Roll Calls, Voting Coalitions, and Possible Agenda Control in the U.S. House of Representatives: 1869 - 2024

Joshua D. Clinton

March 1, 2024

Abstract

How well can roll calls detect the causal impact of majority party agenda setting in the House of Representatives? 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 are themselves endogenous to the congressional agenda. To isolate the effect of agenda changes due to changes in majority status from compositional changes in the members who are serving I evaluate how the congressional agenda affects coalitions of members serving in consecutive Congresses. Using fixed effects to account for time-varying differences between consecutive Congresses and looking at the effect on members who serve in both Congresses helps isolate the effects of the changing agenda using several measures with clear theoretical predictions. Characterizing the overall pattern as well as the variation over time and by various issues both highlights the challenge of consistently estimating the effects of agenda control while also demonstrating that patterns consistent with agenda control are a relatively recent phenomena. The time-varying evidence of patterns consistent with partisan agenda control raises important questions about how to interpret seemingly similar levels of elite polarization across time and how different processes may be responsible for similar levels of polarized voting.

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). Moreover, concerns about levels of elite polarization – as well as investigations into its causes and consequences – arise as a consequence of characterizing the degree to which observed voting coalitions are organized by party.

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), they provide an unambiguous indication of what Congress chooses to publicly record individual positions on. Precisely because roll calls provide a record of public positions, debates over whether members choose to endow political parties with the ability to determine the agenda and constrain which individual choices are recorded for the benefit of the party's collective interest (e.g., Aldrich 1995; Cox and McCubbins 2005, 2007; Harbridge 2015; Pearson 2015; Rohde 1991; Smith 1989) have focused on roll call voting patterns (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). Scholars have learned much about the correlates of voting agendas over time, but identifying the *causal impact* of agenda control faces exceptional, if not impossible, challenges.

Estimating the causal effect of majority party setting requires comparing the world in which agenda control is possible to an all else equal counterfactual where it is not. This condition is never satisfied. Not only does the composition and political, social, and economic conditions vary over time, but the endogeneity of congressional rules themselves means that everything we observe is presumably in equilibrium and nothing is exogenous (or "as if" randomly assigned). Given this, strengthening the conclusions that are possible from estimated associations requires closely connecting the empirical investigation to theoretical predictions and leveraging theory to provide the identification and interpretation of estimated effects and focusing on cases that

limit confounding effects. To this end, I review how measures commonly used to measure key theoretical quantities are themselves endogenous to the theory being tested given that theories of agenda control are theories about the very votes that are being used to estimate legislator ideal points and the necessity of leveraging that connecting to derive predictions in terms of those estimates rather than attempting to use the estimates in secondary analyses. To partially address the fundamental problem of causal inference I leverage variation in the congressional agenda associated with a change in majority party status among members serving before and after the change in majority. In so doing, to limit the confounding effects of compositional changes necessarily associated with such changes (and the possibility that differences arise because of differences in the preferences or circumstances of coalition members rather than differences in the agenda) I estimate the model parameters using only members who serve before and after the change to compare the agenda holding the composition fixed. Comparing theoretically derived predictions related to the percentage of votes splitting partisan coalitions of continuing members as well as the relative percentage of votes occurring in theoretically-relevant portions of the parameter space reveals several conclusions.

First, patterns consistent with majority party agenda control are not consistent across the entire post Civil War (or post-Reconstruction) time period. Only following the Republican takeover of the House in 1994 are the observed differences in the agenda associated with a change in majority control consistent with theoretical predictions. Post World War II patterns are less consistent with theoretical predictions – while it is the case that the majority party Democrats were seemingly far more likely to allow votes splitting off extreme Republicans than those splitting off extreme Democrats there were also more votes splitting moderate Democrats than moderate Republicans such that, on average, the coalition of Democrats serving in consecutive Congresses were as likely to be split as the coalition of re-elected Republicans. Prior to the post-WWII era there is little evidence consistent with theoretical predictions; in fact, most of the patterns are the opposite of what would be predicted. Second, these patters are not somehow attributable to variation in the types of issues being considered – a similar pattern emerges within various content-coded votes using a common issue coding.

Substantively, these results matter because they suggest that although the level of elite polar-

ization we now observe is similar to the levels observed in the past, the extent to which the observed patterns are consistent with agenda control differ greatly. Whereas the current level of polarization can be partially attributable to agenda control, similarly high levels of polarization in early periods cannot. Because the analysis is based on members serving in consecutive Congresses, this suggests – but certainly does not prove – that the level of polarization associated with earlier periods may be more associated with the changing composition of party coalitions and the members serving for shorter terms.

In addition to these substantive conclusions, the analysis also highlights the critical importance of theoretically-motivated analyses for questions in which the stringent demands of causal inference are unlikely ever to be convincingly satisfied. In such cases, interpreting the meaning of the observed patterns and associations is plausibly achieved by comparing theoretically motivated measures and associations to theoretical predictions and determining which theoretically implied mechanism is most consistent with the observed patterns. Even if the identification of causal effects in the conventional sense is illusive, providing a consistent estimate of theoretically relevant associations helps motivate additional theorizing about the causes and consequences of the uncovered patterns and deriving secondary predictions that can be useful for accumulating evidence that is supportive of a causal relationship.

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. Instead, the issue is whether the resulting estimate is a consistent measure of the population parameter of interest. 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, measurement error in any variable correlated with the variables of interest poses a second threat. 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 measurement difficulties and omitted variables.

Estimating a causal relationship between two concepts raises the demands we make of our empirical analyses even higher. Such analyses seek to understand the relative percentage change between two concepts – i.e., how the outcome varies with changes in the "treatment" all else equal. To estimate elasticities, political science largely has relied on exogenous variation in the independent variable – or 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 focus on analyzing the relationship in closely contested elections where scholars can justify the claim that the incumbents were selected as if random. But even if a plausible causal effect can be precisely estimated through such restric-

[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.

tions, it may be difficult to generalize the estimated effect; e.g., how do we know the effect of incumbency in a competitive district is similar to the effect in less competitive districts (e.g., Caughey and Sekhon 2011; Hainmueller, Hall and Snyder 2015)? Generalizing estimated effects requires a theoretical claim about their scope – claims that are difficult to empirically confirm.

Theory is therefore important not only for justifying the extrapolation of causal effects to instances beyond the considered cases, but also because it can provide testable predictions that help interpret empirical associations. Because such theories provide a mechanism for how and why two concepts should be related, the ability to interpret an estimated statistical association in terms of theoretically predicted elasticities arguably is strengthened by associations that are consistent with theoretical expectations. Although hypothesis testing can only disprove rather than prove given the possibility that other explanations may exist, the closer the empirical and theoretical connections the stronger our ability to interpret an estimated association.

2 Predictions regarding party agenda control

Theories of agenda control are theories about the types of votes that should be observed. Positive agenda control refers to the ability of an agenda setter to choose the proposals being voted upon in ways that allow the agenda setter to maximize her utility as much as possible given the status quo and voting rule being used (e.g., Baron and Ferejohn 1989). Negative agenda control – sometimes called “gatekeeping" – is a weaker form based on the ability of the agenda setter to prevent proposals from reaching the floor. Whereas positive agenda control ensures that the agenda setter is always receiving the maximum utility possible from every proposal (given the rules and preferences of other members), negative agenda control ensures that the agenda setter is never made worse off by any proposal.

Whether the majority party in the House of Representatives is willing and able to use rules governing the legislative agenda (perhaps in combination with other inducements) to ensure that it is able to create and maintain a party brand based on its record of legislative accomplishment is a matter of ongoing investigation. Some show evidence of its impact on policy making in state legislatures (e.g., Crosson 2019). Some doubt the frequency with which members would choose to delegate such power to party leadership and whether agenda control produces elec-

toral costs rather than benefits by only allowing certain types of policy change (e.g., Richman 2015; Fortunato and Monroe 2020). Others argue that evidence seemingly consistent with majority party agenda setting is largely observationally equivalent to majoritarian decision-making without knowing the actual preferences of members (e.g., Krehbiel 1993).

Even among those who argue that the majority party regularly wields negative agenda power, differences of opinion exist as to the conditions that make it more or less likely. Some argue that negative agenda is unconditional because it results from stable rules and institutions and an ongoing and ever-present incentive for members to create and maintain an electorally valuable party brand by controlling the agenda (Cox and McCubbins 2005, 2007). Others suggest that the incidence of agenda control varies based on the size or preference homogeneity of the majority. Lee (2016), for example, argues 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 and when the majority has the most incentive to maintain their cohesiveness in the hope of retaining its majority status.[2] The conditional party government account (Rohde 1991; Aldrich 1995; Aldrich and Rhohde 1998) suggests instead that agenda control is most likely to be exercised when the majority party is internally unified and distinct from the minority party (Finocchiaro and Rohde 2008).

Exercising agenda control is itself an act, but we can only observe the potential consequences of that act in terms of the votes that are permitted on the floor. Put differently, conclusions about the prevalence of agenda control are reached by examining the types of votes that are allowed to occur and determining whether those patterns are consistent with theoretical expectations. Insofar as observed patterns match theoretically patterns, the estimated empirical effects are often interpreted as being caused by agenda setting. This interpretation depends critically on ensuring that the empirical patterns are measured in ways that are consistent with the assumptions and predictions of a well-specified theory. Moreover, any interpretation is necessarily limited because theoretically consistent patterns may occur in the absence of agenda setting for other reasons (e.g., the distribution of members' preferences); statistically we can only disprove predictions. Although the existence of clear theoretical predictions may increase our confidence in the ability

[2] 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.

to interpret a statistical association in terms of elasticities, strong interpretations should be made with caution given the possibility of observationally equivalent alternative explanations.

But what patterns are implied by theories of negative agenda setting by the majority party? And are any of those features plausibly "pre-treatment?" Cox and McCubbins (2005, 2007) were among the first to derive how majority party agenda control would affect voting coalitions on the observed votes.[3] To do so, they examine roll rates—the proportion of votes on which a majority of each party votes in opposition to each other; under agenda control 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 the characteristics of votes that should and should not occur in 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 location of the legislator who is indifferent between supporting and opposing the vote – i.e., the threshold separating those who are predicted to vote for and against the proposal.[5]

Theoretical predictions emerge not only with respect to which party coalitions are divided – we should see fewer (and in fact no) votes splitting the majority caucus relative to the minority – but also how the permitted votes are predicted to split the coalitions. Figure 1 summarizes the equivalence between predictions based on cut points and roll rates with perfect spatial voting

[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 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, $\kappa_j = (b + q)/2$.

and a left-leaning majority party (see also Jenkins and Monroe 2015).

