The Dynamics of Legislative Gridlock, 1947–96
SARAH A. BINDER *The Brookings Institution and George Washington University*

*D*avid Mayhew's Divided We Govern *(1991)* sparked an industry of scholars who alternately challenge or confirm the work on theoretical and empirical grounds. Still, we lack a definitive account of the proportions and causes of legislative gridlock. I revisit the effects of elections and institutions on policy outcomes to propose an alternative theory of gridlock: The distribution of policy preferences within the parties, between the two chambers, and across Congress more broadly is central to explaining the dynamics of gridlock. To test the model, I construct a measure that assesses legislative output in proportion to the policy agenda. Using newspaper editorials to identify every salient legislative issue between 1947 and 1996, I generate Congress-by-Congress gridlock scores and use them to test competing explanations. The results suggest that intrabranch conflict—perhaps more than interbranch rivalry—is critical in shaping deadlock in American politics.

We think both parties misread the temper as well as the intelligence of the American electorate if they think it is "good politics" to stall and delay and eventually come up with nothing ("Congressional Record" 1956).

Although "gridlock" is said to have entered the political lexicon after the 1980 elections (see Safire 1993, 305), stalemate is not a modern legislative invention. Indeed, in the very first *Federalist*, Alexander Hamilton complained about the "unequivocal experience of the inefficacy of the subsisting federal government" under the Articles of Confederation (Wills 1982, *Federalist No. 1*). Although stalemate may be endemic to American politics, no definitive account of its proportions or causes yet exists. In this article, I survey recent work, propose a new measure of gridlock, and test several alternative accounts of variation in gridlock over the last half-century.

There is no shortage of scholarship on the politics of gridlock. Most prominent is the work of Mayhew (1991), who rejects the conventional wisdom that divided party control of Congress and the presidency dampens the legislative output of government (Cutler 1988; Sundquist 1988). Subsequent work takes Mayhew as the point of departure, revisiting the questions he raised and researched (see, e.g., Brady and Volden 1998; Edwards, Barrett, and Peake 1997; Fiorina 1996; C. Jones 1994; Kelly 1993; Krehbiel 1998; Oppenheimer 1996; Quirk and Nesmith 1998; Royed and Borrelli 1997; Taylor 1998; Thorson 1998). This project returns as well to Mayhew's work, probing in a new fashion the contours of gridlock in American national politics.

First, I build on Mayhew's null finding and offer an alternative account of variation in gridlock over time.

Most important, I suggest that intrabranch friction may be more significant than interbranch conflict in contributing to policy stalemate. Second, I present a new metric of policy stalemate that measures legislative output in proportion to the policy agenda of Congress and the president. Using data from *New York Times* editorials between 1947 and 1996, I develop a Congress-by-Congress gridlock score and use it to test several hypotheses about the causes of gridlock. The results suggest the fruitfulness of developing new measures and broadening our theoretical focus in assessing the collective performance of Congress and the president.

THE STUDY OF GRIDLOCK IN POLITICS

Judging the performance of Congress and the president is no new endeavor for political scientists. Numerous scholars before Mayhew (1991) had studied legislative and executive effectiveness. In the early twentieth century, for example, session-by-session reporting on Congress by Rogers (1919), among others, appeared in the *American Political Science Review*. Observing Congress over a longer period, others, such as Stealey (1906) and Bates (1936), chronicled and evaluated the achievements of Congress. By mid-century, scholars had turned to more analytical appraisals of legislative performance (Gordon 1966; Sundquist 1968; Truman 1959). There is also no shortage of analysis more explicitly focused on presidential performance (e.g., Bond and Fleisher 1990; Edwards 1989; Peterson 1990; Wildavsky 1966).

Mayhew (1991) is the landmark study in this research tradition due to its focused theoretical perspective and the empirical rigor of its historical sweep. It also departs from much prior work by its focus on lawmaking per se, rather than the performance of any single institution (but see the focus on the "tandem-institutions" of Congress and the presidency in Peterson 1990). Not surprisingly, Mayhew's work has spurred a growth industry in the study of gridlock, with some refining and others challenging the nonpartisan theoretical model underlying Mayhew's empirical results (e.g., Brady and Volden 1998; Krehbiel 1998) and yet others using new measures (e.g., Kelly 1993; Ed-

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wards, Barrett, and Peake 1997) to revisit the effects of divided government on lawmaking. As Fiorina (1996) concludes after assessing much of this recent work, Mayhew's (1991) null finding for the effect of divided party control on government performance has generally held up, albeit with important exceptions.

Nevertheless, there is good reason to revisit the dynamics of policy gridlock. Theoretically, Mayhew was motivated primarily by an interest in the effects of a particular independent variable, namely, divided government. Toward that end, he identified landmark laws in a two-stage process (for details, see Mayhew 1991, chap. 3). In Sweep One, Mayhew used annual end-of-session wrap-up articles from the *New York Times* and *Washington Post* to survey contemporary judgments about the significance of Congress's work each session. In Sweep Two, Mayhew relied on policy specialists' retrospective judgments about the importance of legislation. Using the results of Sweep One to inform his selection of laws during Sweep Two (Mayhew 1991, 45–59), he generated a comprehensive list of landmark laws enacted each Congress between 1946 and 1990.[1] He then tested whether the presence of divided government reduced the number of truly major laws enacted each Congress (Appendix A compares my results with those of Mayhew).

Although others have offered theoretical and empirical alternatives to Mayhew's contributions, no one has yet tackled both challenges simultaneously. As I argue below, no definitive account of the politics of gridlock is possible until a more robust measure has been used to test an array of competing accounts of variation in gridlock over time.

Empirical Considerations

Consider first the empirical challenge left by Mayhew's work. As Fiorina (1996, 89) argues, "an irreducible ambiguity in Mayhew's findings ... remain[s]. Essentially, he has studied the supply of federal legislation and found that the supply is more or less the same during modern unified and divided government periods. But we have no information about the demand for legislation." To be sure, Mayhew recognized this problem, but he concluded that it is "very difficult to see what a denominator for a Congress—an agenda of potential enactments—might be. 'As demanded by the needs of the time,' perhaps? ... That would be hopeless to administer" (1991, 36).

Despite the obvious difficulty of developing an "agenda of potential enactments," such a measure is needed to test theories of political stalemate. Gridlock is not the inverse of legislative output. Certainly, a low level of law production may indicate a high level of political gridlock. Alternatively, it may reflect the response of a Congress and president facing a limited political agenda, in which case it may indicate a low level of gridlock. The point is that we just do not know, absent a metric of the broader political agenda. Indeed,

as much was once suggested by the *New York Times*, which editorially cautioned against admiring Congress "in proportion to the volume of bills it grinds out. The only sane criterion is a comparison of its record with the problems before it" ("The Sluggish 91st" 1969).

Theoretical Considerations

Mayhew's null finding for the effect of divided government on legislative output has spurred others to develop more fine-grained theories of legislative performance. Taken together, such works suggest that party control alone cannot account for variation in the legislative performance of Congress and the president. Instead, institutional arrangements alter the strategies of legislators and presidents and thus affect the character and frequency of policy outcomes. Indeed, a recurrent theme in recent work is the effect of supermajority institutions on policy outcomes. Brady and Volden (1998) and Krehbiel (1998) argue, for example, that the policy preferences of supermajority "veto" players in Congress are central to explaining the dynamics of gridlock. As such, these works formalize the intuition that supermajoritarian, rather than majoritarian, models are central to legislative outcomes.

These works are important efforts to think beyond interbranch conflict. Still, institutional arrangements beyond supermajority rules clearly affect legislative outcomes. In particular, we tend to underestimate the policy consequences of the simple institutional fact of bicameralism. Recent work makes clear the need to account for bicameral features in modeling legislative outcomes (e.g., Tsebelis 1995; Tsebelis and Money 1997) and reflects observations made earlier by Fenno (1982), Longley and Oleszek (1989), and Smith (1988).

Finally, it is important to remember that recent theoretical treatments of gridlock have been tested primarily with Mayhew's data on legislative enactments. That is, theories crafted to account for episodes of legislative stalemate have been tested with data on legislative success. But if policy gridlock is not simply the inverse of legislative output, then the empirical robustness of such theories is difficult to judge. In short, much more needs to be learned about the proportions and causes of legislative gridlock. Integrating new theoretical observations with a more robust measure of gridlock may yield new findings about its political dynamics.

