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# Lawmaking and Roll Calls

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*The ability to generate theories of lawmaking has not been matched by an ability to evaluate the success of these theories for explaining legislative reality. The principal problem in testing lawmaking theories is that many analysts use roll-call votes—or various measures based on roll-call votes—when, in fact, these votes are partly a cause and partly a consequence of the very things the theories seek to explain. This can lead to erroneous substantive conclusions and characterizations. I show how embedding the theoretical predictions of the party gatekeeping and veto pivot theories of lawmaking within a statistical model used to estimate ideal points yields a straightforward test; if the gridlock interval measured using votes on policies predicted by the theories is nonzero, the predictions of the theory are not supported by the observed data (and assumed behavioral voting model). Implementing the test reveals little support for either theory.*

The ability to generate theories of lawmaking has not been matched by an ability to evaluate the success of these theories for explaining legislative reality. Two leading accounts of lawmaking—Cox and McCubbins's (1993, 2005) party gatekeeping theory and the pivot theory of Brady and Volden (2006) and Krehbiel (1998)—yield starkly different predictions about what kinds of bills a legislature will pass. Unfortunately, we currently lack the empirical methods to determine which of these theories better explains empirical reality. The principal problem in testing these theories is that many analysts use roll-call votes—or various measures based on roll-call votes—when, in fact, these votes are partly a cause and partly a consequence of the very things the theories seek to explain.

Most tests use every recorded vote to measure legislators' policy preferences and calculate what appear to be theoretically implied quantities such as gridlock intervals (e.g., Binder 1999; Chiou and Rothenberg 2003; Coleman 1999; Covington and Bargen 2004; Epstein and O'Halloran 1999; Krehbiel 1998; Martin 2001; Segal 1997). Despite recent work focusing on the connections between lawmaking theories and voting behavior (e.g., the work on "roll rates" (Cox and McCubbins 2005)), existing tests of lawmaking theories largely ignore the fact that, in predicting which proposals are successful, the theories also predict who should vote to enact the policy change.

Some work does question the relevance of roll calls for assessing lawmaking theories, but current work fails to fully account for the consequences of the relationship between lawmaking theories and roll-call voting behavior. While some argue that roll calls cannot sensibly be used at all—Shepsle and Weingast note that ideal points "provide an inappropriate basis for testing the hypothesis of party differentiation . . . [because] . . . these scores are . . . endogenous to the legislative context" (1994, 173)—others proceed without addressing the problem (e.g., regressing legislative accomplishment measures on ideal point measures).

I show how relating the predictions of the party gatekeeping and pivot theories to the behavioral voting model used to analyze roll-call voting behavior produces very simple tests of the theories' predictions in terms of the distribution of estimated ideal points.[1] The ability to calculate a gridlock interval using ideal points constitutes evidence either that: (1) the theoretical prediction is unsupported by the observed data or (2) the measure is based upon roll calls outside the scope of the theory and therefore irrelevant for assessing the theory. The fundamental argument is that testing lawmaking theories using roll calls almost

[1]Other attempts to integrate theoretical predictions and statistical roll-call voting models include the examinations of party influence by Groseclose and Snyder (2000) and McCarty, Poole, and Rosenthal (2001), and Clinton and Meirowitz's (2004) study of strategic voting.

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certainly requires a much different, and simpler, test than is currently used.

I implement the tests and examine the support for the party gatekeeping and pivot theories in the 90th–106th Congresses, but the relevance of the argument extends beyond these two theories. First, the principle argument regarding the need to integrate theoretical and measurement models is applicable to many other contexts such as models of committee gatekeeping (Cox 2001; Dion and Huber 1996; Shepsle 1979; Shepsle and Weingast 1987) and presidential vetos (e.g., McCarty and Poole 1995). Moreover, the argument is relevant for assessing similar theories regarding a parliament, court, or deliberative body which implements outcomes via recorded votes (e.g., Schulz and Konig's 2000 investigation of gridlock in the European Union, Londregan's 2000 investigation of presidential agenda control in Chile, and Segal's 1997 study of the impact of the separation of powers on judicial decision making). Second, the argument is also clearly relevant for evaluating the appropriateness of including similar gridlock measures derived from roll-call behavior as explanatory variables in regression analyses.

## Two Lawmaking Theories

A debate of some ferocity in the Congressional scholarship concerns the nature of lawmaking. A prevalent argument is that the majority party leadership in the contemporary House exercises substantial influence through its ability to control the agenda. Although the details vary, Cox and McCubbins (1993, 2005) provide one of the most complete articulations. The argument is that legislators of the same party have correlated electoral fates by virtue of their party affiliation and that these correlated fates offer the possibility of collective electoral gain or loss depending on their record of legislative accomplishment. To solve the resulting collective action problem, party members endow a leader with the ability to punish members who act contrary to party interests through committee assignments and other institutional prerogatives. The party leader undertakes costly monitoring and organizational activities because of the rents resulting from agenda control; in return for solving the collective action problem the leadership can determine the agenda and determine which proposals are given access to the floor.

A competing claim is that the gains from collective action are insufficient to motivate party members to maintain cohesion. The result is a more individualistic environment in which policymaking depends on the preferences of individual members and institutional features such as the possibility of a filibuster and a presidential veto (Brady and Volden 2006; Krehbiel 1998). 1

A fair characterization of the empirical literature is that a consensus has yet to emerge as to which account, if either, is best supported (Smith (2000) provides a nice summary). Although by no means exhaustive, Cox and McCubbins (1993, 2005), Cox and Poole (2002) and Lawrence, Maltzman, and Smith (2006) find support for the party gatekeeping theory, Krehbiel (1998), Schickler (2000), Krehbiel and Wiseman (2001), Wawro and Schickler (2004), and Brady and Volden (2006) find support for the pivot theory, and 1
Krehbiel, Meirowitz, and Woon (2005) find little support for either. As none of this work fully accounts for the theories' implications for legislators' voting behavior, it is unknown whether the conflicting results are due to methodological rather than theoretical errors.[2] In other words, is the lack of consensus a consequence of problematic measures?

