political institutions, such as Henry Jones Ford (1898), were profoundly concerned with the macro-level performance of our political system. Interestingly enough, these concerns emerged at a time when the federal government was much less active than today's government. It is ironic that as government growth exploded in the period after World War II (Higgs 1987), this line of study nearly ceased to exist. This silence is particularly noticeable in recent work within the subfield of congressional studies, which collectively has given surprisingly little attention to long-range perspectives on the policymaking process. To the contrary, the vast majority of contemporary congressional research explores individual-level behavior of members of Congress (MCs).

The trend toward studying micro-level behavior at the expense of consideration of system performance began its reversal with the publication of David Mayhew's examination, in *Divided We Govern*, of policy change under unified and divided government over the last half-century. His seminal study led to a surge in research examining policy performance in American politics, particularly the performance of Congress. Despite the debates about the causes and

consequences of divided government generated by Mayhew's study, there is still little synthesis between studies of legislative production and the theories put forward in the macro-oriented political science literature. Consequently, we felt the need to improve the conversation among scholars working on related topics (i.e., those who study macro policymaking but focus on different inputs that drive policymaking such as institutional change and public opinion) that had previously not been connected in any systematic way. This book is an attempt to facilitate that conversation.

MAKING MACROPOLICY—THE ROLE OF CONGRESS

Congress undeniably plays a special role in forging macropolitical outcomes in the United States. In fact, Congress's privileged position in lawmaking is what distinguishes it from other national legislatures (Katznelson and Lapinski 2004). When we think of politics and policymaking in the United States, such as "foreign policymaking" or "health policymaking," we cannot escape the conclusion that Congress plays an integral role in manufacturing relevant outputs. This is not to say that other institutional players in our separation-of-powers system are not important.⁵ Who would argue that George W. Bush's administration has not played a guiding role in setting the policy agenda in post-September 11 America? But we must also remember that President Bush had much less success controlling the policy agenda in the first part of his administration, primarily because Congress did not agree with many of his proposals and had its own agenda.⁶ In fact, he did not sign his first major piece of legislation into law—the \$1.35 trillion tax cut—until well past his first one hundred days in office. Legislation covering education reform, prescription drugs, and campaign finance reform, among others, had to wait until a new-found bipartisanship sprang from the September 11 tragedy. More recently, many Bush administration proposals in 2003 and 2004 outside the realm of security and defense policy languished in Congress with little or no legislative action (Babbington 2004).

Accordingly, many observers see Congress as the institution where the collective choice of the nation is forged into outcomes. Some have argued that studying Congress is key to understanding how policy inputs map into policy outputs. This means, in brief, that Congress can be seen as the critical link to understanding how the factors that shape the way government operates as a whole (its partisan configuration, the working relationship between branches, etc.) result in aggregate policy outputs. In addition, how do changes in those factors affect changes in overall policy? Two central questions motivate our attempt to better explain this relationship. The first is what macro-outputs are. In other words, what are the dependent variables and what makes them "macro?" The second question asks what mechanisms govern the processes that produce

different macro outputs. Thus we attempt to determine the role and the weight accorded to Congress in producing outputs along with other policymaking bodies.

Interestingly enough, the focus of congressional scholars for decades has been on the actions and behavior of lawmakers and the structure of the institution, with little attention paid to what is removed from the forge. This is what we refer to as macro-outputs. We distinctly see the roots of this tradition in the work of Richard Fenno and David Mayhew. Consider chapter 1 of Fenno's second book, *Congressmen in Committees* (1973), which begins, "A member of the House is a congressman first and a committee member second," after which he examines the individual behavior of MCs inside the committee system. Building on earlier work, Fenno's next pathbreaking study, *Home Style: House Members in Their Districts* (1978), expands on the exploration of the behavior of members of Congress by probing representative-constituency linkages. In his hallmark work *Congress: The Electoral Connection* (1974), Mayhew explores the policymaking activities of lawmakers as they relate to their own reelection strategies.⁷ The focus in this work is on understanding the behavior of individual MCs. While understanding individual behavior certainly provides insights into macro-level phenomena, elaborating on those insights is simply not the primary purpose of this earlier work.