THE DYNAMICS OF GRIDLOCK

Recent work on the politics of legislative productivity encourages us to think along two separable dimensions: partisan and institutional (but see D. Jones 1998, who suggests that the *interaction* between the two is central to modeling legislative productivity). Partisan models focus primarily on the effect of divided party control of Congress and the presidency, while institutional models emphasize the effect of supermajority rules. These works provide an important but ultimately only partial accounting of the electoral and institutional dynamics underlying gridlock in the legislative process.

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[1] Either a contemporary or retrospective judgment of significance was sufficient for a law to be included in the final list. For an alternative approach, see Kelly 1993.

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The Partisan and Electoral Context

Partisan theories of legislative gridlock traditionally have centered on the effect of divided government on policy outcomes. The logic is straightforward: Unified party control of the two branches guarantees an important extraconstitutional link between the legislature and executive, which ensures common interests and shared purpose. "That theory," argues Sundquist (1988, 614), "identified the political party as the indispensable instrument that brought cohesion and unity, and hence effectiveness, to the government as a whole by linking the executive and legislative branches in a bond of common interest." Under unified government, shared electoral and policy motivations of the president and congressional majorities give majority party legislative leaders the incentive and capacity to use their tools and resources to pass legislation. In contrast, under divided government, differing policy and electoral interests are said to reinforce institutional rivalries between Congress and the president, which make it difficult to assemble the coherent policy majorities necessary to forge major legislation (Fiorina 1996, chap. 6). Unified government, in this view, boosts the prospects for legislative success, while divided government makes it harder for Congress and the president to reach agreement on issues before them. Given electoral and policy differences during times of divided government, a simple divided government hypothesis follows:

HYPOTHESIS 1. *Divided party control of government increases policy gridlock, while unified control decreases gridlock.*

But elections do more than simply divide up control of the major branches of government. They also determine the distribution of policy preferences within and between the two major legislative parties. At times, partisan preferences are polarized, with most legislators at the respective ends of the underlying ideological spectrum; at other times, greater numbers of legislators stand closer to the ideological center. The number of moderate legislators is important, because it likely affects the ease with which compromises are crafted and finalized. As Gilmour (1995) suggests, legislators are often likely to prefer disagreement to compromise, particularly if electoral incentives encourage the two parties to differentiate themselves. Thus, the more polarized the two parties, the greater is the incentive for them to distinguish their records and positions, and the lower is the incentive to strive for compromise and make legislative deals.[2] If the presence of moderate legislators affects the ease of compromise, we should observe the following relationship:

HYPOTHESIS 2. *The greater the polarization of the partisan elite, the higher is the level of policy gridlock.*

Elections also affect the distribution of policy preferences across the legislature more generally, independent of the partisan alignment of preferences. Indeed, we might expect different propensities toward gridlock in relatively homogeneous or heterogeneous legislatures. The broader the distribution of preferences, the greater the likelihood that legislators' goals will be incompatible, or at least the more difficult it will be to reach a suitable compromise. As suggested by Axelrod (1970, 5), incompatibility of goals leads to higher levels of conflict and ultimately to episodes of "conflictual behavior." As preferences cohere within a legislature, policy compromise should become easier to achieve. All else equal, we might expect the relative heterogeneity of congressional preferences to affect policy stability. In short:

HYPOTHESIS 3. *The more cohesive legislative preferences, the lower is the level of policy gridlock.*

Finally, the timing of major electoral change is likely to have measurable policy consequences. A unified majority at the onset of an electoral realignment is likely to ensure legislative action, as realignments "provide a basis for relatively integrated, coherent, and effective governmental action" (Clubb, Flanigan, and Zingale 1990, 36). Even major electoral shocks short of a realignment are likely to affect policy stability. The argument was well stated in a *New York Times* editorial in 1948 at the close of the 80th Congress, the first Republican Congress since before the New Deal:

> The Republicans took control of Congress on the basis of an obvious popular revulsion against some of the policies of the Roosevelt-Truman administrations. There was no landslide but there was a perceptible movement of the political terrain. The new legislators certainly had a mandate to liquidate some war measures, to loosen some New Deal controls, to check some New Deal projects and to effect practicable economics ("Eightieth Congress: To Date" 1948).

The effects of such electoral shocks are likely conditioned by the length of time a new congressional majority was in the minority. The longer a new majority was not in control of Congress, the more dissatisfied it is likely to be with the status quo, and the greater is its incentive to make changes. There is also a strong electoral incentive for a new majority to prove that it can govern, which further increases the likelihood of altering the policy status quo.[3] The relationship between electoral shocks and policy outcomes suggests the following hypothesis:

HYPOTHESIS 4. *The longer a new congressional majority has been out of power, the greater is its dissatisfaction with the status quo, and the lower is the level of policy gridlock.*

Elections, in short, affect legislative dynamics in ways other than allocating party control of Congress and the presidency. Indeed, if arguments about the effect of divided government are primarily about the influence

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[2] See also Brady and Volden (1998, 25–6) for a discussion of the expected relationship between polarization and gridlock. Polarization in their account, however, is technically nonpartisan, as it is measured as the distance between the left-side filibuster pivot (the 41st senator) and the right-side veto pivot (the 67th Senate or House member).

[3] A new, inexperienced majority also may face the countervailing difficulty of "learning to govern" (Fenno 1997).

of electoral and policy motivations on legislative outcomes, then theories of gridlock need to account for the multiple ways in which elections align policy and electoral interests within the parties and Congress more generally.

The Institutional Context

In contrast to the partisan models, institutional approaches suggest that structural arrangements alter the distribution of power within Congress and thus weaken the independent effect of party on legislative outcomes. Institutional and partisan frameworks are commonly portrayed as offering distinctive approaches to lawmaking, and the inclusion of supermajority rules represents a break from theories more attuned to the effects of party dynamics. Yet, despite this distinctiveness, most recent institutional work shares a central feature with more traditional views of legislative productivity: Both model legislative-executive interactions as a unicameral game.

To be sure, recent scholarship emphasizing the effect of supermajority rules on policy outcomes is sensitive to the distinctive procedural characteristics of the two legislative chambers (see Brady and Volden 1998; D. Jones 1995; Krehbiel 1998). Nevertheless, Congress tends to be treated as a unitary actor opposing the president. Krehbiel (1998, chap. 2), for example, models a "pivotal politics" game in which the median voter and the "filibuster pivot" sequentially determine whether a bill is sent to the president. Similarly, because filibusters are technically impossible under House rules and thus there is no House filibuster pivot, Brady and Volden (1998) and D. Jones (1998) argue that the House tends to be much less of a constraint on policy outcomes than is the Senate.

Recent formal and comparative studies of legislatures, however, suggest that other institutional features—especially bicameralism—are central in shaping policy outcomes. Hammond and Miller (1987), Riker (1992), Tsebelis (1995), and Tsebelis and Money (1997) suggest that bicameral legislatures alter the dynamics of policy change, which makes changes in the status quo more difficult than in unicameral bodies. Most important, policy stability depends on the distance between the two chambers (Tsebelis 1995). As shown by Tsebelis, movement of the critical players in a bicameral game away from each other shrinks the "winset" of the status quo—the set of all points that can defeat the status quo. Thus, as the preferences of the two chambers diverge policy stability increases, and change in the status quo becomes less likely.

