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The Majority-Party Disadvantage: Revising Theories of Legislative Organization

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ABSTRACT

Dominant theories of legislative organization in the U.S. rest on the notion that the majority party arranges legislative matters to enhance its electoral fortunes. Yet, we find little evidence for a short-term electoral advantage for the majority party in U.S. state legislatures. Furthermore, there appears to be a pronounced downstream majority-party *disadvantage*. To establish these findings, we propose a technique for aggregating the results of close elections to obtain as-if random variation in majority-party status. We argue that the results from this approach are consistent with a phenomenon of inter-temporal balancing, which we link to other forms of partisan balancing in U.S. elections. The article thus necessitates revisions to our theories of legislative organization, offers new arguments for balancing theories, and lays out an empirical technique for studying the effects of majority-party status in legislative contexts.

Keywords: Elections; legislatures; regression discontinuity

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Though parties were absent in the early years of the American Republic, they have come to dominate much of American political discourse. In conjunction with the increase in legislative polarization and partisanship over the past 40 years (e.g., McCarty *et al.*, 2006), political scientists have developed a number of theories to explain the existence of parties and, in particular, to study the hypothesized powers of the majority party in the legislature (Aldrich, 1995; Aldrich and Rohde, 2001; Cox and McCubbins, 2005, 2007). Although the details of these theories differ, they share the view that the majority party organizes its affairs with a gimlet eye towards the electoral fortunes of its members. In such models, the majority party uses its postulated procedural powers in the legislature to create a brand that benefits its members at election time. But does winning majority-party status convey this posited electoral advantage? This is the question we study in this paper. We develop an empirical technique to measure the electoral effects of majority-party status, and we show tentative evidence that, in U.S. state legislatures, there is in fact little or no short-run majority-party advantage and, moreover, a pronounced long-run majority-party *disadvantage*. We discuss what these findings imply for theories of legislative organization, and we argue that this disadvantage stems, at least in part, from a pattern of inter-temporal partisan balancing by voters.

Theories of majority-party power, along with a broader class of theories about outcomes and dynamics that occur at the legislative level and not at the electoral level, have proven difficult to study empirically because of a fundamental problem of selection. Unlike in studies of individual electoral outcomes (e.g., the incumbency advantage), there is often no way to obtain random or quasi-random variation in majority-party status. Without this variation, researchers are unlikely to be able to separate the effects of majority-party status *per se* from other differences between the places that elect one kind of majority (e.g., Democratic) from those that elect another (e.g., Republican).

To make progress on these questions, we develop a "multidimensional regression discontinuity" design (MRD), following previous work on PR electoral contexts (Folke, 2014; Kotakorpi *et al.*, 2013).[1] Like the more typical regression discontinuity (RD) design, this strategy leverages quasi-random variation in the identity of the winning party of close elections. Unlike typical RDs, though, our technique combines variation from multiple close elections to obtain quasi-random variation in majority-party status, taking advantage of the fact that majority-party status is the aggregated result of individual election returns. After presenting evidence for the technique's validity, we apply it to U.S. state legislatures, 1968–2012, and we discuss the implications the results have for legislative and electoral theories.

[1]The approach is also similar in spirit to the geographic RDs presented in Keele and Titiunik (2015).

The remainder of this paper is organized as follows. We begin by offering theoretical perspectives that motivate the research and help interpret our statistical analyses. Following that, we lay out the MRD approach and present empirical tests for its validity in U.S. state legislatures. Next, we employ the technique to estimate the majority-party advantage in the short and long terms, and we consider variation in the effect in an attempt to identify the sources of the majority-party disadvantage. Subsequently, we discuss the revisions that the results necessitate for theories of legislative organization. Finally, we conclude.

Theoretical Perspectives

An enormous literature in American politics studies the partisan organization of U.S. legislatures, for obvious reasons.[2] Legislatures are the center of the policy process, and the manner in which they are organized and run is a crucial determinant of the types of policy the country implements.

The dominant thrust in this literature seeks to explain what parties do in the legislature by explaining "why it is in each [legislator's] interest to support a particular pattern of organization and activity for the party" (Cox and McCubbins, 2007, p. 100). Because of the primacy of re-election concerns (Mayhew, 1974), these theories rest on the notion that legislators create parties and vie for majority-party status in large part because it helps them win re-election (though we will have more to say on policy, rather than re-election, motivations later). Cox and McCubbins (2007), for example, present a formal model in which each legislator's probability of re-election depends on his or her personal characteristics as well as those of his or her party, and use the model to explain decisions about the organization of the majority party in the U.S. House. As they write, "...the best way to maximize the probability that one's party will win a majority next time may very well be to concentrate on getting the current majority reelected (121)." The authors conclude, "...by creating a leadership post that is both attractive and elective, a party can induce its leader to internalize the collective electoral fate of the party" (121). Though differing in important details, the model's focus on the incentives of individuals in forming a party that makes them better off is very much in the same philosophical mold as Aldrich (1995), Aldrich and Rohde (2001), and Cox and McCubbins (2005), among others.

An immediate implication of this argument is that members should receive an electoral boost when their party attains or holds majority-party status. If members use the majority party's voting power to organize the legislature in a way that boosts their electoral fortunes, then gaining majority-party status

[2] The literature on majority-party power is not limited to the U.S. See, for example, Fouirnaies and Mutlu-Eren (2015).

should convey a direct electoral benefit. Put another way, members of the party should perform better in an election when their party is in the majority than in the same hypothetical election — all else held equal — where they are instead members of the minority party. Furthermore, this boost should be observed discontinuously when the party becomes the majority party. Even a narrow majority, in these theories, should be sufficient to control procedural levers and thus to create the party brand and pursue other avenues that aid the party's members on election day. In this way, a narrow majority today should, on average at least, become a less narrow majority down the line.