While the successes in studying congressional organization and legislative behavior have been many, they are only a part of a larger understanding of our democratic institutions and governance. One way to appreciate the remaining void is to look at congressional research from the point of view of the structure-conduct-performance paradigm that has long been part of the industrial organization subfield in economics. This work provides an interesting example of how one might bridge micro-oriented work with macro-level outputs. Roger Myerson (1995), in his review essay "Analysis of Democratic Institutions: Structure, Conduct, and Performance,"⁸ argues that we must fully identify and understand the organization of democratic political systems and the actions of policy actors so that it is possible to appreciate the linkage of structure and conduct to performance. Industrial organization scholars built microfoundations for their subfield through careful inductive work, using industry-specific qualitative case studies followed by empirical analyses. Later, these scholars turned to the study of macro-level performance focusing primarily on efficiency and profits.

In contrast, the literature on Congress has yet to truly connect micro-oriented work to macro-level phenomena. The recent attention given to studying how members of Congress behave is akin to studying how firms act given a certain set of rules governing market structure. The theories explaining why and how individual members of Congress behave have been a real success story, and this success at the micro level makes it possible to expand our horizons and begin to fully explore the consequences for political outcomes. Furthermore, we believe

that the study of macropolitics can simply be thought of as the next logical step for the field of congressional scholarship. Our micro theories that explain member behavior, combined with micro-oriented understanding of internal structure (e.g., the committee system, and organizational rules), provide us with the building blocks for a macro theory of policy performance. Nonetheless, the long-range implications of our micro insights have not been properly explored. It is at this aggregate level where performance must be measured.

The work found in this volume tries to link the microfoundations that already exist in current literature more directly to macro-oriented policymaking and congressional operations. We do this to increase our understanding of how Congress performs over time and in relation to other political actors. Questions central to this concept of the “macropolitics of Congress” include these: Does Congress as a representative body broadly reflect public desires? What are the aggregate implications of Congress as ombudsman for the way people relate to their government? How does Congress manage the administrative state, including its role in the creation of agencies as well the Senate’s special role in filling such agencies (or leaving them empty) with its advise and consent power over nominations? To properly synthesize what is a diverse body of work, it is first necessary to define the key term—macropolitics. It is to this complicated task that we turn next.

TOWARD A DEFINITION OF MACROPOLITICS

Defining what we mean by macropolitics is necessary to further advance this new genre of work. The long tradition of a micro-oriented focus in congressional scholarship means that our first obstacle is coming up with a definition sufficiently narrow to give the field meaning and boundaries, but not so narrow that large areas of congressional research will easily fall between the micro and macro approaches. This is made all the more difficult by the tremendous heterogeneity in the study of macropolitics, as demonstrated in the chapters that follow. Each tries to explain policymaking; however, different emphases are placed on such influences as public opinion, mood, preferences, elections, divided and unified government, committee chairs, bureaucratic agencies, and political parties. To construct a cohesive yet parsimonious definition of macropolitics, we return to the two questions laid out in the previous section: What are macro-outputs, and what mechanisms govern the processes that produce macro-outputs? As alluded to above, studying macropolitics is about developing and testing theories that explain how collective choice maps into policy outputs across time. To understand this mapping process, we first turn our attention to the policy outputs that serve as the dependent variable for macropolitics. Policy outputs, or “system performance,” comprise aggregate views of legislation, impeachment, and ombudsman activities, advice and consent in

shaping the executive branch of government, or approval of matters concerning foreign affairs. What differentiates these outputs from past work on Congress is that we are looking at “outcomes” and, for the most part, think of these in their aggregate form (e.g., the proportion of a party’s platform adopted into law instead of individual enactments passed from a platform considered in isolation).

