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Unpacking pivotal politics: exploring the differential effects of the filibuster and veto pivots

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Abstract Formal models of politics regularly combine assumptions about a variety of actors and institutions to produce equilibrium expectations, which serve as the primary target for empirical testing. Yet the underlying assumptions can vary in their accuracy among actors and across time and context. We focus on the pivotal politics model of lawmaking and argue that a full evaluation of the theory requires a granular analysis of its two primary components: the filibuster and veto "pivots" in Congress. We show that both types of pivots contribute to the success of pivotal politics in explaining postwar lawmaking, but that the relevance of each varies based on institution-specific contexts. Specifically, the filibuster pivot has little explanatory power before the 1970s, when norms of filibuster use were quite restrictive, while the veto pivot's explanatory power is limited to situations in which the president has sufficient public backing to be a force in the legislative process.

Keywords Pivotal politics · Lawmaking · Congress

1 Introduction

Formal models of lawmaking typically combine a multitude of actors and a series of procedural events that extend over months or years into a single, flat picture. Starting conditions—the spatial preferences of members—yield a particular set of equilibrium

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outcomes, and empirically testable predictions flow from them. When we test models with multiple components and find their predictions confirmed in data, we take that as some evidence that the model is broadly "correct". As such, the overall empirical effectiveness of a theory is applied to all of its constituent elements.

Consider, for example, the pivotal politics model of congressional lawmaking (Krehbiel 1998), a one-dimensional spatial model that has been very influential in the study of American politics. A key outcome of the model is that certain pivotal actors (those legislators who determine the success or failure of a filibuster and a veto override) form the boundaries of a "gridlock interval". The gridlock interval is a zone on the spatial line of status quo policies that cannot be modified through legislation owing to disagreement between veto players. Any status quo policy that is located within this interval is insulated from revision, because one of the pivotal actors would veto any policy change. Thus, as this interval gets larger and consumes a larger portion of the policy space, the set of status quos that can be altered decreases, making legislating more difficult.

An alternative way of thinking about—and testing—pivotal politics is that it makes two distinct predictions about congressional voting. The first is that the filibuster pivot will not approve of new legislation on any status quo policy that lies between its ideal point and the median's ideal point. This is because the model assumes an open rule and any new proposal would result in policy being shifted to the floor median, making the filibuster pivot worse off. A logically identical but still independent process happens elsewhere: the veto pivot will not approve of any legislation that addresses a status quo that lies between its ideal point and the median's ideal point. These two gridlocked intervals are then combined into a single "gridlock interval", with a single prediction: the larger this interval, the harder it should be to pass new legislation. Yet, the prediction just as well applies to each component part, where each pivot blocks out space between its ideal point and the median's. The further the filibuster pivot is from the median, the harder it should be to pass laws. And the further the veto pivot is from the median, the harder it should be to pass laws.[1]

This may seem to be a trivial or pedantic observation. However, consider that a range of possible pivotal actors exist in the American lawmaking process: key committee chairs, committee median voters, House majority party leadership, and the median justice of the Supreme Court are just a few reasonable possibilities. The choice to include some, but not all, procedural and institutional features as significant and necessary implies a belief that some may be more essential than others. It may also be that one of the two theoretical components is more important than the other, or that the components vary in their significance over time or by context. The institutions are sufficiently different—one occurring before final passage, the other only after bicameral passage and presidential veto; one formally stipulated in the Constitution, the other a byproduct of changing Senate rules and norms—to suggest that they could play different roles in explaining legislative outcomes.

We test this by separating the typical gridlock interval into its two component pieces—the filibuster interval and the veto interval—and show that each is independently negatively associated with the production of ideological landmark laws in the postwar period. We expand on this testing by looking at timewise and contextual variations in explanatory

[1] This statement assumes that status quos are distributed uniformly, a common assumption in the literature. The "distance" between the pivotal actors and the median is important because we assume that more status quos are gridlocked. With more status quos gridlocked, it is harder to pass new laws. While a uniform distribution assumption may seem to be unrealistic and demanding, Gray and Jenkins (2016) find that it has a relatively limited impact on pivotal politics testing.

power, focusing specifically on when the assumptions about the use of filibusters and vetoes are most likely to be true. With filibusters, we focus on the dramatic shift in norms of filibuster use, and find that the filibuster has no explanatory power before the 1975 cloture rule change, which was the culmination of changing Senate norms around the filibuster to be far more permissive. This result underlines the importance of the malleability of Senate rules, such as the cloture rule, which can be changed with a simple minority vote. With vetoes, we focus on the relative strength of the president, finding that the importance of veto overrides—and thus the pivot—is conditional on the president having sufficient popularity. Each case shows that the applicability of pivotal politics can vary by time and context as the accuracy of its assumptions wax and wane. These results are informative for the process of evaluating and improving formal theories of lawmaking and draw specific attention to the way that norms may alter members' willingness to use all of the procedural tools available to them.

2 Components of pivotal politics

Testing the overall effectiveness of a model in predicting outcomes is a useful endeavor, and pivotal politics has had no shortage of tests both of its predictions as well as its relative success against alternative theories (Krehbiel 1998; Chiou and Rothenberg 2003; Richman 2011; Woon and Cook 2015; Gray and Jenkins 2016). We supplement this testing with a more granular analysis of the individual components.

This is important and necessary because there is substantial reason to expect that each pivot could play a different role in the success or failure of the theory's predictions. The filibuster and veto are entirely different institutions. The filibuster, found only in the Senate, is a procedural hurdle that occurs relatively early in the lawmaking process, before any final-passage vote has been taken (in the Senate, at least). The filibuster provides for unlimited speech on a topic unless a supermajority agrees to end debate (i.e., to invoke "cloture"), and it has evolved over time, as the norms surrounding its use have changed dramatically. Consider that the rate of filibuster activity has increased more than tenfold in the last 50 years. By comparison, the veto override, which is explicitly provided for in Article 1 of the Constitution and requires two-thirds' support in each chamber, has had a less distinct path of normative change; it also occurs at the very end of the lawmaking process, once a bill has been passed in both chambers of Congress, with a final version agreed to, and only after a presidential veto. This can happen many months after a filibuster was avoided.