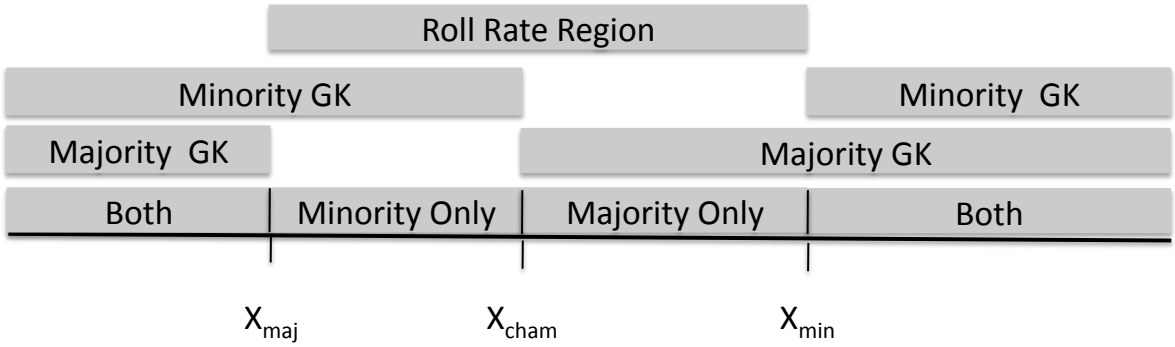

Figure 1: **Agenda Control Predictions:** Given the ideal points of the majority party median member (x_{maj}), minority party median median (x_{min}), and median member of the chamber (x_{cham}), the labelled regions denote the spatial location of cutpoints $\boldsymbol{\kappa}$ producing majority and 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]

Despite relatively clear theoretical predictions, two unavoidable complications emerge. First, these predictions are in terms of the proposed effects of agenda control for a fixed composition assuming that agenda control is being exercised. If agenda control is used selectively – and if its

[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.

use varies over time or based on the issues involved – then conclusions are more difficult because the presence of inconsistent votes cannot disprove its' importance in selected cases. Empirical tests are really an evaluation of whether observed votes are consistent with expectations when agenda control is always – or almost always – being used.

Second, Figure 1 highlights how the incidence of behavior consistent with agenda setting depends critically on the heterogeneity of member preferences relative to status quos and possibly policy proposals. If every member shares the same preferences, for example, patterns consistent with agenda control would occur even in the absence of actual agenda control. Because member preferences are unobservable – and our ideal point measures are a function of the observed agenda – it can be difficult to know whether evidence that is consistent with agenda control is due to the compositional effects related to the homogeneity of preferences or agenda control. Two implications arise. First, because typical measures of party and chamber medians are themselves a function of the congressional agenda it is important to ensure that the theoretical predictions are derived directly in terms of the observed behavior rather than trying to use observed votes to estimate chamber medians for use in a subsequent analysis. Estimated ideal points cannot easily be used to test agenda setting in secondary analyses because they are themselves dependent on the agenda (e.g., Clinton 2012).[7] Second, leveraging changes in majority control to compare how the observed votes differ is complicated by the fact that a change in majority party only occurs because of changes in the composition of the House. As a result, it can be difficult to identify whether differences in the pattern of observed votes are due to differences in agenda setting, differences in the preferences of who is serving in the House, or both.

3 Behavioral and Statistical Voting Models

As noted, predictions related to the prevalence of majority party agenda setting are defined in terms of the votes we should observe given legislative preferences. But the measures typically used to measure theoretical concepts such as the location of party medians are themselves based

[7]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. 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 US Congress–also predict the types of votes we should observe in equilibrium.

on observed behaviors involving the data generating process being theorized about. As a result, using measures derived from observed votes to measure legislative ideal points for use in a secondary analysis is problematic and it becomes important to derive predictions that can be evaluated when analyzing the patterns of voting behavior.

Models of roll call estimation are rather unique in political science because unlike other dimension reduction techniques such as factor analysis or principal components analysis, the statistical measurement models can be directly derived from an underlying behavioral model of voting. It is for this reason that estimates from the analysis of roll calls are often treated as being indistinguishable from the theoretically important preferences, notably elite preferences/ideology. Although the quantities being recovered by the dimensionality-reduction techniques associated with common statistical models are indeed interpretable relative to the underlying behavioral model used to derive the likelihood function, those connections are also important when using and interpreting the estimates to ensure that the assumptions of the statistical model are not inconsistent with the assumptions of the theoretical model being tested.

To help ground the discussion and evaluation of the extent to which the observed patterns of roll call voting behavior is consistent with negative agenda setting by the majority party it is useful to review the theoretical foundations of ideal point estimation. Statistical models of roll call voting are based on a likelihood function derived from a behavioral voting model for legislators $i \in N$ casting votes $t \in T$. Each legislator i is assumed to have a ideal position – "ideal point" – in the parameter space that defines their most preferred outcome in the parameter space (i.e., what they would do if they were a dictator). The actual meaning of this latent space is undefined – it may reflect policy outcomes, policy positions, or a combination of both. It is assumed that members' ideal points x_i are fixed for all T decisions so that we can use all T votes to estimate x_i.[8] All legislators are assumed to vote based on a comparison of the proximity of the outcome associated with voting "yea" ($\theta_{y(t)}$) relative to the proximity of voting "nay" ($\theta_{n(t)}$) according to some some distance (utility) function $f()$. As with $\boldsymbol{x}$, these outcomes could reflect either policy outcomes or public positions. Voting is perfect, however, and it is assumed that legislators experience random and independent utility shocks associated with voting yea and nay – ζ_{it} and

[8]This can be relaxed by estimating different models on different subsets of votes to allow ideal points to vary by issues or over time.

ν_{it} respectively. Given these primitives, if the policy space is unidimensional:

$$\begin{aligned} U_{it}\left(\theta_{y(t)}\right) = &\quad f(x_i, \theta_{y(t)}) + \zeta_{it} \\ U_{it}\left(\theta_{n(t)}\right) = &\quad f(x_i, \theta_{n(t)}) + \nu_{it} \end{aligned}$$

$$\text{which means that Legislator } i \text{ votes Yea if} \quad f(x_i, \theta_{y(t)}) - f(x_i, \theta_{n(t)}) > \nu_{it} - \zeta_{it}$$

$$\begin{aligned} \Pr(y_{it} = 1) = &\quad \Pr(\nu_{it} - \zeta_{it} < f(x_i, \theta_{y(t)}) - f(x_i, \theta_{n(t)})) \\ \Pr(y_{it} = 0) = &\quad \Pr(\nu_{it} - \zeta_{it} > f(x_i, \theta_{y(t)}) - f(x_i, \theta_{n(t)})) \end{aligned}$$

The `NOMINATE` family of ideal point estimates (Poole and Rosenthal 1985) assumes that the utility function is Gaussian, which means that:[9]

$$\begin{aligned} f(x_i, \theta_{y(t)}) = &\quad \beta \exp\left(-\left(x_i - \theta_{y(t)}\right)^2\right) \\ f(x_i, \theta_{n(t)}) = &\quad \beta \exp\left(-\left(x_i - \theta_{n(t)}\right)^2\right) \end{aligned}$$

$$f(x_i, \theta_{y(t)}) - f(x_i, \theta_{n(t)}) = \quad \beta\left[\exp\left(-\left(x_i - \theta_{y(t)}\right)^2\right) - \exp\left(-\left(x_i - \theta_{n(t)}\right)^2\right)\right]$$

It is further assumed that the idiosyncratic stochastic utility realizations ζ_{it} and ν_{it} are independently drawn from a Normal distribution such that $\nu_{it} - \zeta_{it} \sim N(0, \sigma^2)$ means:

$$f(x_i, \theta_{y(t)}) - f(x_i, \theta_{n(t)}) \sim N(\nu_{it} - \zeta_{it}, \sigma^2)$$

which implies:

$$\begin{aligned} \Pr(y_{it} = 1) = &\quad \Pr\left(\nu_{it} - \zeta_{it} < f(x_i, \theta_{y(t)}) - f(x_i, \theta_{n(t)}))\right) \\ = &\quad \Phi\left(\sigma^{-1}(f(x_i, \theta_{y(t)}) - f(x_i, \theta_{n(t)}))\right) \end{aligned}$$

Because

$$\Pr(y_{it} = 1) = \Phi\left(\beta\left[\exp\left(-\left(x_i - \theta_{y(t)}\right)^2\right) - \exp\left(-\left(x_i - \theta_{n(t)}\right)^2\right)\right]\right)$$

[9] In the notation that follows β is a signal-to-noise ratio that is set to 15 in the actual estimation; as the `nominate` package notes: "It is strongly recommended that you do not change the default."

the likelihood function is:

$$\begin{aligned}
\mathrm{L}(\boldsymbol{x},\boldsymbol{\theta}|\boldsymbol{y}) = \;& \Pi_{i=1}^{L}\Pi_{t=1}^{T}\Pr(y_{it}=1)^{y_{it}} \times (1-\Pr(y_{it}=1))^{1-y_{it}} \\
= \;& \Pi_{i=1}^{L}\Pi_{t=1}^{T}\Phi\left[\beta\left[\exp\left(-\left(x_i-\theta_{y(t)}\right)^2\right)-\exp\left(-\left(x_i-\theta_{n(t)}\right)^2\right)\right]\right]^{y_{it}} \\
& \times\left(1-\Phi\left[\beta\left[\exp\left(-\left(x_i-\theta_{y(t)}\right)^2\right)-\exp\left(-\left(x_i-\theta_{n(t)}\right)^2\right)\right]\right]\right)^{1-y_{it}}
\end{aligned}$$

From a $N \times T$ matrix of N legislators making binary decisions over T votes we estimate the vector of N ideal points $\boldsymbol{x}$ and $T \times 2$ location parameters $\left\{\theta_{y(t)}, \theta_{n(t)}\right\}$. A remarkable feature of this measurement model is that *everything is unobserved except for* y_{it}. As a result, we are estimating both legislator and vote parameters simultaneously – effectively estimating x conditional on $\left\{\theta_{y(t)}, \theta_{n(t)}\right\}$ and then estimating $\left\{\theta_{y(t)}, \theta_{n(t)}\right\}$ conditional on $\boldsymbol{x}$ until convergence obtains.[10] The estimated ideal points x therefore clearly depend on the (parameters of the) observed agenda, and the parameters associated with the observed agenda clearly depend on the composition of legislators' ideal points x.

