The effect of majority party agenda setting on roll calls

Abstract

How well can roll calls detect the causal impact of majority party agenda setting in Congress? Estimating the counterfactual required to assess the effects of majority party agenda setting is complicated by time-varying differences in the political environment and the fact that measures commonly used to control for compositional changes may themselves depend on the extent of agenda control being exercised. Using techniques popularized by recent work focused on causal inference, I characterize whether agenda changes occuring during changes in majority party control in the US House of Representatives are consistent with predictions from models of majority party agenda control. Comparing how the same members in consecutive Congresses are affected by changes in party control and using fixed effects to account for time-varying differences between consecutive Congresses helps isolate the changes in the agenda attributable to agenda setting. The analyses highlight the challenge in consistently estimating the effects of agenda control and suggest that although recent transitions produce patterns consistent with the predictions of agenda setting theories, the average effect over the post-Reconstruction period is harder to interpret as being produced by agenda control.

Keywords: agenda control, U.S. Congress, roll call voting

JEL Codes: D72 (Political Processes: Rent-Seeking, Lobbying, Elections, Legislatures,

and Voting Behavior), H11 (Structure, Scope, and Performance of Government)

The study of legislators' voting behavior occupies a prominent place in political science because legislators' votes often are the means by which democratic politicians convert their preferences – and those of their constituents – into policy outcomes. As a result, the study of roll call voting has wide-ranging implications for assessing many critical issues for democratic politics (e.g., lawmaking, representation) and it often is central to investigations into the causes and consequences of elite-level politics (see, for example, the summary of Theriault, Hickey and Blass 2011).

Even if roll calls provide a partial portrait of congressional behavior (VanDoren 1990; Carrubba, Gabel and Hug 2011; Clinton and Lapinski 2011) whose meaning may be difficult to interpret (e.g., Kingdon 1989; Lee 2009, 2016), roll calls provide a clear indication of what Congress chooses to make its members take public positions on. Because roll calls provide a comprehensive record of the public positions that members are asked to take identifiable positions on, studies interested in whether members endow political parties with the ability to constrain individual choices for the benefit of the collective party interest (e.g., Aldrich 1995; Cox and McCubbins 2005, 2007; Harbridge 2015; Pearson 2015; Rohde 1991; Smith 1989) have focused on analyzing patterns in roll call voting (e.g., Carson, Monroe and Robinson 2011; Gailmard and Jenkins 2007; Jenkins and Monroe 2015; Monroe and Robinson 2008; Schickler and Rich 1997 – but see Pearson and Schickler 2009). Much has been learned about the correlates of changing roll call agendas over time, but identifying the *causal impact* of agenda control is exceptionally challenging.

Making a causal claim about how majority party setting affects the congressional agenda requires comparing how the roll call agenda in the presence of agenda setting compares to the roll call agenda in its absence. Addressing the fundamental problem of causal inference consequently requires leveraging over-time variation in the potential use of agenda setting powers by the political parties in the US House of Representatives in the hope of estimating the counterfactual – an estimate that is threatened by difficult-to-measure time-varying differences in the political, social and economic environment.

Although a causal estimate may be impossible to achieve given the difficulties involved, I use a causal-inference-inspired specification to avoid some – but certainly not all – of the difficulties in consistently estimating the association between majority party control and the roll call agenda to build upon the expansive literature on agenda control in Congress. To do so, I examine how the set of votes being considered between consecutive Congresses influence the voting coalitions of continuing members to estimate the change in the agenda that presumably is attributable to a change in majority party control. To account for the many ways that pairs of consecutive Congresses may differ from one another I employ fixed effects – an approach that avoids the difficulties associated with trying to measure and control for the many time-varying differences that may affect the votes being taken.

The efforts I take to identify a consistent estimate of the effect of a change in majority party status on the roll call agenda reveals results that are consistent with theoretical expectations in recent decades, but the association between changes in majority power and changes in the roll call agenda is harder to interpret when averaging across the entire post-Reconstruction period. Probing why recent transitions differ from earlier transitions unfortunately is impossible because the paucity of changes in majority party control make it impossible to determine why larger effects occur in more recent transitions.

For questions in which the stringent demands of causal inference are unlikely ever to be satisfied convincingly – as arguably is the case for questions involving the effect of majority party agenda control given the difficulty of estimating the counterfactual – our best chance of interpreting the meaning of the observed patterns and associations plausibly is achieved by comparing theoretically motivated measurement and description to independently generated theoretical predictions to determine which theoretically implied mechanism is most consistent with the observed patterns. It is to this task that the present paper contributes. Even if identifying causal effects are beyond our grasp, providing a consistent estimate of theoretically relevant associations helps motivate additional theorizing about the causes and consequences of the uncovered patterns.

1 Causality, consistency and confounding

Empirical work in political science faces many challenges – especially when the question involves analyzing historical variation for which data are hard to come by and the concepts are difficult to measure. Measurement-related issues are critically important – not only because measurement is essential for correctly describing the empirical regularities of interest, but also because improved measures can help inspire additional theorizing about the causal mechanisms that may be responsible for the measured patterns and associations. The robust literatures focused on the causes and consequences of elite polarization and also congressional lawmaking, for example, were made possible by work that initially described the patterns of elite voting behavior (e.g., Poole and Rosenthal 1985) and lawmaking activity (e.g., Mayhew 1991).

It can also be difficult to estimate the association between valid measures. The problem is often not one of identification – most statistics (e.g., regression coefficients) are determined by an objective function that has a unique minimum in the population that can be calculated given the observed data. The issue instead is whether the resulting estimate is a consistent measure of the population parameter. Put differently, are we be able to recover the true parameters of the underlying data-generating process even with an infinite amount of the observable data?

Two primary threats to the consistent estimation of statistical parameters arise regardless of whether we seek a causal estimate or not. The most prevalent threat is posed by the omission of variables that partially account for variation in the outcome. Without controlling for every variable that covaries with the outcome we are unable to consistently estimate the true relationship. Even if every confounder is included, however, a second threat is presented by measurement error in any variable correlated with the variables of interest. Mismeasured variables make it impossible to consistently estimate the relationship between variables because the errors propagate to affect the estimate of any variable

correlated with the mismeasured variable.

Concerns about omitted variables and measurement error are especially important when data are hard to come. As a result, questions regarding political institutions and American political development are frequently plagued by such concerns and it is often difficult to consistently estimate a statistical association because of the difficulty of perfectly measuring the confounding variables.

Estimating a causal relationship between two concepts raises the difficulty level even higher. Such analyses seek to understand the relative percentage change in two concepts – i.e., how the outcome varies with respect to the "treatment," all else equal. To estimate elasticities, political science largely has relied on exogenous variation in the independent variable – or else variation that can be justified as being "as if" exogenous conditional on the included covariates.[1] For example, rather than estimating the incumbency advantage using every incumbent and covariates to control for the confounding factors, work interested in estimating the causal effect of incumbency often focuses on analyzing the relationship in closely contested elections where scholars can justify the claim that the incumbents were selected as if random. Even when a plausible causal effect can be precisely estimated through such restrictions, however, it can be difficult to generalize the estimated effects. If the causal effect of incumbency is estimated using close districts, for example, how do we know whether the effects generalize to less competitive districts? Generalizing the estimated effect requires a theoretical claim about their scope – claims that are difficult, if not impossible, to evaluate empirically.

Independently generated theories similarly are important not only for justifing the extrapolation of causal effects to instances beyond the considered cases but also because they can provide testable predictions that can help interpret non-causal interpretations

[1]It also is possible to estimate the elasticity between variables without relying on exogenous variation. Structural modeling, for example, while relatively rare in political science (but see, for example, Canen, Kendall and Trebbi 2018), adopts strong assumptions about theory, functional form and available measures to define a likelihood that directly estimates the elasticity.

of statistical associations. Because such theories provide a mechanism for computing the elasticity of two concepts, the ability to interpret an estimated statistical association in terms of elasticities arguably is strengthened by an association that matches theoretical expectations. Comparing statistical associations to theoretical predictions strengthens our ability to interpret the meaning of an estimated association.

2 Predictions regarding party agenda control

Many models of collective decision-making yield predictions about the roll calls that should be observed in equilibrium, but in the analysis that follows I focus on the party cartel theory of Cox and McCubbins (2005, 2007) to illustrate the difficulty of discerning the causal mechanism behind the selection of roll calls.[2] The party cartel theory argues that the majority party uses the rules governing the legislative agenda and other inducements to ensure that it is able to create and maintain a party brand based on its record of legislative accomplishment. To do so, party leaders anticipate the results of floor votes, allowing votes on the floor only if the outcome is consistent with the preferences of a majority of the majority party.

Some scholars argue that the ability to set the agenda is unconditional because the ability to do sos is derived from stable rules and institutions alongside a continuous incentive for members to defer to the party leadership for the purposes of maintaining an electorally valuable party brand that can help members maintain their seats and their majority status (Cox and McCubbins 2005, 2007). Others suggest that the ability to control the agenda varies over time. Lee (2016), for example, suggests that congressional behavior – and presumably also agenda control – responds to electoral incentives caused by the potential loss of majority status, a risk that increases when vote margins are narrower

[2]Krehbiel's (1998) pivotal politics model, and various versions of committee gatekeeping–see Crombez, Groseclose and Krehbiel (2006) for a review of a portion of the voluminous literature on committee gatekeeping in the USCongress–also predict the types of votes we should observe in equilibrium.

and when the majority has the most incentive to maintain their cohesiveness in the hope of retaining its majority status. Alternatively, agenda control may be easier with larger majorities because of the lesser ability of any individual majority party member to impact the agenda by defecting from the party.

Others emphasize the importance of members' policy preferences for agenda control. The conditional party government account (Rohde 1991; Aldrich 1995; Aldrich and Rhohde 1998), for example, suggests that the effects of agenda setting depend on the extent to which the majority party is unified internally and yet also distinct from the minority party. Krehbiel (1993) takes a contrarian view in questioning the ability to detect agenda setting by noting the difficulty of determining whether observed voting coalitions are a consequence of agenda setting by the majority party or the natural consequence of member preferences and majoritarian decision-making.

Determining whether agenda setting affects congressional behavior requires identifying whether observable patterns match theoretical expectations. Insofar as the observed patterns match the patterns predicted by majority party agenda control, the estimated empirical effects often are interpreted as being caused by agenda setting. The interpretation of any consistently estimated effect depends critically on the ability to compare estimated results to the predictions of a well-specified theory. That said, any interpretation necessarily is limited because theoretically consistent patterns may occur in the absence of agenda setting for other reasons (e.g., the distribution of members' preferences). The existence of clear theoretical predictions may increase our confidence in the ability to interpret a statistical association in terms of elasticities, but strong interpretations should be made with caution given the possibility of observationally equivalent alternative explanations.