Predictions about the importance of the filibuster and veto pivots, such different individual components, could also have differing levels of accuracy. Testing each component helps us understand exactly what parts of our theories work at explaining observed reality. We believe that perfecting and improving theories is most efficient when we can assess how individual components work rather than taking a single resulting prediction of a set of elements and then adjudicating the success or failure of all of them collectively.

3 Testing the basic model

To test the importance of these individual components, we unpack the **Gridlock Interval** and create separate measures of gridlocked space, using first-dimension, common-space DW-NOMINATE scores (Poole and Rosenthal 2007; Carroll et al. 2015). The first

measure, **Filibuster Interval**, represents the absolute distance between the filibuster pivot and the median member of Congress.[2] The second measure, **Veto Interval**, represents the absolute distance between the veto pivot and the median member of Congress. Figure 1 displays the time series of these values for the 80th (1947–1948) through 113th (2013–2014) Congresses. The measures are moderately and negatively correlated ($r = -0.36$). This negative relationship is notable, as polarization does not appear to raise both in lockstep. This is partly because of volatility in the location of the median (see Brunell et al. 2016) as well as because of the 1975 shift in the cloture rule (which we account for in our measure).

The dependent variable is the count of **Ideological Landmark Laws** by Congress. This measure of legislative productivity is based on Mayhew's (1991) Sweep One count of important legislation, and restricted to those laws that tap into a distinct left–right cleavage (following the coding protocol laid out in Lee 2009).[3] We also control for the political, electoral, economic and foreign policy context of each Congress. To accomplish this, we include an indicator variable for **Unified Government**,[4] Stimson's (1991) **Policy Mood** measure of the public's appetite for new federal programs, the national **GDP Growth Rate**,[5] and an indicator variable for whether the United States was at **War**[6] for at least half of the Congress.[7]

The pivotal politics theory suggests that each gridlock-interval component should be negatively associated with legislative productivity. As the gridlocked space widens, the number of ideological landmark laws should decline. In Table 1, we present the results of Ordinary Least Squares[8] regressions, with the full Gridlock Interval (Model 1) and the individual Filibuster and Veto Interval components (Model 2) as the key independent variables. Each model also includes a time trend and a lagged dependent variable, which are not presented in Table 1 for presentational simplicity, but are included in models in the Appendix for completeness.[9] The time trend is entered to account for any systematic rise or fall in the amount of significant ideological legislation over the period analyzed, perhaps

[2] The median member of each Congress is taken as the midpoint between the House median and the Senate median. Numerous ways of conceiving of "The Median" are possible in a bicameral legislature. We opt for the midpoint of the two chambers. Existing theories of legislation struggle to deal with bicameralism, but tend to assume that some median legislator's preference exists to which policies will ultimately converge. We believe members of Congress are sophisticated actors who vote based on reasonable beliefs in the ultimate outcome of the total legislative process, which must include some resolution to the problem of bicameralism. One place where our measurement strategy may matter is that we calculate the Filibuster Interval as the space between the Filibuster Pivot and our created Congress Median (the midpoint between the two chamber medians). We believe this reflects the fact that the Senate is not a unicameral legislature, but instead is one-half of a bicameral legislature—and the ultimate bill that the Senate may pass must reflect a bicameral, rather than unicameral, process. The median we create reflects this.

[3] For a more detailed explanation of this particular measure of significant legislation, see "Appendix 2".

[4] About 38% of our observations represent unified government.

[5] Specifically, we average each one-year growth rate within the Congress. These data come from the US Department of Commerce's Bureau of Economic Analysis and are available at: http://www.bea.gov/national/.

[6] The "War on Terror" is particularly difficult to code. We stop coding the United States as at war in 2010, when combined Afghanistan and Iraq troop deployments fell below 100,000.

[7] Note that the first Congress in which all of these data are available is the 82nd (1951-1952), giving us a 32-Congress time series going up to the most recent complete Congress, the 113th.

[8] The results are robust to using a negative binomial regression rather than Ordinary Least Squares. Table 4 in the Appendix replicates Table 1 with negative binomial models.

[9] This approach is followed for all subsequent tables herein.

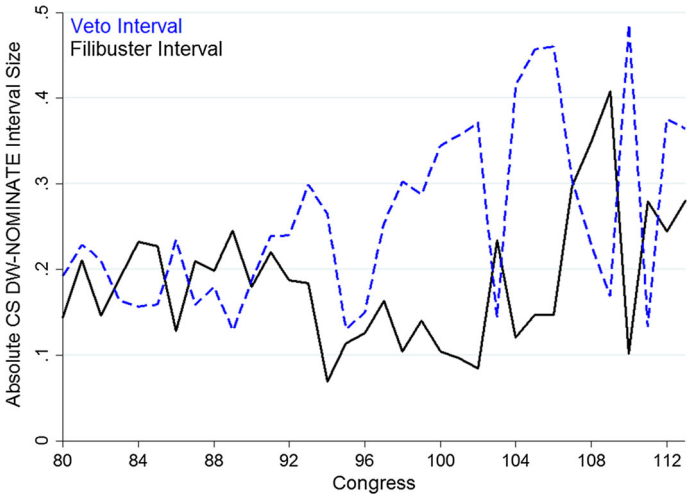

Fig. 1 Veto and filibuster intervals, 80th–13th Congresses

Table 1 Separation of filibuster and veto components

Variable	Model 1	Model 2
Gridlock interval	−20.54** (5.18)	–
Filibuster interval	–	−21.91** (7.07)
Veto interval	–	−18.50** (3.87)
Unified government	−1.36 (1.16)	−1.00 (0.92)
National mood	0.15 (0.11)	0.16 (0.11)
GDP growth	−0.10 (0.18)	−0.13 (0.20)
War	3.63** (0.71)	3.72** (0.75)
N	32	32
R^2	0.46	0.46