These connections highlight the previously noted difficulties. Because theories about the congressional agenda are theories about the data generating process this means that we cannot treat ideal points as being exogenous to the agenda – $\boldsymbol{x}$ is estimated conditional on $\left\{\theta_{y(t)}, \theta_{n(t)}\right\}$. Moreover, because $\left\{\theta_{y(t)}, \theta_{n(t)}\right\}$ are estimated conditional on $\boldsymbol{x}$, changes in the composition of the majority as would result from a change in majority party affect the estimated vote parameters. Not only is it therefore important to test predictions in terms of the pattern of observed votes (rather than using estimates from ideal point models to measure ideal points in a secondary analysis), but it is also important to consider how the leverage seemingly provided by changes in the majority party can also create complications due to changes in the composition of Congress and the subsequent impact of those changes on the parameters being estimated in the statistical voting model.

[10] Hence the derivation of `NOMINATE` – `NOMINAl` Three-step Estimator. The third step was the estimation of β.

4 Research design

The apparently simplicity of theoretical predictions characterized by 1 is therefore offset by the complications of measuring those quantities and estimating the extent to which agenda control is responsible for producing observed outcomes. In general, the causal effects of a treatment are typically identified by comparing the outcomes of randomly assignment treatments to assess the potential outcomes counterfactual out what would occur for the same unit in the absence of the treatment. Because we never observe the same unit being simultaneously treated and untreated, assumptions and statistical controls are required to estimate the all-else-equal counterfactual. This task is challenging for many questions of political science and it is even more so for questions involving American political development and political institutions because the only source of variation comes from either endogenous changes to rules and norms or else from over-time variation that is difficult to interpret because of the difficulty of exhaustively and perfectly measuring the exhaustive set of time-varying confounders.

Ideally, we would estimate the causal effect of agenda control in Congress by comparing how voting coalitions and observed votes vary depending on whether the majority party is randomly endowed with agenda setting power holding all else constant.[11] Holding the composition of Congress fixed is required to eliminate possibly confounding effects of personalities (Hall 1997) and preferences on voting coalitions (and therefore on the estimated cutpoints). Holding the political, social and economic environment fixed removes the impact of external (e.g., wars (Mayhew 2005) and economic crises) and internal pressures (e.g., Smith 1989) on the supply of moveable status quos and the 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 using an empirical specification similar (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 and Clark (2023) for an analysis leveraging the non-partisan Nebraska state legislature):

[11] See Richman and Roberts (2020) for a similarly inspired analysis using simulations to compare actual agendas to simulated agendas in an effort to identify the frequency of “naturally occurring" roll rates.

$$Y_t = \alpha + \beta \text{Dem Majority}_t + \gamma \boldsymbol{X}_t + \epsilon_t,$$

where Y_t is a measure of the fraction of votes that split the Democratic party, $\boldsymbol{X}_t$ is a set of time-varying controls, and β estimates how the percentage of party-splitting votes differs when Democrats are in the majority relative to when Republicans are in control. Perfect agenda control for Democrats would predict $\alpha + \beta = 0$.

Consistently estimating α and β requires extensive and exhaustive statistical controls $\boldsymbol{X}_t$ to account for other time-varying factors affecting the coalitions of observed votes. 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 the included covariates. Such an assumption seems implausibly heroic, but even if we are content with the consistent estimation of β we must still control for all time-varying confounders.

Several threats arise to our ability to consistently estimate the statistical effects of agenda control on 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 – including changes in the size and composition of the majority coalition – could potentially be responsible for the estimated differences being attributed to agenda control. Because agenda control is not measured independently of majority party control, the interpretation that differences are due to agenda control is by assumption (based on a well-specified theory). 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.

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 ($\boldsymbol{X}$). Problems emerge because some of these time-vary differences are directly related to the changing composition of the majority and minority over time. 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 preference variation among the majority coalition – and the distance from the minority coalition is critical for estimating the effects of agenda control consistently. Controlling for the changing composition of Congress over time is also 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 preference heterogeneity is needed to untangle the impact the effect of preference similarity from the impact of agenda control.

The necessity of controlling for such changes raises serious empirical challenges. 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 the votes being analyzed are themselves a function of agenda control.[12] Both the estimated ideal points ($\hat{\boldsymbol{X}}$) and the estimated vote parameters depend on the chosen agenda (i.e., the set of votes $\boldsymbol{Y}$) and the functional form of the spatial voting errors ($\dot{\Phi}()$). 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) depend on the chosen agenda if the frequency of voting error is low (Hirsch 2011; Clinton 2012). Put differently, when roll-call based measures suggest that a party coalition is likely to vote together is that because their underlying true preferences are similar or is it a consequence of agenda setting by party leaders choosing votes that unite the party? (This concern is most clearly revealed by voting coalitions in 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).) It obviously is problematic to use roll-call based measures to control for over-time variation in the composition

[12]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.

of party coalitions if those measures are themselves affected by agenda control.

Given complications with using roll call based measures in secondary analyses and the complications caused by changing compositions over time, I adopt an alternative approach.[13] 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.[14] To do so, I jointly scale the voting behavior of all members serving consecutively in Congresses t and $t+1$ and who also vote on at least 50% of the votes in each Congress. Fixing the composition removes the effect of compositional changes on the estimated vote parameters in consecutive Congresses and reveals how the voting coalitions change among those serving in both. To normalize the space within each pair of consecutive Congresses, I estimate a fixed, unidimensional ideal point for each member serving in each pair of consecutive Congresses using `WNOMINATE` (Poole and Rosenthal 1987; Poole 2005).[15] Because the composition across dyads are fixed, 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 as would be the case with changes in composition. Technically the cardinality of the resulting estimates are not comparable across dyads, but insofar as 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, but comparisons relying on the cardinality of the estimates (e.g., distance in the parameter space) are difficult to compare across dyads because the estimates of each dyad are independently estimated and constrained to lie in the $[-1, 1]$ interval.

Focusing on the differential effects on continuing members limits the effect of compositional changes, but it comes at a cost. Nothing ensures that the median members of the chamber as a

[13] See also Richman and Ryan (2020) who use a simulation approach to estimate the counterfactual roll rates that would be expected in the absence of majority party agenda control.

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

[15] 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.

whole – the theoretically relevant quantities for assessing agenda control in Figure 1– are similar to the medians of members serving in consecutive Congresses. To evaluate the magnitude of this discrepancy, Figure 2 compares how the level of elite polarization measured using the distance between the median Democrat and median Republican varies depending on whether all members or only those serving in consecutive Congresses is used.

As would be expected given that more moderate members are more likely to lose, the measure of polarization is greater when using continuing members. Even so, the two measures exhibit similar ebbs and flows and qualitative characterizations about the relative level of polarization are unaffected. The left panel of Figure 2 reveals the source of the more extreme polarization among continuing members serving between 2021 and 2024 (117th-118th Congresses) by showing that the members who are most likely to serve in consecutive congresses are also those with among the more extreme `DWNOMINATE` scores.

The observed differences highlight an unavoidable tradeoff – holding the composition fixed across majority party transitions to eliminate the effect of changing members on the estimated cutpoint locations means that the median members among continuing members are unlikely to reflect the theoretical members of interest. Even so, several predictions emerge from theoretical accounts.

First, the fraction of observed votes splitting the majority relative to those splitting the minority provides one measure of whether the majority party can maintain a coherent coalition and/or fracture the opposition.[16]

The percentage of votes splitting each party is information about whether the agenda is used to unite the majority party or not, but it does not reveal how the agenda may be used to split the opposition party. If the agenda is used to create a party brand, we would expect the majority party to choose an agenda that both unifies the majority coalition into a single position and which also fractures the opposition party to prevent it from defining a coherent brand in opposition. To characterize the types of votes we observe, I compute the fraction of votes with cutpoints located in theoretically-defined partitions of the space with clear substantive interpretations.

[16]If the most conservative Democrat is more conservative than the most liberal Republican it is possible to observe votes splitting both parties. It is also possible that votes split neither caucus – as would be the case of straight party-line votes.