Cox and McCubbins (2005, 2007) were among the first to show how majority party agenda control would affect voting coalitions on the permitted roll calls.[3] To do so, they examine

[3]To be clear, the record of observed roll calls does not necessarily reflect the record of congressional accomplishment (e.g., Schickler, Pearson and Feinstein 2010; Clinton and Lapinski 2011; Koger and Lebo 2017; Lee 2018) – especially in earlier periods. Focusing on agenda control in terms of observed votes

roll rates—the proportion of votes on which a majority of each party votes in opposition to the other. In particular, if the majority party controls the agenda then minority rolls– i.e., votes in which a majority of the majority party opposes a majority of the minority party and wins (i.e., the minority party is rolled)—should be prevalent and majority rolls —i.e., when a majority of the majority party opposes a majority of the minority party and loses—should be rare.[4]

Krehbiel, Meirowitz and Woon (2005) use an alternative measurement strategy to derive another set of predictions in terms of the types of votes that should not occur given a distribution of members' preferences and spatial voting - work later extended by Stiglitz and Weingast (2010). For roll call vote j in a unidimensional policy space, cutpoint κ_j defines the threshold that separates the legislators who are predicted to vote in favor of the proposal from those predicted to vote against it.[5]

Based on that work, several measures may be constructed to measure the extent to which the agenda is consistent with majority party agenda control.

One measure is the fraction of votes that are predicted to split the majority party. Insofar as the majority party is concerned with producing an agenda that allows the majority party to vote cohesively against a divided minority party, we would expect to observe more cutpoints located among the preferences of the minority party than than of the-majority party. Because the cutpoint of a vote defines the location of the member who

necessarily combines two processes – the choice of which issues to consider and the choice of which issues to resolve by recorded roll call votes (see, for example, Lynch and Madonna 2011). That the characterizations and divisions revealed by roll call voting may differ from members' policy preferences arguably highlights the question – can parties control the set of issues being voted upon to craft a party brand even if the brand is only a partial representation of actual preferences?

[4]As Cox and McCubbins (2007, p. 42) argue, "No dimension j on which the status quo is preferred to the floor median by a majority of the majority party is ever scheduled for floor consideration". In other words, we should observe floor activity only on those proposals that a majority of the majority party prefer to the status quo on a given issue dimension, j. That is, the probability of observing a roll call if a majority of the majority party prefers the status quo to proposal j is zero, and if a majority of the majority prefer proposal j to the status quo the probability not only presumably is greater than zero, but also presumably increasing in the level of majority party support.

[5]If b is the location of the outcome associated with voting yea in the policy space and q is the location associated with voting nay, $\kappa_j = (b + q)/2$.

is indifferent between voting yea or nay, an agenda that maintained perfect party unity would produce a set of cutpoints located entirely to the left of the most liberal Republican during Republican majorities and entirely to the right of the most conservative Democrat during Democratic majorities. Such votes would produce coalitions in which the majority party always was successful and always united.

A second measure is provided by considering the number of votes with cutpoints lying between the ideal policy outcome of the chamber median (x_{cham}) and the ideal policy outcome of the median majority party (x_{maj}). Those are votes that should not be observed if the majority party controls the agenda because they are votes on which a majority of the majority party is defeated by a coalition of majority party defectors and members of the minority party. Because the majority party is rolled on the floor for such votes, a party in control of the agenda would chose to prevent such votes from taking place. The majority party would rather schedule votes that roll the minority party – votes with cutpoints in the interval of $[x_{cham}, x_{min}]$.

Figure 1 summarizes the equivalence between cutpoints and roll rates with perfect spatial voting and a left-leaning majority party (see also Jenkins and Monroe 2015).

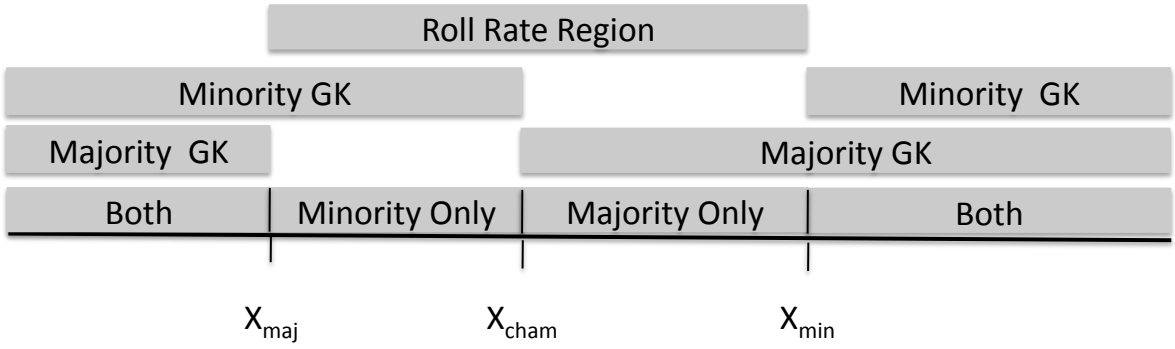

Figure 1: **Predictions of agenda control:** Given the ideal points of the majority party median member (x_{maj}), minority party median median (x_{min}), and median member of the chamber (x_{cham}), the labelled regions denote the spatial location of cutpoints $\boldsymbol{\kappa}$ producing majority and minority rolls ("Roll Rate Region") and that are consistent with majority party agenda control ("Majority GK").

If the majority party never allows a vote on which a majority of the majority votes for an unsuccessful outcome, we should only observe votes with cutpoints located in the region labelled Majority GK. If the agenda somehow was implausibly controlled by the minority party (but see Elgar 2016), we should observe cutpoints occurring only in the region labelled Minority GK. Because regions exist wherein the parties agree with one another – i.e., cutpoints that are more extreme than the party medians are votes that result in bipartisan coalitions – if the majority party seeks to select an agenda that distinguishes the parties from one another we would expect more votes to be located in the the regions labelled Majority Only because those are are votes that split the minority party to produce an outcome supported by a majority of the majority party.[6]

A third measure of agenda control results from comparing the percentage of votes located in the Majority Only region relative to the Minority Only region. Considering relative

[6]The analysis assumes, of course, that the brand of a party is defined by the voting behavior of a majority of the party. If a party brand is able to be defined by the positions taken by party extremists, then such votes may be valuable for branding the opposition party. While it certainly is possible – if not plausible given the willingness of both parties to use the positions of extreme members to characterize one another – I follow existing interpretations and assume that party brands are defined by the voting behavior of a majority of party members.

frequency is important because it is difficult to interpret the frequency of minority party rolls (i.e., votes with cutpoints located in the Majority Only region) without also knowing the frequency of majority party rolls (i.e., votes with cutpoints located in the Minority Only region). If 50% of the observed votes occur in the Majority Only region, for example, the plausibility of majority party agenda control being responsible for it depends on the distribution of the remaining votes. If the other votes all have cutpoints located in the Minority Only region – indicating that they are votes that split the majority party and result in the majority party being rolled on the floor – it is difficult to interpret the overall agenda as being consistent with majority party agenda setting.

3 Research design

Although the outcomes of interest reassuringly are straightforward given theoretical predictions, estimating the extent to which agenda control is responsible for producing such outcomes is exceptionally difficult. In general, the causal effects of a treatment can be identified by comparing the outcomes resulting from the random assignment of the treatment to the outcomes that result for the same unit in the absence of the treatment. Because we never observe the same unit being treated and untreated simultaneously, assumptions and statistical controls must be adopted to estimate the all-else-equal counterfactual. That already challenging task is made even more so for questions involving American political development and political institutions because the variation required to estimate the counterfactual often comes from over-time variation that is threatened by the difficulty of exhaustively and perfectly measuring the time-varying confounders.

To estimate the causal effect of agenda control in Congress we would ideally compare how the outcome measures described in the previous section vary depending on whether the majority party is randomly endowed with agenda setting power holding all else constant. Holding the composition of Congress fixed eliminates the confounding effects of person-

alities and preferences on the observed voting coalitions (and therefore the estimated cutpoints), and holding the political, social and economic environment fixed removes the impact of external (e.g., wars – Mayhew 2005) and internal pressures (e.g., Smith 1989) on the supply of and demand for votes. This is not easily done.

Most existing work attempts to estimate the presence of agenda control using over-time variation in majority party control (but see Jenkins 1999, 2000 for an analysis of the stability of voting behavior using the voting behavior of Southern members serving in the US House relative to their behavior in the party-less Confederate House). Many attempts conceptualize the relationship between majority party status and the roll call agenda using an empirical specification similar to:

$$Y_{D,t} = \alpha + \beta \text{ Dem Majority}_t + \gamma \boldsymbol{X}_t + \epsilon_t, \tag{1}$$

where $Y_{D,t}$ is a measure of the fraction of votes that split the majority party, α is the average percentage of votes splitting the Democratic Party when it is in the minority and the coefficient on majority party status β estimates how the percentage of votes splitting the party differs, on average, when the Democrats are in the majority (for a net effect of $\alpha + \beta$). For analogous regressions predicting the fraction of votes splitting Republican members ($Y_{R,t}$), α denotes the average percentage of Republican splitting votes when Republicans are in the majority and β denotes how that changes when Democrats are in control. Perfect agenda control would predicts $\alpha = 0$ when analyzing $Y_{R,t}$ and $\alpha + \beta = 0$ when analyzing $Y_{D,t}$.

Consistently estimating α and β requires using statistical controls for other other time-varying factors that may impact the types of votes being taken ($\boldsymbol{X}_t$). Moreover, because majority party status is non-random, a causal interpretation depends on whether majority party status can sensibly be assumed to occur “as if" random conditional on covariates. Such an assumption seems implausibly heroic given the context, but even if we are content

with the the consistent estimation of the partial correlation coefficients α and β we must still be able to control perfectly for time-varying confounders.

Several threats arise to our ability to consistently estimate the statistical association between majority party control and the roll call agenda. First, the inability to measure agenda control independently of majority party status means that any factor that covaries with majority party status (e.g., size, composition) potentially could account for the estimated differences being attributed to agenda control. Because the presence of agenda control is not measured independently of majority party control, the attribution of the estimated effects to agenda control is an attribution based on an assumption. Put differently, the fact that the observed pattern of roll call votes matches theoretical predictions cannot rule out the possibility that an observationally equivalent alternative mechanism is responsible (e.g., the similarity of party member preferences which makes it unlikely that a vote could divide the party or actions taken to construct the coalitions in the absence of agenda control on the floor, whipping, for instance). Any causal conclusion about the importance of agenda control necessarily is tentative in the absence of exhaustively considering the predictions of every potential mechanism. (See Krehbiel 1993 on the inferential problems caused by observational equivalence).