The dependent variable is the count of landmark ideological laws. Numbers in cells are OLS coefficients with Newey–West standard errors in parentheses. The model also includes a constant, a time trend, and a lagged version of the dependent variable

* $p < 0.05$; ** $p < 0.01$; one-tailed tests

because Congress took on more issues or because the standards for significance (media attention) became more or less inclusive. The lagged version of the dependent variable (that is, the count of significant ideological bills passed in Congress_{t-1}) represents a standard approach to mitigating temporal autocorrelation in the data.

The results in Table 1 indicate that not only do the data support the overall theoretical prediction of Pivotal Politics (Model 1), but they also support the individual component predictions (Model 2). Specifically, the Filibuster and Veto Intervals are each negatively associated with the production of ideological landmark laws. The marginal effects are approximately equal: a one-standard-deviation increase in the Filibuster Interval corresponds to a 1.70 reduction in expected laws, while a one-standard-deviation increase in the Veto Interval corresponds to a 1.93 reduction in expected laws. Neither component is the overwhelming force in empirical success.

4 Variation in explanatory power

Separating the components is an initial step in a broader process of breaking the static empirical testing of theoretical models. While Table 1 showed that, over more than six decades of congressional lawmaking, both predictions have worked reasonably well and about equally; it does not indicate whether their explanatory power is constant. In this section we explore how the theory might better explain different time periods of lawmaking, specifically focusing on the contexts in which the theory's assumptions are more accurate.

4.1 Exploring the growth in the use of the filibuster

The history of the filibuster poses a risk for assuming constancy. Notably, the cloture rule itself changed in the middle of the time series we analyze. The change from a two-thirds to a three-fifths supermajority requirement to defeat a filibuster draws attention to the possibility that the institution more broadly was not constant. The actual change itself is not the main source of the threat; the measurement of the Filibuster Interval takes into account the relevant vote requirements and chamber sizes (the 96-member Senate before the introduction of the Alaskan and Hawaiian delegations) for each Congress. But the change in the rule points toward something much more threatening: changing norms and strategies related to the filibuster's use.

In fact, filibusters were extremely rare until the 1970s, and began to grow in the years immediately preceding the 1975 rule change.[10] In the four decades since, filibustering has been increasing exponentially.[11] This implies that the way the filibuster was used, and the norms informally controlling its use, changed along the way. And that change has a particular characteristic: the use of the filibuster has come to more closely resemble the filibuster modeled in pivotal politics. In the 1950s, the filibuster, despite requiring fewer senators to maintain, was employed only rarely. The norms for its use were far more restrictive (Wawro and Schickler 2006). Specifically, when we speak of norms, we mean the perception by the senators themselves that the use of the filibuster was a legitimate tactic to prevent bills they disliked from being passed. In the early 1970s, a general rise in the legitimacy of obstructionist tactics and individualism among senators legitimized the filibuster. By 1975, when the cloture rule was changed, the filibuster had become a more regular (if still infrequent) part of the Senate's processes. Over time, under the 60-vote rule, the permissiveness of obstruction and filibustering as legislative strategy has only risen. Today, the filibuster is employed to block almost anything that the minority party in the Senate does not want to see passed. Its legitimacy as a tactic is absolute. Pivotal politics assumes that the filibuster pivot will *always* block any bill that moves policy away from its preferences. Thus, modern norms better fit the theory than the norms of the 1950s and 1960s did. We expect that the filibuster component of the theory has grown in explanatory power over time as Congress came to more closely resemble the assumptions of how filibusters could be used.

Other scholars have identified this change in the filibuster over the 20th century. As Wawro (2010) indicates, "from 1917–1975, the Senate did not have the supermajoritarian

[10] The rule was formally changed on March 7, 1975 (Wawro 2010).

[11] Filibusters are difficult to measure because their mere possibility can alter policy making, thus making them unobserved in some cases. Nevertheless, clear changes are evident in the observed numbers of filibusters. See http://www.senate.gov/pagelayout/reference/cloture_motions/clotureCounts.htm.

character that is has today. Neither the use of filibusters nor the use of the cloture rule was a part of the Senate's day-to-day functions".[12] When the filibuster was used in the 1950s and 1960s, it was overwhelmingly restricted to Southern Democrats opposing civil-rights reforms (Sinclair 2006). The Senate was simply not a super-majoritarian institution in the same way it is today (Mayhew 2003). Yet, between 1970 and 1975, and then continuing past the cloture rule's reform, the filibuster became a more regular tool of both liberals and conservatives to obstruct a broad range of policy proposals (Sinclair 2006; Wawro and Schickler 2006; Wawro 2010). Sinclair (2002) ties the changing norms of the filibuster to a broader rise in obstructionism by individualist senators willing to use all of the procedural tools available to them as well as to rising partisanship incentivizing minority party attacks on the majority's agenda. The net effect was that having 60 approving senators became a regular requirement for passing legislation post-1975. In short, the Senate came to resemble the stylized super-majoritarian chamber envisioned in pivotal politics. Therefore, the pivotal politics expectation that the Filibuster Interval is negatively associated with legislative production should be far more accurate after the shift to permissive norms of filibustering.