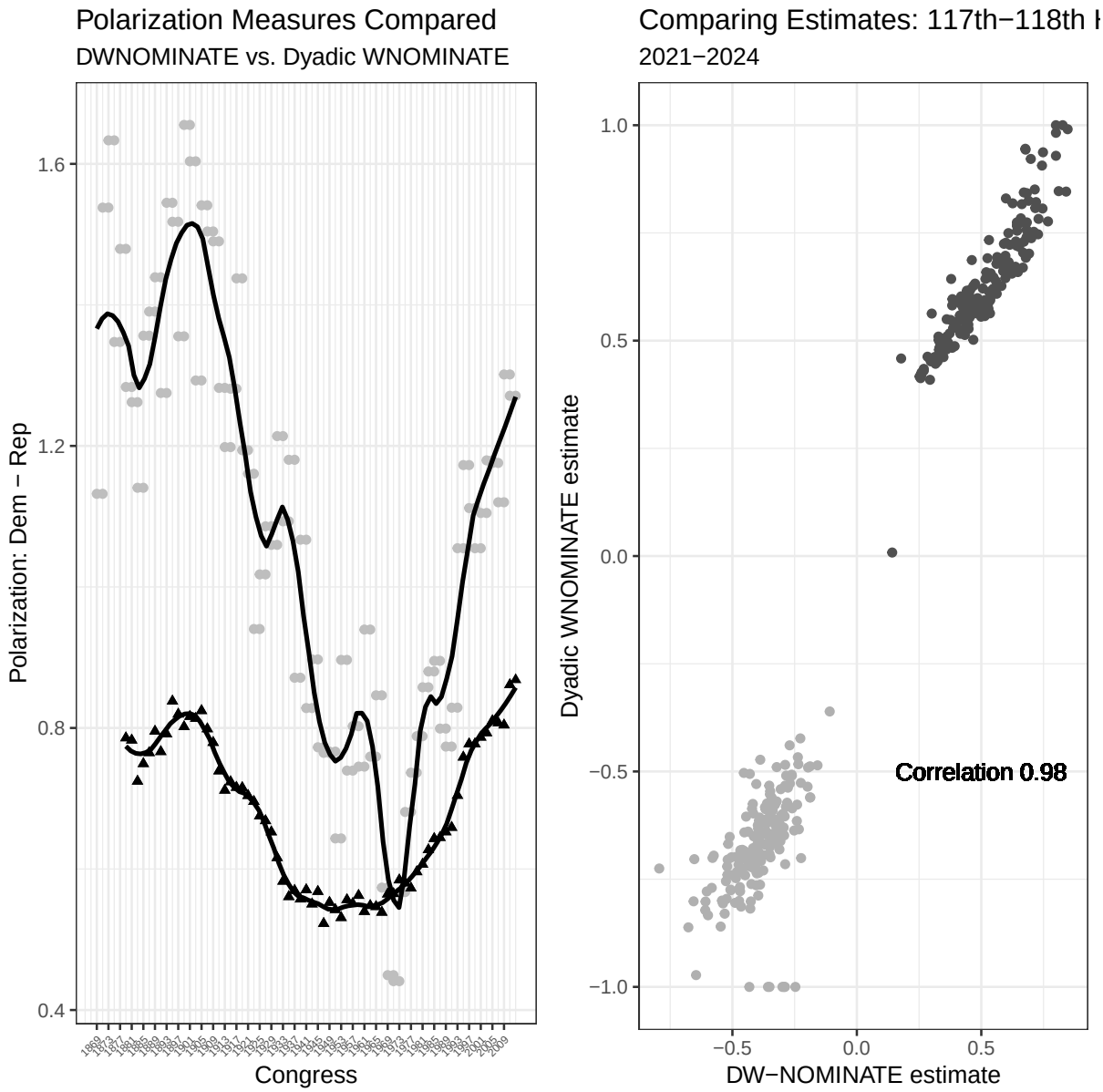

Figure 2: The left panel plots measures of polarization in the House for each congress using all members as well as just those serving in consecutive Houses. Given the dyadic estimates, each House appears twice. The right panel compares the DW-NOMINATE score based on all members to the W-NOMINATE score estimated using just those serving in both the 117th and 118th House – a dyad that went from a Democratic majority to a Republican majority.

While defining partitions relative to the chamber median is impossible given that the sample restriction to those serving in consecutive Congresses, theoretically meaningful regions can be defined using the median Democrat, the most conservative Democrat, the most liberal Republican and the median Republican.[17] Each region is of substantive interest. Votes with cutpoints more

[17]It is obviously possible that the most conservative Democrat may be more conservative than the most liberal

extreme than the Democrat median (Zone 1) are votes that are predicted to produce a bipartisan coalition of Republicans and a majority of Democrats against a minority of Democrats. Cutpoints between the Democratic median and the most conservative Democrat (Zone 2) are votes that split the Democratic party against a united Republican coalition. Cutpoints that fall between the most conservative Democrat and the most liberal Republican (Zone 3) are party-line votes. Votes with cutpoints between the most liberal Republican and the median Republican (Zone 4) are votes splitting Republicans against a united Democratic coalition, and cutpoints that are more extreme than the Republican median (Zone 5) are predicted to be bipartisan votes against a Republican minority.

To illustrate the categorization, Figure 3 plots the location of each of these zones for the 2021-2024 period and the percentage of recorded votes located in each region. As Figure 3 reveals, the most frequent type of observed vote was a straight-party line vote (Zone 3) and votes that were predicted to split moderate Republicans from their more conservative colleagues (Zone 4). The next most likely type of votes were bipartisan votes in which Democrats and a majority of Republicans opposed a minority of Republicans (Zone 5). Despite the fact that Republicans obtained the majority in the 118th House, we few very few bipartisan votes with extreme Democrats in opposition (Zone 1) and relatively few votes splitting more moderate Democrats from their more liberal colleagues (Zone 2). Stepping back, the agenda for these two Congresses were far more likely to target and fracture Republicans than Democrats – the percentage of votes in Zone 5 is much greater than the percentage in Zone 1 and there are similarly many more votes in Zone 4 than Zone 2.

5 Patterns of Voting Over Time

To begin, consider the relative difference with which the party coalitions are split by the recorded votes regardless of how those splits occur. This time series answers the question: are members of the majority party less likely to be divided than members of the minority party on the votes that are recorded in the House? The relative percentage of split coalitions does not indicate whether the recoded votes are splitting extremists or moderates – such requires the analysis of

Democrat.

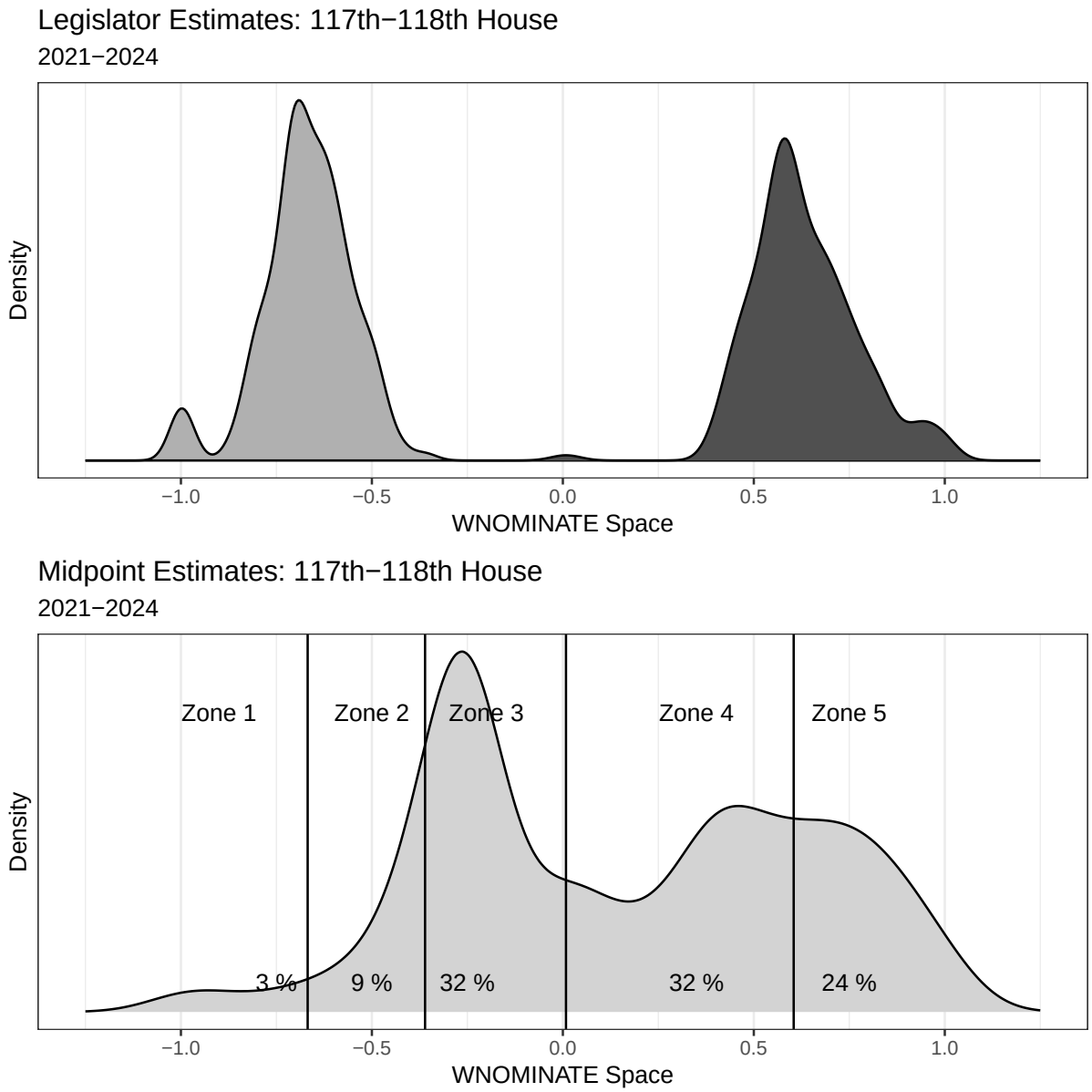

Figure 3: The top panel plots the distribution of estimated ideal points for members serving in both the 117th (2021-2022) and 118th (2023-2024) U.S. House. The bottom panel plots the distribution of cutpoints for recorded roll calls during this period falling into each partition of the parameter space.

the fraction of votes in the various estimate-based partitions of the parameter space – but the overall percentages are informative about the relative ability of parties to vote cohesively on the chosen agenda. The measure also almost certainly *overstates* the fraction of the observed votes consistent with negative agenda setting as a large difference in relative proportions can mask high levels of the majority party being split (e.g., if the minority party is split on 75% of the

votes and the majority party on 25% then even though the difference is 50% there are still 25% of the votes on which the majority party is split contrary to strict predictions).

Figure 4 graphs the percentages using two different units of analysis – Congresses (top) and years (bottom). Recall that the calculation for each dyad of consecutive congresses is based on the relative splits among members serving in both congresses. Although majority status only varies by Congress, conducting the analysis by years allows a sense of how much variation there is in the congressional agenda holding fixed the majority party. The estimates are based on fewer votes – hence the more extreme estimates – but the annual variation allows a more comprehensive characterization of the amount of expected variation in the absence of a change in majority control.

The pattern in Figure 4 reveals several important patterns. First, as expected and reflecting possibly idiosyncratic and event-related demands on the congressional agenda, there is far more variation in the effects of the agenda on party coalitions when the analysis is conducting by year relative to when it is conducted by Congress. This difference suggests that parties are unable to perfectly control the agenda and that party coalitions fluctuate – perhaps due to differences in the issues being considered or the external political environment. Even so, the overall trends in terms of ebbs and flows are similar regardless of whether the data is summarized by Congress or by year. Third, theoretically expected patterns emerge only in the second-half of the time-series. Only starting in the mid-1980s do we observe fewer splits among Democrats compared to Republicans when Democrats are in the majority and visa-versa. Throughout the Post WWII period Democrats and Republicans are equally likely to be split on the observed agenda, and prior to that period the estimates fluctuate considerably in ways unrelated to theoretical predictions. 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.