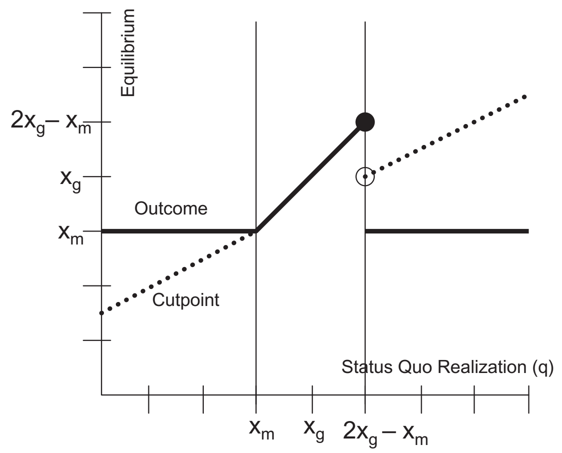
To clarify how the party gatekeeping and pivot theories yield predictions relevant for the behavioral model underlying the analysis of roll-call voting, I consider simple versions of the two theories. To highlight the generality of the argument, I develop the intuition using a general gatekeeping model and a model with veto players even through the specific theories I test assume gatekeeping by the majority party and restrictions imposed by the possibility of filibusters in the Senate and a presidential veto. The results are not novel, but restating the results highlights the connections to roll call behavior.

## Theoretical Predictions: Gatekeeping Theories

Assume a unidimensional policy space $X$. Legislators have single-peaked preferences in $X$ that are defined in terms of spatial proximity to policy outcomes and legislator i's most preferred outcome is $x_i \in X$. Consider gatekeeper $g$ with ideal point $x_g$ and the pivot required for passage—the median legislator in the case of a

[2]Ideal points are one of the most prevalently used measures, but they are clearly not the only measure used to test lawmaking theories. For example, Cox and McCubbins randomly sample 100 reported bills from the 82nd, 83rd, 92nd, and 97th and determine whether a majority party member sponsored the bill (1993, 260), Krehbiel (1998) uses coalition sizes, and Cox and McCubbins (2005) use roll rates and direction of policy change.

**FIGURE 1 Equilibrium Predictions from Gatekeeping Model**




majority voting rule—$m$ with ideal point $x_m$. For simplicity, suppose $x_m < x_g$.

The theoretical result is well known, and it has been widely used in congressional scholarship to model both committee (see, for example, Cox 2001; Crombez, Groseclose, and Krehbiel 2006; Denzau and Mackay 1983; Shepsle 1979; Shepsle and Weingast 1987; Snyder 1992) and majority party (see, for example, Cox and McCubbins 1993, 2005 and Aldrich 1995) agenda control. The play of the game is as follows:

- Nature draws a status quo policy $q \in X$ that is perfectly observed by all.
- Gatekeeper $g$ decides to allow or disallow action on $q$. If action is allowed, play continues. If action is disallowed, status quo policy $q$ is realized.
- If gatekeeper $g$ allows action, the chamber median $m$ makes chooses the proposal $p \in X$ to enact into law.

The Nash equilibrium to this game consists of a set of decisions by the gatekeeper and a set of proposals by the chamber median. The latter is trivial—under majority rule the chamber median is decisive and proposes her ideal point $p = x_m$ in every instance. The set of equilibrium actions by the gatekeeper is likewise trivial. Since any status quo for which action is allowed results in the policy $p = x_m$, the gatekeeper only permits actions for when $x_m$ is closer to $x_g$ than $q$. Consequently, if $x_m < x_g$, inaction should be observed for any realization of $q \in [x_m, 2x_g - x_m]$. The gatekeeper allows action on all other realizations. The solid line in Figure 1 denotes the equilibrium outcome $p^*$ for each status quo realization.[3]

Given an equilibrium prediction $p^*$ and a status quo $q$, the cutpoint for the enacting vote—i.e., the point where a legislator is indifferent between the two outcomes—is $|p^* + q|/2$.[4] The dotted line in Figure 1 plots the equilibrium cutpoint for the vote enacting $p^*$ for each status quo realization. Cutpoints are undefined between $[x_m, 2x_g - x_m]$ because gatekeeping prevents votes on these status quos.

## Theoretical Predictions: Pivot Theories

Whereas gatekeeping theories argue that negative agenda control largely determines the nature of possible policy change, pivot theories argue that policy change is most affected by the need for successful policy to circumvent a series of veto players such as the president (e.g., Cameron 2000; McCarty and Poole 1995) or the president and the filibuster pivot (Brady and Volden 2006; Krehbiel 1998). For exposi- 1
tion, suppose the chamber median $m$ has an ideal policy outcome denoted by $x_m$ and that there are two veto players denoted $l$ and $r$ with most preferred policies $x_l \in X$ and $x_r \in X$ respectively such that $x_l < x_m < x_r$.

With the same assumptions as above, typical play in a pivot game is as follows:

- Nature draws a status quo policy $q \in X$ that is perfectly observed by all.
- Chamber median $m$ decides whether to make a proposal $p \in X$. If so, she reports $p$, if not, the game ends and $q$ is realized.
- Veto player $l$ decides whether to veto $p$. If so the game ends and $q$ is realized. If not, play proceeds.
- Veto player $r$ decides whether to veto $p$. If so the game ends and $q$ is realized. If not $p$ is realized.

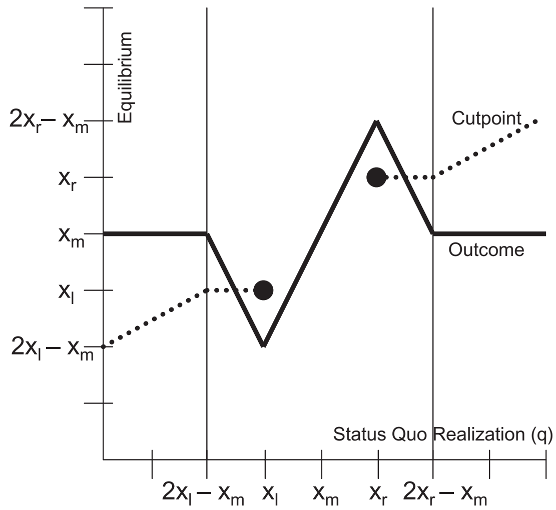
The equilibrium prediction is a unique mapping from the set of status quos to a set of policy outcomes. Figure 2 presents the equilibrium outcome (solid line) and cutpoint (dashed line) for every status quo realization.