For an example, consider Cain, Ferejohn, and Fiorina’s seminal book *The Personal Vote: Constituency Service and Electoral Independence* (1987). They show how constituency service enhances lawmakers’ reelection prospects. Though their focus is on the behavior of individual members of Congress, they speculate about the system-level or macro-level consequences of these activities. An important implication of increased constituency service is that it insulates members’ electoral fortunes from the fate of the party. This leads to the macro implication that parties’ legislative programs might lose cohesiveness and perhaps are less likely to pass. Thinking about these ombudsman activities at the macro level, we might analyze party programs across time and determine whether the success or failure of party platforms and legislative agendas can be explained by changes in constituency service. This illustration demonstrates that a boundary between a micro and macro approach is that the macro perspective looks at the outcomes that are produced out of collective choice. In other words, the emphasis is on the products that flow out of our political institutions.

Creating a list of topics or categories that might be reasonably considered part of the macropolitics genre takes us a step closer to defining what macro-outputs are, but is clearly not sufficient. We need to go further and explore *how* macropolitics is studied. How does analyzing macropolitics differ from micro-level analysis? As should be clear from the preceding paragraph, studying macropolitics makes *outputs* the important dependent variable. This contrasts with studies of policy inputs that use congressional structure or the actions of individual lawmakers as left-hand-side variables. Thus, macropolitics literature is far less interested in discovering the factors that impact such common subjects of research as roll call votes, committee tenure rates, measures of careerism, or the determinants of institutionalism. This is not to say that these factors are not important for output production. They most certainly are. For instance, a macro approach is not necessarily interested in understanding the mechanisms that lead to the disappearance of competitive (marginal) legislative districts. However, those differences in district competitiveness might be quite important from a macropolitics perspective in shifting the location of median preferences in Congress and thereby affecting the complexion of policy.

We certainly are not the first to observe the need for studies of macropolitical performance. Joseph Cooper and David Brady’s underappreciated review essay published over two decades ago in the *American Political Science*

Review (1981) offers perhaps the clearest vision of the utility in focusing attention on congressional organization as an *independent variable*. They argue that this is necessary in order to better understand and pinpoint the impact of institutional changes on the policymaking process. More research, Cooper and Brady contend, is required to evaluate how long-range trends in congressional development and relations with other political actors influence the output side of the political system.⁹ In effect, congressional scholars need to refocus a portion of their attention away from the institutional and behavioral trees that so often occupy us and toward the governing forest.

This leads to a second component critical to defining the boundaries of a macropolitics approach—understanding what is meant by system performance. This concept is central to the viewpoint of congressional structure serving as an independent variable. In effect, the long-term performance of the governing system in general, and Congress in particular, is what we are trying to understand and predict. This is very much related to the questions of both what macro-outputs are and how to study macropolitics. The choices made in defining and conceptualizing system performance are not to be taken lightly. If we decide that system performance is best captured through the implementation of public statutes at the aggregate level, we will likely find that the mechanisms that govern this process differ considerably from those involved in passing legislation. In the former, we might draw on an extensive literature that deals with principal-agent problems between Congress and the bureaucracy (Epstein and O'Halloran 1999), while in the latter we might look at literature that examines the relationship between Congress and the executive branch (Cameron 2000). Cooper and Brady touch on this specific aspect of congressional studies as it relates to macropolitics. While noting the difficulties in analyzing institutional operations, they conceptualize congressional performance in two ways. The first conceives of performance in output or productivity categories, such as lawmaking, oversight, and constituent service. This “concrete approach” attempts to define the possible outputs that could be examined in macropolitics in ways that are familiar to most legislative scholars. Alternatively, in what Cooper and Brady refer to as a more abstract approach, performance is viewed as the relationship or role that Congress plays vis-à-vis other units in the political system. It remains to be determined whether this conceptualization represents a distinct way to define performance or is really more a mechanism. In other words, exploring the relationship of Congress to other institutions might really belong on the left-hand side of the equation. Of course, better understanding these complex relationships is at the heart of the macropolitics enterprise, as again it will lead to us identifying and understanding the mechanisms that lead to political outcomes.