To test this expectation, we operationalize norm changes around the filibuster in two ways. First, we look at the cloture rule change in 1975 (Rule XXII), with the variable **Post-Change**—which takes the value "1" for all Congresses from the 94th through 113th, and "0" for the 80th through 93rd. An interaction term between Post-Change and Filibuster Interval allows two different relationships between the Filibuster Interval and legislative productivity: one before and one after the rule change. This measure is blunt, but relies on the rule change as a landmark moment indicating that a certain amount of change in the norms of filibuster use had occurred.[13] Second, we use a behavioral measure: the count of cloture motions in the previous Congress (**Cloture Motions$_{t-1}$**), which we take as a proxy for the permissiveness of the prevailing norms of filibuster use.[14] As the filibuster becomes more common, this likely reflects more permissive norms that better match the assumptions of pivotal politics. The values are lagged by one Congress to avoid endogeneity. An interaction term between Cloture Motions$_{t-1}$ and Filibuster Interval allows us to estimate the filibuster's effect on lawmaking, conditional on the permissiveness of filibustering norms. This measure is less blunt than the dichotomous measure and reflects the fact that norms changed over time, rather than at a single point in March of 1975. If our expectations are correct, each interaction term should be negative and significant, indicating a stronger relationship in both the years following the rule change, as well as in the years when filibuster use increased. Otherwise, all model specification and variable choices carry over from Table 1. The results are presented in Table 2.[15]

Both models support our expectations. In each, the interaction term is negative and significant, as predicted. Notably, the base term is positive and insignificant in both models. This means that our best assessment of the Filibuster Interval is that it had a weak

[12] This quote is taken from prepared testimony given to the Senate Committee on Rules and Administration, April 22, 2010.

[13] To the extent that the norms actually began to change one or two Congresses earlier, our tests would be biased *against* our expectations—thus creating more conservative interpretations.

[14] We use the count of cloture motions filed, but the results using the number of cloture votes held are very similar. These counts come from: http://www.senate.gov/pagelayout/reference/cloture_motions/clotureCounts.htm These data are presented in the Figure 5 in Appendix.

[15] The results are robust to using a negative binomial regression rather than Ordinary Least Squares. Table 5 in the Appendix replicates Table 2 with negative binomial models.

Table 2 Conditional influence of the filibuster interval

Variable	Model 1	Model 2
Filibuster interval	8.34 (7.64)	3.89 (11.17)
Post-change	3.50** (1.36)	–
Post-change × filibuster interval	−50.92** (6.73)	–
Cloture Motions$_{t-1}$	–	0.17** (0.04)
Cloture Motions$_{t-1}$ × filibuster interval	–	−0.57** (0.17)
Veto interval	−26.83** (2.56)	−25.70** (5.72)
Unified government	−0.72 (0.78)	−1.39 (1.12)
National mood	0.02 (0.07)	0.17 (0.08)
GDP growth	0.02 (0.13)	−0.12 (0.17)
War	3.27** (0.44)	4.34** (0.70)
N	32	32
R^2	0.66	0.58

The dependent variable is the count of landmark ideological laws. Numbers in cells are OLS coefficients with Newey–West standard errors in parentheses. The model also includes a constant, a time trend, and a lagged version of the dependent variable

* $p < 0.05$; ** $p < 0.01$; one-tailed tests

positive relationship with legislative productivity in the earliest years of the time series. Conditioning the effect of the Filibuster Interval on either the rule change or the incidence of cloture motions increases its explanatory power markedly. Both models in Table 2 fit the data substantially better than the model in Table 1.

To better understand the conditional effects of the Filibuster Interval, we present Figs. 2 and 3. In Fig. 2, we show the effect of a one-standard-deviation increase in the Filibuster Interval before and after the cloture rule change. In Fig. 3, we present the marginal effect of a one-standard-deviation increase in the Filibuster Interval as the lagged count of cloture motions increases.

As Fig. 2 indicates, the Filibuster Interval explains substantial variation in ideological landmark lawmaking, but only *after* the Cloture Rule change of 1975.[16] A one-standard-deviation increase in the size of the Filibuster Interval corresponds to a 0.65 *increase* (though not statistically distinguishable from zero) in expected ideological landmark laws before the 1975 rule change. During a time in which the norms of filibuster use were restrictive, the pivotal politics theory's predictions (based on absolutely permissive filibuster use) are not supported. However, after the rule change, the same one-standard-deviation increase in the Filibuster Interval corresponds to a 3.31 *reduction* in expected ideological landmark laws. As the norms of filibuster use came to better match the assumptions of filibuster use in the theory, so too do observed outcomes come to better match the theoretical predictions.

In Fig. 3, we present a complementary story. At the lowest levels of filibuster behavior, the Filibuster Interval has an insignificant relationship with legislative productivity, and the coefficient is in the wrong direction (positive). This level of filibuster use summarizes much of the early postwar period—never more than seven cloture motions were introduced

[16] This result is robust to entering a second interaction in the model, between the Filibuster Interval and the time trend. A separate model with an interaction between the time trend and no rule-change variable returns a significant coefficient similar to those in Table 2, albeit along with poorer model fit.