The effect of chamber differences is not a recent discovery. The framers of the Constitution were careful to design two very different legislative bodies. Simply including an upper house to check the lower house was insufficient. As James Madison argued in *Federalist No. 62*, "I will barely remark, that as the improbability of sinister combinations will be *in proportion to the dissimilarity in the genius of the two bodies*, it must be politic to distinguish them from each other by every circumstance which will consist with a due harmony in all

proper measures, and with the genuine principles of republican government" (quoted in Wills 1982, 315; my emphasis). Madison's comments leave no doubt that the framers intended interchamber differences to come to the fore during the lawmaking process and that they expected such differences to have important policy consequences. Together, these theoretical and historical insights suggest that models of gridlock need to incorporate bicameralism. A simple bicameral hypothesis follows:

HYPOTHESIS 5. *The greater the policy distance between the House and Senate, the higher is the level of policy gridlock.*

Bicameralism is unlikely to be the sole institutional factor that shapes legislative outcomes. Supermajority rules, particularly in the Senate to limit debate, potentially restrict the ability of majorities to secure their preferred policy outcomes. Binder and Smith (1997, chap. 5), for example, show that the rate of death-by-filibuster has increased markedly since the middle of the twentieth century. Of course, the filibuster frequently is used to extract policy and political concessions during consideration of legislation, such that obstructionism need not kill the underlying measure. Still, an additional institutional hypothesis is worth exploring:

HYPOTHESIS 6. *The greater the threat of filibuster, the higher is the level of policy gridlock.*

To the extent that institutional arrangements mediate the effect of legislative preferences on policy outcomes, both bicameralism and supermajority rules may be central to modeling the dynamics of legislative gridlock.

The Policy Context

Partisan and institutional factors alone are unlikely to account fully for deadlock. Because different types of policies yield different patterns of politics (Lowi 1964), the question is whether differences in the broader policy context affect the ease with which legislative compromise is reached. Budgetary slack and broad national trends are key features of the policy environment that can affect policy stability. The logic underpinning each factor is simple. First, the greater the surplus relative to outlays, the easier it is to accomplish legislative goals—whether these include creating new federal programs or cutting taxes. The assumption is that providing new benefits in any form is easier than cutting old ones, and providing new benefits is easier during better fiscal times. For example, according to one recent account of changes in legislative productivity over time, an "environment of constraint" in the 1980s reversed the liberal activism of the previous era (Davidson 1996, 34). This leads us to expect:

HYPOTHESIS 7. *The greater the federal budget surplus relative to outlays, the lower is the level of policy gridlock.*

Second, prevailing national moods are said to have a significant influence on both agendas and outcomes.

> The idea goes by different names—the national mood, the climate in the country, changes in public opinion, or broad social movements. But common to all of these labels is the notion that a rather large number of people out in the country are thinking along certain common lines, that this national mood changes from one time to another in discernible ways, and that these changes in mood or climate have important impacts on policy agendas and policy outcomes (Kingdon 1984, 153).

One such national mood is a climate of opinion that favors governmental solutions to societal problems. As Mayhew (1991) argues, such periods of "public purpose" (Schlesinger 1986) or "creedal passion" (Huntington 1981) may be the cause of extended periods of legislative activism. In other words, movement in public opinion—changes in "global attitudes towards the role of government in society" (Stimson, MacKuen, and Erikson 1995, 544)—may affect policy stability and change. Thus:

HYPOTHESIS 8. *The greater the level of public support for governmental action, the lower is the level of policy gridlock.*

Taken together, these eight hypotheses suggest a broad model of gridlock attuned to electoral, institutional, and policy correlates.

DATA AND METHOD

In this section, I present a measure of policy gridlock and a method for testing competing accounts of policy stalemate (details on measurement appear in Appendix B).

Dependent Variable: Measuring the Proportions of Gridlock

The definition of gridlock largely shapes how it is measured.[4] I have in mind the simple idea suggested by C. Jones (1994, 196), who argues that our primary concern should be to evaluate the "success of the system in treating public problems." Gridlock reflects the relative ability of the political system to reach legislative compromises that alter the status quo. Implied here is what Mayhew (1991, 34) terms "some actually-did-pass numerator over some all-that-were-possibilities-for-passage denominator." The question is how the denominator—possibilities for passage—should be defined and operationalized. Mayhew, after giving the matter considerable thought, decided to focus exclusively on the numerator. As argued earlier, however, a denominator is crucial to an evaluation of legislative performance over time.

My approach to identifying a denominator of potential enactments builds on the work of Cobb and Elder (1983, 85), who define the "systemic agenda" as "all

issues that are commonly perceived by members of the political community as meriting public attention." Similarly, Kingdon (1984, 3) defines the agenda as "the list of subjects or problems to which governmental officials, and people outside of government closely associated with those officials, are paying some serious attention to at any given time."[5] What needs to be measured is the proportion of the agenda that fails to be enacted in any given period, in this case each two-year Congress.

The main task is to determine what constitutes the systemic agenda. I rely on daily unsigned editorials appearing in the *New York Times* between 1947 and 1996. The choice rests on the assumption that the nation's paper of record responds to issues under consideration in Washington and highlights public problems that deserve attention.[6] As former *Washington Post* editorial board member E. J. Dionne expressed it, an editorial writer's job is "to tack a notice up on the board ... to put an issue on people's radar screen" (personal conversation, April 3, 1998, Washington, DC). Current and former members of the *Times* editorial board concur. Their goal, they say, is to "get out in front of the news ... jump out in front of an issue before it gets covered in the news," although they recognize that they are often "driven by the news and reacting to the news" (interview with Steven R. Weisman, April 1, 1998, Washington, DC). Editorials, in short, capture issues at the "much talked about stage" (Mayhew 1991, 36) as well as issues that may be considered the "agenda of potential enactments" (pp. 35–6). "I concern myself with the things my neighbors don't have time to think about," one former *Times* editorial writer observed, "with issues that are very important to our common life together" (interview with N. Don Wycliff, Editorial Page Editor, *Chicago Tribune*, April 23, 1998, Chicago, IL).

From the editorial pages of the *New York Times* I extract the issues that plausibly constitute the systemic

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[4] The pejorative connotation of *gridlock* is unfortunate but seemingly unavoidable. My use of *gridlock*, *stalemate*, or *deadlock*, implies no normative preference for legislative activity.

[5] Focusing on "agendas" rather than "public demands" is preferable because of the inherent difficulty of determining what constitutes a demand. Quirk and Nesmith (1994, 192) define deadlock as "a failure to act, for whatever the reason, in the face of a pressing need or demand for action." Yet, they also recognize that it is difficult to determine demand in face of "countervailing demands or interests of comparable magnitude" that may in fact be served by the status quo (p. 209 n. 6). Moreover, it is not clear that public demand per se exists exogenous to the policy process and media coverage of such activity. On the interplay of public opinion, media coverage, and political activity, see, among others, Iyengar and Kinder 1987.

[6] In recent decades, the *Times* is widely considered to espouse relatively liberal views, but because the editorials both support and oppose issues, there is no need to balance the data with information from a more conservative newspaper, such as the *Wall Street Journal*. A regional newspaper such as the *Chicago Tribune* (also considered conservative) was not a viable option: Its attention to national issues competes with coverage of local, state, and regional matters to a much greater extent than is the case for the *Times*, and its decisions about which national issues to cover are often strongly shaped by regional considerations (interview with N. Don Wycliff, Editorial Page Editor, *Chicago Tribune*, April 24, 1998, Chicago, IL). Moreover, Page (1996) suggests that the *Times* is an agenda-setter for other media outlets. Finally, the *Washington Post* proved unusable because it is so tightly focused on the daily life of Congress that almost no legislative activity escapes its notice. In 1997, for example, the *Post* ran nearly 900 editorials mentioning Congress, the House, or the Senate; in contrast, the *Times* ran about 500.

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agenda, albeit the agenda of the political elite.[7] Specifically, I coded the legislative content of each editorial that mentioned Congress, the House, or the Senate and then used the issues mentioned to compile a list of agenda items for each Congress, tallying as well the number of editorials the *Times* ran on each issue. For example, the *Times* editorialized 65 times about the successful Civil Rights Act of 1964 (which it favored) and 48 times about the Tax Reform Act of 1986 (which it favored). Eight times it discussed the failed constitutional amendment to require a balanced budget (which it opposed) in the 97th Congress (1981–82). With these lists of potential enactments for each Congress, I then determined the fate of each issue: whether it died in committee, on the Senate floor, in conference, and so on, or whether it was enacted. Gridlock scores for each Congress were then calculated as the percentage of agenda items that were not enacted by the close of that Congress. Appendix B provides further details on this measure.