Again, we agree that evaluations of congressional performance should address output as well as interinstitutional relations. We believe, however, that most of the progress that will be made in this area in the short run will come

from studying performance and other productivity-related categories. This is largely due to the fact that most of the existing theoretical work (e.g., Krehbiel 1998; Cameron 2000) deals with lawmaking. Some of this structure clearly requires that we make additional progress in devising adequate measures to properly test theory. This is no simple task. Cooper and Brady contend that this hurdle is probably the fundamental reason why relatively little research on macropolitics has ever been conducted (1981, 999). The good news is that since Cooper and Brady published their article, considerable progress has been made in the area of measuring legislative productivity (Mayhew 1991; Howell et al. 2000; Lapinski 2000; Clinton and Lapinski 2006).

What may encourage scholars to take up the mantle of macropolitics is to construe performance broadly enough to include a variety of outputs, rather than simple counts. For instance, we might think of measuring performance as not merely the amount of legislation but also its direction—liberal versus conservative (see, for example, Erikson, MacKuen, and Stimson 2002). Or, as Mayhew did in *Divided We Govern*, we can explore the extent and type of oversight of federal agencies as it occurs over long swaths of time. Similarly, in the Freedman and Cameron chapter included in this volume, performance can be taken to be the type and direction of government regulation within a given policy arena over time. Expanding beyond simple counts of important legislation will be vital to bringing in scholars from other areas, including American political development and comparative politics, where the content and direction of policy is considered very important (Skowronek 1982; Smith 1997; Skocpol 1992; Orren and Skowronek 2004; Huber and Shipan 2002).

Even if the primary interest of studying macropolitics remains within the subfield of congressional studies, the increased interest in historical research on Congress will require serious thought about what measures can be developed to test ideas that might be best located within particular historical moments. For instance, it is reasonable to think of performance in different periods as a combination of both *change* and *continuity*. For example, if we are interested in studying the period from the mid-1890s through the 1920s, we might focus on the role of political parties within Congress. In some policy areas, however, influence for the Republican Party meant *not* passing legislation.¹⁰ Several Republican lawmakers, including much of the leadership, were often opposed to (or split) when it came to legislative proposals that would change national labor laws during this time period. Consider labor policy. For several years Democratic representative William Sulzer (N.Y.) annually introduced one or more bills that would have created a Department of Labor with a cabinet-level secretary (Chamberlain 1946, 144). These bills languished in committee until the Democratic Party gained control of the House in 1911. Even labor issues that garnered bipartisan support, such as

legislation strengthening child labor laws, cut against the Republican leadership's desire to keep the federal government out of issues that impacted business interests. As Chamberlain writes, "President Taft though keenly aware of the need for protecting children in industry was not favorable to the establishment of a federal children's bureau because he disapproved the 'disposition to unload everything on the federal government that the states ought to look after'" (146). Measuring the status quo, coding for policy direction, and disaggregating policy by substantive issue area are all critical steps for studying legislative productivity in this and in many historical periods. Consequently, scholars should not take lightly the conceptualization and construction of appropriate performance measures as they apply to the questions and periods of interest.

We see Brady and Cooper's essay as the first major call for the study of the macropolitics of Congress. Their primary interest in studying Congress across time was that such an approach nicely captures change—including change in rules, norms, and preferences—all of which are likely to impact policymaking. Political change, for Cooper and Brady, might take the form of new institutions (e.g., the Australian ballot, the Seventeenth Amendment, or changes in internal organization spurred by the Legislative Reorganization Act of 1946) or transformations in the preferences of crucial players in the policy process as a result of events like realigning elections (see also Brady 1988; Mayhew 2002). Identification of change in congressional structure and operations holds particular importance to this longitudinal view of congressional scholarship and congressional performance more generally. It leads us to a critical question, does long-term change in institutional organization and congressional interactions with other political actors affect the process of policymaking and subsequent outcomes?