Alternative theories of legislative organization, however, predict no effect of majority-party status. Krehbiel (1998), to choose the most well-known example, offers a model in which the spatial logic of the legislature dominates and parties are absent. In such a model, majority-party status is irrelevant because only the spatial positions of atomized legislators matter. Being in the majority party conveys no special advantages because, at the end of the day, all procedural choices are at the whim of the median legislator. There is therefore no mechanism for any majority set of legislators to pull policies in their preferred direction, and thus no creation of a party brand.[3]

A final set of theories, in contrast to those just described, could predict a majority-party *disadvantage*. This potential explanation comes from the literature on partisan balancing (Alesina and Rosenthal, 1989; Bafumi *et al.*, 2010; Erikson, 1988; Erikson *et al.*, 2015), which argues that voters will prefer the out-party in other electoral offices in order to preserve ideological balance. Though the argument was originally applied to U.S. House midterm elections after presidential elections, the logic is quite general and has received empirical support in many contexts, including U.S. statewide elections (Erikson *et al.*, 2015), U.S. state legislative elections (Folke and Snyder, 2012), and a variety of international contexts (e.g., Erikson and Filippov, 2001; Kern and Hainmueller, 2006). The basic idea is that moderate voters react to overly partisan policy moves by rewarding the other party in other elections. By creating balance *across* offices, they therefore secure more moderate policy than they could through unified control of the executive and the legislature. The theory does not necessarily require voters to be sophisticated — in the sense of remembering which party controls which offices and reacting accordingly — but simply that they observe the current partisan direction of policy and react.

[3]In addition, dynamics of state vs. federal elections might also predict no effect of majority-party status in low salience, state-level elections like those we will study in this paper. Rogers (2015), for example, argues that national elections drive state-level elections. In the most extreme case, if state legislative elections were entirely determined by preferences for presidential candidates, then there would be no effects of majority-party status in the legislature. This is probably too extreme a hypothetical. We know, for example, that incumbency advantages are large in state legislative elections (Ansolabehere and Snyder Jr, 2004; Fowler *et al.*, 2014), which suggests that there is more to the story than only national elections.

This balancing literature has focused, by and large, on what we call *inter-office* balancing: the balancing of partisan control across offices. However, the logic can be extended to a separate phenomenon that we call *inter-temporal* balancing: the balancing of partisan control within an office over time. If the majority party pulls policy away from the middle and towards the desires of its members, then moderate voters may prefer that the party control of the office alternate over time, just as they might prefer balancing control across offices.[4]

Importantly, a balancing argument for the majority-party disadvantage is not entirely at odds with theories that predict that the majority party is powerful in the legislature. Indeed, it could be that the majority party's power is precisely what induces voters to prefer balance, if the majority party pushes policy too far from the middle. The theory of Conditional Party Government, for example, leaves room for members to have preferences over policy and not just re-election (Aldrich and Rohde, 2001). The theory then predicts that members will make the majority party powerful (by centralizing power in leadership) in times of preference homogeneity in order to change policy (and not just their own re-election chances). However, observing a majority-party disadvantage under such a theory would require that majority-party members' preferences are such that they are willing to take an electoral hit in exchange for *temporary* policy shifts towards the extreme, since a majority-party disadvantage will make the other party more likely to become majority in the future and engage in the same exercise of policy change. Thus, while balancing theories need not conflict directly with theories of legislative organization that posit majority-party power, adding a balancing tendency in the electorate into such theories would require changes to the postulated goals or strategies of majority-party members.

In this section we have reviewed theories of partisan legislative organization and the electoral predictions they proffer. We have offered theoretical views consistent with a positive, negative, or non-existent majority-party advantage, and we have explained why an empirical test of this advantage could force important revisions of these theories. We now explain the empirical strategy we employ to test these theories.

Aggregating Close Elections to Obtain Variation in Majority-Party Status

Obstacles to Estimating Majority-Party Effects

To understand the degree to which majority-party members are advantaged or disadvantaged, we need to estimate the effects of majority-party status on

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[4]This kind of alternating representation is discussed in other contexts in, for example, Bafumi and Herron (2010).

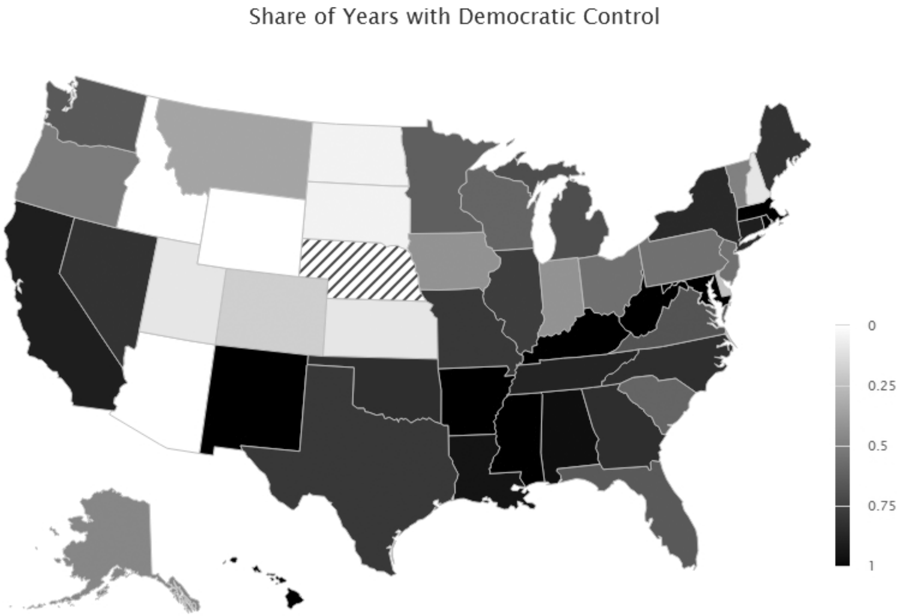

Figure 1: **Stability of Majority-Party Control of the Lower Chamber of State Legislatures Across U.S. States, 1968–2012.** States are shaded from black (always Democratic legislatures) to white (always Republican legislatures). NE is omitted due to non-partisan legislature.

electoral outcomes. Specifically, we are interested in comparing how a given party performs in an election cycle in which it is the majority party, relative to the counterfactual in which it is instead the minority party, but all else is held equal. This idealized counterfactual ensures that we capture the *per se* advantages of majority-party status but not any of the other factors that can make members of the majority party perform better electorally.

As an example to fix these ideas, consider Figure 1, which shows a map representing the stability of majority-party control of lower houses across states in our sample. States colored in darker shades have legislatures always controlled by the Democratic party; those colored white have legislatures always controlled by the Republican party. The intensity of the blue or red coloring indicates the degree to which one party or the other typically controls the legislature. As the map shows, many legislatures have a very stable majority party, i.e., a single party maintains majority-party status for many years.