Second, even if we are willing to assume that all majority-party related effects are explained by agenda control, identifying situations that are all else equal with the exception of the party in control requires identifying and measuring perfectly the relevant set of time-varying statistical controls ($\boldsymbol{X}$). The conditional party government account, for example, argues that majority party agenda setting (β) depends on the extent of within-party and between-party preference homogeneity. If so, controlling for such variation is critical for estimating the effects of agenda control consistently. Controlling for the changing composition of Congress over time is required to eliminate the confounding effects of preference alignment. Because members with identical policy preferences will vote together regardless of whether the party is endowed with agenda setting power, controlling for varying

preference heterogeneity in the party caucus is needed to untangle the impact the effect of preference similarity from the impact of agenda control.

It is unclear whether available measures are up to the task. Extant work often relies on ideal points estimated using the observed roll calls to control for compositional changes over time, but ideal point estimates are problematic because, as Figure 1 makes clear, they are themselves a function of agenda control.[7] Recall that the likelihood function for a roll call estimator is:

$$L\left(\beta,\alpha,\boldsymbol{X}|\boldsymbol{Y}\right)=\Pi_{i=1}^{n}\Pi_{j=1}^{m}\Phi\left(x_{i}'\beta_{j}-\alpha_{j}\right)^{y_{ij}}\times\left(1-\Phi\left(x_{i}'\beta_{j}-\alpha_{j}\right)\right)^{(1-y_{ij})}$$

where the only observed parameter is the roll call vote of member i on vote j – y_{ij}. Both the estimated ideal points ($\hat{\boldsymbol{X}}$) and the estimated vote parameters (with midpoint $\frac{\alpha}{\beta}$) depend on the chosen agenda (i.e., the set of votes $\boldsymbol{Y}$) and the functional form of the spatial voting errors ($\Phi(\dot{)}$). As a result, measures commonly used to describe how the composition of Congress varies over time (e.g., distance between median members, the standard deviation of party ideal points) may depend on the chosen agenda if the frequency of voting error is low (Hirsch 2011; Clinton 2012). Put differently, when a roll-call based measure suggests that a party is likely to vote together, is that because the underlying preferences of the party members are very similar, or is the similarity in roll call measures a result of agenda setting by a party choosing to favor votes that unite the party? It obviously is problematic to use roll-call based measures to control for over-time variation in preferences if those measures also are a function of the treatment. That concern is perhaps most clearly illustrated by considering the patterns of roll call votes in

[7]In the analysis that follows, I use ideal points to summarize the propensity for members to vote together, on average, on the observed agenda regardless of their motivations. Because the probability that two members vote together on an issue is a function of the proximity of their estimated ideal points, a one-dimensional representation of ideal points summarizes the average likelihood that members will vote together, on the observed roll call votes. A unidimensional ideal point consequently is precisely the right measure for analyzing the ability of a party to create an agenda that fosters purposeful coalitions and splits.

parliamentary democracies – even though same party members likely vary in their policy preferences, the observed party-bloc voting behavior we observe cannot often distinguish between party members voting as they do because of agenda control, party pressure, and so on (e.g., Tsebelis 1994).

Given such complications, I adopt an alternative approach to account for compositional changes over time. To minimize the confounding effect of compositional changes when estimating the effects of agenda control, I focus on how the agenda affects the voting behavior of members serving together in consecutive Congresses.[8] To do so, I jointly scale the voting behavior of all members who serve in the pair of consecutive Congresses t and $t+1$ and who also vote on at least 50% of the votes in each Congress. That sample restriction identifies how the voting coalitions agenda in Congress t and Congress $t+1$ differentially affect the voting coalitions among the members common to both Congresses. To normalize the space within a pairing of consecutive Congresses, I estimate a fixed, unidimensional ideal point for each member in each pair of consecutive Congresses using `W-NOMINATE` (Poole and Rosenthal 1987; Poole 2005).[9] Because the outcomes are normalized as a percentage of the observed agenda – i.e., the fraction of votes that lie in an interval defined by partisanship and medians – the fact that the scale of the estimated ideal points varies between congressional pairings is inconsequential.

Fixing the set of members being compared within a Congressional pairing eliminates the effect of compositional changes on the estimated policy space and ensures that the estimated differences in the agenda effects between the two Congresses are attributable

[8]The idea conceptually is similar to a design used by Nokken and Poole (2004) to evaluate whether members change their voting behavior over time.

[9]Because unanimous and near-unanimous votes provide no and very little information for identifying the unknown parameters, respectively, such votes are omitted from the estimation of ideal points and midpoints. While they are uninformative for distinguishing between members' ideal points - and therefore for quantifying the extent to which agenda control is used to unite or expose the parties – they arguably are relevant for characterizing the amount of political conflict that occurs – a Congress with 100 unanimous votes and 10 party-line votes arguably differs qualitatively from a Congress with 0 unanimous votes and 10 party-line votes. Ignoring such votes is unproblematic for studying agenda control because unanimous votes are not obviously related to the desire and ability of the majority party to establish a party brand, but that consideration is important for scholars studying the level of polarization or partisan contestation.

to differences in the agenda. Because the membership is fixed, so too is the width of the partitions being used to classify the agenda. As a result, differences in the fraction of votes occurring in some region of the space result from differences in the votes being considered rather than differences in the width of the regions.

Describing how the agenda differentially affects voting coalitions among a fixed set of legislators is important for distinguishing the effects of the agenda from compositional changes, but it is not without risk. Because the estimated party and chamber medians for those serving in consecutive Congresses differ from the overall medians relevant for characterizing agenda control in Figure 1, the theoretically relevant partitions may differ from the analogous partitions defined using the set of continuing members. Nothing ensures that the median members of the chamber as a whole – which theoretically are relevant quantities for assessing agenda control – are similar to the medians computed using members who serve in both Congresses.

To evaluate the magnitude of that discrepancy, Figure 2 compares how the distance between the chamber median and the median Democrat (left) and Republican (right) using `DW-NOMINATE` applied to the set of all members in a given Congress (y-axis) compares to the analogous distance for members serving in consecutive Congresses computed using `W-NOMINATE` (x-axis). The distance measures graphed along the y-axes use the membership of the entire Congress, but the distance measures plotted along the x-axes use only the set of members who cast votes in consecutive Congresses.

Despite being measured on slightly difference scales, the distance measures are reassuringly similar – correlating at 0.73 for Democrats and 0.57 for Republicans. A closer inspection of the resulting patterns reveal the predictable consequences of focusing on continuing members when estimating the partition widths. When the agenda of the 104th Congress (1995-1996) is compared to the 103rd (1993-1994) using the 191 Democrats and 127 Republicans who serve in both Congresses, the median continuing member is -0.17

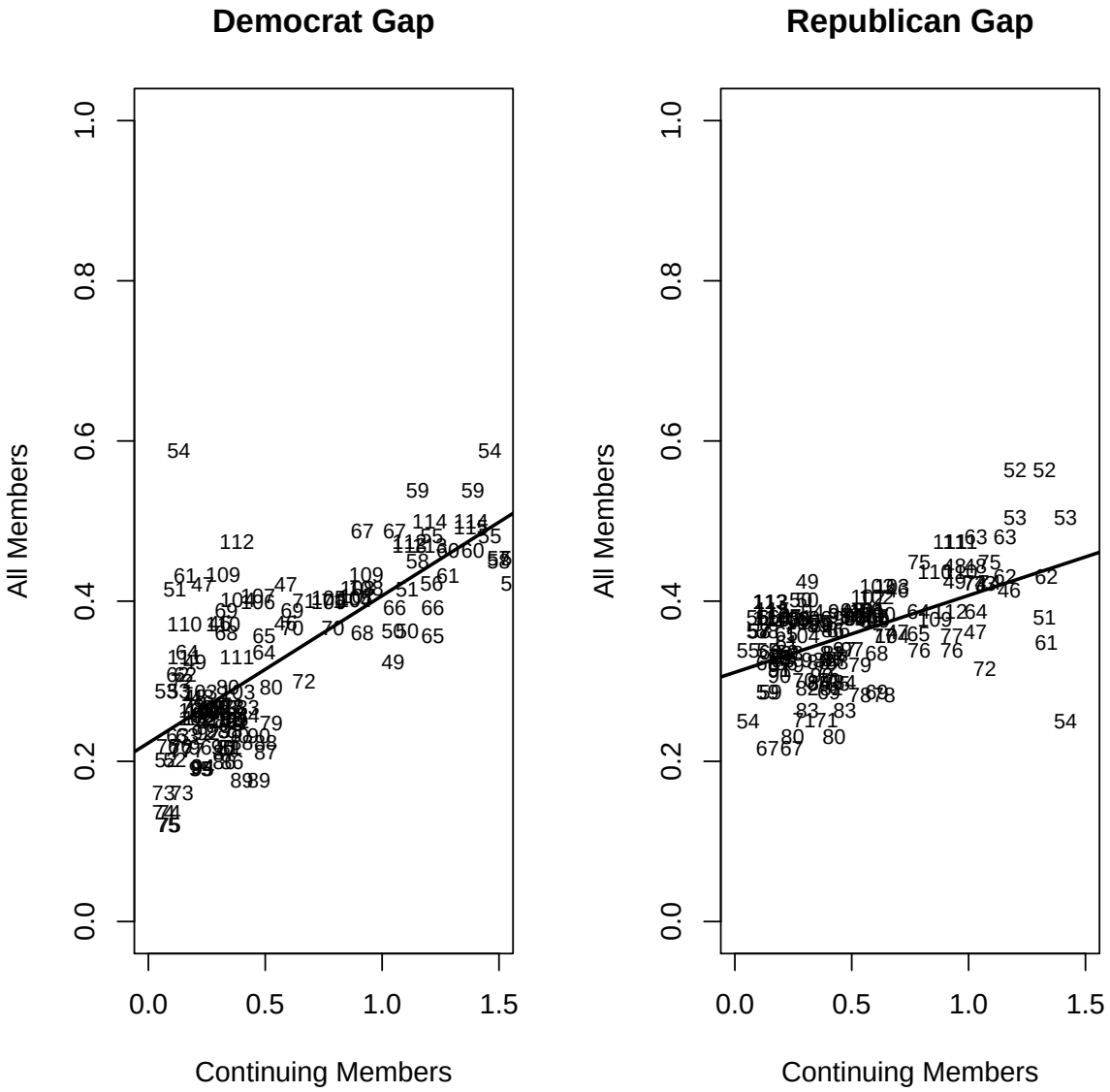

Figure 2: Comparing theoretically relevant partition widths. The x-axis plots the difference between the overall median and the relevant party median among members who serve in consecutive Congresses computed using `W-NOMINATE`. The y-axis plots the distance between the overall median and the relevant party median using all members serving in each Congress computed using `DW-NOMINATE`. Because each point is a single Congress, comparing every pair of consecutive Congresses produces two different distance measures because of compositional differences between congressional pairings (x-axis). The measured distances correlate at 0.73 among Democrats and 0.54 among Republicans.