The agenda list was smallest in the 86th Congress (1959–60), a low of 70 issues, and largest in the 99th Congress (1985–86), a peak of 160 issues.[8] Most recently, in the 104th Congress (1995–96) the total was 118, a modest increase over the 94 issues during the Democratic 103d Congress (1993–94). Although one might expect the agenda to be significantly larger during periods of unified control, there is no significant difference in agenda size under unified and divided control: On average, unified governments have faced 107 issues, divided governments 122. Agenda size has increased steadily over time, however, as shown in Table 1. It jumped sharply at the onset of the activist era identified by Mayhew (1991), starting in 1961, nearly doubling between the 86th Congress (1959–60) and the 87th (1961–62).

Because the agendas developed from the editorials include issues that the *Times* supported as well as those it opposed, the lists are not a record of liberal initiatives supported by the newspaper. Furthermore, there is no bias toward issues that ended in stalemate rather than enactment: Of the 2,899 agenda items discussed over the fifty years, exactly half were enacted.[9] Also, the *Times* did not write more often about gridlocked issues than those clearly headed for passage; the Pearson's *r* correlation between the number of editorials written per issue and the final outcome of the issue is

Congress (Years)	Number of Issues on Agenda
80 (1947–48)	85
81 (1949–50)	85
82 (1951–52)	72
83 (1953–54)	74
84 (1955–56)	84
85 (1957–58)	89
86 (1959–60)	70
87 (1961–62)	129
88 (1963–64)	102
89 (1965–66)	96
90 (1967–68)	119
91 (1969–70)	144
92 (1971–72)	135
93 (1973–74)	133
94 (1975–76)	138
95 (1977–78)	150
96 (1979–80)	144
97 (1981–82)	127
98 (1983–84)	138
99 (1985–86)	160
100 (1987–88)	140
101 (1989–90)	147
102 (1991–92)	126
103 (1993–94)	94
104 (1995–96)	118

TABLE 1. Size of the Congressional Policy Agenda, 1947–96

only .11. The *Times* did write significantly more often about topics that made Mayhew's list of landmark laws. On average, the *Times* editorialized nine times about the 281 issues that became landmark laws, but fewer than three times about all other issues, which suggests that the number of editorials on an issue is a valid indicator of policy importance.

My measure of gridlock seems to avoid some of the vulnerabilities that Mayhew notes about his count of landmark enactments. First, Mayhew (1991, 41) states that his compilation of contemporary judgments ("Sweep One") "singles out one kind of legislative action and ignores others. It looks for the major direct innovative thrust ... but it overlooks the practices and logics of the appropriations process, imaginatively placed amendments, and incrementalism by way of small bills." My editorial method, in contrast, tends to capture these smaller legislative moments, including such issues as child vaccination (enacted in the 103d) and repeal of the oleomargarine tax (enacted in the 81st after stalemating in the 80th).

Second, the editorial measure does not favor certain policy areas over others, which Mayhew considers to be a weakness of his approach. For example, my method does not slight defense weapons buildup, a policy area that tends to be overlooked by Mayhew's approach; the editorial measure detects fights over an alphabet soup of weapons production: the A-12 Avenger, the B-1 and B-2 bombers, the MX and Midgetman missiles, and so on.[10] Third, the editorial measure is not weakened by

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[7] I make no claim that the agendas generated from the editorials are exogenous to the policy process. Cook (1998, 13), among others, has observed that "the political agenda is set not by the media by themselves or by the members [of Congress] by themselves but by the two sides, whether working together or in competition."

[8] The peak in the 99th Congress seems to result from a higher than average appearance of certain types of policy issues on the pages of the *Times* in 1985 and 1986. In particular, the newspaper editorialized more frequently than normal on energy and public lands issues, and it voiced opinion on a larger than usual number of executive and judicial nominations pending before the Senate.

[9] Because many issues appear in more than one Congress, the 2,899 items are not a count of the number of *different* issues discussed by the *Times* over the period. For example, motor voter registration appears in the data set three times (101st, 102d, and 103d Congress) and was mired in gridlock until the 103d, when it was enacted.

[10] The editorials also pick up important regulatory statutes enacted

FIGURE 1. Level of Policy Gridlock in Congress, 1947-96
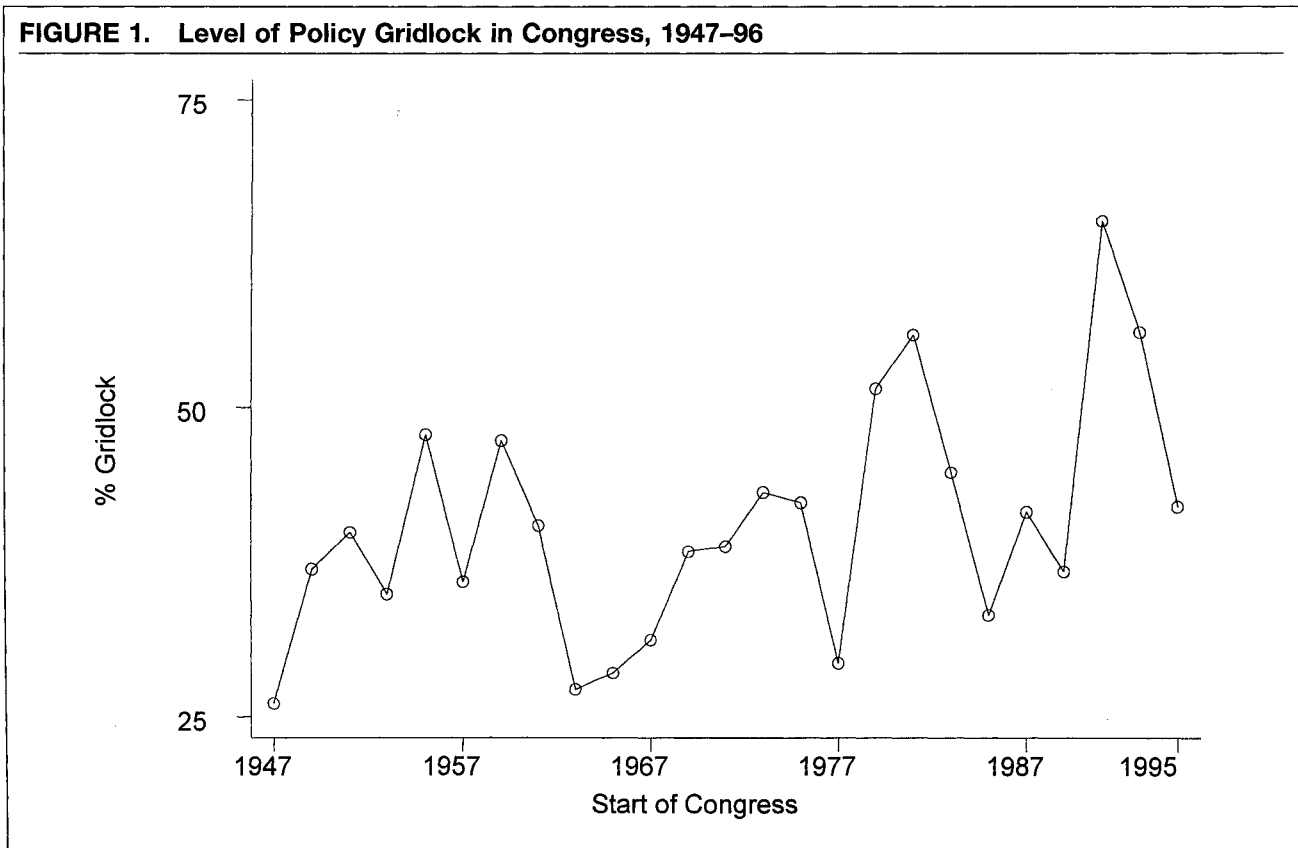
the problem of omnibus bills in the 1980s because the agendas for each Congress are based on particular issues, rather than final legislative packages. Finally, my method reduces the subjectivity problem encountered by Edwards, Barrett, and Peake (1997, 551), who note that identifying the potential significance of bills that, if passed, would have been important "remains a series of judgments rather than straightforward codifications." The problem of subjectivity is reduced by relying instead on the judgments of the nation's newspaper of record to identify salient policy issues.