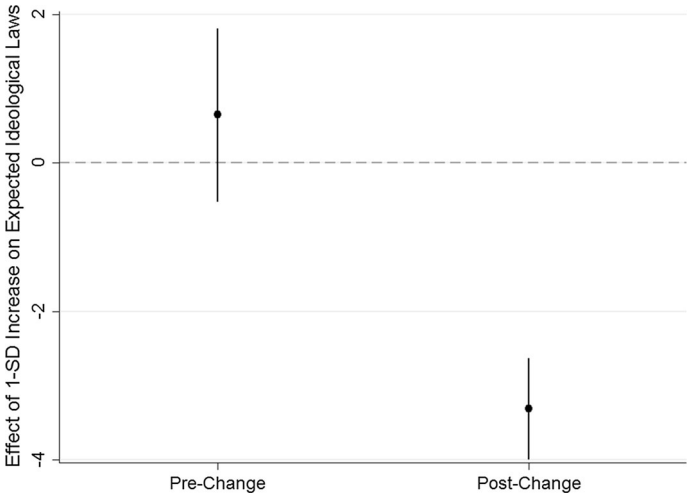

Fig. 2 Landmark ideological laws before and after 1975 cloture rule changes

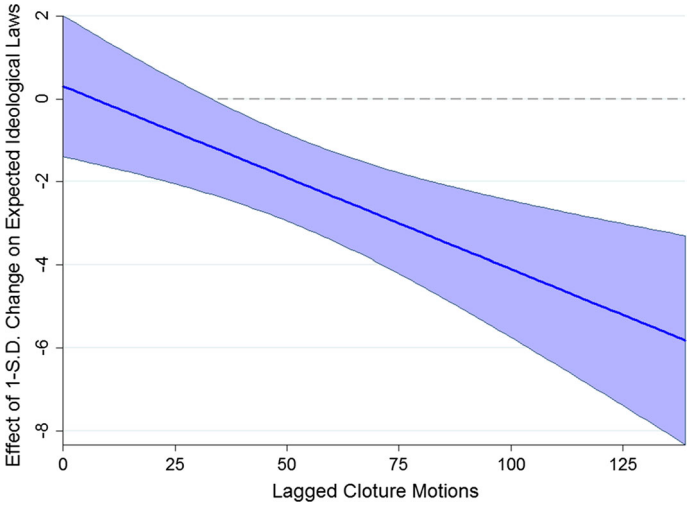

Fig. 3 The impact of the filibuster interval on production of landmark ideological laws as filibuster use increases

in a full Congress until the 92nd (1971–1972). However, when the number of lagged cloture motions increases into the 30 s and 40 s (numbers found only after 1970), the Filibuster Interval has a significant, negative relationship with the production of ideological landmark laws, as pivotal politics predicts. At the highest levels of filibuster activity, found during the Obama presidency, a typical increase in the Filibuster Interval[17] is associated with a reduction of more than five ideological landmark laws per Congress.

[17] We define a "typical increase" as a one-standard-deviation increase, or about 0.08 on the DW-NOMINATE first-dimension scale.

4.2 Variation in the relevance of veto politics

Another core assumption implicit in pivotal politics is that the president will always choose to veto a policy that moves a status quo away from his own ideal point. Such a veto necessitates a veto override vote in Congress and thus creates the veto pivot. However, much like the assumption that voters *always* filibuster a bill they do not like, an assumption that presidents *always* employ the veto is unrealistic; rather, it is more likely to be true under certain circumstances. In this section, we analyze when presidents may be more legislatively powerful and have more capacity to use their veto prerogative. We find that the success of pivotal politics' predictions is conditioned on presidents enjoying at least a moderate level of legislative power.

Presidential popularity is a key dimension that determines a president's ability to throw his weight around in the legislative process. We specifically discuss legislating on significant issues, the types of laws that have the capacity to be landmarks. Vetoing such prominent legislation is costly (Groseclose and McCarty 2001; Cameron and McCarty 2004). Presidents are unlikely to win the "blame games" that follow significant vetoes and are likely to suffer for it with voters. Unpopular presidents, all else equal, are more likely to "choose their battles," not wanting to drive their popularity even lower with vetoes of major legislation. This means that, all else equal, a more popular president should be more likely to exert legislative influence in the way that pivotal politics assumes. This yields a simple prediction: the importance of the Veto Interval should be conditional on the popularity of the president. When the president is sufficiently popular, the Veto Interval should be important in determining whether Congress has the necessary votes to override the president's veto and pass landmark legislation. However, when presidents are very unpopular, their role in the lawmaking process should be weakened.

To test this hypothesis, we conduct an additional analysis that conditions the effect of the Veto Interval on the president's popularity. We measure **Presidential Popularity** as the percentage responding positively[18] to Gallup's traditional "Presidential Approval" question, which has been asked in surveys dating back to President Harry Truman. Specifically, we use the first score of the calendar year that begins each Congress. Thus, the 80th Congress (1947–1948) uses President Truman's 1947 approval rating. When a new president takes office, such as with President Dwight D. Eisenhower in 1953, we use that new president's first rating in that Congress's calendar year.[19] Given that behavior within a Congress affects approval ratings, taking the first rating eliminates most or all of the endogenous information that would be included by other strategies, such as a 2-year average approval rating.[20] To test the extent to which Presidential Popularity conditions the effect of Veto Interval, we include both terms and an interaction of the two, **Presidential Popularity × Veto Interval**. We argue that the significance of Congress's capacity to override a veto rises with the president's popularity, and thus we expect a negative coefficient on the interaction term. This would mean that the same interval of ideological space gridlocked by the veto pivot would have a stronger negative effect (pivotal politics' predicted impact) on legislative productivity as the president becomes more popular. The

[18] Our results are robust to using a net measure as well, subtracting disapproval from approval ratings.

[19] Two Presidents—Lyndon Johnson and Gerald Ford—took over in the middle of their predecessor's term. In these cases, we use the scores of Presidents John F. Kennedy and Richard Nixon rather than their successors.

[20] These data are presented in the Figure 6 in Appendix.