To study Congress in this longitudinal manner, David Brady sketched out what he believes are the necessary components of a theory of macropolitics.¹¹ Specifically, Brady focuses on the mechanisms that force change in macropolitical outputs. In many ways Brady's concluding chapter in this volume is a sequel to his original essay on the subject. He acknowledges that building such a theory will take time, but that enough work has been completed in macropolitics to begin to outline what a theory might look like. He asserts that a theory would have to address at least the following questions: When will the government be active and what kinds of activities will it undertake? How do the parts of government—elected and appointed—work together or against each other to either limit activity or enhance it? How do moods, opinions, and elections interact with government actors to yield policy activity or inactivity? Regardless of the motivation, we agree that macropolitics must be studied across time and, potentially, across place. How can one adequately assess performance if a particular Congress cannot be compared to another?

IMPORTANT NEW WORK IN MACROPOLITICS

A number of recent works on Congress fall within the macropolitics genre. A few of these studies stand out—some for their theoretical contributions and others for their empirical efforts, and a few for both. As mentioned above, David Mayhew's work on the consequences of divided government was pioneering, not for its theoretical offerings, but for providing a first cut at the complicated task of measuring governmental performance—here, measured as the productivity of important legislation—over a long time horizon. His simple count of public laws makes the critical distinction between landmark legislation and everything else, and challenges the long-standing belief that unified party government leads to moments of high legislative productivity, while divided government contributes to legislative gridlock.

Of course, Mayhew's controversial work opened up an extensive and interesting debate on the effect of unified and divided government, making the study of divided government without question the most active area of research in the macropolitics realm. While some scholars have questioned his measures of legislative importance and, to a certain extent, his conclusions (Kelly 1993; Howell et al. 2000), others have argued that the broader political context is ignored when one analyzes simple counts of important enactments (Binder 1999, 2003; Edwards, Barrett, and Peake 1997).

Krehbiel's theory of pivotal politics (1996, 1998) offers insights as to when we will see individual and aggregate policy activity (see also Brady and Volden 1998). His approach to policy activity is based on micro-level assumptions regarding preference-driven legislative action. In essence he sees the policy positions of two key legislative players—the cloture pivot and veto pivot as the critical factors in determining when there will be activity or inactivity within a given sphere of public policy. The configuration of these vital political actors in relation to the location of the status quo and the president's position for a given policy dictates when changes in that policy will be adopted. The portion of policy "space" within which the key actors are not able to improve their well-being by enacting new policy is considered the "gridlock interval." Crucial to the pivotal politics model is the role of elections as a catalyst for policy change. If we believe that members of Congress do not change their ideological positions across time (Poole 1998),¹² then electoral turnover is the only real mechanism for making significant alterations in the size and location of the gridlock interval and thus increasing the likelihood of policy change. More broadly, however, this configuration of actors also determines the likelihood of sweeping policy changes in the aggregate. For example, according to Krehbiel's theory, we should observe policy surges at the beginning of new presidential administrations (particularly if we observe a party change), as this is likely to have produced a larger shift in the gridlock interval. This work offers

one of the strongest supplements to Mayhew's research on legislative productivity as related to the incidence of unified and divided government, which is not considered to be theoretically related to gridlock *per se*.

While Krehbiel's model is a powerful one—suggesting a number of hypotheses as to when we should expect surges and slumps in overall legislative output—scholars have only begun to test its empirical predictions. In addition, Krehbiel's model leaves considerable room for other approaches to fill in the gaps. For instance, the pivotal politics theory does not tell us why some policies that have status quo points clearly outside of the gridlock interval are not immediately changed once the gridlock interval shifts. Nor does it say anything about what accounts for timing in relation to policymaking. The theory remains silent on such questions.

Other important work falling within the domain of macropolitics that deals more explicitly with the question of interbranch cooperation and conflict is that of Charles Cameron and his colleagues. Again, motivated by rational choice models of political action, Cameron studies veto bargaining by exploring the incentives of both Congress and the president in the process of negotiating policies (Cameron 2000; Cameron, Lapinski, and Riemann 2000). Cameron shows that vetoes do not inevitably spell the demise of policy proposals but are simply a tool in the give-and-take between branches in shaping government outputs.