(reflecting the fact that Democrats are in the majority in the 103rd), and the median Democrat and Republican are located at -0.56 and 0.50, respectively. Comparing the agenda of the 104th (1995-1996) to the 105th (1997-1998) using the 162 Democrats and

194 Republicans who serve in both Republican-led Congresses predictably shifts the overall median to 0.39, the Democrat median to -.50, and the Republican median to 0.68. Those changes are a direct consequence of compositional changes – relative to the members who serve in both the 103rd and 104th Congresses, 67 more Republicans and 29 fewer Democrats serve in both the 104th and 105th Congresses. Given those changes, the distance between the median Republican and median continuing member is .067 in the 103rd-104th pairing and 0.31 in the 104th-105th pairing.

The nobserved shifts highlight an unavoidable tradeoff – holding the composition fixed across majority party transitions means that the partition widths will fail to reflect the compositional change associated with the new majority. Because the overall median in the 103rd-104th pairing reflects the Democratic majority present in the 103rd Congress, the overall median for continuing members is too Democratic for the 104th Congress. As a result, it is possible that votes located between the overall median and the Republican median in the 104th Congress actually are consistent with majority party agenda control because they are votes that split Democrats and unite Republicans, making it hard to derive a precise point prediction as we no longer can predict to observe no votes in that interval. That said, we would still expect the fraction of votes occurring between the Republican median and the continuing member to vary in response to a change in majority status because Republicans presumably are less likely to allow such votes than Democrats. Although the resulting difference likely underestimates the actual magnitude of how much the agenda changed in response to agenda setting considerations, the direction of the change arguably is still informative about the existence of agenda control.

To ensure that the substantive conclusions are not affected adverselyby that complication, I also calculate the percentage of recorded votes that split the party caucus of continuing members (i.e., have a cutpoint more liberal than the most conservative Democrat or more conservative than the most liberal Republican). While the identity of the most liberal Republican or the most conservative Democrat varies based on compositional changes,

changes in that fraction provide another measure of the extent to which the agenda fractures the coalition of continuing party members. Insofar as the majority party seeks to use its agenda setting power to unite members behind a common brand or fracture the minority party caucus, the percentage of party-splitting votes should decline unambiguously when a party gains control of the agenda.

In addition to the confounding effects caused by compositional changes, many other time-varying effects may differentially affect a party's ability or desire to exercise agenda control and that consequently are important to account for. If the desire to use the legislative agenda to define a party brand (perhaps as opposed to the desire to legislate for commonly held goals) varies in response to changes in the political, social and economic environment – perhaps because of wars (Mayhew 2005), economic recessions, the increasing power of the executive branch vis-a-vis Congress, or the changing media environment – consistently estimating the relationship between majority party control and the observed roll call agenda requires identifying and measuring the potential confounding effects. That is a daunting, if not impossible, task. As an alternative, I rely on fixed effects to account for any systematic time-varying differences that may influence the congressional agenda between congressional pairings.

Relying on congressional pair fixed effects to identify the consequences of majority party status produces a difference-in-difference identification strategy wherein the effect of majority party control is identified by comparing the difference in the agendas occurring during a change in majority party control within a consecutive congressional pair relative to the within-pair differences in the absence of a change in majority party. To be sure, scholars have examined previously the effects of specific changes in majority control (e.g., Aldrich and Rohde 1998; Lee 2018), but the use of fixed effects allows me to estimate the average difference associated with a change in majority party status over the entire post-Reconstruction time period.

To identify how the agenda changes alongside changes in the majority party I estimate

the percentage of votes that occur in each pair of consecutive Congresses using three closely related outcome measures: the percentage of votes that split continuing party members, the percentage of votes between the continuing median and the median continuing member, and the difference in the percentage of votes splitting the medians by party. If $Y_{D,t}$ denotes the percentage of votes with cutpoints more liberal than the most conservative Democrat serving in both Congresses – i.e., the percentage of votes that split the Democrats voting in both congressional pairs – then the estimating equation is:

$$Y_{D,t} = \alpha + \beta \text{Dem Majority}_t + \delta_n + \epsilon_t \tag{2}$$

where t indexes the time period (e.g., Congress, congressional session) and δ_n denotes a fixed effect for each of the n consecutive congressional pairings. Given the outcome measure being used, if Democrats are in control of the majority we would expect to observe relatively fewer votes splitting the coalition of Democrats that serve in both Congresses relative to instances in which the Republicans are in control. Because α denotes the fraction of votes splitting the Democrats when the Republicans are in the majority, α should be close to 1, β should be close to -1 and the net effect of $\alpha + \beta$ should be near 0 if agenda control is complete – revealing that both parties prevent any votes that split their own caucuses when they are in control of the agenda.

The inclusion of the consecutive pairs fixed effects δ_n means that the identification of β results from from changes in majority party control within a consecutive pair; consecutive Congresses sharing the same majority party are unable to identify the effects of majority party control because it is impossible to distinguish the effects of majority party control from other time-varying effects. Repeating the analysis for $Y_{R,t}$ provides the Republican effect, although now α represents the effect of being in the majority (hence α should be -1) and β is the difference when in the minority (and therefore close to 1).

Replicating the analysis for the fraction of votes lying between the continuing median and

the party median – i.e., the fraction of votes between $x_{Cham,n}$ and $x_{Dem,n}$ for Democrats in pairing n and between $x_{Cham,n}$ and $x_{Rep,n}$ for Republicans in pairing n – should reveal a similar pattern. Even though the estimated partitions are most problematic during majority party transitions because of composition changes, we would still expect to observe a shift in the fraction of votes occurring in each region if the parties are more likely to allow votes that split the opposition party and unite their own party. Because the parameters denote the average change in the fraction of votes occurring in specified interval, the regression coefficients can be used to characterize the changing roll call agenda. In particular, we would expect that $\alpha > 0$ and $\beta < 0$ when predicting the fraction of votes between $x_{Cham,n}$ and $x_{Dem,n}$, and we would expect $\alpha < 0$ and $\beta > 0$ when predicting the fraction of votes between $x_{Cham,n}$ and $x_{Rep,n}$.[10]

A closely related alternative specification is provided by differencing the impact between Democrats and Republicans (i.e., the difference in the percentage of votes occurring between $x_{Cham,n}$ and $x_{Dem,n}$ and $x_{Cham,n}$ and $x_{Rep,n}$). So doing examines whether the set of recorded votes is more likely to unify the majority and also split the minority. Because positive values of those differences indicate a larger percentage of votes likely to split Democrats then Republicans, we would expect $\alpha > 0$ and $\beta < 0$. Examining that difference is important for interpreting the ability of the observed agenda to create a distinctive party brand. If the majority and minority party members are equally likely to be split by the chosen agenda it seems hard to conclude that the majority party is controlling the agenda to define a party brand. In contrast, sizable differences in the percentage of midpoints splitting the two parties should exist if the agenda purposefully is being set by the majority to protect the majority party caucus and fracture the minority party.

[10] Although it is possible to derive the expected direction of the parameters in the presence of agenda control it difficult to know what the precise point estimate should be in light of the complications caused by the potential for theoretically valid votes to occur within the interval during periods when majority control changes.

4 Estimating the effect of majority control on roll calls

To begin, Figure 3 summarizes the percentage of votes with cutpoints between the median Democrat and the median continuing member for each Congress in each pairing of consecutive post-Reconstruction Congresses. For example, when comparing the impact of the agenda on the members serving in both the 45th (1877-1878) and 46th Congresses (1879-1880), we find that those two Democrat-led Congresses each had fewer than 20% of recorded votes on which the median Democrat and the median continuing member were predicted to vote differently from one another. Because the same members are being analyzed in both Congresses, the slight, statistically indistinguishable, increase in the percentage of such votes occurring during the 46th Congress can be attributed to changes in the agenda rather than to changes in the composition.

In general, the pattern that emerges in the top plot of Figure 3 reveals that fewer votes with cutpoints dividing the overall median and median Democrat occur when the Democrats are in the majority (Congresses with a Democratic majority are circles) – suggesting that fewer votes are taken on which the Democrats are expected to lose when they are in the majority relative to when they are not. Even so, the pattern is far from universal and significant variation occurs both across time and also within specific pairings. Consistent with the findings of Schickler and Pearson (2009), for example, the Democratic Party during the 1940s frequently took votes that split the party caucus.

The bottom graph of Figure 3 reports the difference in those percentages between the second and first Congresses in each pairing. Differences that are greater than zero indicate instances in which the percentage of votes splitting the Democratic Party increases between the first and second Congress. To help highlight the changes during periods of majority party change, dark vertical lines denote a pairing when the Democrats gained the majority in the second Congress of the pairing and dark gray lines denote a pairing where the Democrats lost the majority in the first Congress of the pairing.