For status quo realizations in the interval bounded by the minimum and maximum of the veto players' policy preferences $[x_l, x_r]$—the gridlock interval—policy change is impossible and no votes (and therefore cutpoints) are observed in equilibrium.

3 [3]Figures 1 and 2 summarize the results presented in Krehbiel, Meirowitz, and Woon (2004).

[4]Assuming both $p^*$ and $q$ are greater than (or less than) 0.

FIGURE 2 **Equilibrium Predictions from Pivot Model**




## A Statistical Voting Model

Legislators' policy preferences in the models of the previous section are latent primitives defined independently of actions. Measuring policy preferences is highly desirable because the models' equilibrium predictions are directly related to the preferences of certain legislators. To measure legislator preferences, scholars postulate a behavioral model and analyze the roll of legislators' actions—typically roll calls—using a statistical model assuming the behavioral model is true. The resulting ideal point estimates are then frequently used to measure the intervals of the prior section. Because the party gatekeeping and pivot theories' predictions are explicitly related to unobserved policy preferences, measuring policy preferences using legislators' votes requires: postulating a behavioral voting model, translating the assumed behavioral model into a statistical model, and analyzing observed votes to produce estimates of legislators' policy preferences.

To explicate the link between the lawmaking theories and the statistical voting model, it is useful to consider the behavioral and statistical voting model in some depth. For exposition, I use the model of Clinton, Jackman, and Rivers (2004), but the argument is not sensitive to the particulars of the statistical model.

Associated with each roll-call vote $t$ ($t = 1 \ldots T$) is a pair of locations in the policy space—one associated with a successful roll-call vote ($\theta_{y(t)}$), and one associated with an unsuccessful roll-call vote ($\theta_{n(t)}$). Given that the lawmaking theories of the previous section assume a unidimensional policy space, I restrict attention to a unidimensional statistical model.[5] The utility for legislator $i$ ($i = 1 \ldots L$) with ideal point $x_i$ is assumed to be given by: $U_i(\theta_{y(t)}) = -\|x_i - \theta_{y(t)}\|^2 + \eta_{it}$ and $U_i(\theta_{n(t)}) = -\|x_i - \theta_{n(t)}\|^2 + \nu_{it}$ where $\eta_{it}$ and $\nu_{it}$ represent the stochastic portion of the utility function and $\| \bullet \|$ represents the Euclidean norm.

Legislator $i$ votes yea if the utility from the policy resulting from a yea vote ($\theta_{y(t)}$) is greater than the utility they get from voting nay ($\theta_{n(t)}$). The latent utility differential for legislator $i$ on roll call $t$ is therefore: $y^*_{it} = U_i\left(\theta_{y(t)}\right) - U_i\left(\theta_{n(t)}\right) = -\|x_i - \theta_{y(t)}\|^2 + \eta_{it} - \theta_{n(t)}\|^2 + \nu_{it}) = 2x_i\left(\theta_{y(t)} - \theta_{n(t)}\right) - \theta^2_{y(t)} + \theta^2_{n(t)} + \varepsilon_{it}$ where $\varepsilon_{it} = (\eta_{it} - \nu_{it})$. Assuming $\varepsilon_{it}$ is iid $N\left(0, \sigma_t^2\right)$, and letting $\Phi()$ denote the standard normal CDF yields: $P_{it}(y^*_{it} = 1 | x_i, \theta_{y(t)}, \theta_{n(t)}) = \Pr(y^*_{it} > 0) = \Phi(\sigma_t^{-1}(2x_i(\theta_{y(t)} - \theta_{n(t)}) - \theta^2_{y(t)} + \theta^2_{n(t)}))$.

Assuming that legislators vote independently with respect to both indexes, conditional on $x$ and the $T$-length vectors of proposal locations $\theta_{y(t)}$ and $\theta_{n(t)}$, yields the probability of observing the $L \times T$ matrix of roll-call votes $\boldsymbol{Y}$:

$$\begin{aligned} \mathrm{L}(x, \theta) = \prod_{i=i}^{L} \prod_{t=1}^{T} \Phi(\sigma_t^{-1}(2x_i\left(\theta_{y(t)} - \theta_{n(t)}\right) - \theta^2_{y(t)} \\ + \theta^2_{n(t)}))^{Y_{it}} \times (1 - \Phi(\sigma_t^{-1}(2x_i\left(\theta_{y(t)} - \theta_{n(t)}\right) \\ - \theta^2_{y(t)} + \theta^2_{n(t)}))^{1-Y_{it}}) \end{aligned} \tag{1}$$

where only $\boldsymbol{Y}$ is observed. Simplifying to the underlying item response model yields the item discrimination parameter $\beta_t = 2\sigma_t^{-1}\left(\theta_{y(t)} - \theta_{n(t)}\right)$, the item difficulty parameter $\alpha_t = \sigma_t^{-1}\left(\theta^2_{n(t)} - \theta^2_{y(t)}\right)$ and the likelihood: $\mathrm{L}(x, \theta) = \prod_{i=i}^{L} \prod_{t=1}^{T} \Phi(\beta_t x_i - \alpha_t)^{Y_{it}} \times (1 - \Phi(\beta_t x_i - \alpha_t)^{1-Y_{it}})$. A choice of priors and a set of identifying restrictions completes the specification in the Bayesian context (see Clinton, Jackman, and Rivers 2004 for details).[6]

The cutpoint associated with each vote is the ideal point at which the probability of voting yea or nay is equal (i.e., the position of the indifferent legislator). In

[5]Cox and McCubbins (2005) note that they envision a multidimensional policy space (e.g., chap. 3), but they also assume a unidimensional space when they use first dimension DW-NOMINATE scores in their analysis (e.g., chap. 5).

[6]A Bayesian model enables the computation of any statistic of the posterior and yields an immediate assessment of the precision of each estimate. This offers two benefits. First, ideal point standard errors are estimated directly, and it is straightforward to determine whether ideal points differ from one another (see Lewis and Poole 2004 for a means of generating bootstrapped standard error estimates for NOMINATE). Second, an estimate and standard error for pivots can be computed while accounting for the uncertainty of the identity of each.