Table 3 Conditional influence of the veto interval

Variable	Model
Veto interval	32.12* (14.85)
Presidential popularity	0.30** (0.05)
Presidential popularity × veto interval	−0.85** (0.27)
Filibuster interval	−24.29** (6.30)
Unified government	−1.18 (0.95)
National mood	0.01 (0.09)
GDP growth	0.03 (0.17)
War	4.15** (0.70)
N	32
R^2	0.60

The dependent variable is the count of landmark ideological laws. Numbers in cells are OLS coefficients with Newey–West standard errors in parentheses. The model also includes a constant, a time trend, and a lagged version of the dependent variable

* $p < 0.05$; ** $p < 0.01$; one-tailed tests

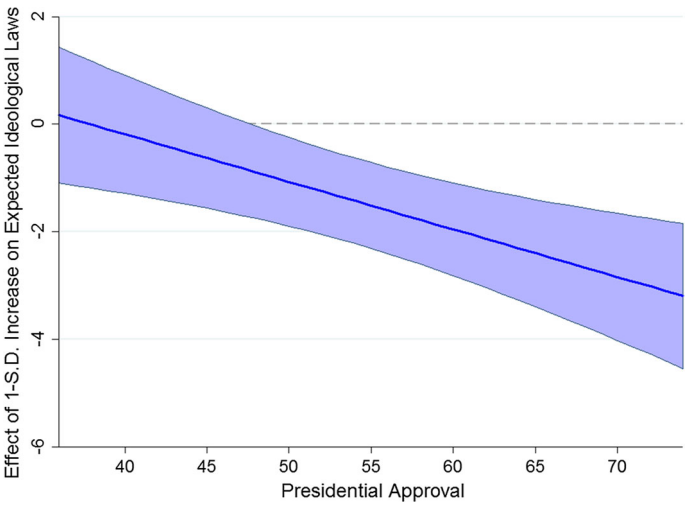

Fig. 4 The impact of veto politics on production of landmark ideological laws as presidential approval varies

results of this model, otherwise similar to those used in Table 1, are presented in Table 3.[21]

The results support our expectations: the interaction term between Veto Interval and Presidential Popularity is negative and significant, implying that the negative relationship between the space gridlocked by veto politics and the production of ideological landmark laws is greater when the president is more popular. Because this interaction features two

[21] The results are robust to using a negative binomial regression rather than Ordinary Least Squares. In Table 6 in the Appendix, we replicate Table 3 with negative binomial models.

continuous variables and neither of them is observed at or anywhere close to zero, the constituent terms of the interaction cannot be interpreted. To better understand the interactive effect, we plot (in Fig. 4) the marginal effect on ideological landmark law production of a one-standard-deviation increase in the size of the Veto Interval as Presidential Approval increases.

As Fig. 4 indicates, when less than half of the country approves of the president, the Veto Interval has an insignificant relationship with legislative productivity. At the lowest levels, the best approximation is actually of a positive, rather than negative, relationship. However, for presidents who enjoy 50% or more approval, veto override politics within Congress take on important and statistically significant explanatory power. When the veto pivot is more polarized from the congressional median in these contexts, fewer ideological landmark laws are passed. At the highest levels of Presidential Approval, a typical increase in the size of the Veto Interval is associated with a reduction of three ideological landmark laws per Congress.

5 Conclusion

Collectively, these results support our core argument that theories can be evaluated in a more granular way than simply assessing their overall ability to explain variation in observational data. They can be evaluated for how well their individual component predictions perform under empirical testing. They can also be evaluated for how well they explain reality over time, as norms and institutions change and the assumptions that undergird a theory vary in their suitability. We show that both the Veto and Filibuster Intervals have played a part in the predictive success of Pivotal Politics, but that the effect of each is conditional.

The filibuster component of pivotal politics has grown in significance over time, most especially with the shift in norms in the 1970s, culminating in the Rule XXII change in 1975. Our results, as well as the judicial confirmation politics of the 113th and 115th Congresses, reinforce the importance of accounting for norms and easily-changed rules. As these norms and rules change, so might the underlying dynamics of legislation that scholars theorize about. The veto component's change is far less linear, waxing and waning as the importance of the president in policy making varies. In each case, a normal (one standard deviation) increase in the size of the gridlocked space in contexts that best match the assumptions of pivotal politics corresponds to a loss of about three landmark ideological laws in a Congress (compared to an average of about six in the postwar period). These are significant losses of major federal policy on issues such as economic distribution and regulation.

We offer these tests as examples of thinking of theories in more dynamic and detailed ways. This is useful not only for their empirical evaluation, but also for pinpointing their weaknesses to find areas on which they can be developed and improved. Our results indicate that both the filibuster and veto pivots contribute to the success of pivotal politics, but that their relative values have changed over time. The filibuster pivot result most sharply shows that relying exclusively on procedural rules misses the fact that informal norms can limit the willingness of members to use all of the procedural tools available to them. Likewise, the veto pivot result shows that presidents are willing—and able—to use their constitutional power more when their standing among the public is higher; a

politically unpopular president has less ability to compete effectively with Congress in the lawmaking process.

Appendix 1: Additional models and data visualization

See Figs. 5, 6 and Tables 4, 5, 6.