Also drawing on the separation-of-powers theme, Erikson, MacKuen, and Stimson's (2002) wide-ranging study of citizen preferences and government activity utilizes years of research on the purposive actions of policymakers to examine how changes in public "policy mood" over time influences the legislative activity and liberalism of Congress, the president, and the courts. The notion behind this body of work is that Congress (and the president) is responsive to constituent wishes. Erikson et al. offer a very good first step in the process of measuring ideological direction of policy output over time.

While macropolitics work was quite rare a decade ago, it is becoming ever more common as we attempt to better understand what drives policy outputs. Collectively, the works highlighted here have turned the study of macropolitics in the United States into a more coherent research tradition that seeks understanding of the mechanisms that drive policy outputs. The objective of this book is to help develop this new, important body of work and provide an impetus for a new wave of macro-oriented research, specifically involving Congress.

ORGANIZATION OF THE VOLUME

The timing and structure of this volume is not accidental. Prior to the last decade or so, scholars had made little headway in the study of macropolitics, partially because of the lack of a theoretical infrastructure. We now have the

beginnings of good macropolitics theory, the bulk of it coming from micro-level work focused on the behavior and actions of individual MCs. It is no coincidence that the first part of this volume, “Theoretical Approaches to the Macropolitics of Congress,” draws upon the work of three highly regarded political scientists who are best known for their research on formal models dealing with legislatures and the separation-of-powers system. Krehbiel’s past work, particularly that examining the role of information in a legislative setting, often used deductively derived rational actor models to provide testable predictions regarding the purposive behavior of individual lawmakers. In chapter 1, Krehbiel goes beyond thinking about a single model of legislative policymaking, and instead provides the tools necessary to help us develop empirical methods to test “competing models” of macropolitics (along with demonstrating the importance of determining whether alleged competing theories produce non-observationally equivalent predictions). His work brings us back to an important and ongoing debate about the role of political parties as organizing cartels inside Congress (and has implications for the role of parties across different types of legislatures).

Huber and McCarty’s contribution is an ambitious chapter that demonstrates the relationships between bureaucratic capacity, legislative expertise, and legislative output. This is accomplished by deriving a game theoretic model that truly possesses macro-level predictions. This type of work is important for two reasons. First, the model puts America’s evolving political eras into their historical context, as it is designed to predict when institutional change will affect congressional performance. In other words, the model offers the conditions under which we are likely see changes in the macro-outputs Congress produces. The “under what conditions” question is tailored to take into account different moments of congressional history. This modeling is important, because if we are interested in understanding legislative outputs, preference-based theories of macropolitics work well for the post-1946 United States, but are less useful for earlier eras. Clearly, Congress-centered reforms must matter, including the development of the committee systems in both chambers, the direct election of senators, and procedural reforms like the introduction of cloture in the Senate or the expansion of subcommittee powers in the House. Second, this model offers true macro-level predictions, thus moving away from theoretical work that is centered on individual lawmakers. The Huber and McCarty chapter is an excellent example of the type of theoretical work we hope to see more of in the future.

Part 2, *The Macropolitics of Representation*, builds on the idea that Congress is the most representative political institution in our separation-of-powers system. Legislators, who must represent the specific interests of their constituencies, make binding decisions on public policy.¹³ Erikson, MacKuen, and Stimson argue that public demand plays a formative role in determining the policies enacted. In the words of Heinz Eulau, legislators are “responsive”

(Eulau 1993). Public opinion regarding federal policy is measured through the familiar conceptualization of “mood,” which exists in a left-right (liberal-conservative) continuum. Erikson, MacKuen, and Stimson find overwhelming evidence to support the notion that the causal mechanism underlying the enactment of liberal or conservative legislation is public support. If we believe that legislators should receive direct signals from constituents and translate these messages into policy, then this chapter suggests that our political system is working as it should.