Figure 1 plots the path of gridlock over fifty years. In calculating the gridlock scores, I applied a "salience filter" to eliminate minor agenda items (the filter is discussed in Appendix B). I selected only those issues on which the *Times* wrote four or more editorials and then calculated the percentage of these that failed to be enacted into law by the close of Congress. The filter yields numerators (number of failed agenda items) that range from 6 to 16 per Congress and denominators (total number of agenda items) that range from 16 to 39 per Congress.

Most important, the editorial measure generates gridlock scores that comport with the received wisdom about levels of policy stalemate. Using my method, two of the three most productive congresses were the 88th (1963-64), under presidents John F. Kennedy and

Lyndon Johnson,[11] and the 89th (1965-66), the Great Society Congress under Johnson, which stalemated on only 27% and 29% of their agendas, respectively.[12] In contrast, the two least productive were the 102d (1991-92), under President Bush, and the 103d (1993-94), under President Clinton, which failed to resolve 65% and 56% of their agendas, respectively.[13]

The gridlock scores also help resolve a puzzle noted by Mayhew (1991) when he compared the record of the Great Society Congress with that of the 93d (1973-74).

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[11] With typical bravado and remarkable foresight, Johnson predicted in 1964 that "when the record of this [88th] Congress is completed, it will place the 88th Congress in the record books as the most constructive in the twentieth century" ("Salute to Congress" 1964).

[12] The third and most productive is the 80th Congress (1947-48), with a gridlock score of 26%. Although President Truman termed this the "do-nothing" Congress, McCullough (1992, 696) argues in his biography of Truman that the record of the 80th suggests otherwise. Legislation passed included the Marshall Plan, National Security Act, Taft-Hartley Labor Act, Reciprocal Trade Agreements, and Selective Service Reform, while issues such as Hawaii statehood and antipoll tax legislation failed to be enacted.

[13] The *Washington Post's* assessment of these two congresses helps establish the reliability of the *Times* editorials in measuring gridlock. The *Post* stated that the 103d "will go into the record books as perhaps the worst Congress—least effective, most destructive, nastiest—in 50 years" ("Perhaps the Worst Congress" 1994). On the 102d: "When the final gavel fell Friday, Congress left behind one of the thinnest records of legislative achievement within memory.... Lawmakers brought a heavy load of legislation on a wide variety of issues to the verge of enactment, only to be thwarted by Republican filibusters ... or by presidential vetoes that could not be overridden" (Dewar 1992).

during the early 1970s that Mayhew notes his method misses. Of the seven such laws mentioned by Mayhew (1993, 488), the editorial method detects five.

Both produced 22 landmark laws (making them the most productive by Mayhew's count), but the 93d failed to earn "much of a reputation for legislative achievement" (p. 89). Mayhew reasons that because of the presence of divided government under Nixon and Ford, "Journalists . . . deprived of an opportunity for a carry-out-the-mandate-script, tended to reach for a deadlock-between-institutions counter-script that probably under-reported real legislative motion" (p. 90). But the 89th and 93d also differed in another important way: The legislative agenda of the 93d was 40% larger than that of the 89th, yielding a gridlock score of 43% for the 93d and only 29% for the 89th. Journalists likely were inclined to stress legislative conflict in the divided years of the 1970s, but they also probably emphasized deadlock in the 93d because it stalemated on so much of its large agenda. Accounting for both numerators and denominators, in short, affects the interpretation of legislative performance.

Independent Variables

Electoral/Partisan Hypotheses. Hypothesis 1 (divided government) is tested with a dummy variable denoting the presence or absence of divided party control of Congress and the presidency (*Divided Government* coded 1, 0 otherwise).[14]

Hypothesis 2 (party polarization) requires a measure that establishes legislators' relative ideological positions along a Left-Right spectrum. I use the first dimension of Poole and Rosenthal's (1997) W-NOMINATE scores to place legislators along such a continuum and to identify "moderates": those closer to the ideological midpoint between the two party medians in each chamber than to their own party median. This produces a count of the number of conservative Democrats and liberal Republicans in each House and Senate party in each Congress, which can be averaged to calculate the percentage of moderates in each chamber for every Congress. I take the mean percentage of House and Senate moderates to produce a single *Percentage of Moderates* for each Congress, a measure of legislator moderation over time. The lower the score, the smaller is the number of moderate legislators and the more polarized are the political parties. Ideally, the percentage of House and Senate moderates would be included in the analysis as separate independent variables, but the two series are highly colinear (Pearson's $r = .88$ over the past half-century), which would introduce an unacceptable level of multicolinearity into the analysis. Still, the resulting measure comports well with conventional wisdom about the recent disappearance of the political center (see, e.g., Cohen 1996; Silbey 1996), as it ranges from a high of 35% in the 91st Congress (1969–71) to less than 12% in the last two congresses.[15]

Hypothesis 3 requires a measure of homogeneity in

policy preference across the House and Senate (*Ideological Heterogeneity*). I average the standard deviation of the first-dimension W-NOMINATE scores for each chamber in each Congress (the House and Senate series correlate at .53), which produces an overall measure of relative policy preference homogeneity in Congress.

Finally, Hypothesis 4 (party mandate) requires a measure of the length of time a new congressional majority has been out of power (*Time Out of Majority*). I count the number of congresses a new majority (controlling both the House and Senate) spent in the minority, averaging the experiences of House and Senate parties. This averaging into a single series is again necessary due to the colinearity between the House and Senate series (Pearson's $r = .70$). When the new Republican majority took control in 1995, for example, Senate Republicans had been in the minority through four congresses and House Republicans through twenty, yielding a mean time out of power of twelve Congresses.[16]

Institutional Hypotheses. Hypothesis 5 requires a measure of the relative policy space between House and Senate over time (*Bicameral Distance*). Again, first-dimension W-NOMINATE scores are used to calculate the median score on the Left-Right dimension for each chamber in each Congress. The distance is thus measured as the absolute difference between chamber medians in each Congress (see Table 2).[17] The two bodies were closest during the 98th Congress (1983–84), when boll weevil Democrats dominated the House center and Republicans controlled the Senate. They were farthest apart in the 104th Congress (1995–96), when Senate Republicans lacked enthusiasm for the House Republicans' Contract with America. Indeed, the House passed 65% of all the issues on the legislative agenda in the 104th, while the Senate passed only half—the greatest such gap in the past fifty years.

To test Hypothesis 6 (filibuster), I calculate the *Severity of Filibuster Threat* in each Congress. I start with the number of filibusters in each Congress (see Binder and Smith 1997) to tap the extent to which this procedure is likely to limit legislative performance. Because filibusters vary in intensity and policy consequence, I next multiply the quantity in each Congress by the ideological distance between the median senator

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[14] The three congresses between 1981 and 1986 that couple a Democratic House and a Republican Senate and president are treated as a period of divided government.

[15] Note that although the conventional wisdom usually refers to the

size of the "political center," the "center" actually moves across time (Groseclose, Levitt, and Snyder 1999).

[16] For the congresses between 1981 and 1986, when the two parties split control of the House and Senate, the variable is coded 0, as no party had a majority in both chambers. When the Democrats assumed control of both the House and Senate in 1987, the variable takes the value of 1.5 (averaging three congresses spent in the minority for Senate Democrats and none spent in the minority for House Democrats.

[17] The assumption of a single dominant dimension of conflict is based on Tsebelis and Money (1997, chap. 3), who show that bicameralism (assuming Euclidean preferences) tends to privilege a single dimension of conflict, given the transaction costs of assembling alternative coalitions along other dimensions of conflict. "This [privileged] dimension," they argue, "expresses the differences between the two chambers, or the differences of the median voters of the two chambers" (1997, 89).

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TABLE 2. Bicameral Distance, 1947–96

Congress (Years)	Distance between House and Senate Medians
80 (1947–48)	.169
81 (1949–50)	.022
82 (1951–52)	.094
83 (1953–54)	.174
84 (1955–56)	.021
85 (1957–58)	.052
86 (1959–60)	.163
87 (1961–62)	.298
88 (1963–64)	.207
89 (1965–66)	.230
90 (1967–68)	.358
91 (1969–70)	.172
92 (1971–72)	.235
93 (1973–74)	.349
94 (1975–76)	.229
95 (1977–78)	.105
96 (1979–80)	.225
97 (1981–82)	.186
98 (1983–84)	.008
99 (1985–86)	.023
100 (1987–88)	.190
101 (1989–90)	.066
102 (1991–92)	.163
103 (1993–94)	.295
104 (1995–96)	.340

Note: Based on first-dimension W-NOMINATE scores (absolute distance between House and Senate medians).

and the farthest filibuster "pivot."[18] I assume that the greater the distance between the median and the filibuster pivot, the tougher it is to resolve policy and political differences between the majority and the filibustering minority.