**TABLE 1 Theoretical Predictions: Gatekeeping (top) and Pivot (bottom)**

| Status Quo Location (q) | Proposal Attempted | Eqm. Outcome | Eqm. Cutpoint |
|---|---|---|---|
| $q < x_m$ | yes | $x_m$ | $q+\left(\frac{x_m-q}{2}\right)$ |
| $q \in [x_m, 2x_g - x_m]$ | no | $q$ | None |
| $q > 2x_g - x_m$ | yes | $x_m$ | $q-\left(\frac{q-x_m}{2}\right)$ |
| $q < 2x_l - x_m$ | yes | $x_m$ | $q+\left(\frac{x_m-q}{2}\right)$ |
| $q \in [2x_l - x_m, x_l]$ | yes | $2x_l - \text{q}$ | $q+\left(\frac{(2x_l-q)-q}{2}\right)=x_l$ |
| $q \in (x_l, x_r)$ | no | $q$ | None |
| $q \in [x_r, 2x_r - x_m]$ | yes | $2x_r - \text{q}$ | $q+\left(\frac{q-(2x_r-q)}{2}\right)=x_r$ |
| $q > 2x_r - q$ | yes | $x_m$ | $q-\left(\frac{q-x_m}{2}\right)$ |

terms of the model, this implies $\Phi(\beta_t x - \alpha_t) = .5$, or $\beta_t x - \alpha_t = 0$. Solving for x yields: $\alpha_t/\beta_t$.[7]

Having outlined the behavioral and statistical model used to analyze roll-call behavior, I now demonstrate how the predictions of the party gatekeeping and pivot theories can be integrated, and therefore tested, within this statistical model.

## Theoretical Implications for Roll-Call Estimates

Because roll calls are the mechanism by which policy change is enacted, predictions about which policies are successful also yield predictions about which legislators should support the change. That is, a prediction about whether policy $p$ passes is also a prediction about which legislators vote to enact $p$.

Cutpoints describe the ability to distinguish between legislator preferences using roll calls in a spatial voting model because they denote the location of a legislator indifferent between the status quo $q$ and the policy proposal $p$. If the cutpoint for vote $t$ lies between the ideal points of legislator $i$ and $j$, then $i$ and $j$ should vote differently from one another on vote $t$ because, by definition, one legislator is closer to $q$ and the other is closer to $p$.

Table 1 summarizes the equilibrium policy prediction $p^*$ and the implied cutpoint for every status quo realization $q$ for each theory.[8]

As Table 1 makes clear, predictions about equilibrium proposals are also predictions about which cutpoints should be observed and therefore which legislators should vote together. Put differently, because ideal points are a function of legislator induced preferences and the observed agenda, predictions of the party gatekeeping and pivot theories regarding equilibrium outcomes are also predictions about the distribution of estimated ideal points on votes enacting the outcomes.

The intuition is straightforward. Gatekeeping theories predict we should never observe a cutpoint in the interior of $[x_m, x_g]$ resulting in policy change.[9]

[7]Because cutpoints are computed using the ratio of the estimates $\hat{\alpha}/\hat{\beta}$, they are somewhat unstable (Bafumi et al. 2005). Consequently, the reported cutpoint estimates should be interpreted with caution. As the cutpoints are of second-order interest—inspecting the distribution of ideal points constitutes a sufficient test—the choice of estimator is based on computation speed. The parameterization in Clinton, Jackman, and Rivers (2004) can be estimated in C using *ideal .5* (Jackman 2004); estimating the alternative parameterization currently requires using winBUGS (Bafumi et al. 2005).

[8]The predictions assume that legislators are policy oriented and that proposals are costly. This rules out the possibility of non-successful proposals (as might happen if legislators are motivated by position-taking considerations).

[9]Testing whether the cutpoints lie in theoretically impossible locations of the policy space is an inferior test because it requires estimating legislators' preferences in the space. Krehbiel, Meirowitz, and Woon (2005) use ideal points based on the set of all votes for

Consequently, $g$ and $m$ should never vote differently on votes enacting a policy because $g$ will never allow an issue that divides them on the agenda. Because the gatekeeper and chamber median vote identically on votes enacting policy, their ideal points are indistinguishable. The width of the gatekeeping gridlock interval using ideal points based on these votes is *zero*. A nonzero interval indicates the presence of roll calls in which the party and chamber medians vote differently—an event which should not occur according to the theory.

Denoting the ideal point on the relevant roll-call agenda as $\hat{x}$ yields the prediction:

**Gatekeeping Theory:** $\hat{x}_g = \hat{x}_m$.

The predicted relationship holds only if we analyze roll calls for which the behavioral assumptions of the party gatekeeping and pivot theories apply. As the theories assume that preferences are defined over policy outcomes (as opposed to position-taking considerations), to identify the set of votes with the closest connection to the theories I only analyze successful final passage votes. This assumes that voting decisions on successful final passage votes reflect policy preferences.[10] Assessing the party gatekeeping theory requires estimating ideal points using all successful final passage votes and determining whether the ideal points of the chamber and majority party medians are statistically distinguishable.[11]

Using a subset of the roll-call record is unproblematic for two reasons. First, the votes I examine are the ones most closely related to the theories. Using additional votes requires reaching beyond the theories and imposing additional behavioral assumptions (e.g., votes on amendments and procedural issues reflect sincere policy outcome preferences). This results in a noisier test because of the possibility of incorrect behavioral assumptions. Second, increasing the number of relevant roll calls only increases the precision of the estimated ideal points. Holding the data-generating process constant, analyzing fewer votes produces more imprecise estimates and increases the likelihood that ideal points are indistinguishable (i.e., we cannot reject the theory). Because statistically distinguishable differences are evident in the results below, the results cannot be due to small samples.

In fact, a test of predictions result is immediately available if one is willing to use all roll calls to measure policy preferences. If so, the fact that existing gridlock interval estimates are nonzero is problematic for the theories. (Gridlock intervals constructed using DWNOMINATE and Common Space scores are decidedly nonzero; see, for example, Chiou and and Rothenberg 2003). Restricting the analysis to successful final-passage votes investigates whether the nonzero intervals are the result of including votes for which the predictions of the theories are not binding.

An analogous argument yields the prediction for pivot theories.