Katznelson and Lapinski believe that the substance of policy is a key, and often missing, element in the study of the policy process. In their chapter, they argue that representation and public policy proceed in tandem but that our political institutions often prevent median voter policies from becoming law. Building on a rich history of early public policy research, they introduce a coding scheme to parse statutes into distinct categories by policy area. Arranging policies in distinct categories allows scholars to identify like policies across time and helps us better identify the primary dimension of policy issue areas. Consequently the coding scheme is an empirical device that should help us better understand how the policy process differs across issues (e.g., preference intensity, absent in most theoretical accounts of policymaking, surely varies across issue areas). In other words, Katznelson and Lapinski argue that if we are to analyze theories of macropolitics, such as Krehbiel’s theory of gridlock or Huber and McCarty’s model that predicts when legislators will legislate, we need to think about how specific policy areas fit into the picture (e.g., what explains the political ordering of the policy agenda in Krehbiel’s model of gridlock?).

The part titled “Testing Theories of Macropolitics across Time,” introduces new ideas about testing extant theories of legislative productivity across history. As noted earlier, Cooper and Brady lamented that researchers were largely stymied in their effort to examine institutional performance by the difficulty in formulating appropriate variables. The payoff, however, they surmised, would be quite high if we could devise measures that captured institutional performance across such time periods as varied as the Civil War, the Progressive Era, and the New Deal. Heitshusen and Young take a step in the right direction with their chapter. They argue that legislation that changes the U.S. Code denotes a level of legislative significance. Consequently, they are able to leverage this rich source to create a list of significant enactments back in time, which they use to test existing preference-based theories of macropolitics. This suggestive work provides us with many answers (along with a few new questions) about whether it is appropriate to read the present backwards when testing theories of policymaking.

Shipan’s work moves away from studying the effect of partisan control on the passage of important legislation toward a more inclusive study of its effect on the political agenda. The agenda in this case is thought of as being endogenous

and an alternative conceptualization of system performance. Shipan contends that understanding the denominator (the number of established objectives of the legislature) will allow us to determine the institutional performance of Congress because it allows us to measure how much of the agenda passes (see Binder 1999, 2003). He finds that divided control of government leads to sharp increases in the size of the political agenda.

The fourth part, “Macropolitics and Public Policy,” uses the macropolitics framework to examine the course of legislative change in three very different policy environments. Freedman and Cameron offer a new kind of history of the administrative state—one that is theoretically driven yet has a deep understanding of the logic of individual regulatory regimes. Using a simple spatial model to map the relative ideological positions of the principals and agents involved in telecommunications policy, they are able to predict when significant changes in regulation or enactments occur. Their findings indicate that Congress is willing to act when its agent—in this case the Federal Communications Commission—diverges too far from the ideological preferences of the relevant congressional committees.

Taking a slightly different angle on the macropolitics of the congressional-bureaucratic relationship, Canes-Wrone explores the relative ability of Congress *and* the courts to influence bureaucratic decisions with regard to the issuing of permits for the development of wetlands. Again, examining differences in the ideological inclination of representatives on the relevant oversight committees and in the ideological position of district and appellate courts, Canes-Wrone finds that both branches affect the issuance of permits by the Army Corps of Engineers. For our purposes Canes-Wrone’s findings have implications beyond the specifics of this case study by demonstrating that the various principals in this separation-of-powers environment are able to greatly influence the decisions of the bureaucratic agent without directly mandating specific kinds of actions.

Adler and Leblang examine the collective governance of the economy by the executive and legislative branches. Dissatisfied with the existing literature on political cycles that often underestimates the role of Congress as a factor in economic performance, they develop models of ideological and partisan compromise between Congress and the president that are tested against existing theories that most often highlight only the partisanship of the president or the existence or absence of unified party government. Performance of the domestic economy is seen as an additional way to gauge performance of the governing structure. Using economic indicators over most of the postwar period, Adler and Leblang find that their measure of interbranch compromise helps to explain peaks and troughs in economic performance. Specifically, the economy performs better—in terms of increased growth and decreased unemployment—when there is a Democrat in the White House and when there is the likelihood of a “liberal” compromise between executive and legislative branches.