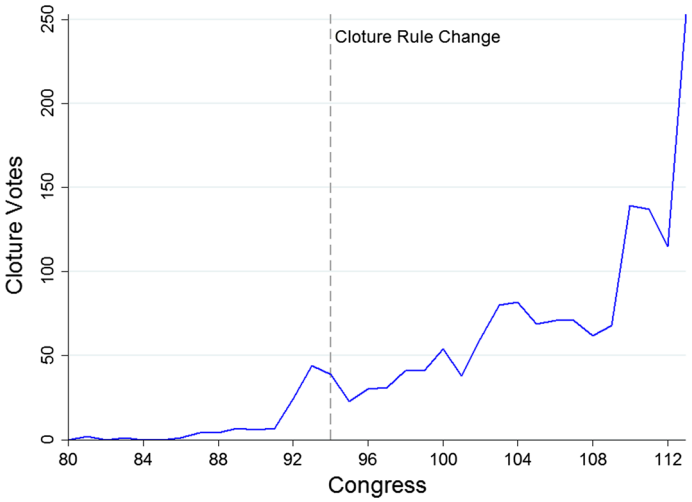

Fig. 5 Count of cloture motions, 80th–113th Congresses

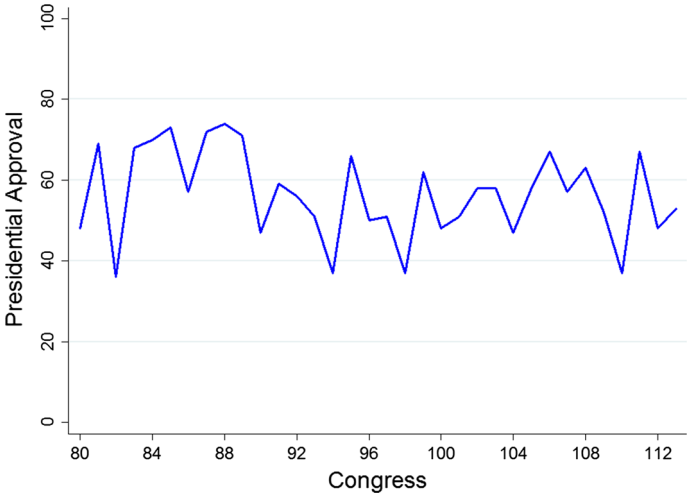

Fig. 6 Initial presidential approval, 80th–113th Congresses

Table 4 Separation of filibuster and veto components: negative binomial results

Variable	Model 1	Model 2
Gridlock interval	−3.40** (0.99)	–
Filibuster interval	–	−3.62** (1.24)
Veto interval	–	−3.07** (1.02)
Unified government	−0.21 (0.21)	−0.15 (0.20)
National mood	0.03 (0.02)	0.03 (0.02)
GDP growth	−0.02 (0.04)	−0.02 (0.04)
War	0.53** (0.12)	0.54** (0.13)
Sig. Ideo. $laws_{t-1}$	0.03 (0.02)	0.03 (0.02)
Time trend	0.03* (0.01)	0.03* (0.01)
Constant	−1.76 (2.38)	−1.61 (2.63)
N	32	32

The dependent variable is the count of landmark ideological laws. Numbers in cells are negative binomial regression coefficients with Newey–West standard errors in parentheses

* $p < 0.05$; ** $p < 0.01$; one-tailed tests

Table 5 Conditional effect of the filibuster interval: negative binomial results

Variable	Model 1	Model 2
Filibuster interval	1.95 (1.26)	1.12 (2.07)
Post-change	0.78** (0.26)	–
Post-change × filibuster interval	−8.85** (1.30)	–
Cloture $Motions_{t-1}$	–	0.02** (0.01)
Cloture $Motions_{t-1}$ × filibuster interval	–	−0.09** (0.03)
Veto interval	−4.31** (0.64)	−4.22** (1.29)
Unified government	−0.12 (0.18)	−0.22 (0.24)
National mood	0.01 (0.01)	0.03 (0.02)
GDP growth	−0.01 (0.03)	−0.02 (0.03)
War	0.46** (0.09)	0.57** (0.14)
Sig. Ideo. $laws_{t-1}$	0.00 (0.01)	0.02 (0.03)
Time trend	0.09** (0.02)	0.02 (0.02)
Constant	−6.14** (1.95)	−1.49 (3.11)
N	32	32

The dependent variable is the count of landmark ideological laws. Numbers in cells are negative binomial regression coefficients with Newey–West standard errors in parentheses

* $p < 0.05$; ** $p < 0.01$; one-tailed tests

Table 6 Conditional influence of the veto interval: negative binomial results

Variable	Model
Veto interval	6.30* (3.26)
Presidential popularity	0.05** (0.01)
Presidential popularity × veto interval	−0.16** (0.06)
Filibuster interval	−3.61** (1.31)
Unified government	−0.21 (0.22)
National mood	0.01 (0.02)
GDP growth	−0.00 (0.03)
War	0.54** (0.13)
Sig. Ideo. $laws_{t-1}$	0.04 (0.03)
Time trend	0.03* (0.01)
constant	−3.35 (2.33)
N	32

The dependent variable is the count of landmark ideological laws. Numbers in cells are negative binomial regression coefficients with Newey–West standard errors in parentheses

* $p < 0.05$; ** $p < 0.01$; one-tailed tests

Appendix 2: The ideological content of landmark legislation

In this Appendix, we briefly describe the process of getting from Mayhew's counts of significant legislation to the counts of ideological legislation we use in this paper. This section is a condensed version of what appears in Gray and Jenkins (2016).

Mayhew (1991) developed a list of significant enactments based on a two-sweep method. In Sweep One, laws are marked as significant based on their appearance in newspaper recaps of major acts of Congress. In Sweep Two, additional bills are picked as significant by expert evaluators of policy areas. Sweep One, being contemporaneous, covers the entire period from the 80th to 113th Congresses. Sweep Two, being retrospective and taking considerable time for the impacts of policy to become apparent, only covers legislation up through the 1980s. This poses the first problem for using Mayhew's total counts. Because both sweeps are not applied to the full time sequence, the potential for bias exists owing to extra laws being counted in earlier periods. This is not something that simply "dummying" these earlier years can be guaranteed to correct. Moreover, Howell et al. (2000) persuasively argue that the two sweeps have different time-series dynamics that make combining them problematic. Therefore, we rely exclusively on Sweep One counts, which have been applied across the entire time range.

