The Filibuster and Legislative Discussion

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We investigate whether the filibuster stimulates public debate and discussion within Congress, as its advocates argue, or whether, instead, it discourages legislators from devoting time and attention to bills they know will not pass, as its critics attest. To do so, we exploit multiple sources of variation in the filibuster, measures of legislative discussion, and identification strategies. In the preponderance of analyses, we observe null effects. Where significant differences are observed, they nearly always suggest that a strengthening (weakening) of the filibuster coincides with a reduction (increase) in the volume of floor speeches or time devoted to legislative affairs. Whatever benefits the filibuster may confer, they do not appear to include enhanced discussion on the floors of Congress.

Does the filibuster enhance legislative discussion? Defenders of the Senate rule respond decisively in the affirmative. By giving a platform for skeptical colleagues to speak and by slowing the pace of legislative change, they argue, the filibuster ensures that disagreements over proposed bills are properly aired. When arguing against a 1975 proposal to weaken the filibuster by reducing the number of votes needed to invoke cloture, Senator James Allen (D-AL) insisted that the filibuster guarantees “extended debate” and thereby brandishes the Senate’s “reputation as a deliberative body.”1 Or as a Congressional Research Service report (2013, 3) summarized the views of the rule’s advocates, the filibuster promises to “cool passions and force deliberation.”

Two mechanisms undergird the claims made by these advocates for the filibuster. First, by enabling legislators to hold the floor for extended periods of time, the “talking filibuster” mechanically prolonged debate. But even after the demise of the talking filibuster in the early 1960s, the voting rule may have stimulated discussion. By requiring a supermajority of senators to invoke cloture and thereby lift the hold placed on pending legislation, advocates of the filibuster insisted, members of the majority party must curry the support of at least some opposition figures. To do so, they must moderate their policy claims, of course, but they also must listen to criticism, sharpen their arguments, and engage in discussion aimed at winning over skeptics of their legislative proposals. The filibuster, as such, does not merely slow the pace of lawmaking. As Marcus (2010) explains, the filibuster also “enhances the opportunity for real debate” and thereby ensures the enactment of “a better end product.”

Critics of the filibuster see things very differently. Rather than stimulating discussion, they argue, the primary effect of the filibuster is to block policy change. As a result, substantive policy debates in the Senate give way to political posturing and nearly constant electioneering, rendering the floor of this putatively deliberative body nearly devoid of meaningful exchange. As Fredrickson (2020) argues, “for decades, the filibuster has ceased to serve the purpose of allowing contrary ideas to be aired and promoting debate. The simple threat of objection simply ends all discussion.” And according to Klein (2020), “The irony of the modern filibuster is that it rarely includes debate, and often prevents it. Indeed, senators often filibuster the motion to begin debate on legislation, which reveals how thin the commitment to deliberation actually is.”

Which perspective is closer to the truth? It is hard to say. Although the existing scholarly literature broadly recognizes

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the relevance of supermajoritarian rules for legislative discussion and debate, no empirical study has systematically evaluated their relationship to one another. Participants in popular debates over the filibuster, meanwhile, only supply impressions that invariably align with their normative arguments.

This article initiates the process of building an empirical foundation for assessing these competing claims. To do so, it tracks patterns of legislative discussion before and after the filibuster underwent significant changes in the nineteenth, twentieth, and twenty-first centuries. Making use of multiple sources of variation in the filibuster, measures of legislative discussion, and identification strategies, we recover reasonably consistent findings. In the preponderance of analyses, we do not find any credible evidence that the filibuster enhances legislative discussion and debate. Where significant differences are observed, they nearly always suggest that strengthening (weakening) the filibuster coincides with a reduction (increase) in legislative discussion.

The filibuster may deteract from legislative discussion, or it may have no meaningful effect at all. But it does not appear to increase the volume—and hence, one might reasonably infer, the quality—of legislative deliberations on the floors of Congress. Arguments on its behalf, we conclude, must rest on alternative grounds.

DATA

To assess the merits of these competing arguments, we build a variety of data sets that link multiple changes in the filibuster with multiple measures of legislative discussion. We exploit five sources of variation in the rules and norms governing the filibuster: the 1917 adoption of Senate Rule XXII, which established cloture and thereby limited the ability of a single senator to hold the floor indefinitely; the 1975 reduction in the number of votes needed to invoke cloture from two-thirds to three-fifths; Mitch McConnell’s assumption of leadership in the Senate in 2007, when the norms governing the use of the filibuster relaxed significantly (Mann and Ornstein 2012) and the number of filibusters increased dramatically (see fig. A.1); and the elimination of the “disappearing quorum” in the House in the late nineteenth century, which, according to Krehbiel (2017, 352), functioned as a form of “background supermajoritarianism.” With the exception of the 2007 shift in norms, all of these changes weakened the filibuster and hence, if its advocates are correct, should have coincided with a decrease in legislative discussion.