This stability is *not* evidence for a majority-party advantage. Though a large majority-party advantage could be a sufficient condition for majority-party stability, many other factors could also produce this stability in the absence of such an advantage.[5] The underlying partisanship of voters could ensure that one party wins most seats in most years, even if majority-party status itself conveys no additional electoral advantage.

The figure also reveals which states have seen shifts in majority-party status. Texas, for example, is shaded in grey because, while it has historically had a Democratic lower chamber, it has since had a sustained run of Republican-controlled lower chambers. Yet this example highlights the central empirical obstacle of studying majority-party advantage. No one would claim that the switch in control of the Texas House of Representatives was random. The changes in Texas correspond quite clearly to marked shifts both in the parties — reflecting the Southern realignment — and in the underlying preferences of Texan voters.[6]

To address these empirical issues, we need to isolate variation in majority-party status that is not correlated with the underlying observable and unobservable characteristics of legislatures and time periods. In an ideal world, with no constraints on our abilities, we could *randomize* election outcomes to ensure that some legislatures received a Democratic majority and others received a Republican majority. We would then be able to compare future electoral outcomes across these two sets of cases to learn about the majority party's advantage. The randomization of majority-party status would ensure that we are capturing these *per se* advantages of majority-party status and not any of the other confounding factors. Since we cannot (and should not) run such an experiment, we must instead develop a quasi-experimental technique based on observational data. The next subsection outlines this approach.

The Multidimensional Regression Discontinuity Design

To isolate quasi-random variation in majority-party status we follow the literature on regression discontinuity designs and focus on the outcomes of close elections, which can, in the limit, be thought of as-if random (Lee, 2008). While in isolation the outcomes of close elections only inform us about dynamics at the district level — like the incumbency advantage — we can combine *multiple* close elections to study outcomes aggregated at the legislative level.

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[5] For related work on understanding the stability of majority-party control, see Folke *et al.* (2011).

[6] While we could certainly attempt to control for shifts in public opinion using recent advantages in voter scaling — see for example Tausanovitch and Warshaw (2013, 2014) and Warshaw and Rodden (2012) — such an approach would still not isolate the majority-party advantage. Suppose we could match two states that have the same underlying voter ideology, but one elects a Republican majority and the other a Democratic majority. Even though we have held fixed the underlying voter preference, there are still likely to be many unobserved factors that led one state to choose a Democratic majority and the other a Republican one.

In this paper, we focus on majoritarian legislatures with single-member districts. We take our cue from a closely related literature that focuses on combining multiple elections in proportional representation systems. In a study of Swedish municipalities, Folke (2014) introduces a "minimal distance" MRD approach. We use this minimum distance as one of our distance measures, as explained below. Solé-Ollé and Viladecans-Marsal (2013) adapt this approach to the study of Spanish municipalities, and Fiva *et al.* (2014) apply it to Norwegian local governments. In a study of Finnish elections, Kotakorpi *et al.* (2013) instead use a resampling-based technique to leverage multiple election outcomes in a different way.[7] However, as we show in the Appendix, this latter approach is not as well suited for the majoritarian context and results in extremely small sample sizes.[8]

To explain our approach in detail, we first describe the traditional, single-variable regression discontinuity design, before considering the multivariable case with more than one running variable — i.e., more than one election outcome. We term the multivariable case the multidimensional regression discontinuity design or MRD. We use the potential outcomes framework throughout, with notation following Imbens and Lemieux (2008) and Zajonc (2012). We observe an outcome Y and a continuous scalar covariate X. The potential outcomes are $Y(0)$ and $Y(1)$, the values the outcome would take without and with treatment. For our application, these will be the party's vote share when it is the minority vs. when it is the majority, respectively. In the sharp regression discontinuity framework, we have W, the treatment indicator, as a deterministic function of X:

$$W = 1(X \geq c)$$

for some cutoff c. Thus, the observed outcome is

$$Y = (1 - W) \cdot Y(0) + W \cdot Y(1).$$

The variable X is known as the running variable. For the traditional application to elections, X is the vote share for a candidate in a district (usually centered at 0 so that positive X indicates a win and negative a loss), W is whether or not the candidate wins, and c is 0. There are no units for which we observe both $Y(1)$ and $Y(0)$; instead we compare units within ϵ of the cutoff, c, as we make ϵ arbitrarily small.

The traditional RD is a very powerful tool to estimate the effects of (for example) one party controlling a given seat on incumbency advantage or roll-call votes. However, the traditional RD method is insufficient if the final outcome of interest is at a more aggregated level than the observed running

[7] Tukiainen and Lyytikäinen (2013) also use the resampling technique to study Finnish elections.

[8] Nevertheless, we show that results are similar using it instead.

variables.[9] Clearly, the vote share of any one candidate does not determine a party's majority status. Instead, majority status is a more complicated function where there are many running variables, namely the vote shares in each district. However, with a few modifications, the RD tools can still be applied.

We still observe some outcome Y. However, we also observe $\mathbf{X}$, a vector of d covariates, rather than a simple scalar covariate as in the univariate RD case.[10] Treatment is a deterministic function of some or all of the running variables, $W = \delta(X)$, where $\delta : \mathcal{X} \to \{0,1\}$ is the assignment rule (Zajonc, 2012).[11]

Following Zajonc (2012), we define the treatment assignment set $\mathbb{T} \equiv \{x \in \mathcal{X} : \delta(x) = 1\}$. The complement of $\mathbb{T}$, $\mathbb{T}^c$, is the control assignment set. Let $\bar{A}$ be the closure of some set A. Then, we define the assignment boundary $\mathbb{B}$ as

$$\mathbb{B} \equiv bd(T) \equiv \bar{T} \cap \bar{T}^c$$

or as the intersection of the closures of the treatment assignment set and the control assignment set.

In many applications of MRD, heterogeneity across the treatment boundary may be an important object to study. The sharp conditional treatment effect is defined as

$$\tau_{SBRD}(x) \equiv E\left[Y(1) - Y(0)|\mathbf{X} = x\right], x \in \mathbb{B}.$$

In the education case, researchers might want to know the effect of some treatment on students that failed one test but not another or the difference in treatment effects between these two types of students. However, in our case, the more natural object of study is the sharp average treatment effect, defined as

$$\tau_{SBRD} \equiv E\left[Y(1) - Y(0)|\mathbf{X} \in \mathbb{B}\right].$$

Here, τ_{SBRD} would indicate the average effect of the treatment, averaged over all portions of the treatment boundary.