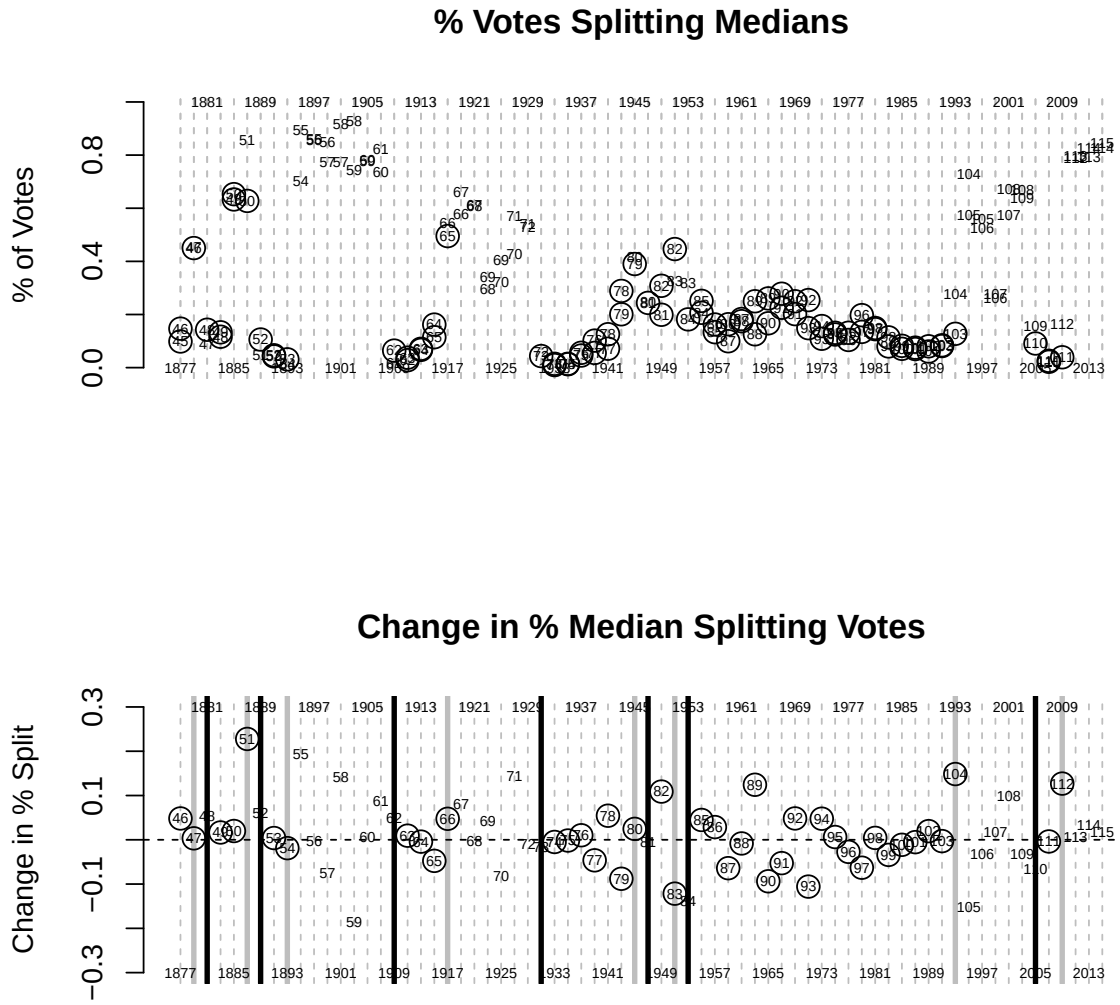

Figure 3: Percentage of votes splitting the median and Democratic median: The figures plot the percentage of midpoints falling between the median ideal point of Democrats and the median among members of each congressional pair. The percentage in each House is labelled accordingly in the top figure, and the bottom figure reports the difference between the second House (labelled) and the first House for each adjacent Congress. Circles indicate a Democratic majority.

If the agenda changes in response to a change in party control, we should observe an increase in the fraction of party-splitting votes when Democrats lose the majority (light grey lines), and a reduction in party-splitting votes when they gain the majority (black lines). While that pattern sometimes is apparent – following the 1994 midterm elections

in which Republicans gained control of the House, for example, the percentage of votes with midpoints falling between the median Democrat and the continuing median increased roughly 20% between the 103rd (1993-1994) and the 104th (1995-1996) Congress – it also is clear that non-trivial variation in the agenda existseven in the absence of a change in majority party within a consecutive pair. The variation in the agenda occurring in the absence of a change in majority control arguably provides a placebo test of the extent to which the agenda may vary in the absence of a change in majority control. This variation helps reveal whether the change that occurs during a majority party transition exceeds the agenda changes that occur in the absence of such change.

Table 1 reports the results. Specifications (1) and (2) compare the fraction of votes with midpoints that are estimated to lie between the party median and overall median for (1) Democrats and (2) Republicans, respectively. If we ignore all potential confounding effects, specification (1) suggests that 56% of the observed votes have midpoints between the median Democrat and the median continuing member when Democrats are in the minority – a fraction that falls by 39% to only 17% when Democrats control the majority. When Republicans are in the majority, 21% of the observed votes are expected to split the median Republican and the median member – a percentage that increases to 47% when they are in the minority.

Party:	Democrats	Republicans	Democrats	Republicans	Democrats	Republicans	Democrats	Republicans
% Cutpoints:	$\in [x_D, x_{Mdn}]$	$\in [x_{Mdn}, x_R]$	$\in [x_D, x_{Mdn}]$	$\in [x_{Mdn}, x_R]$	$\in [x_D, x_{Mdn}]$	$\in [x_{Mdn}, x_R]$	$<$ max $\boldsymbol{x}_{Dems}$	$<$ in $\boldsymbol{x}_{Dems}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem majority	-0.39	0.26	-0.16	0.05	-0.033	0.017	-0.031	0.078
	(0.03)	(0.04)	(0.04)	(0.04)	(0.021)	(0.048)	(0.030)	(0.026)
% Dems			-1.50	1.35				
			(0.15)	(0.19)				
log(# votes)			0.00	-0.00				
			(0.00)	(0.00)				
Intercept	0.56	0.21	1.21	-0.39				
	(0.03)	(0.03)	(0.07)	(0.09)				
Pair FE?	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.50	0.24	0.71	0.44	0.99	0.99	0.99	0.99

Table 1: Percentage of votes splitting overall and party medians: All estimates computed using `WNOMINATE`. Specifications (7) and (8) predict the fraction of votes that are to the left of the most conservative Democrat and to the right of the most liberal Republican, respectively. Standard errors are in parentheses.

To control for potential time-varying confounders, specifications (3) and (4) include the percentage of continuing members that are Democrats and the logged number of recorded roll call votes. The fraction of Democrats coarsely accounts for compositional differences between consecutive congressional pairs – although it is is unclear whether larger majorities should produce more or less agenda control. The number of roll calls accounts for the change in the use of roll calls over time – especially following the reforms of the 1970s – and perhaps also the emergence of new issues.

Including those crudely measured time-varying covariates highlights the difficulty of consistently estimating the effect. Controlling for the percentage of Democrats being analyzed shrinks the estimated effect of majority control among Democrats by more than half, and it suggests a near-zero effect among Republicans. Moreover, fewer party-splitting votes among Democrats occur when Democrats are more numerous – if the fraction of Democrats in a consecutive pair increases by 10%, the percentage of midpoints splitting the medians declines by 15% among Democrats (specification (4)) and increases by 13.5% among Republicans (specification (5)). Although majority control is obviously closely related to the percentage of Democrats, the relative effect of a change in Democratic control relative to the change in the percentage of Democrats suggests that the size of the party caucus is more consequential than the presence (or absence) of majority party status. Such a result is hard to square with expectations about an unconditional effect of agenda control.

Specifications (5) and (6) probe the relationship further by using consecutive pair fixed effects to control for over-time variation. So doing identifies the effect of majority party status by leveraging majority party transitions within a consecutive pair. Using within-pair differences to identify how the agenda changes in response to a change in majority party status reveals effects that are larger substantively and statistically distinguishable from zero at conventional levels.

Although specifications (1) through (4) seem to suggest a pattern that is consistent with

majority party agenda control when comparing levels over time using statistical controls, the reason why specifications (5) and (6) fail to find a statistically distinguishable effect is revealed by the bottom graph of Figure 3. Although the Democrats' loss of majority status in the 112th (2011-2013) and 104th (1994-1995) Congresses resulted in many more votes that split their party caucus relative to when they were in the majority, such changes are far from universal. Although similar shifts in the agenda occurred during the change associated with the 51st and 66th Congresses, the percentage of midpoints splitting the Democrats essentially was unchanged following the loss of majority control in the 80th, 54th and 47th Congresses and it actually fell following the loss in the 83rd. Moreover, although the percentage of votes splitting Democrats sometimes increase when Republicans took control, the percentage of votes splitting the Democrats does not routinely shrink when Democrats obtain the majority. When the Democrats took over in the 111th Congress, for example, the agenda was equally likely to split the set of Democrats serving in both the 110th and 111th Congresses as the agenda in the 110th Congress..

To further characterize the relationship between majority party control and the roll call agenda, Figure 4 plots the estimated fixed effects from specifications (5) and (6). Each fixed effect represents the average percentage of votes that is estimated to occur between the party median and overall median for Democrats (top) and Republicans (bottom).

Figure 4 reveals that each party is indeed less likely to be split by the recorded roll call agenda when they are in the majority relative to when they are in the minority – a pattern that is broadly consistent with agenda control by the majority party. However, a non-trivial percentage of votes continue to split the majority party – especially Democrats throughout the 1950s, 1960s and 1970s – and the variation in the percentages over time suggest that even if the patterns are explained by agenda control, the ability to control the agenda certainly is not unconditional.

Because the effects of agenda control are underestimated owing to the inability to account