Policy Hypotheses. As did Mayhew (1991), I measure the health of the budgetary climate (*Budgetary Situation*) as the size of the federal budget surplus or deficit as a percentage of federal government outlays (Ragsdale 1996, 354). To tap national sentiment, I use Stimson's annual measure of domestic policy mood, which is derived from opinion surveys (Stimson 1991, 1999).[19] Higher values reflect stronger preferences for an activist federal government.[20] Following Stimson, MacKuen, and Erikson (1995) and Taylor (1998), I lag the *Public Mood* variable by one Congress. As Stimson, MacKuen, and Erikson (1995, 546) note, "this cannot rule out the possibility that opinion responds to policy, but it definitely precludes the possibility of opinion responding to current policy."

Estimation

Because the dependent variable is constructed from grouped data (total number of failed legislative issues per Congress, divided by the total number of policy issues on the agenda each Congress) with unequal size groups, the OLS assumption of uniform variance is violated. That is, given agendas of varying size over the 25 congresses, heteroskedasticity will be present across the disturbances. The solution in this case is to model variation in gridlock with weighted least-squares estimates in a grouped logit equation.[21]

RESULTS AND DISCUSSION

The results of the grouped logit model are shown in Table 3, column 1. Overall, the model performs well, explaining 54% of the variance in gridlock levels over the past half-century.[22] Moreover, the model estimates show strong support for the partisan/electoral hypotheses and mixed support for the institutional and policy accounts. Most prominently, the coefficient for divided government is positive and statistically significant: Divided governments are prone to higher levels of gridlock. Although Mayhew (1991) convincingly shows that divided and unified governments do not differ in the quantity of landmark laws enacted, divided party control does appear to affect the broader ability of the political system to address major public problems. In this sense, the "party-government school" (Ranney 1954) that advocates "responsible" parties is vindicated. As Wilson (1911), Key (1942), Schattschneider (1942), Sundquist (1988), and many other scholars have argued over the past century, deadlock is more likely when the two major parties split control of Congress.

But interbranch conflict is not the sole factor. Partisan polarization and ideological diversity both contribute to policy stalemate. The effect of party polarization is perhaps the most striking. Despite the faith of responsible party advocates in *cohesive* political parties, the results here suggest that policy change is *less* likely as the parties become more polarized and the percentage of moderate legislators shrinks. Clearly, there are limits to the power of political parties to break policy deadlock. Indeed, it appears that intense polarization can be counterproductive to fostering policy change. Still, the semblance of a party mandate matters: The longer a new congressional majority has been out of power, the lower is the level of policy gridlock under the new majority.

The magnitude of these effects can be seen by simulating expected levels of gridlock, given specified changes in the values of the independent variables. The

[18] Senate medians are determined from Poole and Rosenthal's W-NOMINATE first-dimension scores. Filibuster pivots before 1975 are located at the 33d and 67th percentiles; after 1975, at the 40th and 60th.

[19] Like Taylor (1998), I average Stimson's annual mood scores to create a public mood score for each Congress. Because the data start in 1952, the variable takes the value for 1952 for the 82nd Congress (1951–52).

[20] Mayhew (1991) used a dummy variable to denote the Congresses between 1961 and 1976 as a period of particularly activist government.

[21] The models below are run with the glogit routine of Stata 6.0. Glogit estimates weighted least squares, accounting for the different sized denominators (total number of agenda issues) and, in principle, different variances across congresses. Because the percentage data are bounded between 0 and 1, the logit function is more appropriate than weighted least squares through OLS.

[22] A Dickey-Fuller test strongly rejects the possibility that a unit root exists. Durbin-Watson, Breusch-Godfrey Lagrange multiplier, and Portmanteau Q tests fail to reject the null hypothesis of no first- or second-order autoregressive or moving average (ARMA) errors (see Greene 1997, chap. 13).

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TABLE 3.   Determinants of Policy Gridlock, 1953–96

Hypothesis	Variable	(1) Coefficient (SE)	(2) Change in x (from, to)	(3) Net Change in Expected Probability of Gridlock
Partisan/ Electoral	Divided government	.340* (.142)	(0, 1)	+8%
	Percentage of moderates	−.027* (.013)	(18.47, 33.67)	−10%
	Ideological diversity	6.263* (2.710)	(.47, .55)	+11%
	Time out of majority	−.177** (.049)	(0, 2)	−9%
Institutional	Bicameral distance	2.263** (.818)	(.07, .30)	+13%
	Severity of filibuster threat	.035 (.039)	(0, 7.5)	+6%
Policy	Budgetary situation	−.006 (.009)	(−19.02, −2.09)	−2%
	Public mood (lagged)	−.034* (.016)	(55.76, 65.20)	−8%
	Constant	−1.509 (1.587)		
	N	22		
	F	4.10**		
	Adjusted R^2	.5413		
	Durbin-Watson d	1.921		
	Breusch-Godfrey			
	LM test (lag 1)	.0484		
	LM test (lag 2)	1.7842		
	Portmanteau Q	15.3574		

Note: The entries in column 1 are weighted-least-squares logit estimates for grouped data (standard errors in parentheses). Time series model diagnostics based on OLS-generated residuals. $*p < .05$, $**p < .01$; one-tailed t-tests. Net change in the expected probability of gridlock is calculated as the independent variables change between the values in column 2 (i.e., between one standard deviation below and above the mean value for each of the continuous variables and between 0 and 1 for the dichotomous variable). Simulated probabilities are based on the exponential linear predictions generated by the adjust routine in Stata 6.0, and they are calculated assuming the presence of divided government (all other variables are set at their mean values).

ranges of the independent variables appear in Table 3, column 2, and the associated changes in gridlock appear in column 3. Of the four partisan/electoral variables, a change from unified to divided party control has the smallest effect (increasing the level of gridlock by 8%), although the range of predicted probabilities of gridlock for the four variables is relatively small. Increased ideological diversity has the greatest influence (boosting gridlock 11%), followed by the effect of a larger number of moderate legislators. As the percentage of moderates in Congress climbs from less than one-fifth to one-third of all House and Senate members, the predicted level of gridlock falls by 10%. Such results confirm the sentiments of the many members of Congress and observers who claim that partisan polarization limits the legislative capacity of Congress (see Cohen 1996; Grove 1996; Serafini 1995). The "incredibly shrinking middle"—as Senator John Breaux called it (Serafini 1995)—seems to hamper substantially the ability of Congress and the president to reach agreement on the issues before them.

Turning to the institutional variables, the bicameral context matters greatly. Even after controlling for the effects of elections on partisan alignments, when ideological differences between the House and Senate increase, Congress finds it harder to reach agreement on pressing policy issues, and policy stalemate climbs. Indeed, bicameral differences have the greatest substantive influence on the level of gridlock: As the distance between the House and Senate increase fourfold along the Left-Right spectrum, gridlock rises by 13%. This helps explain why students of Congress may have been "overly optimistic" (Sundquist 1995, 10) about the prospects for governance under unified government in the 103d Congress (1993–94). Only twice before in the postwar period were the House and Senate as far apart ideologically as they were in the 103d, and the last occurrence was twenty years previously (see Table 2). Given the high level of partisan polarization, it is no wonder that seasoned observers concluded at the close of the 103d: "The only good news as this mud fight finally winds down is that it's

hard to imagine much worse" ("Perhaps the Worst Congress" 1994). While others have highlighted the constraining effects of supermajority rules to account for "unified gridlock" (Brady and Volden 1998; Krehbiel 1998), bicameral constraints clearly help determine the level of stalemate under unified regimes.