Beyond the relative frequency with which party coalitions were split by the allowed recorded votes we can also examine how the permitted votes were predicted to split the party caucus by comparing the relative incidence of estimated cutpoints in the various partitions. The top-panel of Figure 5 graphs the relative incidence of bipartisan coalitions voting against a minority of

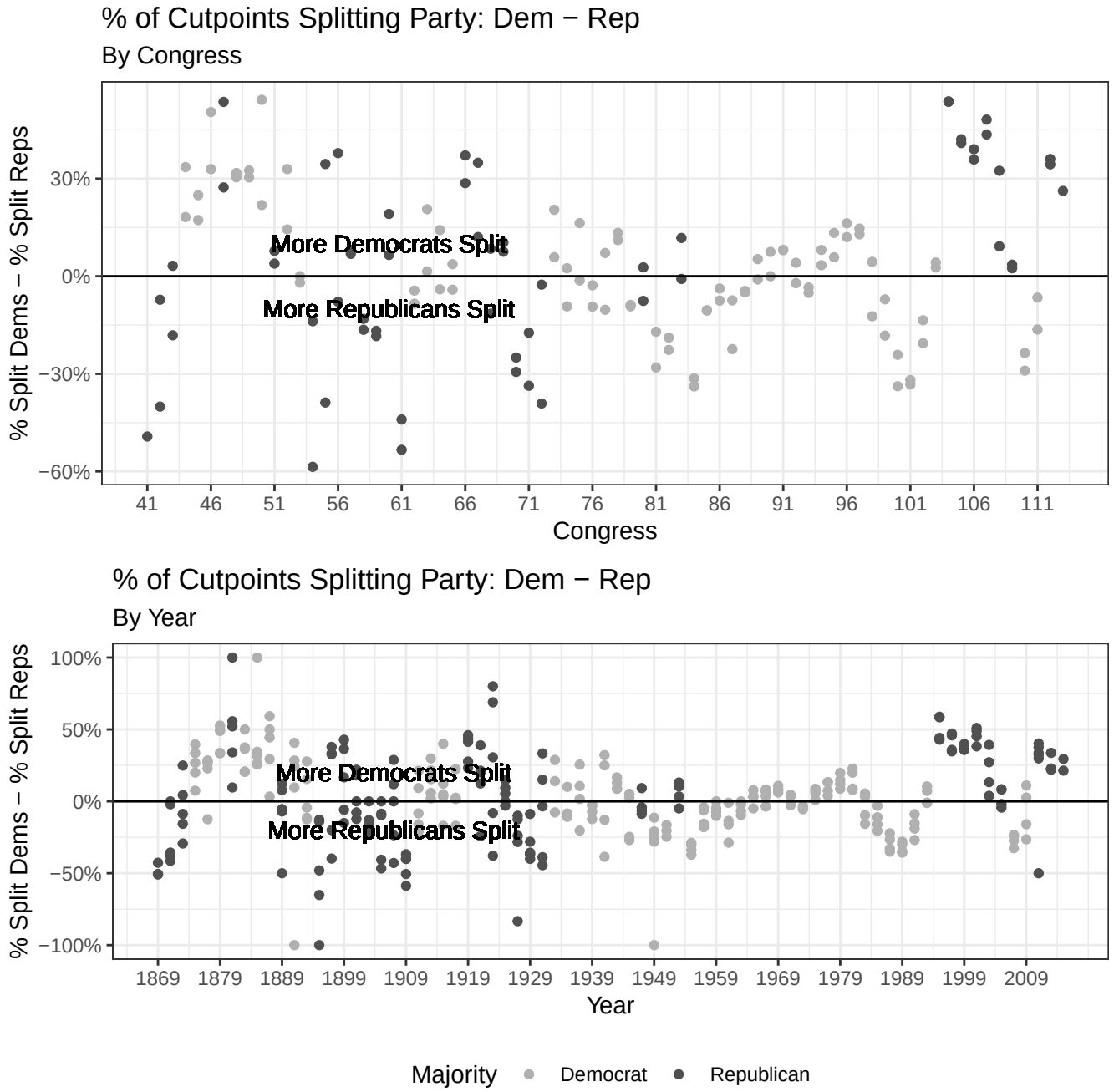

Figure 4: Relative Percentage of Recorded Votes Splitting Party Coalitions of Continuing Members: The top-panel characterizes the relationship by Congress and the bottom-panel characterizes the relationship by year. Because the estimates are based on a dyad, each Congress appears twice when analyzing the results by Congress (top panel) and four times when analyzing the data by year (bottom panel).

Democrats (Zone 1) relative to a minority of Republicans (Zone 5).

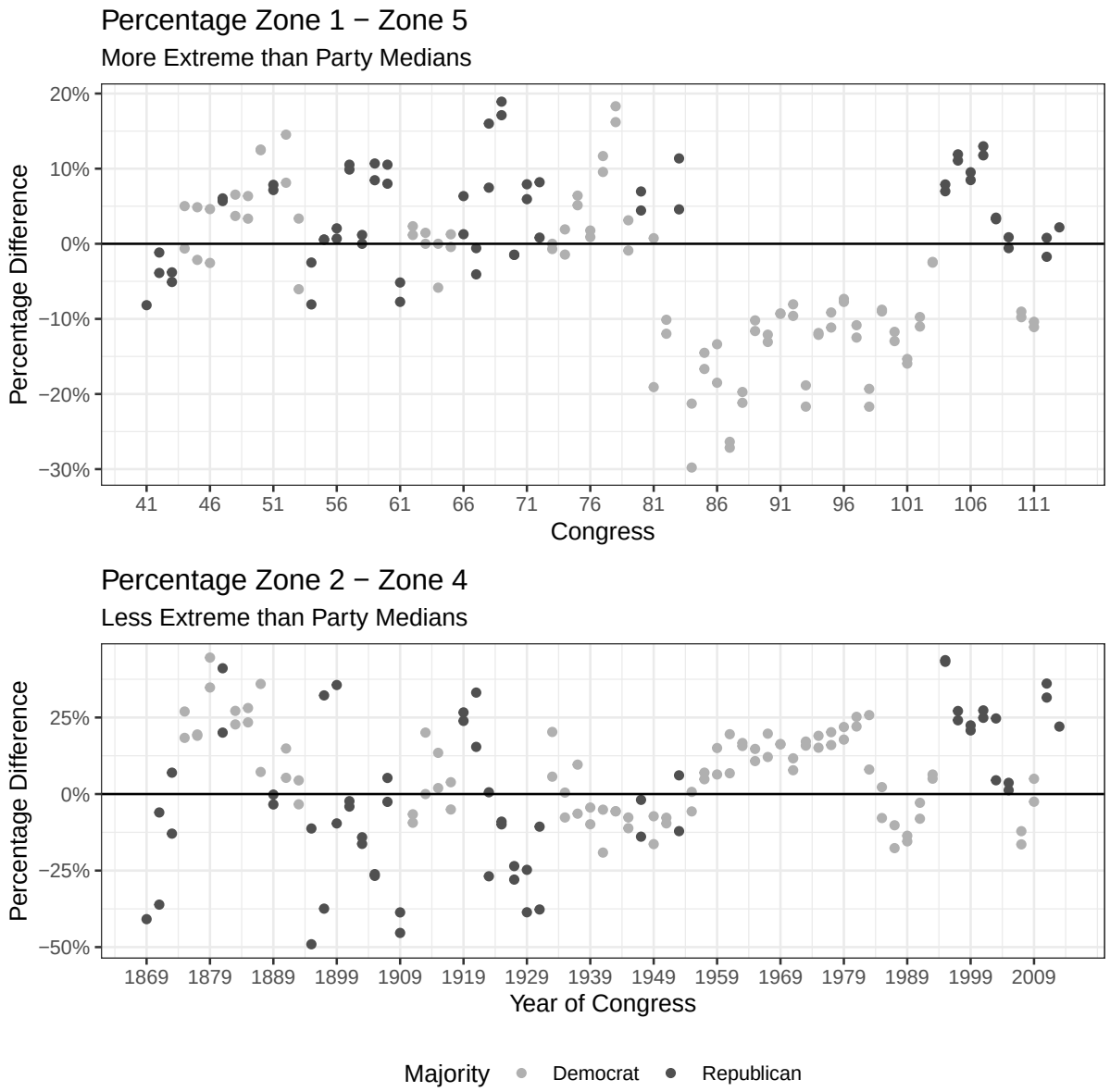

Figure 5: The top panel plots the difference in the percentage of votes splitting more moderate Democrats (Zone 2) relative to the percentage splitting more moderate Republicans (Zone 4) among those serving in consecutive Houses. The bottom panel plots the relative percentage of votes where a bipartisan coalition opposes extreme Democrats (Zone 1) relative to the percentage where a bipartisan coalition opposes extreme Republicans (Zone 5). The analysis is at the Congress level.

Here again we observe a sharp temporal variation with the patterns observed in the post WWII era being much more consistent with majority party agenda control than earlier periods. As the Figure reveals, both Democrats and Republicans were much more likely to allow bipartisan votes splitting the minority party's caucus than they were to allow bipartisan votes splitting their own

caucus when in the majority.

Inspecting the incidence of votes that split moderate members from the party caucus reveals a more troubling pattern for accounts of majority party agenda setting. As the bottom panel makes clear, only since the mid-1980s do we see patterns consistent with majority parties scheduling votes to fracture the minority party by exposing more moderate members. Between 1960 and 1980, Democrats allowed more votes splitting moderate Democrats than votes splitting moderate Republicans. Although this arguably reflects the well-known dominance of racially conservative Southern Democrats in the House, it nonetheless demonstrates the inability of the majority to maintain a cohesive party caucus on the votes that were recorded.

These patterns raise immediate questions not only about the overall support for majority party agenda setting powers over this period, but also about the nature of politics. Given the obvious temporal variation in Figures 4 and 5, how should we interpret similarly high levels of elite polarization in Figure 4 given the evident differences in the agenda being considered? Although beyond the scope of this paper, the temporal differences in the agenda strongly suggests that different processes are perhaps responsible for the similarly high levels of polarization we observe at different points in Congressional history.