When just Sweep One counts are used, they represent one justifiable means of measuring important legislation: what journalists remarked upon at the end of a session. However, when Sweep One counts are incorporated as the dependent variable *in a pivotal politics model*, a disconnect between theory and testing occurs.[22] Pivotal politics describes the process of producing *ideological* laws. Legislators have preferences over policies based on their ideal points on a single dimension of *ideological* positions. If a status quo cannot be placed on the line because it does not invoke political ideology, then the theory cannot make predictions about it.[23] An ideal test of pivotal politics should assess whether the

[22] The same argument holds for a dependent variable based on both Sweep One and Two laws.

[23] The theory could make predictions about productivity of non-ideological laws, however this would require separate measurement and analysis of alternative dimensions. If we are to use existing single-dimension ideological measures, then the outcome measured must also be ideological. In short, the revealed preferences must match the measured outcomes.

ideological distance between pivotal actors in Congress influences the production of ideological legislation. Yet, Mayhew's laws were chosen *only* for their importance, *not* for whether they represent any genuine conflict between liberal and conservative values. As Lee (2009) argues, not all issues generate conflict, and not even all issues for which there is partisan conflict can be coherently placed on an ideological line.

This weakness provides an opportunity for constructing a new dependent variable, but does not require departing from Mayhew. Instead, we create *subsets* of Mayhew's Sweep One laws, separating those that fit into a liberal-versus-conservative ideological conflict from those that do not. In this, we rely on Lee's (2009, pp. 63–64) definition of ideology and resulting coding scheme for classifying Senate roll-call votes as ideological or non-ideological. We code based only on the content of legislation; we do not consider the size or partisan composition of voting coalitions.

Lee identifies four issue categories of ideological conflict: economic, social, hawk-versus-dove, and multilateralism versus unilateralism.[24] The economic category includes laws that change levels of economic regulation (such as environmental regulations for businesses) or redistribution (for example, changing tax schedule income brackets or expanding Medicaid funding) or affect the overall level of government spending and share of the economy (such as large economic stimulus spending). The social category includes civil rights legislation and criminal punitiveness, as well as laws that push policy away from traditional gender, family, sex, and race norms (such as "Don't Ask, Don't Tell," abortion rights, or school prayer). The hawk-versus-dove category involves authorizations for the use of military force, weapons investment, and limitations on weapons testing. Finally, the multilateralism versus unilateralism category includes debates over the importance of international organizations to America's foreign policy (for example, policies that promote the United Nations).

Laws outside of these four categories do not have a clear place in modern American ideological debates. In these cases, placing a policy alternative to the "left" or "right" of a status quo is very difficult or impossible, which makes the common logic of Pivotal Politics theory testing inapplicable.[25] Many laws fall into this non-ideological category, including those that deal with good governance (such as Freedom of Information policies) and departmental reorganization, non-redistributive and non-regulatory programs (such as the anti-cancer efforts begun by the National Cancer Act of 1971), disaster relief, and the distribution of power between the branches of the federal government (such as the War Powers Resolution).

For each law that Mayhew identified in his Sweep One, we determine whether it fits into one of Lee's categories of ideological conflict.[26] If so, the law is coded as ideological; if not, it is coded as non-ideological.[27] Figure 7 illustrates the resulting time series of

[24] This definition of ideology is time-bound to the debates between mainstream liberals and conservatives in the postwar era. Lee provides more detail on each category and what should be included, as well as many examples.

[25] Members' preferences on these non-ideological issues may be orthogonal to their ideological preferences and thus the use of ideological preference measures potentially introduces substantial measurement error.

[26] There are alternative ways to identify how topics fit on an ideological dimension. While we rely on human coding of topics based on a scholar-defined coding scheme, others might instead analyze which topics regularly produce high error rates in classification using existing measures, such as DW-NOMINATE. More opportunities exist for greater levels of empirical validation of Lee's work.

[27] Most laws have many sections and components that are difficult to evaluate in such a dichotomous way. We focus on the core features of the law rather than any add-ons or unrelated provisions—specifically, the aspect that Mayhew identified in his brief note on each law. When necessary, we use other historical descriptions of the laws to provide supplementary information.

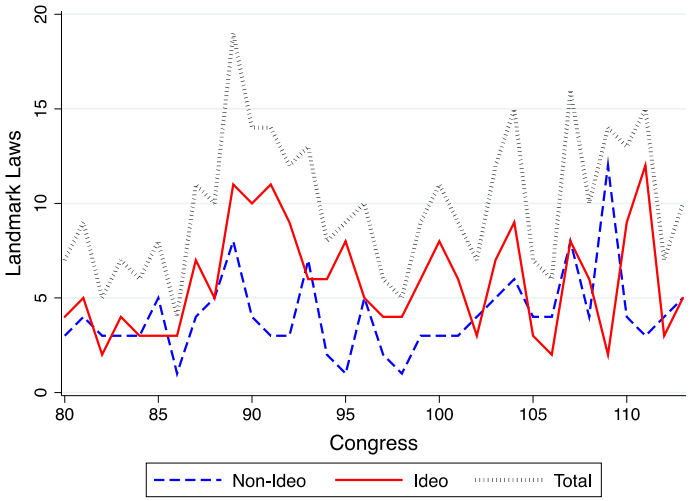

Fig. 7 Landmark laws split into ideological and non-ideological categories, 80th–113th Congresses

ideological and non-ideological landmark laws. The mean level of ideological laws (5.85) is higher than non-ideological laws (4.09), but both series display meaningful variation (standard deviations of 2.83 and 2.19, respectively). The minimum and maximum for ideological laws are two (106th and 109th Congresses; 1999–2000 and 2005–2006) and 12 (111th Congress; 2009–2010), while the minimum and maximum for non-ideological laws are one (86th, 95th, 98th Congresses; 1959–1960, 1977–1978, and 1983–1984) and 12 (109th Congress; 2005–2006). Overall, the two series exhibit no meaningful correlation ($r = 0.03$) (See Fig. 7).

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