**Pivot Theory:** $\hat{x}_l = \hat{x}_m = \hat{x}_r$.

Testing the predictions of a pivot theory, of which the theories of Brady and Volden (2006) and Krehbiel (1998) are perhaps the most widely analyzed, requires estimating ideal points on the set of successful final passage votes and determining whether it is possible to distinguish between the pivotal legislators.[12] Because status quos in the gridlock interval cannot be changed, veto players should never vote against one another on successful enactments because such measures will never be proposed. Consequently, the ideal points of pivotal legislators should be indistinguishable.

Calculating the gridlock interval appears to require directly comparing Senate, House, and Presidential preferences which entails assuming either that members voting in both chambers have identical induced preferences or else that legislation considered in both chambers have identical status quos (see Bailey 2005 for a discussion of possible complications). As an alternative, I follow Krehbiel (1998, chap. 4) and calculate the pivot gridlock interval in the Senate assuming that the veto-override pivot is binding (i.e., the distance

this purpose, but nothing guarantees that the policy preferences relevant for lawmaking also structure behavior on procedural and amendment votes (see, for example, Smith and Roberts 2003). As it is unclear whether the ideal point estimates derived using the set of all votes represent policy outcome preferences, interpreting the relationship between the estimated cutpoints and the estimated ideal points becomes difficult.

[10]On nonfinal passage votes—and perhaps even on "insignificant" final passage votes (see, for example, Cox and McCubbins 2005, chap. 6)–it is clear that this relationship need not hold because of the possibility of other behavioral motivations on such votes (e.g., position-taking considerations may take precedence over policy outcome preferences). Neither theory precludes unsuccessful amendments from being offered and voted upon—amendments whose cutting line might lie between the pivots. Consequently, if the entire set of roll calls is analyzed, neither theory necessarily predicts a gridlock interval of width 0.

[11]An analogous test could be used to test theories of committee gatekeeping: identify the set of votes for which the theory is applicable (presumably policy proposals reported out of the committee), and determine whether the ideal points of the chamber median and the committee gatekeeper estimated on these votes are distinguishable.

[12]The theory makes slightly different predictions for budget-related legislation since they have statutory limits that preclude filibusters.

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between the 41st and 66th most liberal or conservative legislator depending on the party of the president). This test is sensible because it represents a necessary condition for the validity of the theory—if the theory is true, it should be true in the Senate—and it avoids the strong assumptions required to establish the comparability of the recovered space across institutions.

## Empirical Tests of Theoretical Predictions

To test the theoretical predictions of the models summarized above, scholars frequently use ideal point estimates. A prevalent application is to use ideal point estimates $\hat{x}$ to calculate the party gatekeeping (i.e., $|\hat{x}_m - \hat{x}_g|$) and pivot (i.e., $|\hat{x}_l - \hat{x}_r|$) gridlock intervals for use as independent variables in a subsequent regression analysis.[13] Measures of legislative accomplishment such as number of enacted important statutes or descriptions of the legislative environment such as roll rates are then often regressed on these, or related, gridlock measures to determine which quantity best is more correlated with the legislature's observed behavior. Despite widespread use, at least two problems exist.

First, because lawmaking theories have implications for both the dependent variable and for ideal point measures of the gridlock interval, using ideal points to measure the gridlock interval creates severe endogeneity problems. Roll calls, and roll-call based measures, are not exogenous because the theories being evaluated have direct implications for voting behavior on the enacting votes. Despite claims to the contrary, the consequences are severe—resulting in inconsistent coefficient estimates of unknown magnitude and direction (see, for example, Achen 1983). It is *not* the case that using ideal point estimates to test lawmaking theories necessarily results in attenuated coefficient estimates for the ideal point measure.[14]

A second problem is that the entire endeavor of calculating gridlock intervals for use in a regression analysis is of uncertain worth. Because the party gatekeeping and pivot theories yield predictions about the distribution of ideal points, the existence of a nonzero gridlock interval indicates: (1) the theory as characterized above does not perfectly describe the observed data or (2) the theory is true but the votes used to generate ideal points are irrelevant to the theory.[15] It is entirely unclear what introducing the width of a gridlock interval in a regression analysis accomplishes. Finding a nonzero gridlock interval using the set of final passage votes is either: (1) the only evidence required to assess the theory or (2) evidence that the votes and method are insufficient.

To implement the alternative tests I propose, I examine behavior in the 90th–106th Houses (1967–2000) and 90th–100th Senates (1967–89). The point of the analysis is not to resolve the "pivots-vs.-party" debate, but rather to suggest that at least some of the debate is being waged on the wrong terrain using problematic measures.

The first task is identifying the set of nonunanimous roll calls that are most theoretically relevant—the set of successful final passage votes.[16] Successful final passage votes are theoretically appropriate so long as legislators vote their policy outcome preferences (but see recent work by Krehbiel and Woon 2005). Using the determination of Rohde (2004) to identify the set of final passage votes in the House, I determine whether the vote passes (accounting for the two-third passage requirement for votes considered under Suspension of the Rules).[17] For the Senate, I use Katznelson and Lapinski's (2006) determination whether a vote was a final passage vote on a law.

[13]See, for example: Krehbiel (1998), Binder (1999), Epstein and O'Halloran (1999), Schickler (2000), Schulz and Konig (2000), Cox (2001), Cox and McCubbins (2002), Chiou and Rothenberg (2003), Covington and Bargen (2004), and Cox and McCubbins (2005).

[14]The results follows directly from any textbook once the consequences of endogeneity are accounted for. In terms of the proof (and notation) presented in section 5.6.1 of Greene (2003), for endogenous ideal points $x^*$ the result occurs because plim $(1/n)u_i\varepsilon_i = \sigma_{u,\varepsilon}$ (not 0 as in the printed proof). Consequently, plim $b = (\beta + \sigma_{u\varepsilon}/Q^*)/(1+\sigma_u^2/Q^*)$, not $\beta/(1+\sigma_u^2/Q^*)$ as in the attenuated case.