6 Estimating the Relationship

Having characterized the main measures over time, to better identify how the agenda changes alongside changes in the majority party I estimate the relationship using three closely related measures based on patterns we should observe if majority party agenda setting occurs: the relative percentage of votes that split continuing party members, the difference in the percentage of cutpoints lying in Zones 1 and 5, and the difference in the percentage of cutpoints estimated to lie in Zones 2 and 4. The first measure relies only on the observed pattern of votes, but the other two assessments use parameter estimates from `WNOMINATE` applied separately to each congressional dyad. Because the outcome measures are in terms of the relative difference between Democrats and Republicans – i.e., how many more votes split Democrats than Republicans or how many more votes are contained in Zone 1 relative to Zone 5 – the assessment is arguably biased in favor of finding support for agenda control because focusing on the relative difference rather than

the absolute levels allows for theoretically unpredicted levels of each. If, for example, 75% of the votes split Democrats and 25% of the votes split Republicans under a Republican majority, although the 50% difference is consistent with the agenda splitting the minority coalition more than the majority, the fact that the majority is split on 25% of the observed votes is theoretically unexpected. As a result, the estimates obtained almost certainly overstate the degree to which the observed agenda is consistent with majority party negative agenda control.

As previously noted, controlling for all of the ways in which circumstances may change over time is simply impossible with available measures. But absent this ability it is impossible to consistently estimate the associations of interest – to say nothing about the even more challenging empirical task of inferring causal effects. Complications arise not only because of the confounding effects caused by compositional changes - changes that may be correlated with changes in majority status – but also because of other time-varying effects that may differentially affect a party's ability or desire to exercise agenda control. 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 each of these potential confounding effects to limit the consequences of omitted variables bias when estimating the effect of majority party agenda control. This task seems hopeless.

As an alternative, I conduct two assessments in the hopes of triangulating on a qualitative estimate of the prevalence of majority party agenda control. To begin, I use regressions to characterize empirical regularity associated with the correlations over time and asses the extent to which the outcomes of interest are related to majority party control, the number of recorded votes, the number of continuing legislators, and secular time trends. Such an assessment provides no causal leverage, but it does highlight relationships of theoretical interest and the difficulties of extant approaches. To do so regressions employing the following specification are used for outcome $Y_{t,d}$ in time period t – either a Congress or year – within dyad d.[18]

[18]This notation is clunky as it is used to denote the fact that a Congress or year can occur twice in the data due to the `WNOMINATE` estimations being performed at the dyad level.

$$Y_{t,d} = \alpha + \beta_0 \text{Dem Majority}_t + \beta_1 \log(\text{Num. votes})_t + \beta_3 \text{Pct. Democrat})_t + \beta_4 \text{Dyad}_t + \epsilon_{t,d} \quad (1)$$

To control for potential time-varying confounders, specifications include the percentage of continuing members that are Democrats, the logged number of recorded roll call votes, and a secular time trend. The fraction of Democrats coarsely accounts for compositional differences between consecutive congressional pairs – although it 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 the emergence of new issues. The dyad time trends accounts for other (linear) trends that may affect the outcomes of interest - e.g., the changing media environment.

In addition to estimating the levels of the outcome, I also utilize dyad fixed effects to better account for the numerous time-varying differences that may influence the congressional agenda using:

$$Y_{t,d} = \alpha + \beta_0 \text{Dem Majority}_t + \beta_1 \log(\text{Num votes})_t + \beta_3 \% \text{ Democrat})_t + \delta_d + \epsilon_{t,d} \quad (2)$$

where δ_d denotes a fixed effect for each of the D congressional dyads.

Dyadic fixed effects helps (partially) limit the effect of time-varying influences, but it does so at the cost of restricting the identification of the parameters to within-dyad variation. Put differently, the inclusion of the dyad fixed effects δ_d means that the identification of β_0 results from changes in majority party control within a dyad – i.e., comparing how the agenda varies when Democrats are in control relative to when Republicans are in control. The dyadic fixed effects effectively 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 previously examined the association between the congressional agenda and changes in majority control (e.g.,

Aldrich and Rohde 1998; Lee 2018), but the use of fixed effects estimates the average difference associated with a change in majority party status over the entire post-Reconstruction time period.

Most analyses of agenda setting have focused on Congresses as the unit of analysis, but it is not obvious that this is the only level of interest. Congresses are natural units of analysis because they are defined by a period of (relatively) stable composition and elites are presumably acting knowing that they have two years to create the record of accomplishment that they wish to take to voters. Even so, analyzing the results by year analyzes the extent to which agendas vary within a House and how the magnitude of the year-to-year variation in congressional agenda compares to the magnitude of between-Congress variation. This is important for better estimating how the variation we observe in the agenda when majorities change compare to the variation we observe *among the same continuing members* relative to the variation we observe in the agenda when they do not – i.e., how the variation in the agenda occurring in the first two and last two years of a dyad under a stable majority compares to the variation in the two years when majority control changes.

	Relative Split	Zone 1 - Zone 5	Zone 2 - Zone 4	Relative Split	Zone 1 - Zone 5	Zone 2 - Zone 4
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic Majority	-0.140^{***}	-0.117^{***}	-0.001	-0.106^{**}	-0.104^{***}	0.021
	(0.051)	(0.020)	(0.041)	(0.043)	(0.020)	(0.032)
Log Number of Votes	0.121^{***}	-0.004	0.115^{***}	0.060^{***}	0.015^{**}	0.042^{***}
	(0.028)	(0.011)	(0.022)	(0.015)	(0.007)	(0.011)
Percentage of Democrats in Dyad	0.550^{***}	0.170^{**}	0.337^{**}	0.440^{**}	0.049	0.334^{***}
	(0.209)	(0.081)	(0.166)	(0.175)	(0.080)	(0.127)
Dyad Time Trend	-0.003^{***}	-0.001^{***}	-0.001	-0.002^{**}	-0.002^{***}	-0.0003
	(0.001)	(0.0004)	(0.001)	(0.001)	(0.0004)	(0.001)
Constant	-0.784^{***}	0.034	-0.761^{***}	-0.366^{***}	0.004	-0.346^{***}
	(0.170)	(0.066)	(0.136)	(0.100)	(0.046)	(0.073)
Unit:	Congress	Congress	Congress	Year	Year	Year
Observations	144	144	144	325	325	325
R^2	0.160	0.325	0.226	0.063	0.165	0.114
Adjusted R^2	0.136	0.306	0.203	0.051	0.154	0.103
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 1: Associations Over Time: Specifications (1) - (3) are at the congressional level, specifications (4) - (6) are at the yearly level (within Congress). Positive values of the outcomes indicate more splits among Democrats than Republicans.

In the cross-sectional analysis results reported in Table 1, we see correlations that initially appear broadly consistent with majority party agenda control regardless of whether the analysis is

conducted at the congressional or annual level. When Democrats are in the majority, there are 14% fewer votes, on average, splitting Democrats relative to Republicans when analyzed at the congress level and 11% fewer votes when analyzed on the annual level. However, Democrats are more likely to be split the larger the percentage of Democrats there are – a 10% increase in the percentage of Democrats in the dyad is predicted to increase the relative percentage of votes splitting Democrats relative to Republicans by around 5% – and the more votes that are taken. While this perhaps suggests that Democrats are willing to allow dissent when they have a larger coalition, this finding is contrary to claims about unconditional agenda setting power and it also raises questions about the relative importance of compositional effects on voting behavior given that Democratic control of the House is directly related to a higher percentage of Democrats in the dyad – in terms of specification (2), a 30% increase in the percentage of Democrats completely offsets the predicted effects of majority control on the relative incidence of observing votes splitting Democrats.

Similar questions arise when looking at the distribution of the observed votes relative to the preferences of continuing members. As specifications (2) and (5) reveals, there are indeed fewer votes splitting Democrats who are more extreme than the continuing median – i.e., votes on which a bipartisan coalition is predicted to oppose the extreme Democrats – relative to votes splitting extreme Republicans when Democrats have a majority. While not strictly predicted by the theories of majority agenda control, it is also not inconsistent with the incentives of the majority party as it suggests that Democrats are better able to vote cohesively in a bipartisan coalition against extreme Republicans when they are in the majority. But the lack of difference in specifications (3) and (6) related to the frequency of votes that are predicted to split the party median and the most moderate partisan are much harder to reconcile. The fact that there are no differences regardless of the party in control suggests that parties are not able to either better protect their more moderate (and therefore electorally vulnerable) members from having to break with the party or target the more moderate members of the opposition party to expose divisions within the majority party. Put differently, partisans who are more moderate than their party median – and who are therefore both more likely to agree with the opposing party and represent a district that the opposing district can win – are equally likely to be targeting by a roll call regardless of whether they are in the majority or minority. (Recall that the location of

the cutpoint is the location of a member who is indifferent between voting yes or no.) Given that these are the most vulnerable members of the majority coalition – and also the members of the minority whose seats are arguably most like to flip – the fact that we see no differences is hard to reconcile with theoretical expectations.

In addition to these concerns is the fact that the specifications reported in Table 1 almost certainly suffer from omitted variable bias due to the difficulty of account for other time-varying correlates of the congressional agenda. To examine whether these hard-to-reconcile results are a possible consequence of unobserved variation I employ dyad fixed effects and estimate the associations using within-dyad variation. While similar results obtain at the congress level, I focus on the annual results to increase the precision of the estimates and allow for the estimation to leverage within dyad variation in majority party status.

	Relative Split	Zone 1 - Zone 5	Zone 2 - Zone 4	Relative Split	Zone 1 - Zone 5	Zone 2 - Zone 4
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic Majority	−0.083	−0.057***	−0.011	−0.072	−0.069**	0.014
	(0.051)	(0.020)	(0.036)	(0.048)	(0.027)	(0.029)
Log of Votes	0.058	−0.011	0.071	0.020	0.031***	−0.010
	(0.071)	(0.028)	(0.051)	(0.014)	(0.008)	(0.008)
Percentage Democratic Dyad	−0.260	0.209	−0.562	0.306	−0.383**	0.628***
	(1.201)	(0.466)	(0.853)	(0.291)	(0.161)	(0.171)
Unit:	Congress	Congress	Congress	Year	Year	Year
Dyad FE?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144	144	144	325	325	325
R^2	0.824	0.860	0.874	0.630	0.527	0.777
Adjusted R^2	0.639	0.712	0.742	0.518	0.385	0.710
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 2: Within-Dyad Differences Over Time: Specifications (1) - (3) are at the congressional level, specifications (4) - (6) are at the yearly level (within Congress). Positive values of the outcomes indicate more splits among Democrats than Republicans.