We also deploy multiple measures of legislative discussion, including the total volume of House and Senate floor speeches delivered by each senator and House representative in the 43rd–114th Congresses (1873–2016), as consolidated by Gentzkow, Shapiro, and Taddy (2018); the length of time devoted to landmark laws enacted by Congress, as determined by Mayhew (1991), and all elements of majority party agendas, regardless of whether they ultimately became law, as compiled by Curry and Lee (2020); and the number of words each senator delivered on the floor about every appellate and district court nominee during the Obama administration (2009–16).

ANALYSIS

Two classes of identification problems confront this project. First, changes in filibuster rules and norms may coincide with changes in other practices and procedures, such as how filibusters are tracked or the amount of time allotted to debate, which themselves covary with our measures of legislative discussion. Second, and as already indicated, the topics of Senate debate—be they bills or nominees—may depend on the rules of consideration. Changes in these rules, therefore, may alter the subjects of the congressional agenda, further complicating our efforts to estimate the effects of the filibuster on specific topics of legislative discussion.

To address these identification challenges, we deploy a variety of strategies that intermittently exploit within-member variation in speech patterns, leverage the House as a control group, limit the sample to particular classes of congressional activity, vary the windows of analysis, and control for observable features of Senate votes. Despite their variable strengths

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2. While arguably the most important, these five sources of variation do not exhaust the full complement of procedural changes to the filibuster. Carve-outs in 1970 for trade and in 1974 for budgetary reconciliation bills, e.g., are excluded. For a longer list, see Binder and Smith (1997), table 1-1.

3. On inspection, we found that speeches with fewer than 30 words were generally procedural, such as members yielding time, appreciating the speaker, recording a vote, etc. We therefore dropped these speeches and then calculated the remaining number of words spoken by each legislator in a given year.

4. From both samples, we drop all reconciliations bills, which are not subject to a veto. Including them in the analysis, we note, has no bearing on our main substantive findings. In both samples, we identified the dates when the House and Senate reported every bill out of the legislative committees, discussed the bill on the floor, cast votes, and considered conference reports to resolve chamber differences. We then calculated the total number of days spent on each bill. Because congress.gov provides scheduling information at the daily level, we cannot calculate the precise amount of time spent on a bill within any day.

5. Among 323 confirmed judicial nominees, 207 were confirmed before the filibuster was eliminated for judicial nominees, and 116 were confirmed afterward. Using an automated algorithm, we filtered all senatorial floor speeches about each nominee and then validated them by human reading.

6. When considering the larger public benefits of enhanced discussion, this latter concern no longer arises, as agenda changes are properly understood as occurring posttreatment.
and weaknesses, which we clarify below, these strategies collectively yield reasonably consistent findings of the effects of the filibuster on legislative discussion.

**All floor speeches**

To begin, we assess how changes in the filibuster correlate with the overall volume of legislators’ speeches. Here, we limit the analyses to four of our sources of variation in the filibuster: the cloture adoption in 1917, the cloture threshold change in 1975, norm changes in 2007, and the House filibuster rule changes in 1889 and 1893. To account for unobserved heterogeneity of legislators, we exploit within-member variations in speech patterns. Because the filibuster rule changes in 1917 and 1975 as well as the norm change in 2007 focused narrowly on the Senate, and because the rule changes in 1889 and 1893 were limited to the House, we treat legislators in the adjoining chamber (the House in 1917, 1975, and 2007 and the Senate in 1889 and 1893) as control groups.7

We implement the following difference-in-differences design:

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\text{Log(Word Counts)}_{it} = \alpha_i + \delta_t + \beta \text{Filibuster Rule/Norm Change}_{it} + \epsilon_{it},
\]

where subscript \(i\) denotes each individual legislator and \(t\) denotes the year. The dependent variable is the log number of words spoken by a legislator in a given year; \(\alpha\) represents legislator fixed effects, which account for all time-invariant individual characteristics of their baseline speech patterns and their correlates; and \(\delta\) represents year fixed effects, which flexibly control for secular changes in legislative discussion over time. Legislators who switched chambers are given two identifiers. The filibuster rule or norm change is the key treatment, which in each regression applies to only one chamber and which is coded 1 in those years after the rule or norm changed. The coefficient \(\beta\) tells us how the rule or norm change correlates, on average, with legislative discussion by each treated lawmaker.7 Since rule and norm changes are applied at the chamber level, we report the standard errors clustered at the chamber-by-session level. Finally, we estimate these regressions over incrementally larger time windows that vary from one to five congressional sessions immediately before and after the rule or norm change.

Table 1 presents our main results. When examining discussion trends around 1917, when the Senate first established the cloture rule, we find very little evidence of broader changes in discussion patterns, a finding that is consistent with Burdette’s (1940, 7) observation that “the provision authorizing cloture has had less effect on filibustering than might have been anticipated.” Very little also appears to have changed around 1975, when the Senate lowered the threshold from two-thirds to three-fifths. For all window sizes, we recover null results with point estimates that hover around zero.