[15]A third possibility—that the theory is true, the correct votes are used, and that the probabilistic voting model used to estimate ideal points introduces sufficient error into the test—does not appear to matter. The reasoning is as follows. If every legislator is equally and independently likely of making a mistake then the ideal points based on an error-ridden roll-call matrix will be unbiased but imprecise. The consequence is that we may incorrectly fail to reject the theory because we cannot distinguish between the ideal points. If moderates are more likely to make a mistake than extremists than the same result is true so long as moderates are the relevant pivots. If extremists are more likely to err the error should not affect the estimation of the quantities of interest.

[16]Excluding unanimous votes—which are uninformative for estimating ideal points because of the lack of variation in voting—is unproblematic for theory testing because these votes are consistent with both theories.

[17]Using Rohde's (2004) classification of House roll calls for the 83rd through 106th Congresses, I use roll calls classified as: Final Passage/Adoption of a Bill, Final Passage/Adoption of a Conference Report, Final Passage/Adoption of a Resolution, Passage/Adoption of a Bill under Suspension of the Rules, or Passage/Adoption of a Resolution under Suspension of the Rules. To examine whether proposals considered under suspended rules sys-

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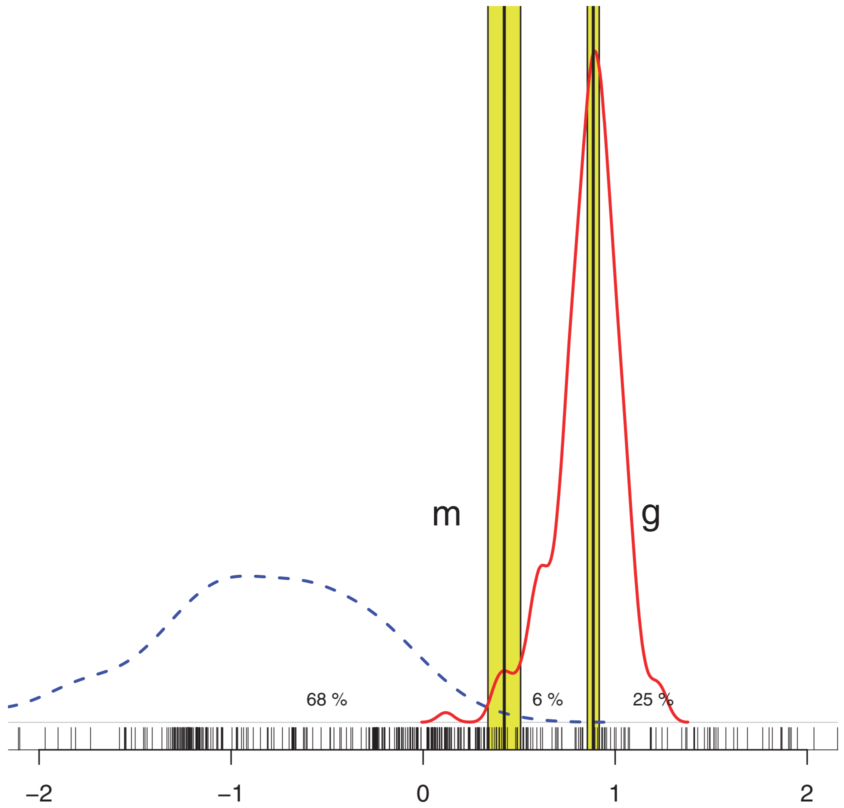
**Figure 3 Theoretically Implied Estimates for the 106th House: The figure graphs the density of Democratic (dashed) and Republican (solid) ideal points, the 95% quantiles for the chamber (m) and majority party (g) medians, the distribution of estimated cutpoints and the percentage located in each partition**

Colour Figure




Testing the predictions involves estimating two statistical ideal point models using the theoretically relevant roll calls—one that is unconstrained and one that imposes the ideal point equality constraints noted above—and testing whether the constraints are valid (Clinton and Meirowitz 2003). This appears difficult because the identities of the legislators whose ideal points should be constrained are unknown. An alternative approach involves estimating an unconstrained model and testing whether the relevant ideal points are statistically distinguishable.

Figure 3 presents the results graphically for the 106th House based on the 241 successful final-passage votes.[18]

The results are immediately evident and discouraging for the party gatekeeping theory. There is no doubt that the estimated chamber median (m) and majority party median (g) are not identical. The fact that the illuminated 95% regions of highest posterior density (i.e., Bayesian "confidence interval") for the party and chamber medians does not overlap indicates that the 241 successful final-passage votes contain enough information to distinguish between the chamber and majority party medians. In fact, the estimated difference between the chamber and Republican party median is .46 with a 95% HPD interval of [.40,.55]—far from 0 as the theory predicts. The distribution of estimated cutpoints (plotted along the x-axis) reveals that 6% have posterior medians in the gridlock interval and many more have a substantial probability of lying in the interval.

tematically differ—perhaps because they are of lower salience—the online appendix demonstrates that omitting these votes fails to change the results.

[18]I estimate ideal points using the statistical model imposing the identification restriction that the ideal point estimates have mean zero and unit variance (Rivers 2003). For each analysis, I generate 1,000,000 samples thinning by 2,000 using IDEAL .5 (Jackman 2004). Although the identification restriction means that comparisons across congresses and chambers is impossible, such comparisons are unnecessary for the tests I conduct. NOMINATE results are substantively identical.

TABLE 2 Testing Party Gatekeeping Using Successful House Final-Passage Votes

| Congress | Roll Calls Analyzed | Avg. Coalition Size | Distance [95% HPD] | Pct. Invalid Cutpoints |
|---|---|---|---|---|
| 90 (1967–1969) | 208 | 302 | .79 [.72, .86] | 25.3% |
| 91 (1969–1971) | 207 | 302 | .59 [.52, .66] | 23.2% |
| 92 (1971–1973) | 254 | 305 | .53 [.46, .59] | 9.9% |
| 93 (1973–1975) | 387 | 325 | .63 [.57, .70] | 9.2% |
| 94 (1975–1977) | 458 | 319 | .53 [.48, .58] | 5.9% |
| 95 (1977–1979) | 447 | 324 | .46 [.41, .51] | 6.6% |
| 96 (1979–1981) | 317 | 304 | .51 [.45, .57] | 6.8% |
| 97 (1981–1983) | 176 | 315 | .56 [.49, .62] | 19.4% |
| 98 (1983–1985) | 218 | 323 | .47 [.41, .53] | 19.5% |
| 99 (1985–1987) | 142 | 317 | .57 [.50, .64] | 24.0% |
| 100 (1987–1989) | 210 | 324 | .54 [.47, .60] | 25.1% |
| 101 (1989–1991) | 178 | 328 | .56 [.49, .63] | 25.6% |
| 102 (1991–1993) | 198 | 318 | .46 [.41, .53] | 25.6% |
| 103 (1993–1995) | 200 | 318 | .47 [.42, .52] | 19.0% |
| 104 (1995–1997) | 205 | 320 | .44 [.39, .49] | 17.1% |
| 105 (1997–1999) | 210 | 323 | .57 [.51, .62] | 16.2% |
| 106 (1999–2001) | 241 | 332 | .46 [.40, .55] | 6.2% |