The key difference from traditional RD is that, rather than considering a bandwidth around the cutoff, we use ϵ-neighborhoods. Formally, the ϵ-neighborhood of a point x, $N_\epsilon(x)$, is the set of all points no farther than ϵ away

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[9] Papay *et al.* (2011) highlight many potential applications of MRD. In education, students may take multiple tests with treatment of a complex function of all scores. Similarly, teachers may be rewarded on the basis of many different tests taken by their students. In public finance, Leuven *et al.* (2007) test the effects of government transfers that are conditional on both the total minority group population share and the maximum minority group share. Papay *et al.* (2011) also mention the potential application of MRD to elections, as in this project.

[10] In the majority-party case, d will be the number of state house or state senate races in a given state.

[11] Note that MRD embeds the univariate RD case when $d = 1$ and thus $\delta = 1(X \geq c)$.

from x.[12] Of particular interest in MRD, we have B_ϵ as the ϵ-neighborhood around the boundary:

$$B_\epsilon \equiv \{\mathbf{X} \in \mathcal{X} : \exists x \in \mathbb{B} | \mathbf{X} \in N_\epsilon(x)\}.$$

Let $B_\epsilon^+ \equiv B_\epsilon \cap \mathbb{T}$ and $B_\epsilon^- \equiv B_\epsilon \cap \mathbb{T}^c$ be the boundary sets for treatment and control units respectively.

The two assumptions from Zajonc (2012) are:

1. Boundary positivity: For all $x \in \mathbb{B}$ and $\epsilon > 0$, $Pr(X \in N_\epsilon^-(x)) > 0$ and $Pr(X \in N_\epsilon^+(x)) > 0$
2. Continuity: $E[Y(1)|X = x]$ and $E[Y(0)|X = x]$ are continuous in x and f_X is continuous in x as well.

With the first assumption, we guarantee that there exist both treated and untreated units along the boundary. With the second, we ensure that treated and control observations along the boundary are, in the limit, comparable to each other in terms of their potential outcomes.[13] With these assumptions, Zajonc (2012) then proves that the sharp average treatment effect is the limit of two expectations, just as in the univariate RD case:

$$\tau_{SBRD} = \lim_{\epsilon \to 0} E\left[Y|\mathbf{X} \in B_\epsilon^+\right] - \lim_{\epsilon \to 0} E\left[Y|\mathbf{X} \in B_\epsilon^-\right]$$

To see how MRD is applied in practice to multiple elections, consider first the second-simplest case of majority rule where there are three elections, $i = 1, 2, 3$, between Democrats and Republicans.[14] This three-district case is illustrated in Figure 2. Each dimension represents the Democratic vote share margin in one of the three districts, and the Democrats have a majority if they win at least two districts. Thus the Democrats win the majority of the seats for any vote share vector located in one of the blue (dark) cubes, whereas the Republicans win the majority of the seats for any point in the red (light) cubes.

Let X_i be the triplet of Democratic vote share less than the Republican vote share in the three elections and let x_i be an element of X_i. The elections are standard in that if $x_i > 0$, the Democrat wins. Thus the number of Democrats elected will be $\sum_i \mathbf{1}(x_i > 0)$. With three seats, a party holding two or three seats will be in the majority and the indicator function $\mathbf{1}[\sum_i \mathbf{1}(x_i > 0) \geq 2]$ will indicate if Democrats are in the majority.

[12] In two dimensions, this is a sphere of radius ϵ. In more dimensions, this is a hypersphere of radius ϵ.

[13] See p. 55 in Zajonc (2012).

[14] The simplest case, of course, is a one-seat legislature and this collapses to the standard RD problem.

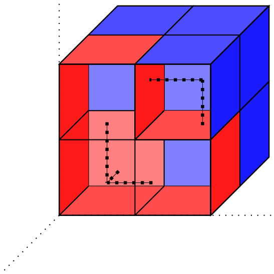

Figure 2: **MRD Example: A three-seat legislature.** The large cube represents outcomes in a three-dimensional space, where each dimension is the Democratic share of the two-party vote in one of the three seats in the hypothetical legislature. The blue (or darker shaded) sub-cubes are those in which the Democratic party has won the majority, i.e., those areas of the larger cube in which the Democrats win at least two of the three seats. The red (or lighter shaded) sub-cubes are those where the Republican party has won the majority.

Again, $\delta : \mathcal{X} \to \{0, 1\}$ is the assignment rule. In the case of a simple RD, $\delta(x) = \mathbf{1}(x > c)$. In the case of the majority party rule,[15]

$$\delta(\mathbf{X}) = \delta(x_1, x_2, x_3) = \mathbf{1}\left[\sum_i \mathbf{1}(x_i > 0) \geq 2\right].$$

In practice, to estimate a multivariable regression discontinuity, we need not just the treatment boundary but a concept of distance and a method to calculate the distance between the running variables and the treatment boundary. Both Wong *et al.* (2013) and Reardon and Robinson (2012) describe a number of methods for estimating a multivariable regression discontinuity. We focus on the centering method as the most straightforward solution to the problem of majority effects.[16] This method collapses the multiple dimensions of running variables to a single dimension that describes the distance to the treatment boundary.[17] Reardon and Robinson (2012) show that the estimated treatment effect is the same as the average treatment effect at all

[15]In the more general case, let the number of seats in the legislature be I. Let X be the set of all x_i. Then the assignment rule is simply: $\delta(X) = \mathbf{1}[\sum_i \mathbf{1}(x_i > 0) \geq I/2]$.

[16]We draw the "centering" name from Wong *et al.* (2013). Reardon and Robinson (2012) term this method "binding score".

[17]Zajonc (2012) shows that the sharp average treatment effect can also be found by path-integrating over the conditional treatment effect. While this method may have better finite sample properties than centering, Zajonc (2012) suggests that the two methods "will rarely differ" and that "[g]iven the additional complexity involved in integrating explicitly, we recommend estimating average effects using scalar RD methods...By using distance to the nearest boundary the scalar forcing variable binds along the entire boundary."

discontinuities, which fits the majority-party question because the ordering of different seat-races is arbitrary.[18]

However, both Wong *et al.* (2013) and Reardon and Robinson (2012) argue that the applied distance metric will vary between applications. What measure best captures how far a party is from winning majority status? We explore three measures of distance. The first is the minimum Euclidean distance between the vector of running variables and the treatment boundary. If the Democrats lose all three seats by 2 points, for example, they are $\sqrt{8} \approx 2.83$ away from a majority.[19] This distance measure has the advantage of familiarity — it is taught in high school geometry — but lacks convenient interpretation. We refer to this measure as the Euclidean distance throughout.