Relying on dyadic fixed effects to account for omitted time-varying confounders possibly present in the results reported in Table 1 does not strengthen the support for majority party agenda setting. As is immediately evident, the use of dyad fixed effects removes any relationship between the outcomes and majority party control of the House. While Democrats are still less likely to be split when they are in the majority, the difference is no longer statistically distinguishable from zero (likely because of the loss of precision associated with estimating the effect using within-dyad variation). Relatively minor differences in the percentage of votes splitting extreme partisans in

specifications (2) and (5) remain statistically distinguishable from zero at conventional levels of significance, but the magnitude of the estimated association is much less than those of Table 1.

7 Variation By Time? By Issue?

To explore the relationship further it is useful to conduct two addition analyses to allow the nature of the association to vary. To be clear, such explorations are beyond the strictly derived theoretical predictions as the theories of agenda control does not explicitly define scope conditions related to either time or issue content. Even so, it seems plausible that such aspects may be related to unobservable differences relevant for characterizing the empirical applicability of the theoretical predictions – e.g., the extent to which party brands are valuable and worthy of securing using agenda control.

To begin, I allow the effect to vary over time to relax the assumption of constant agenda control over time given the temporal variation evident in Figures 4 and 5. So doing allows the value of party brands – and therefore the value of agenda setting – to vary based on unaccounted for changes in the social, economic and political environment over the lengthy time period I examine (e.g., perhaps the importance of agenda control changes as a result of changes in the electoral environment (e.g., the adoption of the Australian secret ballot) or the nationalization of politics associated with the changing media landscape).

To explore the possibility of time-varying effects I estimate a version of a naive change-point model by re-running every specification after iteratively and sequentially dropping all prior dyads. In other words, I begin with a model using every dyad included in the specifications reported in Tables 1 and 2 and end with a model that includes only the most recent dyad. Figure 6 plots how the estimated coefficient on the indicator for a Democratic majority (β_0) varies after dropping all dyads prior to each dyad in sequence. Because the coefficient is identified using within-dyad variation, the estimates only change when dropping an observation containing a change in majority control - hence the discontinuous “jumps" in the coefficient and the total number of changes in the estimated parameter depends on the number of changes in majority control (and the average within dyad effect in the remaining dyads).

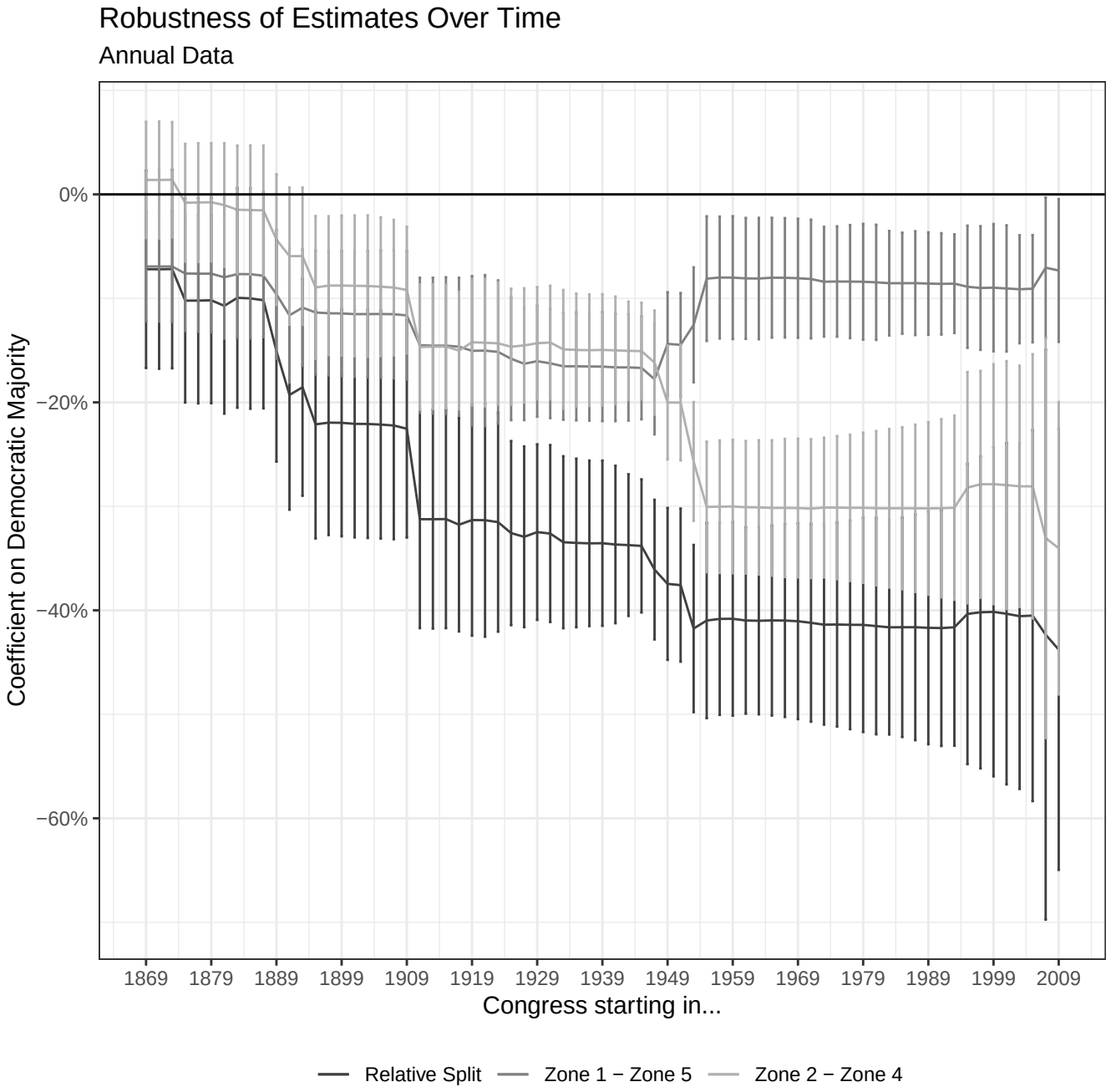

Figure 6: Temporal Sensitivity of Coefficients: Each graph plots the coefficient for Democratic Majority β_0 in specification (2) including all dyads following the dyad noted on the x-axis. The use of dyad fixed effects means that the plotted coefficients are identified by the average within-dyad variation in majority party control. The unit of analysis is a year.

Highlighting the changing relationship over time, the estimates graphed in Figure 6 immediately reveal that patterns consistent with majority party agenda control are driven by the changes we observe in more recent Congresses. Dropping congresses meeting prior to 1900 suggests

that Democrats are less likely to be split than Republicans when they are in the majority and that there are also fewer votes in both Zones 1 and Zone 2 than Zones 5 and 4 respectively. The fact that the coefficients are generally decreasing over time suggests that the effects are most pronounced in more recent Congresses. Replicating the qualitative differences evident in the pooled regressions, the largest effects are associated with votes splitting extremists (i.e., Zone 1 - Zone 5) rather than moderates (i.e., Zone 2 - Zone 4) even though the majority should presumably be more concerned with protecting more vulnerable moderate members and exposing more moderate members of the minority.

The patterns evident in Figure 6 raises several additional questions beyond the scope of this paper. First, because of the need to assume that agenda control and majority party status are synonymous, we cannot evaluate empirically whether those seemingly important temporal 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 leadership to 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.

Second, it is unclear why the nature of Congress, the electoral environment, or society has changed in ways that would seem to favor an increase in agenda control power as the results suggest. In fact, the contemporary environment seems more likely to favor the independence of members rather than conformity given rampant decentralization in communication, fundraising, and access to candidates and voters and the ability of legislators to develop a personal brand that is independent of party.

Third, precisely because the effects consistent with agenda control only emerge more recently, how should we interpret the similar levels of elite polarization between these periods? Put differently, if we are willing to ascribe at least some of the contemporaneous levels of polarization to agenda

control, how should we interpret the meaning of similarly high levels of partisan disagreement when the correlates of partisan agenda control were not present? Does this suggest that the nature – and therefore the possible consequences – of elite polarization differ in ways that make it difficult to reach for the past when trying to interpret the present?

7.1 Variation by Issue?

But perhaps the temporal variation evident in Figure 6 is attributable to variation in the issues being considered. Although the theories of agenda control do not offer clear predictions about the importance of issue content beyond the effects due to preferences on agenda control, if some issues are more electorally valuable than others then perhaps the desire and incentive to exercise agenda control depends on the issues involved – especially if it is costly to do so (perhaps because of monitoring or vote-buying costs). To explore this possibility I compare the patterns over time based on the issues associated with each vote. Many ways of coding the issue content of the congressional agenda exist, but for simplicity and consistency I rely on the readily available codes devised by Clausen. Given that there are no strong theoretical expectations as to precisely how issue content may affect agenda control conditional on elite preferences, the question is simply whether there is evidence of agenda control occurring within a subset of issues in ways that are obscured by the aggregates analyses performed so far.

Figure 7 recreates Figure 4 for each issue and shows that it is not the case that the aggregate patterns obscure clear evidence of agenda control once issue content is accounted for. While the predicted associations are harder to discern in some issues than others, it is not obvious that prior conclusions based on the entirety of the roll call record are affected by differences in the issue content of the votes being analyzed. While some issues see to exhibit patterns that are more theoretically consistent than others – e.g., Social Welfare votes compared to votes on Foreign and Defense Policies – other prominent and consequential issue areas show even an less obvious connection between majority party control and voting coalitions (e..g, Civil Liberties). That said, the patterns appear more similar than dissimilar across issues than different and it is not clear that theoretical expectations are much better satisfied when considering votes within a single issue domain.