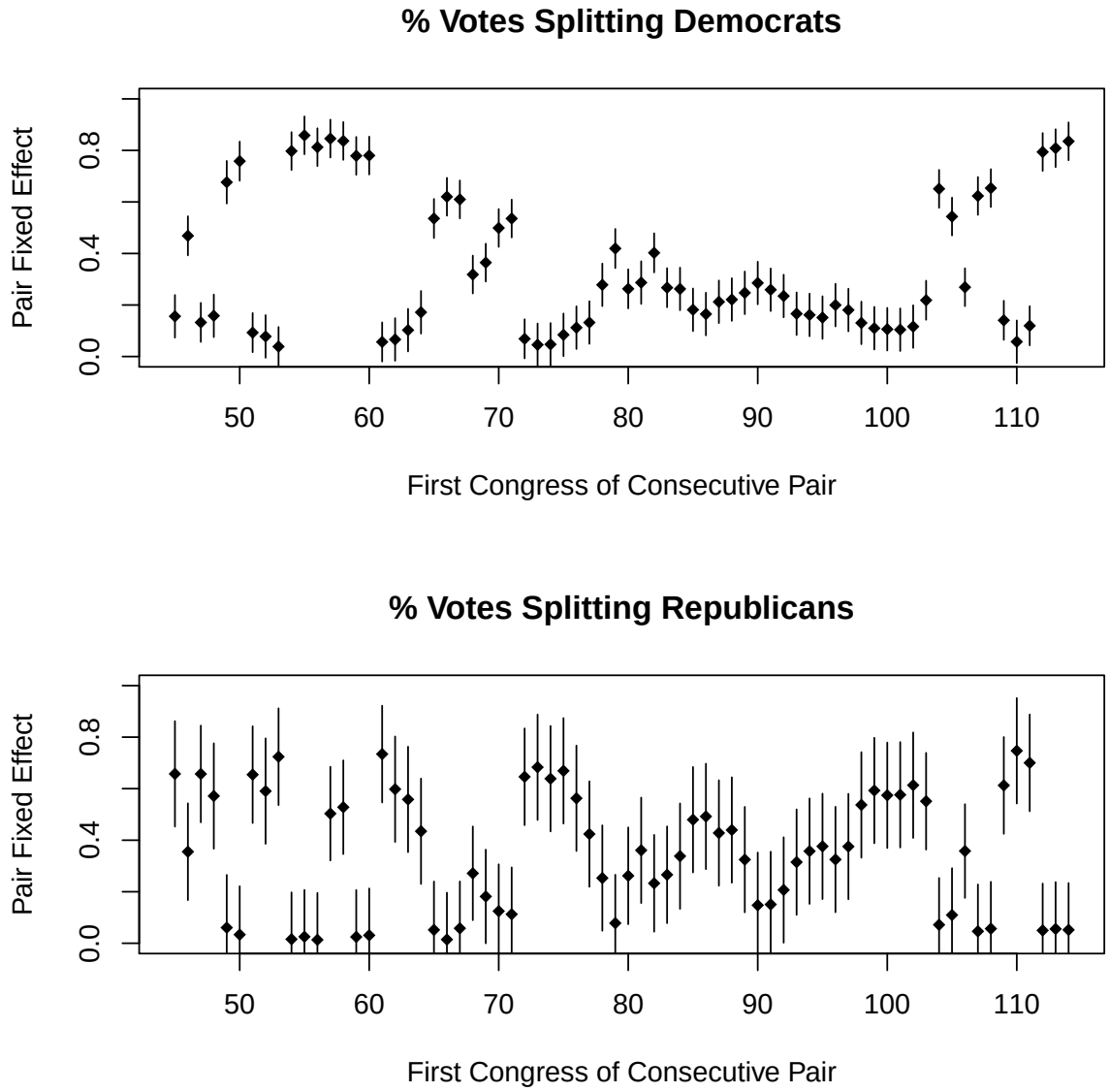

Figure 4: The figures plot the coefficient estimates (and 95% confidence intervals) for the fixed effect estimates in specifications 5 (Democrats) and 6 (Republicans) of Table 1. Each fixed effect identifies the average percentage of votes that split the party median and overall median for the pair of consecutive Congresses.

for shifts in the medians of theoretical interest when holding the composition fixed across consecutive Congresses, specifications (7) and (8) employ an alternative measure of party-splitting behavior – the percentage of roll calls that are estimated to split the set of party members serving in both Congresses. While we are less likely to observe votes that split Democrats when Democrats are in the majority (7) and more likely to observe votes that

split Republicans (8), the magnitude of the estimates effects are far from compelling. The fact that the percentage of votes splitting Republicans is predicted to increase by only 7.8% when Democrats obtain the majority relative to the agenda chosen by Republicans among the same set of members seems rather insignificant – especially considering that no changwe is predicted in the percentage of votes splitting Democrats.

5 Estimating the differential effect

The percentage of votes splitting each party is information about whether the agenda is used to unite the majority party or not, but it provides a partial measure of agenda setting insofar as we also care about how the agenda may be used to split the opposition party. If the agenda is used to create a party brand, we would expect a majority party to choose an agenda that not only unites itself, but also fractures the other opposition party to prevent it from being able to define a coherent brand. An agenda with a small percentage of party-splitting votes because the remaining votes produce bipartisan coalitions is politics of a different type than an agenda that seeks to expose splits within the minority party. Examining the difference in party splitting votes helps characterize those possibilities.

To identify how the chosen agenda differentially impacts the minority relative to the majority party, I calculate the percentage of votes splitting the median from the party median for the two parties. Again using the set of members casting votes in consecutive Congresses, I calculate: $\Delta\%Midpoints \in$ [Dem. Median, Chamber Median] − $\Delta\%Midpoints \in$ [Chamber Median,Rep. Median]. Differences near zero occur when the parties are equally likely to be split (unlikely if agenda control exists), differences greater than zero indicate instances in which Democrats are more likely to be split (expected under Republican agenda control), and differences less than zero indicate instances in which Republicans are more likely to be split (expected under Democratic agenda control).

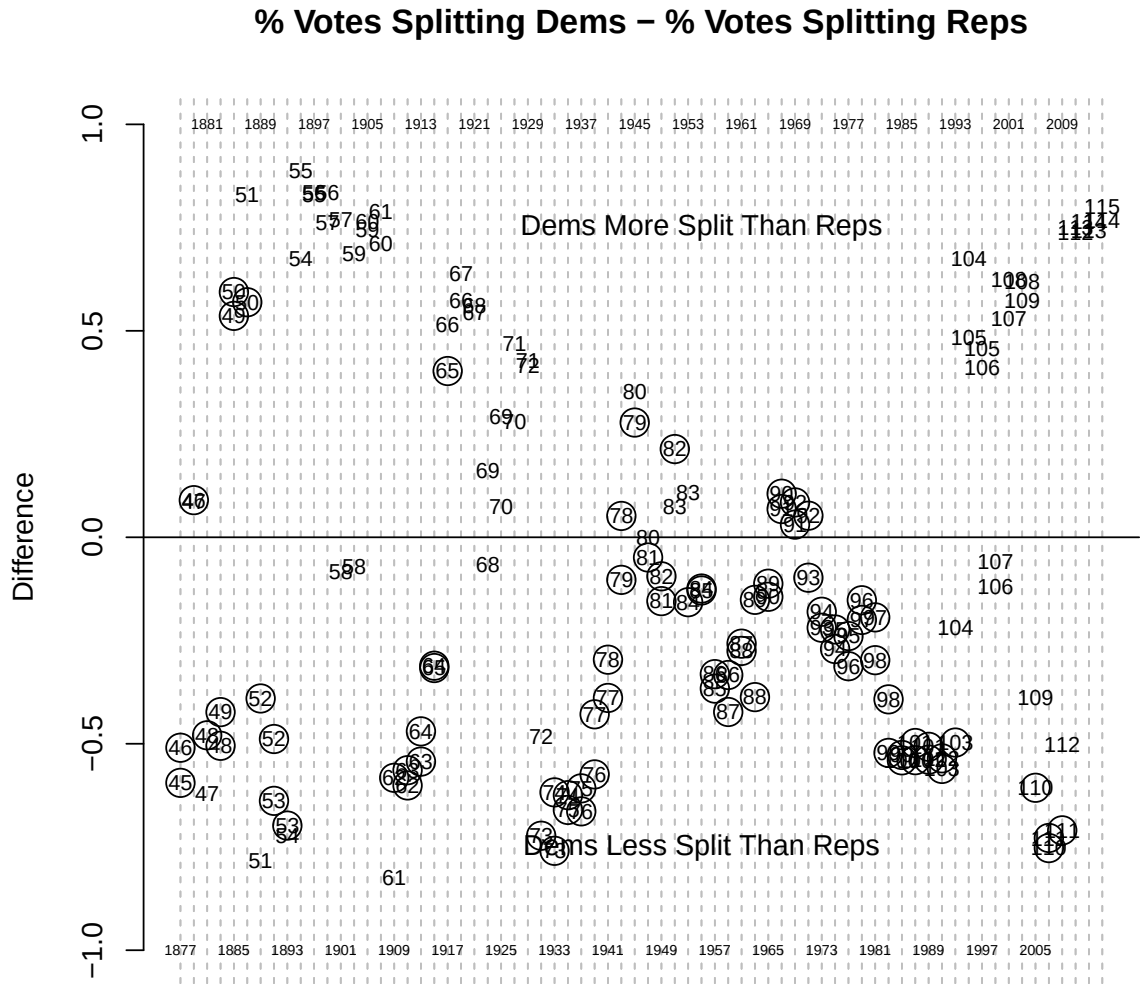

Figure 5: Differential impact by party: The figures plot the difference in the percentages of midpoints falling between the median ideal point of Democrats and the median among members of each congressional pair and the corresponding percentages for Republicans. Circles indicate Houses with a Democratic majority.

Figure 6 graphs the difference over time. Because each consecutive pair contains two Congresses, each point reports the difference in the percentage of recorded roll calls that are predicted to fall into that region for each party for each Congress.

The raw relationship of Figure 5 appears broadly consistent with majority party agenda

setting. When Democrats are in the majority (indicated by a circled Congress), the observed votes are more likely to split Republicans than Democrats. Conversely, the set of observed votes are less likely to split Republicans than Democrats when Republicans are in the majority. That said, a non-trivial amount of variation is observed in the difference over time and in quite a few Congresses the agenda splits the majority and minority parties similarly. Although some of those instances reflect the influence of the Conservative Coalition in the Democratic Party, the larger point is that the variation is not always obviously related to changes in agenda control.

Table 2 probes the foregoing relationship in more detail to confirm the lack of a systematic correlation between the types of votes being taken and the party that presumably is in control of the agenda. Specification (1) analyzes the variation without statistical controls to reveal that party-splitting votes do appear to covary with majority party status. When Democrats are in the minority, 30.9% more roll calls have midpoints lying between the Democrat median and the common median than between the Republican median and the common median. When Democrats are in the majority, Democrats hold roughly 12% fewer roll calls between the Democrat median and common median than fall between the Republican median and common median. Although broadly consistent with what we would expect from majority party agenda setting, the differences are surprisingly small and it seems that parties are constrained in their ability to hold votes that unite their own party and split the opposition.[11]

Entering covariates to account for time-wise differences in specification (2) again reveals the importance and difficulty of estimating the relationship between majority party status and the percentage of party-splitting votes consistently using over-time variation. Once we control for the percentage of Democrats in a consecutive pair of Congresses the independent effects of majority status decline dramatically. While a statistically distinguish-

[11]While the effects for Republicans appear to be largervthan for Democrats (perhaps because of the Conservative Coalition), investigating the potential reasons for this difference is difficult given the available data.

	Difference (1)	Difference (2)	Difference (3)	Difference (4)
Dem majority	-0.65	-0.21	-0.05	-0.11
	(0.07)	(0.07)	(0.05)	(0.05)
% Dems		-2.87		
		(0.29)		
log(# votes)		0.02		
		(0.03)		
Intercept	0.34	1.50		
	(0.05)	(0.22)		
Pair FE?	No	No	Yes	Yes
R^2	0.42	0.65	0.97	0.83

Table 2: Estimating the difference in party-splitting Votes: Specifications (1) - (3) model the differences in the percentage of votes between the Democratic median and the overall median and the percentage of votes between the Republican median and the overall median. Specification (4) predicts the difference in the percentage of cutpoints more liberal than the most conservative Democrat and the percentage of cutpoints more conservative than the most liberal Republican. Standard errors are in parentheses.

able effect remains, the size of the difference between Democrat-led and Republican-led Congresses shrinks by more than half. Moreover, as with the undifferenced results, the results suggest the importance of numerical superiority – a 10% increase in the fraction of Democrats reduces the difference in the percentage of votes splitting the two parties by 28.7%. Given the meaning of the difference, the implication is that votes are more likely to split Republicans and less likely to split Democrats. That a 10% shift in the composition of the chamber is predicted to affect the agenda more than the predicted impact of a Democratic majority is hard to square with an account of unconditional agenda control.