The second distance measure is the minimum rectilinear distance between that same point and the treatment boundary, the main measure introduced in Folke (2014).[20] This measure has the convenient feature of describing how many additional percentage points the party would have to be given to flip majority status.[21] Consider again our simple, three-seat legislature. If the Democrats lose the first seat by 0.5 points, the second by 2 points, and the third by 10 points, they would need 2.5 points to gain majority status. Throughout this paper, we will refer to this measure as the Manhattan distance.

The third distance measure that we consider is the minimum uniform partisan swing required to change the identity of the majority party.[22] Implicitly, this measure assumes perfect correlation across elections and that the effective distance to the majority for a party losing by two seats depends only on the distance of the second-closest loser, not the first.[23] We refer to this distance measure as the Uniform Swing distance.

How do we compute these three distance measures and how do they compare? While all three are technically the minimum distance from a vector

[18]The MRD literature has been developed for use in education applications. Reardon and Robinson (2012) detail a summer school treatment program where students are treated if they fail either a math or a reading test. Thus, there are two treatment boundaries (students that are sent to summer school because they fail the math test and students that are sent to summer school because they fail the reading test. The causal effect of summer school on some later outcome may differ across these two groups and that difference may be of interest to researchers. However, in the elections case, whether the Democrats are denied a majority because of a loss in one district versus another is of less interest, especially when the districts are ordered arbitrarily and differently across states or time.

[19]That is, they need to win any two races and the shortest path from the point $(-2, -2, -2)$ to the treatment boundary is $\sqrt{2^2 + 2^2} = \sqrt{8}$ units.

[20]The rectilinear distance is also known as the L_1 distance, ℓ_1 norm, or Manhattan distance as it corresponds to the distance between two points on a city grid, travelling only along the grid.

[21]Assuming, of course, that those points are efficiently distributed.

[22]This distance is related to the distance-to-majority estimated via simulation in Fiva *et al.* (2014), which captures the average vote shock necessary to shift majority control.

[23]This contrasts with the efficient distribution interpretation of the Manhattan distance.

in $\mathcal{X} \subset \mathbb{R}^d$ (the space of running variables) to a complex surface in $\mathcal{X}$ (the treatment boundary), this minimization process is trivial in all cases. For the Euclidean and Manhattan distances, it is equivalent to finding the distance (Euclidean or rectilinear) from a vector of the K closest losses, where K is the number of seats needed to win the majority, to the origin.[24] For the Euclidean distance, we can write $x_j^E = \sum x_{ji}^2$. For the Manhattan distance, we can write $x_j^R = \sum |x_{ji}|$. The uniform partisan swing distance is even simpler. We write the distance, x_j^{US}, as the size of the Kth closest loss where K is again the number of seats needed to win the majority. Naturally, these three distance measures are highly correlated: in our sample, the pairwise correlation coefficients range from 0.839 for the Manhattan and Uniform Swing distances to 0.976 for the Euclidean and Manhattan distances.[25] Because they are so highly correlated, we typically only present results using a single distance metric selected through balance tests in the paper (with a few exceptions to demonstrate robustness). In the Appendix, we provide a more detailed account of the different measures (including histograms and rank correlations between the variables).

Once we have these distance measures computed for each observation — in our case, each state-election cycle — the rest of the MRD procedure follows the standard RD procedure. We can use standard methods to calculate the appropriate bandwidth and estimate the effects of majority status on our outcomes, Y, using either local-linear approximation within the bandwidth or a polynomial control function in the running variable across a larger sample.

Seat Share: Not a Valid Running Variable

The running variables we have considered (Manhattan, Euclidean, and Uniform Partisan Swing) are all distance measures that map individual race outcomes into overall majority measures. Each distance measure attempts to describe how far (or close) a party is from majority status. One simple alternative, however, is the seat share itself: how many seats did the party win divided by the total number of seats in the legislature? Given that the majority party is, by definition, the party with the majority of seats, the seat share is a possible running variable: when seat share passes the 50% cutoff point, the party will be in the majority.

There are two reasons why we do not use the simple seat share measure as our forcing variable. First, the balance results are not promising. In a parallel exercise to Table 1 (see below), we regress the lag of treatment

[24]Of course, if the party in question is in the majority, K is the number of seats to lose the majority and the races of interest are the K narrowest wins.

[25]The correlation coefficient between the Euclidean distance and the Uniform Swing distance is 0.933.

(majority) status on Democratic seat share in the following election, as well as regressing previous seat share on future seat share. In the first balance test, with majority status as the outcome, we estimate a 10 percentage point increase in the probability of majority status with a standard error of 0.10. In the second balance test, with previous seat share as the outcome, we estimate a treatment effect of 1.46 points with a standard error of 1.34. Though neither of these effects are significant at conventional levels, both appear far from balanced at the discontinuity.

The second argument against using seat shares is more general, as other samples may have different balance test results. Using the seat share measure throws away a lot of potentially important data. Consider a five seat legislature with three seats won by Republicans and two by Democrats. The seat share for the Democrats is 40% and that would be the potential seat share running variable regardless of how close (or not) any of the actual races were. However, whether the closest Democratic loss was by 1 percentage point or by 20 percentage points should affect how close we consider the Democrats are to a majority. In the extreme case of a single seat legislature, using seat share running variable is akin to calling races won 51 to 49 just as close to flipping as races won 99 to 1. In addition, the distribution of possible seat shares varies across states based on variations in the total number of seats. Depending on the total number of seats — the denominator in the seat share calculations — certain values of seat share are impossible and the resulting discrete nature of the distribution could violate the traditional RD assumption of smoothness in the running variable density around the discontinuity. For these reasons, we focus in the subsequent analyses on the distance variables from the previous subsection.