The results offer no support for hypothesis 5. The filibuster threat coefficient, although positive, fails to reach statistical significance. As shown in columns 2 and 3 of Table 3, moderate increases in the severity of the filibuster threat boost gridlock only 6%. Still, simulating levels of gridlock across more extreme differences in filibustering activity is instructive. Comparing a Senate with no filibusters (the 84th Congress in 1953–54) to the one with the most (22 in the 104th Congress in 1995–96), the predicted level of gridlock jumps from 42% to 53%. Although the effect of supermajority rules is swamped by other sources of variation in the legislative arena, it is premature to reject the hypothesis that Senate supermajority institutions have strong policy and political consequences.

Finally, there is mixed support for hypotheses 7 and 8. Mayhew (1991) found that major lawmaking was significantly and substantially higher during the activist era of the 1960s and 1970s. Substituting a measure of public opinion for Mayhew's "activist era" dummy variable still yields statistically significant results: A ten-point jump in public preference for activist government lowers gridlock by 8%. Conversely, as Mayhew (1991) also found, the budgetary situation has little effect on the legislative record of Congress and the president: As the budget shifts from large deficits to near surplus levels (column 2), gridlock eases by a mere 2%.

Table 4 presents alternative specifications of the model to test whether the results are artifacts of the period and measurements chosen. First, I reran the analysis using an alternative measure of public mood (column 1). Because the Stimson data begin in 1952, including a lagged public mood variable limits the model in Table 3, column 1, to the period from the 83d Congress (1953–54) through the 104th (1995–96). To test the model since 1946, I substituted Mayhew's activist era dummy variable for the lagged public mood variable (Table 4, column 1). The results show that the estimates are robust to the two alternative indicators of public mood: Gridlock is significantly lower when national sentiment favors governmental solutions to policy problems. Moreover, each of the statistically significant coefficients in Table 3, column 1, remains significant in the new model, with the exception of moderates and ideological diversity. Because the percentage of moderate legislators and the activist era dummy variables are correlated at .65, multicolinearity between the two may account for the differences across the two models.

Second, in Table 4, column 2, I test an alternative measure of party control, using two dummy variables to distinguish different forms of divided government. *Quasi-divided Government* is coded 1 during the three congresses between 1981 and 1986 in which the Republicans controlled the Senate and the Democrats controlled the House, 0 otherwise. *Pure Divided Government* is coded 1 during the 12 congresses in which a

TABLE 4. Determinants of Policy Gridlock, Alternative Specifications			
Hypothesis	Variable	(1) Coefficient (SE)	(2) Coefficient (SE)
Partisan/ electoral	Divided government	.311* (.130)	—
	Pure divided government	—	.364* (.141)
	Quasi-divided government	—	.053 (.278)
	Percentage of moderates	−.017 (.014)	−.025* (.013)
	Ideological diversity	4.339 (2.563)	7.591** (2.891)
	Time out of majority	−.152*** (.042)	−.189***
Institutional	Bicameral distance	2.358** (.867)	2.203** (.807)
	Severity of filibuster threat	.016 (.032)	.032 (.039)
Policy	Budgetary situation	−.005 (.008)	−.014 (.001)
	Activist era	−.396* (.210)	—
	Public mood (lagged)	—	−.040* (.017)
	Constant	−2.582* (1.404)	−1.892 (1.595)
	N	25	22
	F	4.12**	3.92*
	Adjusted R^2	.5095	.5556
	Durbin-Watson statistic	2.18	2.04

Note: The entries in each cell are weighted-least-squares logit estimates for grouped data (standard errors in parentheses). The Durbin-Watson statistic is based on OLS-generated residuals. *$p < .05$, **$p < .01$, ***$p < .001$; one-tailed tests.

single party controlled Congress (and the other party controlled the presidency), 0 otherwise. In this specification, pure divided party control of government yields significantly higher levels of policy gridlock than does unified or quasi-divided control. In contrast, the level of policy stalemate under quasi-divided control is indistinguishable from that of other periods.

Finally, the results help make sense of the divergent conclusions reached by Mayhew and many of his critics. Although Mayhew found no effect of divided government on the production of important legislation, it clearly dampens the legislative performance of Congress and the president: A greater percentage of the agenda is likely to be killed under divided than unified government. Focusing solely on what is enacted, rather than on the agenda facing Congress, risks understating the effects of divided government. As much was suggested by Edwards, Barrett, and Peake (1997) in their analysis of the effect of divided government on the

passage of legislation opposed by the administration. Still, as they conjecture (p. 562)—but do not model—divided government is "only one of the many obstacles that legislation faces on the path to enactment." In this sense, Mayhew (1991) is precisely right: The effects of divided government are challenged by alternative sources of variation in American politics. The theory of gridlock presented here confirms the effect of divided government on legislative performance, as well as the contours of alternative influences.

CONCLUSION

Unified party control of government cannot guarantee the compromise necessary for breaking deadlock in American politics. As Mayhew (1991, 198–9) argues, looking solely at the *structural* component of the political system—the separation of powers between Congress and the president—tends to obscure important dynamics in American lawmaking. As the analysis here suggests, the *pluralist* component deserves more attention. The distribution of policy views within and across the two major political parties has predictable and important effects on the legislative performance of Congress and the president. The temporal dimension of party politics matters as well; after long frustration as the minority, a new majority may capitalize on electoral mandates after gaining unified control of Congress. Intrabranch politics, it seems, may be as important to explaining policy deadlock as the usual suspect, conflict between the branches.

Perhaps the most important source of intrabranch disagreement is bicameralism. This structural component, rather than the separation of power between executive and legislative branches, seems particularly relevant to the dynamics of policy gridlock in the postwar period. To be sure, both the separation of powers and bicameralism were central to beliefs about the proper construction of political institutions in 1787 (Lee and Oppenheimer 1999). Still, with important recent exceptions, the policy consequences of divided government, not bicameralism, feature prominently in theoretical and empirical treatments of legislative gridlock. But treating bicameral bodies as if they were unicameral risks overlooking important differences. House-Senate differences, not simply legislative-executive conflicts, have structured patterns of gridlock in postwar American politics. Of course, this raises the question of whether the model presented here is robust across earlier eras of American politics, a topic that requires considerably more data on historical agendas and policy outcomes, but one that deserves a careful look in the future.

APPENDIX A: A COMPARISON WITH MAYHEW

Comparison of Dependent Variables

As discussed above, Mayhew (1991) uses a two-stage process to determine the quantity of major legislation passed by each Congress. His method generates 300 landmark laws enacted

TABLE A-1. Predicting the Production of Landmark Laws, Comparison to Mayhew (1991)

Variable	Coefficient (SE)
Divided government	−.389 (1.663)
Percentage of moderates	.513** (.150)
Ideological heterogeneity	−46.814 (29.897)
Time out of majority	.359 (.531)
Bicameral distance	5.382 (9.619)
Severity of filibuster threat	1.149 (.467)
Budgetary situation	.332** (.111)
Public mood (lagged)	−.015 (.183)
Constant	22.108 (16.661)
N	22
F	5.79**
Durbin-Watson d	2.01

Note: The entries in each cell are unstandardized regression coefficients (standard errors in parentheses). *$p < .05$, **$p < .01$; one-tailed t-tests. Because Mayhew's count of landmark laws is the dependent variable in this analysis (instead of the level of gridlock), the expected direction of the coefficient estimates is reversed from Table 3 (e.g., a larger percentage of moderates should now lead to higher levels of landmark enactments).

between 1947 and 1996 (Mayhew 1991, 1997). My method of using *New York Times* editorials to determine the legislative agenda for each of the 25 congresses in that period captures 94% of Mayhew's landmark laws; for nine of the congresses, the editorials discuss 100% of the landmarks. Of the 15 landmark laws that received no editorial mention by the *Times* between 1947 and 1986, 11 were identified by Mayhew from retrospective judgments of policy experts during his Sweep Two (such evaluations were not yet available for the period after 1986 at the time Mayhew published his 1991 work). To some extent, then, our lists differ due to Mayhew's use of retrospective policy judgments. Still, the editorials do mention numerous laws identified by Mayhew during Sweep Two, such as ratification of the Nuclear Nonproliferation Treaty in 1969, the National Forest Management Act of 1976, and the Job Training Partnership Act of 1982.