If we rely on fixed effects to account for over-time variation, we find even less support for agenda control. Specification (3) reveals that the difference in the percentage of votes splitting the two parties within a consecutive congressional pair does not change systematically when majority control reverses within that pairing. Examining the effect of changes in majority control within a consecutive pairing in terms of how the agenda differentially affects the set of members serving both before and after the change in majority control does not provide evidence consistent with a change in the agenda. Given the com-

plications caused by holding the composition fixed during a change in majority control, specification (4) examines an outcome that arguably is less sensitive to that concern – the difference in the percentage of votes that are are more liberal than the most conservative Democrat (and which therefore split the Democrats) and the percentage of votes that are are more conservative than the most liberal Republican (and which therefore split the Republicans). We would expect that Democrats are less likely to be split by the agenda when the Democrats are in control and that is precisely what the results of specification (4) reveal. That said, the magnitude of the expected effect is rather small – changing from a Republican majority to a Democratic majority is predicted to result in an 11-point shift in the difference in the percentage of party-splitting votes. Given the nature of the differencing, that result is consistent with either 11% fewer votes splitting Democrats and an unchanged percentage of votes splitting Republicans, 11% more votes splitting Republicans and an unchanged percentage of votes splitting Democrats, or 5.5% fewer votes splitting Democrats and 5.5% more votes splitting Republicans. Although statistically distinguishable from zero, the magnitude of the estimated effects seem underwhelming.

Figure 6 reveals the reason for low-magnitude effects by graphing the difference in the difference over time. Large shifts in the differential impact of the agenda on the continuing members for each party indeed are associated with changes in majority control in Congresses during recent transitions – the change between the 103rd and 104th and the 111th and 112th produce many more cutpoints splitting Democrats than Republicans and the change between the 109th and 110th increases the number of votes splitting Republicans greatly– but the effects of earlier transitions are far less dramatic. As a result, the large changes in the agenda that occur in recent Congresses are muted considerably by the lack of similarly large effects during earlier transitions.

Because of the need to assume that agenda control and majority party status are synonymous, we cannot evaluate empirically whether those seemingly important temporal

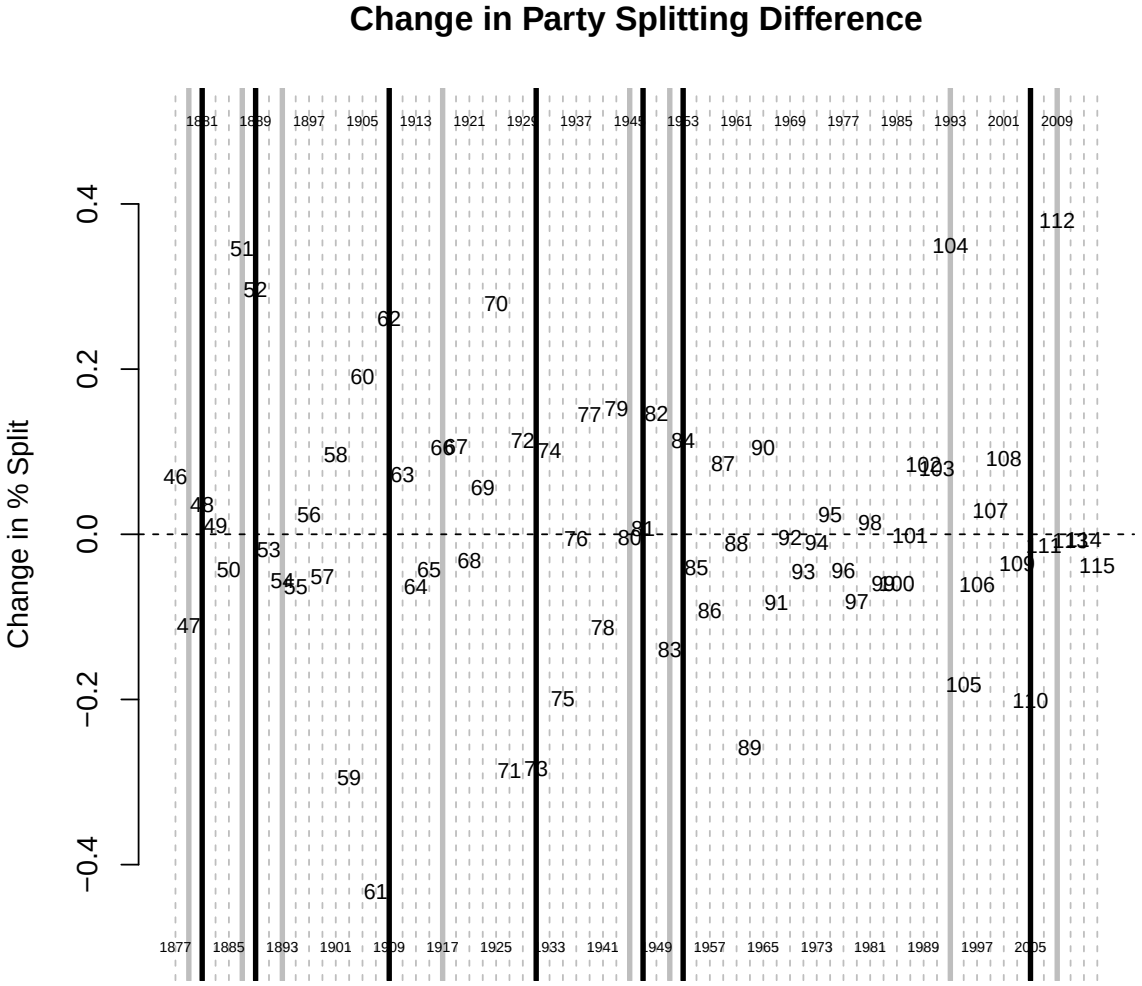

Figure 6: The difference in party-splitting votes between congressional pairs: The points depict the difference in the difference in party splitting votes graphed in Figure 5 between the Congresses within each congressional pair. Black vertical lines denote a difference in which the Democrats obtain the majority and grey vertical lines denote a difference in which the Democrats lose the majority.

differences are attributable to a change in the use of agenda control (e.g., only recent majorities are able to control the agenda because of changes in the institutional environment or the incentives for members to defer to the majority party's leadershipto create a party brand given the electoral and media environment) or whether the difference indicates a

change in the effects of agenda control (e.g., even if earlier majority parties had the ability to control the agenda, they choose to use such power more sparingly than majorities in recent Congresses). Such differences are qualitatively important for characterizing congressional behavior and majority party power, but in the absence of measures of agenda setting that are independent of majority party control, such investigations are impossible.

Another limitation is the paucity of changes in majority control. The historical record provides relatively few instances of party transitions and those changes provide the best opportunity for identifying the association between majority party status and the agenda of recorded votes. While we might expect that the ability to control the agenda for the purposes of creating and maintaining an electorally valuable party brand might affect the the probability that a majority party would lose its control over the process – i.e., if parties truly are effective at wielding agenda control, then we should observe relatively few changes in majority party status – an absence of such changes hampers our efforts at identifying the effects of agenda control. Another consequence of the lack of changes in majority party status is the impossibility of determining why the effects are larger in recent periods; potential explanations for the time-varying difference far exceed the number of data points (transitions).

6 Discussion

Political scientists have increasingly focused on estimating causal relationships, but the requirements for making causal interpretations are not easily met for those interested in national institutions or American political development. It often is difficult to interpret the treatment as being as if random and to measure all of the potential confounding relationships to conclude that that the variation is as if random conditional on included covariates. While abandoning the study of national institutions may sometimes yield dividends for those interested in causal effects (see, for example, Feigenbaum, Fouirnaies

and Hall 2017), the ability to generalize across contexts is unclear.

The response to such difficulties cannot be to abandon questions for which causality may be impossible to determine. So doing would limit the scope of political science to questions that would almost certainly exclude questions of first-order importance. Even if the resulting analyeses are descriptive, characterizing how the political environment varies in terms of the potential inputs and outputs of political processes frequently is often critical for describing the nature of politics and prompting further inquiries. Consider, for example, the enormous literature on the nature of lawmaking prompted by Mayhew's (1991) work comparing the outputs of unified and divided governments, or the scholarship probing the causes and consequences of the elite polarization that was made possible by the landmark work of Poole and Rosenthal (1984, 1997, 2004).

Of course, not every characterization and measure is equally valuable. To minimize the threats posed by spurious associations and characterizations, the task of measurement and description must accompany the development of well-specified theories. The importance of empirical characterizations depends on whether the relationships correspond to extant theories or else help develop new theorizing about about the nature of the political process.

This paper contributes to that effort by examining the statistical association between changes in majority control and the changing roll call agenda in the post-Reconstruction US House of Representatives. Interpreting changes in the roll call record over time as being a result of agenda control is extremely challenging from a causal inference perspective. In addition to the usual concerns about non-random treatment assignment and the difficulty of distinguishing agenda control from other aspects that are closely related to majority status, the need to leverage over-time variation when estimating the counterfactual creates a difficult measurement task in light of the numerous time-varying differences in context.

Building on a robust existing empirical literature, I identify how changes in majority status affect how recorded congressional votes affect members serving before and after a change

in the majority party. Rather than employing statistical controls to make the required over-time comparisons – a task that arguably is impossible given the lack of measures that can adequately control for the many ways in which the circumstances change over time – I use specifications inspired by attempts to estimate causal effects. In particular, I identify how members serving in consecutive Congresses are affected differentially by the roll call agenda and whether those effects covary with changes in majority control as theory would predict. Fixed effects models are used to estimate the average effect and account for the numerous ways in which the political, economic and social circumstances have changed.

The raw patterns are broadly consistent with theoretical predictions regarding majority party agenda setting – fewer votes are taken with cutpoints dividing the median party member and the continuing floor median when a party is in the majority relative to when it is not – but the estimated relationship is far from robust. Including additional controls – fixed effects to identify the effects of a change in majority status between consecutive Congresses – halves the estimated estimate and suggests that the agenda also varies depending on the size of the majority party. Using fixed effects to examine the change within a congressional pairing suggests an even smaller overall effect. Although sizable effects are observed during the last three changes in majority control that are consistent with theoretical predictions, the pattern is far from universal and the paucity of such transitions make it impossible to ascertain empirically why the effects occur only in recent Congresses.