Incumbency Effects and Majority-Party Effects

For our purposes, as we discussed above, we define $Y_{it}(0)$ to be party i's electoral outcome at time t after receiving the control, i.e., being the minority party, and $Y_{it}(1)$ to be the party's electoral outcome at t after receiving the treatment, i.e., holding majority-party status. The causal effect of interest is simply $Y_{it}(1) - Y_{it}(0)$, estimated using the technique we have just described. This definition makes clear that individual effects of incumbency are mainly distinct from this quantity. We know that individual incumbents possess marked advantages, both in Congress (Erikson, 1971) and also in state legislatures (Fouirnaies and Hall, 2014; Fowler *et al.*, 2014) like those we study in this paper. Most of the party's members will be incumbents whether or not the party is the majority-party. The comparison therefore differences out most of the incumbency advantage that is separate from majority-party effects. However, if there were some interactive boost — if the incumbency advantage is larger for majority-party members — then this latter component will be included. This latter component is simply another way to think about the legislative theories we discussed above. If the majority party successfully takes actions to boost its members re-election efforts, then incumbents of the majority party will do better than incumbents of the minority party. In addition to this possible interactive effect, when a party possesses majority-party status, it has more incumbents than the minority party by definition. One mechanism for an effect of majority-party status on downstream electoral outcomes is thus the slight, extra amount of incumbency advantage it receives for being the majority vs. the minority.

Although this particular part of the incumbency advantage, flowing through the fact that the majority has more incumbents than the minority, is included in the quantity of interest, we might be interested in knowing the degree to which it drives the estimated effects of majority-party status, especially in light of the legislative theories we discussed in Section 2. Explicitly disentangling them is difficult, but we should not expect them to play a major role because the comparisons the MRD will make will be among cases where the party just barely won or just barely lost majority status. These are cases where the number of incumbents is almost the same. Once we present our estimates in subsequent sections, we present a simulation to help discern how much different the estimate would be if we took account of these personal incumbency effects. As we will show, these personal effects may mask a slightly larger majority-party disadvantage, aggregating over all other mechanisms, but they are unlikely to be a main part of the story.

Data on U.S. State Legislative Elections

The U.S. state legislatures provide an ideal laboratory for studying the effects of majority-party status; they offer a large sample size for the MRD, and variation in the organization of the legislatures can provide further information about when and why the majority party is advantaged or disadvantaged.

We follow a large and growing literature that turns to state legislatures in order to answer, in a comparative American context, more general questions about legislative organization and behavior (e.g., Aldrich and Battista, 2002; Gamm and Kousser, 2010, 2013; McGhee *et al.*, 2013; Shor and McCarty, 2011). Indeed, Gamm and Kousser (2010, p. 1) write: "the states represent ideal arenas for considering the interplay of institutional rules, party competition, degrees of professionalism, and the variety of bills..." Although there are no doubts in a variety of differences between state and federal legislatures — in their professionalism, in the amount of money spent in campaigns, in the salience of their work and its coverage in the media, and so on — the states give us an excellent opportunity to look broadly at how parties operate in differing institutional settings.

To study the state legislatures, we use the Klarner *et al.* (2013) dataset, as extended by Carl Klarner, which covers all state legislative elections in the years 1968–2012. Among these cases, we focus on general elections that take place in partisan, single-member districts. This means that we exclude the non-partisan, unicameral legislature of Nebraska as well as those state chambers that feature multimember districts.[26] In addition to these restrictions, we focus on the effects of lower-chamber majority-party status. This is purely for mathematical convenience; because lower chambers do not have staggered elections, it is easier to compute distance variables for the MRD in these settings.

Finally, in investigating parties in state legislatures over this time period, we should keep in mind the unusual partisan shifts induced by the Southern realignment. Although the quasi-randomization from the MRD should prevent any such secular trends from biasing the results, given how widespread the resulting partisan shifts are, it seems wise to ensure that they do not somehow drive our subsequent findings. After presenting our main results, we turn to a careful account of this issue, and we show that it does not drive our results.

Estimation

Using the MRD technique described previously, we estimate equations of the form

$$Y_{i,t+k} = \beta Dem\ Maj_{it} + f(Dist_{it}) + \epsilon_{it}, \tag{1}$$

where $Y_{i,t+k}$ is an outcome of interest in state i, usually the Democratic party's majority-party status in subsequent electoral cycles. We investigate many downstream observations in order to track the majority-party effect over time. To ensure that estimates are comparable, we choose a maximum value of $k = 10$, and we perform all analyses on the set of data where $Y_{i,t+10}$ is non-missing. Thus for the main analysis the final year of treatment that we consider is 1990, since 10 terms downstream corresponds to 20 years downstream.[27]

The variable $Dem\ Maj_{it}$ is the treatment indicator,[28] taking the value 1 when the Democrats win a majority in the election at time t. The variable $Dist_{it}$ is the Euclidean, Manhattan, or Uniform Swing distance to the treatment boundary, and following the usual RD approach, f represents some function of this running variable — typically a local linear kernel-smoothed function at a so-called optimal bandwidth as implemented in Calonico *et al.* (2014). The quantity of interest is β, the MRD estimator for the effect of majority-party status.

As a plausibility test for the MRD, we compare the estimates we obtain from it with those from a difference-in-differences design. Specifically, we

[26] For a discussion of the consequences of excluding multimember districts, see Eggers and Fouirnaies (2014).

[27] In Tables 4 and 5 in the appendix, we also consider the possibility that the effect of majority-party status has changed over time.

[28] Note that our choice of focusing on the Democratic party is entirely arbitrary. Estimates would be identical if the Republican party were used, due to symmetry.

estimate equations of the form

$$Y_{i,t+k} = \beta Dem\ Maj_{it} + \gamma_i + \delta_t + \epsilon_{it} \tag{2}$$

where variables are defined as before and γ_i and δ_t represent state and year fixed effects, respectively. Although we may still think this specification is biased — the decision to switch majority parties still seems non-random even in the difference-in-differences setup — we might think this bias is relatively small and that the resulting estimates therefore provide a useful sanity check for the MRD estimates. Indeed, we find that the two strategies provide similar estimates in almost all cases. Since the difference-in-differences approach has a higher degree of statistical power, using as it does much more data than the MRD, it is an especially helpful comparison when the two approaches display similar point estimates.

Drawbacks to MRD Considered

As with any regression discontinuity technique, the MRD estimates are *local* to competitive legislatures. The majority-party advantage or disadvantage could be smaller or larger in lopsided legislatures; the results in this paper simply do not speak to such contexts. However, while it is important to acknowledge this limit to the analysis, there are two reasons to value the results.