Given the sharp increase in the use of the filibuster in 2007, we might expect more discussion and debate on the Senate floor. The empirical evidence, however, does not bear this out. In the aftermath of McConnell’s rise to power, we find significantly lower levels of legislative discussion. Depending on the size of the windows examined, we find that speeches declined by somewhere between 13.9% and 28.1%. The new norm of subjecting nearly every bill to a filibuster did not encourage legislative discussion; if anything, it appears to have depressed it.10

This pattern of findings carries over into the nineteenth century. As the bottom panel of table 1 shows, legislators spoke more in the aftermath of the disappearing quorum’s elimination than they did before. Apart from the one-session window, all estimates are positive, although none are statistically significant.11

These results appear reasonably constant across a variety of subpopulations. As shown in tables A.3–A.8, similar findings are recovered from models that separately examine the deliberative practices of Democrats and Republicans, of members of the majority and minority parties, and of ideological moderates and extremists. Throughout, we do not see any clear evidence that the filibuster enhances legislative discussion.

**Legislation**

The preceding analyses aggregated all floor speeches to the member-by-year level. We now focus on the amount of time

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7. Given the possibility of spillovers, of course, the adjoining chamber never functions as a pure control group. The strength of this research design, as such, hinges on the relative degree of independence across the House and Senate.

8. Models that characterize the dependent variable in levels without the log transformation yield findings that broadly conform with those reported here. Where differences arise, as shown in table A.1, they run contrary to the claims of advocates of the filibuster.

9. Our estimates of the effect of the filibuster crucially depend on the parallel-trends assumption. Reassuringly, in the period leading up to the rule change, as shown in fig. A.2, the two time series track each other well.

10. It is possible, of course, that the norm change did not take hold immediately upon McConnell’s assumption of the majority party leadership. We therefore also estimated models that set the cut point at mid-2007, early 2008, and mid-2008 (see table A.2). In none of these models do we find any evidence that the expanded use of the filibuster enhanced legislative discussion.

11. Recall that the House killed the filibuster at the beginning of the 51st Congress, reinstated it in the 52nd Congress, and then permanently eliminated it in the 53rd. Consequently, the estimates in table 1 cols. 4 and 5, which incorporate all of this variation, may be preferred.
each chamber spent discussing and debating landmark legislation and elements of the majority party agendas. Using ordinary least squares, we estimate the following regression:

$$\text{Senate Days}_i = \beta_0 + \beta_1 \text{Filibuster Norm Change}_i + \beta_2 \text{House Days}_i + \beta_3 \text{Introduced in Senate}_i + \epsilon_i,$$

where subscript $i$ now represents each bill, the dependent variable is the number of days spent in the Senate discussing a given bill, and the key variable of interest identifies those bills that were considered after the change in filibuster norms. We include controls for the number of days spent in the House discussing each bill as well as an indicator for whether a bill was first introduced in the Senate. As in the previous section, we estimate these regressions during five different time windows. Because detailed scheduling data are only available after 1980, we limit our analysis to the 2007 change in norms.

Table 2 presents the results. For landmark legislation, the positive coefficients indicate that somewhere between 0.5 and 2.5 more days were spent discussing landmark bills in the aftermath of the norm change. None of the point estimates, however, even approach conventional levels of statistical significance. Moreover, the size of the point estimates is smaller for the longer windows, which included larger numbers of observations and, consequently, more precise estimates. For majority party agendas, which include a mixture of bills that failed and passed, the coefficients remain statistically insignificant and are even smaller in magnitude.

When disaggregating the majority party agendas, we find that the Senate spent less time on bills that were not enacted into law in the aftermath of the norm change and no difference on successfully enacted bills (tables A.9 and A.10). For both samples of bills, similar results are recovered when we include additional covariates for divided government, divided chambers, and measures of Senate polarization (table A.11). On net, we do not find any evidence that the Senate’s more expansive reliance on the filibuster coincided with an increase in legislative discussion.

### Judicial nominees

Our final analysis focuses on the 2013 elimination of the filibuster for nominees to the federal judiciary. As explained in the appendix and accompanying tables, this rule change coincided with a slight, but statistically insignificant, decline in the length of senators’ floor speeches on appellate and judicial nominees. When disaggregating the data, we find that effects vary markedly for appellate and district court nominees. These latter findings, however, do not hold up to a variety of robustness checks and placebo tests. We do not recover any compelling evidence that the elimination of the filibuster systematically altered the volume of senatorial discussions about Obama’s nominees.

### CONCLUSION

While a robust literature documents the filibuster’s relevance for coalition building and lawmakers (see, e.g., Binder and
Smith 1997; Koger 2020; Wawro and Schickler 2006), this article provides the first systematic evidence of its effects on congressional discussion and debate. Relying on a wide variety of measurement and identification strategies, we do not find any evidence that the filibuster enhances the Senate’s consideration of laws or judicial nominees. Most of our analyses suggest that changes in the filibuster did not significantly alter the volume of speeches or time devoted to congressional debate. Where differences are observed, they usually indicate that the filibuster detracts from, rather than bolsters, public discussion on the floors of Congress. On the floors of Congress, at least, the filibuster does not encourage senators to slow down, scrutinize the merits of proposed laws and judicial nominees, and participate in what Mayhew (2000) calls the “public sphere.” If anything, it may degrade legislative discussion.

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