TABLE 3 Testing Pivot Theory Using Successful Senate Final-Passage Votes

| Congress | Roll Calls Analyzed | Avg. Coalition Size | Distance [95% HPD] | Pct. Invalid Cutpoints |
|---|---|---|---|---|
| 90 (1967–1969) | 83 | 68 | .77 [.59, .97] | 16.4% |
| 91 (1969–1971) | 94 | 67 | .98 [.76, 1.17] | 27.8% |
| 92 (1971–1973) | 118 | 69 | .82 [.63, 1.03] | 27.2% |
| 93 (1973–1975) | 205 | 72 | .13 [.10, .19] | 1.2% |
| 94 (1975–1977) | 173 | 69 | .15 [.10, .20] | 5.5% |
| 95 (1977–1979) | 149 | 72 | .32 [.23, .44] | 1.0% |
| 96 (1979–1981) | 148 | 71 | .19 [.13, .29] | 1.7% |
| 97 (1981–1983) | 93 | 73 | .67 [.47, .90] | 13.0% |
| 98 (1983–1985) | 60 | 76 | .58 [.41, .74] | 19.3% |
| 99 (1985–1987) | 48 | 74 | .55 [.40, .74] | 21.1% |
| 100 (1987–1989) | 86 | 79 | .75 [.56, .93] | 5.8% |

Having illustrated the analysis using the 106th House, I replicate the analysis for the 90th through the 105th Houses. For each House, Table 2 reports: the number of successful final-passage votes, the average size of the enacting coalition, the distance between the chamber and majority party medians (with 95% Highest Posterior Density estimates), and the percentage of votes with a (posterior median) cutpoint in this interval.

Although the average coalition sizes are all greater than 50% because the sample includes only successful votes, the question of interest is: Are the enacting coalitions across these votes consistent with theoretical predictions? The results are unambiguous. Table 2 reveals nonzero gridlock intervals in every case and the intervals are sizable—ranging from .44 (104th) to .79 (90th). In contrast to theoretical predictions, the chamber and party medians are always statistically distinguishable. As is to be expected given these differences, the percentage of successful final passage votes with cutpoints estimated to lie in the interior of the party gridlock interval ranges from 5.9% to 25.6%. (These are lower bounds because votes with cutpoint estimates exterior to the interval have substantial probability mass over the interval).

To test the corresponding prediction for the pivot theory, I replicate the analysis for the 90th-100th Senate and calculate the difference between the filibuster and veto-override pivot. Table 3 presents the results. In every instance, the size of the gridlock interval is distinguishable from zero. The differences in Table 3 appear smaller than those of Table 2, but recall that the identification strategy makes both cross-chamber and over-time comparisons impossible.[19]

Using theoretically suggested votes and a measurement model of roll-call voting consistent with the party gatekeeping and pivot theories reveals no support for the predictions of either theory. Extending the analysis in several ways fails to alter this conclusion. As the online appendix (at http://journalofpolitics.org) reports, using an alternative coding scheme of final-passage votes and excluding votes considered under suspended rules because they might be less relevant to the theories (perhaps because they are less salient?) fails to change the results. Using only successful final passage votes on domestic, nonappropriation policy to examine whether the differences are due to incorrectly assuming that votes are structured by a single underlying dimension in the statistical voting model also fails to change the results.

These results are not intended to resolve whether lawmaking is shaped by party institutions—such a resolution would require moving beyond roll calls, replicating the analysis across longer periods of congressional history, reconciling discrepancies in what constitutes "final passage," and potentially restricting the analysis to further subsets of roll calls (e.g., classifying votes according to the issues they raise or by the "importance" of the legislation being considered)—but the results suggest that neither performs particularly well.

The analysis does demonstrate the importance of accounting for the relationship between theoretical and measurement models. Ignoring the relationship between party gatekeeping and pivot theories and the behavioral assumptions underlying the analysis of roll-call behavior yields very different conclusions and may explain the conflicting substantive conclusions of prior studies making use of roll calls. Integrating the theoretical implications of the two theories into the roll-call measurement model reveals no support for either theory. For every subset of votes I consider in either chamber, the ideal point based gridlock interval is always nonzero.

[19]When interpreting the small percentage of cutpoints in the pivot gridlock interval recall that the percentage is a lower bound and excludes votes whose median may lie outside the interval but which still have a high probability of lying in the interval. Recall also that the cutpoint estimates are unstable for the reasons noted in footnote 7.

## Problems with Theory, Estimation or Data?

The tests of the previous section treat the two theories as exclusive. The possibility that the true account combines majority party gatekeeping with the constraints that the pivot theories suggest does not confound the analysis because the equilibrium preference based gridlock interval is either identical to the party gatekeeping gridlock interval (if the veto pivots lie between the party and chamber medians) or else the equilibrium interval is even larger. In either case, if the gridlock interval measured using ideal points is problematic for the party gatekeeping theory, it is also problematic for an integrated theory.

An obvious objection to the test I implement is that the estimator gives incorrect answers because the wrong votes are analyzed. Even though I use information to restrict the set of votes to those dealing with nonappropriation, domestic issues in the appendix, the estimated differences may be due to incorrectly projecting a multidimensional preference space into a single dimension. It is impossible to conclusively dismiss this possibility, but several reasonable reactions exist. First, if the estimated differences are due to incorrectly estimating the relevant policy space, existing uses of ideal points based on a more expansive usage of the roll-call record are also invalid for the same reasons. That is, if this is a problem for the analysis I conduct, it is presumably even more of a problem for existing measures using every roll call (i.e., measures constructed using DW-NOMINATE or Common Space scores). Second, the fact that we do not know the dimensionality of policy preferences underlying the roll-call record may make implementing the tests I propose difficult, but it does not invalidate the legitimacy of the tests applied to the properly restricted set of votes. The difficulty of implementing the test does not diminish its superiority over current uses of ideal points.