Finally, the volume concludes with chapters by David Mayhew and David Brady. In typical fashion, Mayhew provides us with rich insights about the work contained within the volume along with some new data (an update of his landmark legislation for the 107th Congress) and a few fresh ideas of his own. Instead of attempting to summarize an eclectic set of chapters, Mayhew offers extensive thoughts on two chapters that deal with subjects very close to his own work: the effect of divided government on policy agendas and how public opinion affects the direction (liberal or conservative) of policymaking. Giving his conclusion his signature stamp, he brings to the forefront ideas that will engage scholars for the next few years. First, he asks how the status quo policy reversion point is affected under conditions of crisis. This is an interesting question, especially since our country has revisited crisis over and over again. Does crisis make the existing status quo point irrelevant? Mayhew thinks so. He argues that “a shift of the status quo policy outside the congressional ‘gridlock interval’ can be spectacularly caused by events other than elections.” In other words, the status quo point is thrown out of the window under crisis conditions. How we incorporate this discovery into our theoretical and empirical work is left to the discipline to figure out. Second, he argues that the disappearance of partisanship under conditions of crisis is important and deserves our attention. Mayhew urges scholars to dig deeper into the circumstances surrounding the movement of Congress toward unanimous or near unanimous action.

Brady shows that studying macropolitics is compatible with two major approaches to American politics: the rational choice tradition and the American political development approach. He delineates the differences between the two genres: the former puts heavy emphasis on the role of elections and political preferences, while the latter is more interested in social movements and the origins and development of political institutions. He points out that both share an interest in understanding the dynamics of American politics over time. His chapter also sketches out an overview of the progress we have made in developing a theory of macropolitics since he published his initial probe in 1981, and provides a road map for areas of research where good progress can be made in the short term.

NOTES

1. Senator James Jeffords renounced his Republican Party affiliation on May 24, 2001. The “Macropolitics of Congress” conference was held in Boulder, Colorado, June 1–3, 2001.

2. By micro-oriented work we simply mean that the unit of analysis is the individual member of Congress. Most micro-level theories, of course, are ultimately interested in the collective behavior of individuals. The distinction between collective (macro) behavior and macro-outputs is an important one. Aggregate behavior in many

ways serves as a bridge between micro- and macro-oriented work. We return to this linkage later in the introduction.

3. It is even more important in that divided government has been prevalent throughout the post–World War II period, and with the current competitive political environment, the probability is high that it will be with us into the foreseeable future.

4. While Wilson conceded that our separation-of-powers system worked well in a more simple time, he believed that it did not function well as society faced more complex problems than existed at the Founding.

5. See Whittington and Carpenter 2003 for an argument for placing more emphasis on studying the role of the executive branch in public policy.

6. Much recent research has pointed out that Congress, particularly the Senate, is not a majoritarian institution (Krehbiel 1998). See the *Wall Street Journal* editorial “Daschle’s Dead Zone” (July 22, 2004, A12).

7. Research on Congress that until the early 1970s had been rooted in social-psychological analyses shifted its focus by embracing rational actor models heavily influenced by economics. This transformation, documented in several excellent review essays (Gamm and Huber 2002; Polsby and Schickler 2002), has lasted until today.

8. The structure-conduct-performance paradigm is as old as the New Deal with roots in the Harvard Department of Economics. See Mason and Lamont 1982 or Tirole 2003 for a historical review of the concept. Cameron (2000) speculates that this paradigm is well suited to studying policy outputs.

9. Cooper and Brady prefer the word *change* to *development* because of the normative component of the latter.

10. This is somewhat of a simplification as Progressive Republicans were interested in many national level reforms. Furthermore, southern Democrats were against intrusions by the federal government across a number of policy issues.

11. Taken from Brady’s comments during a roundtable presentation at the Colorado-Yale conference.

12. Redistricting might lead members to change their preferences, as they consider themselves to be delegates of their districts.

13. Of course, representation is provisional in that public policies can be changed by future Congresses (Katznelson and Lapinski 2004).

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