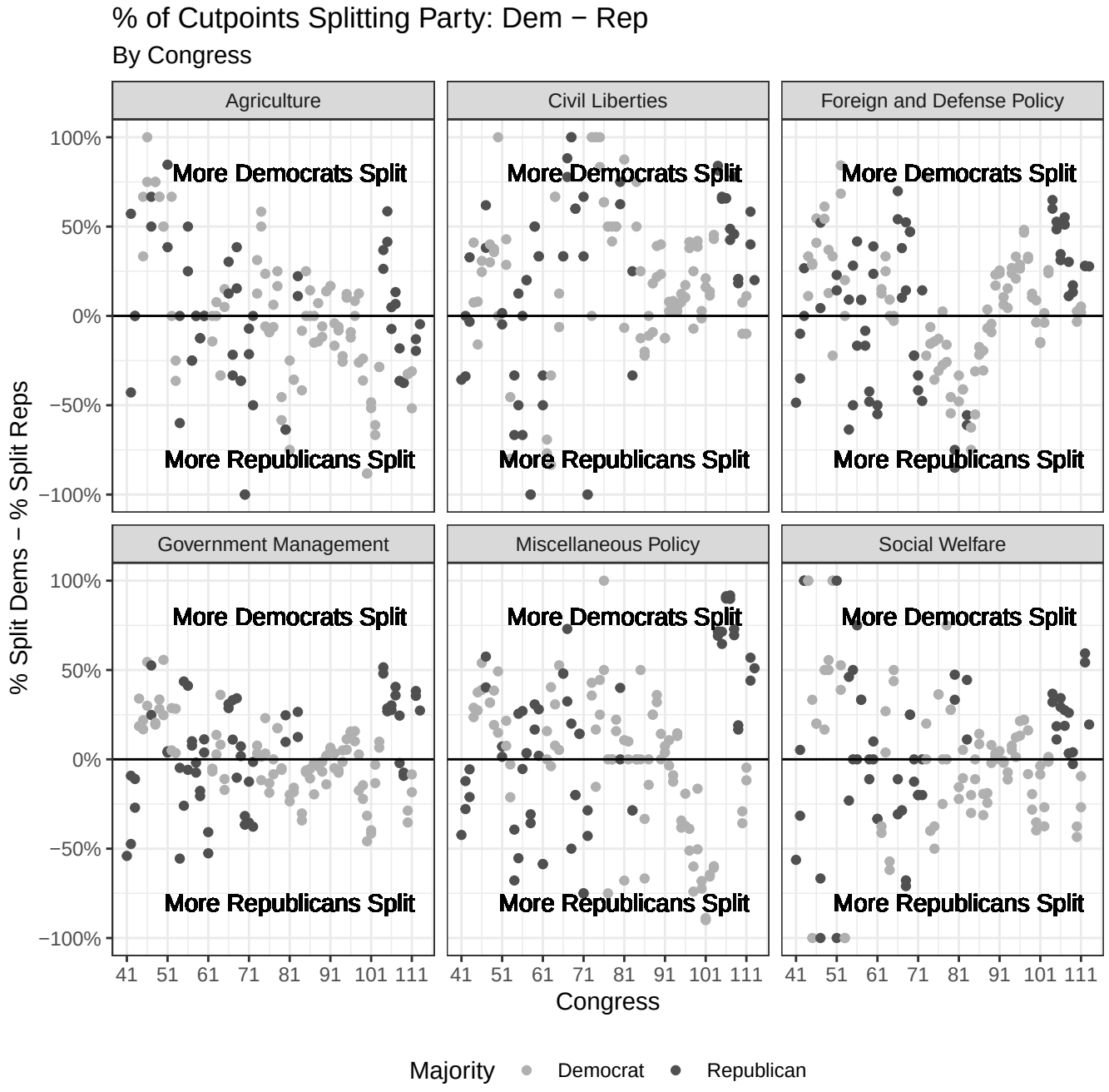

Figure 7: Temporal Sensitivity of Coefficients: Each graph plots the coefficient including all dyads following the dyad noted on the x-axis. The top-panel characterizes the relationship by Congress and the bottom-panel characterizes the relationship by year. The use of dyad fixed effects means that the plotted coefficients are identified by within-dyad variation in majority party control.

8 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 and leveraging variation in state institutions may sometimes yield dividends for those interested in causal effects (see, for example, Feigenbaum, Fouirnaies and Hall 2017), the ability to generalize between national and state 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 analyses 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 be informed by the insights from well-specified theories to help identify possible confounders and observational equivalence. Given the abundance of spurious associations, the importance of empirical characterizations largely depends on whether the relationships either correspond to extant theories or else help they help develop new theorizing about the nature of the political process underlying the uncovered empirical regularities.

This paper contributes to this effort by examining the statistical association between changes in majority control and the changing roll call agenda in the US House of Representatives. Inter-

preting observed changes in the roll call record as being due to majority party agenda control is extremely challenging for several reasons. In addition to the usual concerns about non-random treatment assignment and the difficulty of distinguishing agenda control from other aspects closely related to majority status (e.g., coalition size), the need to leverage over-time variation when attempting to estimate the counterfactual creates a difficult, if not impossible, measurement task given the many difficult to measure time-varying differences.

Building on a robust existing theoretical and empirical literature, I examine how changes in majority status affect how recorded congressional votes affect members in consecutive congresses before and after a change in majority control. 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 and the fact that many such measures are themselves a product of the observed agenda – I use specifications inspired by attempts to estimate causal effects to limit possible confounders. In particular, I identify how members serving in consecutive Congresses are differentially affected differentially by changes in the roll call agenda in theoretically predictable ways to limit the impact of compositional changes on the estimated parameters and I employ dyadic fixed effects to estimate the average effect of changing agenda control using the average within dyad change to limit the confounding effects of the numerous ways in which the political, economic and social circumstances have changed.

Only in recent Congresses is there evidence consistent with agenda control by the majority party and even then the evidence is rather limited. Although fewer votes are taken which split the majority party than the minority party, the largest differences are found in votes where a bipartisan coalition opposes the most extreme members of the minority party. There is little evidence that majority parties are able to prevent votes exposing their own more moderate members from having to take positions contrary to the median continuing member or which seek to isolate the more moderate members of the opposition from their continuing median. Moreover these patterns are not obviously impacted by the issues being voted upon as similar characterizations emerge when conditioning on the issues being voted upon.

There is also clear evidence of a dramatic temporal shift; the patterns are far more consistent

with negative agenda setting starting in the mid-1980s. (Recall that Republicans were able to gain control of the U.S. Senate in 1980 – the first time since the Republicans held a chamber of Congress since 1952.) The reasons for the temporal changes are beyond the scope of this paper, but the fact that the observed record of roll call votes is only consistent with theoretical predictions in recent congresses suggests the difficulty of reaching strong conclusions about the unconditional willingness or ability of the majority party to exercise agenda control in the U.S. House. It also raises important questions about how scholars should interpret similar levels of elite polarization across time. Because the pattern of recorded votes is consistent with agenda setting only in the contemporaneous period, this suggests either that some other mechanism is responsible for the ebbs and flows we observe or else that the polarization of the present is unrelated to the polarization of the past.

The characterizations I offer also highlight several broader considerations. First, the existence of a well-specified theory can be is critical for interpreting an estimated association – 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 estimated statistical associations. 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. Particularly in the case here where 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 perfectly covaries with majority status.

Second, work focused on national institutions must often leverage over-time comparisons when estimating statistical associations but 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 makes it impossible to identify the true relationship even with an infinite amount of data. While some concerns may be rectified by better measures or an alternative identification strategies that limit the necessity of explicit measures (e.g., the use of fixed effects), 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 patterns consistent with agenda control are only observed in more recent Congresses. Despite the temptation, our response to such limitations cannot be to abandon asking questions where inference is difficult, but rather to acknowledge how such limitations may affect the conclusions that are possible and what assumptions are required in the hopes that other sources can help validate those assumptions and ameliorate the concerns.

An expansion of scholarship focusing on causal inference appropriately has appropriately highlighted attention on what must be true to interpret an estimated association as being a causal relationship. Recognizing and acknowledging whether empirical correlations are causal or not certainly is essential for understanding the current state of knowledge, but measurement and theorizing remain important for characterizing empirical regularities whose causality is not easily addressed. The significance of determining causality is clear, but it is only because of well-specified theories and estimated associations that we are able to interpret (and generalize) causal estimates more broadly. It is to this continuing effort that this paper contributes.

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.

Baron, D.P, & Ferejohn J.A. (1989). Bargaining in legislatures. *American Political Science Review.* 83(4): 1181-1206.

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.

Caughey D., & Sekhon, J.S. (2011). Elections and regression discontinuity design: lessons from close U.S. House races, 1942-2008. *Political Analysis* 19: 385-408.

Clark, J. (2023). Parties, preferences and influence in legislative decision-making. Forthcoming *Party Politics.* https://doi.org/10.1177/13540688231169394

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.

Crosson, J.M. (2019). Stalemate in the States: Agenda Control Rules and Policy Output in American Legislatures. *Legislative Studies Quarterly* 44: 3-33.

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.

Finocchiaro, C.J. & Rohde, D.W. (2008). War for the floor: partisan theory and agenda control in the U.S. House of Representatives. *Legislative Studies Quarterly* 33(1): 35-61.

Fortunato, D, & Monroe N.W. (2020). Agenda Control and Electoral Success in the US House. *British Journal of Political Science* 50(4):1583-1592.

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.

Haimmueller, J., Hall A.B. & Snyder, J.M. (2015). Assessing the External Validity of Election RD Estimates: An Investigation of the Incumbency Advantage. *Journal of Politics* 77(3): 707-720.

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.

Hall, R.L. (1997). *Participation in Congress* New Haven: Yale 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.

Richman, J.T. (2015). The Electoral Costs of Party Agenda Setting: Why Hastert Rule Leads to Defeat. *Journal of Politics* 77 (4) , pp. 1129-1141.

Richman, J.T. & Roberts, R. (2020). When Do Low Roll Rates Indicate Party Influence? Evidence from Counterfactual Roll Rates. *Legislative Studies Quarterly*, 45: 177-206.

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 procedural coalitions 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 votes, 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.