If identifying the set of votes relevant for theory testing is difficult, this provides reason for caution in using ideal points to test lawmaking theories; statistical models must be properly restricted to the set of votes for which the behavioral assumptions apply. I examine the results' robustness to alternative definitions of final passage, and I restrict the sample of analyzed votes in several ways in the online appendix, but the roll-call record can obviously be parsed in many alternative ways.

One "solution" would be to abandon ideal points and test theories using patterns evident in voting behavior without a statistical framework (e.g., party roll rates). Because the party gatekeeping theory predicts that the chamber median and majority party median should never vote differently on final passage votes, we can examine whether a majority of the majority party votes together. Testing the pivot theory similarly reduces to examining whether enacting coalitions on successful final-passage votes are consistent with the need to invoke cloture and override a presidential veto. The dimensionality of policy preferences is unconstrained in such investigations.[20]

Avoiding ideal points may escape the problems introduced by using estimates based on the wrong dimension, but the tests are crude and incorporate so little of the theoretical explanation so as to be almost uninformative with respect to the suggested causal mechanism. Is it really surprising that a majority of the majority party votes together on successful final-passage votes? Although consistent with the predictions resulting from party gatekeeping, this fact says nothing about the underlying mechanism (e.g., which legislators vote together). Likewise, is the fact that the smallest average coalition size is 67 for the Senates I examine strong or weak evidence for a self-described simple theory based on veto pivots? Finally, any performance pales in comparison to the analogous prediction of the median voter theory because a majority always votes together on successful final passage votes.

These reactions are not intended to dismiss the importance of alternative investigations, but rather to suggest that testing theories using measures only weakly related to postulated causal mechanisms results in weak conclusions and subsequent debates about the interpretation of the results (see, for example, Krehbiel 2000). In contrast, tests that utilize theoretically implied concepts are more powerful for evaluating claims concerning the causal mechanism. If measuring theoretically implied covariates introduces error as a consequence of making measurement assumptions that cruder tests avoid, both examinations are useful. If so, the arguments and conclusions of this paper are important and informative.

An additional caveat regards the interpretation of the results. Strictly speaking, the findings demonstrate that the data generating process is not perfectly described by the theoretical predictions. However, perfection is clearly a high, and perhaps unreasonable, standard. It is not completely unreasonable to argue that the observed differences are "small" given the crudeness and simplicity of the models, but absent additional work defining evidentiary standards, however, it is unclear what constitutes strong or weak disconfirming evidence for a theory.[21] The fact that a consensus has yet to emerge regarding the standard, and indeed the proper null model, for evaluating empirical evidence does not detract from the need to empirically assess theoretical predictions using measures that assume behavioral models consistent with the theoretical accounts being assessed.

Finally, the concerns I raise do not affect all uses of ideal points. If legislators' voting behavior is of interest rather than the preferences structuring voting behavior—as is the case for testing theories about voting behavior (e.g., Snyder and Ting 2003) and the literature on shirking (see, for example, Rothenberg and Sanders 2000) then the theoretical and measurement models do not conflict. In fact, ideal points are arguably exactly the right measure to assess accounts of position-taking (see, for example, Clinton 2006) because voting is the behavior of interest.

## Conclusion

The ability to generate theories of lawmaking has not been matched by the ability to conclusively evaluate theoretical predictions. One difficulty in achieving scientific consensus is the absence of theoretically consistent measures and disagreement about the evidence for theories of interest. The lack of consensus is consequential because the accumulation of knowledge and scientific progress depends on the ability to identify measures whose relationship to lawmaking theories is clear and uncontested (Kramer 1986).

Proper tests of theoretical predictions require that measures used to test predictions are generated using models that, at the very least, are not inconsistent with the theory being tested. This point has important implications for theory testing using roll calls because the theories being tested typically have implications for behavior on the enacting roll calls. In generating predictions about congressional lawmaking, the party gatekeeping and pivot theories necessarily generate predictions about legislator behavior on roll calls enacting the predicted policies. Existing uses of roll calls and roll-call measures to explain lawmaking activity fails to fully account for the fact that roll calls

[20] In fact, they assume voting is perfect and that every unique voting profile (i.e., enacting coalition) indicates an additional dimension in the policy space.

[21] For example, one might assess the strength of the conclusions by determining how much error must be introduced to the behavioral model to satisfy the equality constraints.

are partly a cause and partly a consequence of the very things lawmaking theories seek to explain.

Because the party gatekeeping and pivot theories have predictions for the legislative agenda, a nonzero gridlock interval measured using ideal points estimated on the set of theoretically relevant votes indicates either: (1) the theory does not (perfectly) characterize the data or (2) the theory is true but the votes used to generate the ideal points are irrelevant to the theory. In either case, it is unclear what the prevalent practice of using ideal point based measures of gridlock intervals in regression analysis accomplishes—if the gridlock interval is nonzero either the theory or the data is problematic. Accounting for the relationship between the theoretical and measurement models in the contemporary Congress reveals little support for either theory.

Despite this paper's focus on the party gatekeeping and pivot theories, the underlying argument is relevant for any use of roll calls to test theories with implications for voting behavior. That is, if a theory has implications for legislative outcomes, it also has implications for voting behavior on those outcomes that cannot be ignored when analyzing and using those votes.

Assessing the support for theoretical predictions using measures that are questionably related to theoretically implied concepts results in ineffective tests. Achieving consensus on measures is difficult in political science, however, because theoretically relevant quantities are almost never observed and the observed data is often faintly related to theoretical concepts. Similar to Poole's caution that "anyone can construct a spatial map . . . but the maps are worthless unless the user understands both the spatial theory that the computer program embodies and the politics of the legislature that produced the roll calls" (2005, 209), the arguments of this paper demonstrate that recognizing and accounting for the connections between the roll calls and lawmaking theories is essential for the proper use of ideal point estimates to assess lawmaking theories.

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