Comparison of Results

In Table A-1 I replicate the analysis in Table 3, column 1, substituting Mayhew's count of landmark laws per Congress for the dependent variable. The model explains 65% of the variance in landmark laws. The coefficients for the percentage of moderate legislators and the budgetary situation are statistically significant and in the expected direction. Just as a larger pool of moderates lowers the level of gridlock, so ideological moderation spurs greater production of landmark

laws; and budgetary slack increases the output of important legislation. My analysis confirms Mayhew's finding that divided government does not have a significant effect on major lawmaking, and the other coefficients in the analysis do not reach statistical significance in the expected direction. This suggests that a consideration of the broader universe of "potential enactments" significantly alters our view of legislative dynamics. Although divided party control of government, bicameral differences, and other factors may have little effect on the *quantity* of landmark laws enacted, these forces significantly enhance the ability of legislators to block salient legislative proposals they oppose.

APPENDIX B: MEASURING GRIDLOCK

Generating Gridlock Scores

To measure gridlock, I generated a list of salient agenda items in each Congress between 1947 and 1996 and determined whether each item was resolved by enactment of legislation in that Congress. The level of gridlock for each Congress is thus calculated as the number of failed agenda items in each Congress, divided by the total number of items on its agenda. I used daily editorials from the *New York Times* between 1947 and 1996 to identify the agenda issues, selecting every editorial that mentioned Congress, the House, or the Senate (editorials were retrieved from microfilm for 1947–80 and from Lexis-Nexis for 1981–96). The editorials were coded to identify only those issues for which the *Times* mentioned, advocated, or opposed legislative consideration. This eliminated incidental mention (e.g., editorials discussing Supreme Court rulings that overturned a law enacted by Congress and editorials endorsing congressional candidates).

Editorials were then coded to determine the legislative issue at stake and the number of editorials written on the issue. As did Mayhew (1991), I dropped routine appropriations bills. Although the politics of appropriations can be used to study gridlock (Oppenheimer 1996), judgments about their legislative fate in this context are problematic, because it is impossible to code objectively whether the final level of funding constituted success or failure. In all, nearly 15,000 editorials were collected and coded by a team of five college interns, a research assistant, and the author. To ensure the reliability of the coding, editorials coded by interns were also coded independently by the research assistant or the author. Intercoder reliability averaged 87%. Coding discrepancies were reviewed and resolved by the research assistant and/or the author.

Each agenda item was then coded as a legislative success or failure. For most cases, legislative fate was readily apparent from the yearly editions of *Congressional Quarterly Almanac*. If the *Almanac* provided no or ambiguous coverage, numerous sources were consulted, including the Thomas website at the Library of Congress, Legi-Slate's on-line bill retrieval service, and the U.S. Congressional Hearings Data Set maintained by the Center for American Politics and Public Policy at the University of Washington. In most cases, it was relatively easy to match the *Times* discussion of an issue with its legislative fate. In the 104th Congress, for example, the *Times* called for passage of campaign finance reform, and no such legislation was enacted. Similarly, the *Times* opposed enactment of a bill making English the official language of the United States, and no legislation was enacted. In contrast, the *Times* advocated telecommunications, welfare, and immigration reform, and broad legislative packages were enacted on each by the 104th.

More difficult coding decisions arise when Congress and the president enact legislation that addresses only a portion of a larger issue. Many of these cases are not difficult to code, however, because the *Times* tends to adjust its editorials over the course of a Congress to discuss both legislative realities (small bills) and larger unresolved issues. The 104th Congress, for example, enacted a relatively narrow health care bill, the so-called Kennedy-Kassebaum Health Care Portability Act. It did not, however, address broader issues of medical insurance reform that have been on the agenda in recent congresses. In this case, the *Times* ran separate editorials on health insurance reform (eight editorials) and Kennedy-Kassebaum (six editorials). The former issue was coded as a failure; the latter, a success. Similarly, in the 101st Congress (1989–90), a tax package was passed that addressed only some of the broad issues. In this case, however, the *Times* ran separate editorials on both a capital gains tax cut (twelve editorials), which was excluded from the final package, and expansion of the earned income tax credit (four editorials), which was included. Thus, although Congress addressed only some of the tax matters, the two issues are easily coded as failure and success, respectively.

More difficult coding decisions are posed by legislation that significantly amends the original proposal. For example, in the 103d Congress (1993–94), Clinton proposed a major economic stimulus package that was later whittled down to a minor extension of unemployment benefits after a prolonged Senate filibuster. The outcome for the stimulus package was coded as failure, guided by analysis in *Congressional Quarterly Almanac*. When the *Almanac* proved inclusive about an outcome, news stories in the *New York Times* and *Washington Post* were consulted. If the result was still ambiguous, I erred on the side of coding the outcome as a success if elite opinion suggested that Congress had made a substantial effort to address the underlying issue. This minimized the risk of penalizing Congress and the president for compromising on legislation. Moreover, since I am interested in relative rather than absolute levels of gridlock, my primary concern was consistency in determining legislative success or failure.

Choosing a Salience Filter

To compare my analysis with previous research, it is essential to filter the agenda. First, to maintain consistency with Mayhew's (1991, 40) approach, I dropped all agenda items concerning executive and judicial nominations, internal congressional procedures, and foreign aid appropriations that involved no major statutory changes. As did Mayhew, I included treaties and constitutional amendments. Second, because the editorials generate agendas that encompass minor, major, and landmark issues, I developed the "salience filter." As shown in Table 1, the editorials generate legislative agendas ranging in size from 70 to 160 issues per Congress, with enactments ranging from 34 to 91 issues. In contrast, Mayhew's (1991) method of determining landmark laws generates a range of 5 to 22 laws per Congress.

Fortunately, the number of *Times* editorials per issue is a proxy for issue salience (for the 2,899 issues discussed over the 25 congresses, the mean number of *Times* editorials on an issue is 3.33, with a standard deviation of 4.85; the number of editorials on an issue ranges from 1 to 65). The proxy relationship is readily established by correlating the number of editorials per issue with several indicators of policy salience. First, based on a Pew Research Center database on public attentiveness to major news stories, the percentage of the public following an issue "very closely" rises with the number of editorials written on that subject by the *Times*. For 1993–96, $r = .341$ ($p < .05$, one-tailed test) (Pew Research

Center 1999). Second, issues that receive more *Times* editorials also generate more network news stories. For 1993–96, $r = .446$ ($p < .01$, one-tailed test) (Vanderbilt Television News Archive 1999). Third, as the number of editorials on an issue increases, the length of network news coverage for that topic increases as well. For 1993–96, $r = .437$ ($p < .01$, one-tailed test) (Vanderbilt News Archives 1999). Fourth, on average the *Times* wrote 8.7 editorials on issues that became landmark enactments (Mayhew 1991) but only 2.8 on all others. Finally, issues attracting only one or two editorials frequently had minimal national importance. The *Times*, for example, wrote one editorial on creation of Dinosaur National Monument in the 84th Congress (1955–56) and on zip code reform in the 97th (1981–82). More recently, establishing uniform poll closings was discussed once during the 103d Congress (1993–94). The relationship between salience and number of editorials, however, is not perfect. Of the 281 landmark laws identified by Mayhew that also appear in the *Times* editorials, 9.6% received only one editorial.

Using the number of *Times* editorials as an indicator of issue salience, I selected the minimum number to qualify an issue for inclusion in the analysis. To determine the cut-off point, I used several criteria. First, I sought to maximize the correlation of number of legislative enactments per Congress with Mayhew's count of landmark laws per Congress. Second, I sought to minimize the number of Mayhew landmark laws excluded by the choice of the cut-off point. Third, I sought to maximize variation in the dependent variable of gridlock. These criteria led me to choose four or more editorials as the threshold in calculating the gridlock scores. The slight cost of the filter is that 30% of the Mayhew laws received fewer than four editorials. (The median number of *Times* editorials per Mayhew law included in the agenda database is six.)

There is no uniform effect of choosing one cut-off point over another. When gridlock scores are based on one or more *Times* editorial per issue and the analysis in Table 3 is replicated, bicameral distance and divided government remain statistically significant, but the lagged public mood, ideological heterogeneity, time spent out of the majority, and percentage of moderates are not significant. In contrast, if the cut-off point is five or more editorials, each of these variables except ideological heterogeneity is significant. A full set of results for six different cut-off points is available from the author and at the *APSR* web site.

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