First, majority-party effects in competitive legislatures are likely to matter far more than effects in lopsided legislatures because in the latter case, one party is likely to control the legislature no matter what. In a legislature dominated by the Democratic party, for example, whether or not the Democrats possess an advantage based on their majority-party status is not as relevant since they will control the legislature regardless. Second, there is simply no way to evaluate majority-party effects — or even to conceive of what they would mean — in a largely one-party context. What would it mean to assign the Rhode Island House of Representatives (92% Democratic) to have a Republican legislature today, or to assign Idaho (80% Republican) to have a Democratic legislature today? We cannot observe such a remote counterfactual in the real world, and no empirical technique exists, MRD or otherwise, to estimate a plausible majority-party effect in such a context.

In addition to the issue of locality, we should also consider the assumption that underlies the MRD — namely, that close elections and their aggregation truly are, in the limit, as-if random. A recent literature has offered evidence that this assumption does not hold in the U.S. House, where incumbents seem to win very close elections at an abnormally high rate (Caughey and Sekhon, 2011; Grimmer *et al.*, 2012; Snyder, 2005). Eggers *et al.* (2015), however, offer broad evidence that the election RD estimate is plausible. And more importantly, Eggers *et al.* (2015) show that state legislative elections do not exhibit any evidence of this kind of sorting. The balance tests in the next subsection are consistent with the notion that close elections in U.S. state legislatures are, indeed, as-if random.

Using Balance Tests to Select Distance Metric

Validating the MRD procedure is especially important due to the uncertainty over the correct distance metric. Error in the distance metric due to incorrect specification is akin to error in a control variable (see Equation (1)), and thus can cause bias in the estimated treatment effect. In order to select from among the proposed distance metrics, we therefore rely on the results of balance tests using the lagged treatment and lagged seat share variables.

In so doing, we must make a philosophical tradeoff between a purely design-based estimation procedure and a purely observational matching procedure. In the limit, with an infinite proliferation of proposed distance metrics, searching over them to find the best balance on these lagged variables would be a matching strategy and no longer a design-based approach. On the other end of the spectrum, choosing only a single distance metric *a priori* would require a strong theoretical assumption over the correlation structure among electoral districts in a legislature. By choosing three plausible distance metrics, we restrict the space to avoid a purely algorithmic search for balance on the lagged outcomes and thereby maintain most of the design-based advantage of the RD approach. Practitioners should keep in mind this tradeoff if they add more possible distance metrics to their applications, however.

Table 1 presents results of the balance tests for each of the three distance metrics. These results come from estimating Equation (1) where $k = -1$, implemented using rdrobust in Stata with the Calonico *et al.* (2014) optimal bandwidth and a local linear specification. In the first two columns, we see that the Manhattan distance does a relatively poor job in controlling for the distance to majority-party status. Treated state-years in the MRD using this distance metric are substantially more likely to have held majority-party status and to have had higher seat shares in the previous electoral cycle. The other two distance metrics, on the other hand, appear to do quite well.

The strongest balance results are for the Euclidean running variable, shown in the middle two columns. Here, treated and control units are shown to be quite similar in terms of lagged majority-party status and lagged seat share. This suggests, in the context of the MRD applied to U.S. state lower houses, that the Euclidean measure effectively captures the distance to majority-party status and thus avoids bias in the treatment variable due to error in the distance metric. While the Uniform Swing variable also does a solid job in the balance tests (final two columns), the measured imbalances are a bit larger in magnitude than the Euclidean tests.

Table 1: **Balance tests for possible distance metrics.** Evaluates the performance of the three proposed distance metrics by testing for balance on lagged majority-party status and lagged seat share. The Euclidean RV and the Uniform Swing RV perform best.

	RV=Manhattan		RV=Euclidean		RV=Uniform Swing	
	Lag majority	Lag seat %	Lag majority	Lag seat %	Lag majority	Lag seat %
Dem majority	0.11	2.23	–0.01	0.49	0.02	–1.49
	(0.08)	(1.39)	(0.09)	(1.62)	(0.12)	(1.98)
Optimal BW	96.96	114.59	27.63	33.06	13.81	12.60
N	381	409	347	390	352	337
Specification	Local Linear	Local Linear	Local Linear	Local Linear	Local Linear	Local Linear

Estimates using Calonico *et al.* (2014) optimal bandwidth implemented with rdrobust in Stata. Standard errors from this procedure in parentheses. Bandwidth sizes are reported in the units of each running variable and therefore vary in magnitude across the three.

Results: Majority-Party Disadvantage

Based on the balance tests in the previous section, we report all results using the Euclidean running variable. Before we perform formal estimation, we follow the usual RD practice by presenting plots of the discontinuities in the data. Figure 3 presents these plots, using the Euclidean MRD distance variable, for $k = 1, \ldots, 9$ (we omit $k = 10$ for the moment to preserve a convenient grid arrangement of the graphs). The vertical axis of the plots represents the indicator variable for downstream Democratic majority-party status. The grey dots on the plots are the raw, binary data; the larger black points represent averages within 2-point bins of the running variable. Finally, the lines represent OLS fits to the raw data on each side of the discontinuity.

Consider the top left plot, which represents the effect of majority-party status at t on majority-party status at $t + 1$, i.e., the immediate effect of majority-party status on the very next election. A large majority-party advantage would be represented in this plot by a large upwards jump in the fitted line when we look just to the right of zero on the horizontal axis — when the Democrats just barely win majority at time t. Instead, we see no jump in the graph whatsoever.

What is more, as we look across and down the figure, at the plots for downstream majority-party status, we see either no jump or *downward* jumps, indicating, if anything, a majority-party disadvantage. No meaningful positive jumps are observed in any of the plots.