The characterizations I offer contribute to our understanding of the extent to which the roll call agenda may respond to the incentives of the majority party by examining the relationship using a slightly different approach than does the extant literature, but they also highlight several broader considerations. First, the existence of a well-specified theory is critical for interpreting any estimated effect – not only because it helps define the measurable outcomes relevant for assessing the effects of agenda control, but also because

it is essential for interpreting the estimated statistical associations. Particularly in the case here wherein agenda control and majority party status are assumed to be synonymous, it is impossible to determine whether the identified effects are a consequence of agenda control or some other trait that covaries with majority status. Interpreting the mechanism responsible for the identified effect depends critically on whether the estimated associations match theoretical predictions; only when the estimated associations match theoretical predictions should we be confident in attributing the effects to agenda control (and even then the potential for observationally equivalent alternative interpretations cannot be eliminated; see Krehbiel 1993).

Second, work focused on national institutions often must leverage over-time comparisons when estimating a statistical associations. A consistent estimate of a statistical association must account for all of the ways in which the social, political and economic environment may influence the relationship. Moreover, the presence of measurement error in any of the correlated measures make it impossible to identify the true relationship even with an infinite amount of data. Those limitations occur even if we are content to estimate non-causal statistical associations. Some concerns may be rectified by better measures or an alternative identification strategy that removes the need for explicit measures (e.g., the use of fixed effects), but a paucity of data often makes it impossible to precisely estimate the relationship. The lack of changes in majority control in the post-Reconstruction US House, for example, makes it impossible to know why more effects consistent with agenda control are observed in more recent Congresses. The response to such limitations cannot be to abandon asking questions, but rather to acknowledge how such limitations may affect the the conclusions that are possible.

An expansion of scholarship focusing on causal inference appropriately has highlighted what must be true to interpret an estimated association as being a causal relationship. Recognizing and acknowledging whether empirical correllations are causal or not certainly is essential for understanding the current state of knowledge, but measurement and the-

orizing are equally important. The significance of determining causality is clear, but it is only because of well-specified theories that we are able to interpret (and generalize) causal estimates. Only if the discipline recognizes – and rewards – the contributions of each of those endeavors will political science advance.

Aldrich, J. H. (1995). *Why parties? The origin and transformation of party politics in America*. Chicago: University of Chicago Press.

Aldrich, J. H., & Rohde, D. W. (1998). The Transition to Republican rule in the House: Implications for theories of congressional politics, *Political Science Quarterly*, 112(4), 541-567.

Binder, S. A. (2003). *Stalemate: causes and consequences of legislative gridlock*. Washington, D.C.: The Brookings Institution Press.

Carson, J. L., Monroe, N. W., & Robinson, G. (2011). Unpacking agenda control in Congress: individual roll rates and the Republican revolution, *Political Research Quarterly*, 64(1): 17-30.

Carrubba, C., Gabel M., & Hug, S. (2011). Legislative voting behavior, seen and unseen: a theory of roll-call vote selection, *Legislative Studies Quarterly* 33(4): 543-572.

Clinton, J. D. (2007). Lawmaking and roll calls, *Journal of Politics* 69(2): 455-67.

Clinton, J. D. (2012). Using roll call estimates to test models of politics, *Annual Review of Political Science*. Vol. 15:79-99.

Clinton, J. D., Jackman, S., & Rivers, D. (2004). The statistical analysis of roll-call voting: a unified approach, *American Political Science Review* 98(2): 355-70.

Clinton, J. D., & Lapinski, J. S. (2011). Laws and roll calls in the U.S. Congress, 1891-1994, *Legislative Studies Quarterly* 33(4): 511-541.

Clinton, J. D., Bateman, D., & Lapinski, J. S. (2017). A House divided? Political conflict and polarization in the U.S. Congress, 1877-2011, *American Journal of Political Science*. 60(5): 866-898.

Cox, G., & McCubbins, M. (2005). *Setting the agenda: responsible party government in the U.S. House of Representatives*. New York: Cambridge University Press.

Cox, G., & McCubbins, M. (2007). *Legislative leviathan: party government in the House, 2nd Edition*. New York: Cambridge University Press.

Egar, W. T. (2016). Tarnishing opponents, polarizing congress: the House minority party and the construction of the roll-call record, *Legislative Studies Quarterly* 41(4): 935-964.

Feigenbaum, J. J., Fouirnaies, A., & Hall, A. B. (2017). Majority-party disadvantage: revising theories of legislative organization, *Quarterly Journal of Political Science*, 12(3):

269-300.

Gailmard, S., & Jenkins, J. A. (2007). Negative agenda control in the Senate and House of Representatives: fingerprints of majority party power, *Journal of Politics* 69 (3): 689-700.

Jenkins, J. A. (1999). Examining the bonding effects of party: a comparative analysis of roll-call Voting in the U.S. and Confederate Houses, *American Journal of Political Science* 43(4):1144-1165.

Jenkins, J. A. (2000). Examining the robustness of ideological voting: evidence from the Confederate House of Representatives, *American Journal of Political Science* 44(4): 811-22.

Jenkins, J. A., & Monroe, N. S. (2012). "Partisan agenda control in the U.S. House: a theoretical exploration, *Journal of Theoretical Politics* 24(4): 70:555-570.

Jenkins, J. A., & Monroe, N. S. (2015). "On measuring legislative agenda setting power, *American Journal of Political Science* 60 (1): 158-174.

Jenkins, J. A., & Stewart, C. III. (2013). *Fighting for the speakership: the House and the rise of party government.* Princeton: Princeton University Press.

Harbridge, L. (2015). *Is bipartisanship dead? Policy agreement and agenda-setting in the House of Representatives.* New York: Cambridge University Press.

Hirsch, A. V. (2011). A monte carlo study of theory driven bias in ideal point estimation, *Political Analysis* 19 (1): 87-102.

Kingdon, J. W. (1989). *Congressmen's voting decisions. 3d ed.* Ann Arbor, MI: University of Michigan Press.

Koger, G., & Lebo, M. J. (2017). *Strategic party government: why winning trumps ideology.* Chicago: University of Chicago Press.

Krehbiel, K. (1993). Where's the party?, *British Journal of Political Science* 23 (2):235-66.

Krehbiel, K. (1998). *Pivotal politics: a theory of U.S. lawmaking.* Chicago, IL: University of Chicago Press.

Krehbiel, K. (2007). Partisan roll rates in a non-partisan legislature, *Journal of Law, Economics, and Organization* 23 (1): 1-23.

Krehbiel, K., Meirowitz, A., & Woon, J. (2005). Testing theories of lawmaking, Social

Choice and Strategic Decisions, number XVI in *Studies in Choice and Welfare.*

Krehbiel, K., Crombez, C., & Groseclose, T. (2006). Gatekeeping, *Journal of Politics* 68(2):322-34.

Lee, F. E. (2009). *Beyond ideology: politics, principles and partisanship in the U.S. Senate.* Chicago: University of Chicago Press.

Lee, F. E. (2016). Patronage, logrolls, and polarization: congressional parties of the Gilded Age, 1876 - 1896, *Studies in American Political Development* 30(2): 116-117.

Lee, F. E. (2016). *Insecure majorities: Congress and the perpetual campaign.* Chicago: University of Chicago Press.

Lee, F. E. (2018). The 115th Congress and questions of party unity in a polarized era, *Journal of Politics* 80(4): 1464-1473.

Lynch, M. S., & Madonna, A. J. (2012). Viva voce: implications from the disappearing voice vote, 1865 - 1996, *Social Science Quarterly* 94(2):530-550.

Madonna, A. J. (2011). Winning coalition formation in the U.S. Senate: The effects of legislative decision rules and agenda change, *American Journal of Political Science* 55(2): 276-288.

Mayhew, D. R. (1991). *Divided we govern: party control, lawmaking, and investigations, 1946-1990.* New Haven, CT: Yale University Press.

Mayhew, D. R. (2005). Wars and American politics, *Perspectives on Politics* 3 (3): 473-93.

Monroe, N. W., & Robinson, G. (2008). "Do restrictive rules produce nonmedian outcomes? A theory with evidence from the 101st-108th Congresses, *Journal of Politics* 70(1): 217-31.

Nokken, T. P., & Poole, K. T. (2004). Congressional party defection in American history, *Legislative Studies Quarterly* 29(4): 545-68.

Pearson, K., & Schickler, E. (2009). "Discharge petitions, agenda control, and the Congressional committee system, 1929-1976, *Journal of Politics* 71(4): 1238-56.

Pearson, K. (2015). *Party discipline in the House of Representatives.* Ann Arbor: The University of Michigan Press.

Poole, K. T. (2005). *Spatial models of parliamentary voting.* New York: Cambridge

University Press.

Poole, K. T. & Rosenthal, H. (1984). The polarization of American politics, *Journal of Politics* 46(4): 1061-1079.

Poole, K. T. & Rosenthal, H. (1997). *Congress: a political-economic history of roll call voting.* New York: Oxford University Press.

Poole, K. T. & Rosenthal, H. (2007). *Ideology and Congress.* New York: Transaction Press.

Rohde, D. W. (1991). *Parties and leaders in the postreform House.* Chicago: University of Chicago Press.

Schickler, E., & Rich, A. (1997). Controlling the floor: parties as proceduralcoalitions in the House, *American Journal of Political Science* 41(4):1340-75.

Schickler, E,. & Pearson, K. (2009). Agenda control, majority party power, and the House committee on Rules, 1939-1952, *Legislative Studies Quarterly* 34(4): 455-91.

Schickler, E., Pearson, K., & Feinstein, B. (2010). Congressional parties and civil rights politics from 1933-1972,, *The Journal of Politics* 72(3): 672-689.

Smith, S. S. (1989). *Call to order: floor politics in the House and Senate.* Washington, DC: Brookings Institute.

Snyder, J. M., Jr., & Groseclose, T. (2000). Estimating party influence in Congressional roll call voting, *American Journal of Political Science* 44(2): 193-211.

Stiglitz, E. H., & Weingast, B. R. (2010). Agenda control in Congress: evidence from cutpoint estimates and ideal point uncertainty, *Legislative Studies Quarterly* 35(2): 157-185.

Theriault, S., Hickey, P., & Blass, A. (2011). Roll-call cotes, in the *Oxford Handbook of the American Congress*, Eric Schickler and Frances Lee (eds.). New York: Oxford University Press. pp. 575-597.

Tsebelis, G. (1994). The power of the European Parliament as a conditional agenda setter, *American Political Science Review* 88 (1): 128-42.

VanDoren, P. M. (1990). Can we learn the causes of Congressional decisions from roll call data? *Legislative Studies Quarterly* 15(3): 311-40.