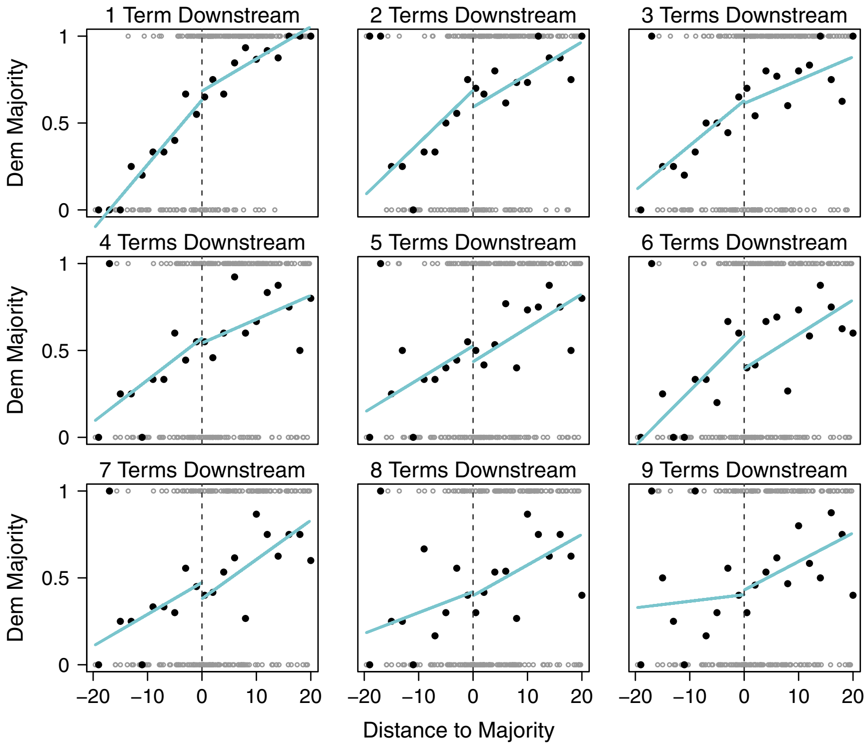

Figure 3: **The downstream majority-party disadvantage: U.S. state legislative lower chambers, 1968–2012.** Presents MRD plots for the effect of Democratic majority-party status at time t on downstream Democratic majority-party status. No large positive jumps are seen at the discontinuity, and downward jumps consistent with Table 2 appear starting two terms downstream.

Note: Grey points are raw data; black points are averages in 2-point bins of the running variable. Lines are OLS regression lines fit to the raw data on either side of the discontinuity.

Table 2 presents the formal results, estimated using the automated procedure from Calonico *et al.* (2014).[29] The rows of the table correspond to estimates for different values of k from Equation (1). The first row, for example, corresponds to the immediate majority-party effect, i.e., the effect of majority-party status at time t on the very next election at $t + 1$. The first column presents the MRD estimates, while the second column presents the difference-in-differences estimates for comparison. The third column reports difference-in-differences estimates with state-specific linear trends to relax the

[29]In the Appendix, we show that results are robust to other choices of both specification and bandwidth.

Table 2: **Effects of majority-party status on downstream majority-party status.** Both the MRD and the Diff-in-Diff show a pronounced downstream disadvantage.

Terms downstream	MRD	Diff-in-Diff	Diff-in-Diff w/trends
$k = 1$	0.03	0.13	0.04
	[−0.27, 0.33]	[−0.01, 0.27]	[−0.11, 0.19]
	$N = 191$	$N = 354$	$N = 354$
$k = 2$	−0.28	−0.09	−0.11
	[−0.55, −0.01]	[−0.25, 0.07]	[−0.27, 0.04]
	$N = 152$	$N = 354$	$N = 354$
$k = 3$	−0.09	−0.07	−0.05
	[−0.41, 0.23]	[−0.17, 0.04]	[−0.20, 0.10]
	$N = 170$	$N = 354$	$N = 354$
$k = 4$	−0.12	−0.11	−0.07
	[−0.43, 0.20]	[−0.17, −0.05]	[−0.16, 0.02]
	$N = 189$	$N = 354$	$N = 354$
$k = 5$	−0.23	−0.16	−0.11
	[−0.56, 0.10]	[−0.30, −0.02]	[−0.29, 0.07]
	$N = 161$	$N = 354$	$N = 354$
$k = 6$	−0.32	−0.18	−0.11
	[−0.64, 0.00]	[−0.34, −0.02]	[−0.30, 0.09]
	$N = 172$	$N = 354$	$N = 354$
$k = 7$	−0.18	−0.08	−0.00
	[−0.50, 0.15]	[−0.20, 0.04]	[−0.12, 0.11]
	$N = 184$	$N = 354$	$N = 354$
$k = 8$	−0.09	−0.08	−0.04
	[−0.40, 0.22]	[−0.26, 0.09]	[−0.21, 0.14]
	$N = 195$	$N = 354$	$N = 354$
$k = 9$	−0.04	−0.05	−0.04
	[−0.34, 0.27]	[−0.23, 0.13]	[−0.24, 0.16]
	$N = 186$	$N = 354$	$N = 354$
$k = 10$	0.15	0.07	0.08
	[−0.16, 0.46]	[−0.08, 0.22]	[−0.05, 0.20]
	$N = 186$	$N = 354$	$N = 354$

MRD estimates use Calonico *et al.* (2014) optimal bandwidth implemented with rdrobust in Stata. Sample sizes differ due to variation in optimal bandwidth. 95% Confidence intervals in brackets; Difference-in-differences standard errors clustered by state.

parallel trends assumption. 95% Confidence intervals are presented below each estimate, as are the sample sizes.

In the first row, we again see surprisingly little evidence for a majority-party advantage. The MRD estimate is very close to zero, and we cannot reject the null of no effect (although the confidence interval is quite wide, we should note). The difference-in-differences estimate is more precise and positive, indicating approximately a 13 percentage-point increase in the probability of holding majority-party status after the election at $t + 1$, on average. However, we have good reason to believe this latter estimate is upward biased; in the third column, when we add state-specific trends, the coefficient shrinks markedly and is quite similar to the non-effect estimated in the MRD. Although the confidence intervals are wide in all three cases, on balance the evidence does not suggest a noticeable majority-party advantage in the short term.

The picture becomes a little clearer as we go farther downstream. In the second row, both techniques indicate a large *disadvantage.* In the first column, we see that the MRD estimates that majority-party status at time t causes roughly a 28 percentage-point decrease in the probability of holding majority-party status after the elections at $t + 2$, i.e., after two subsequent election cycles. And unlike the $k = 1$ estimate, in this case we can reject the null hypothesis that this effect is zero. The difference-in-differences design, with or without state-specific trends, also indicates a negative effect. Though the estimates range, all three agree that there is a negative and it appears substantively large; even the smallest of the three estimates indicates an almost 10 percentage-point decrease in the chance the party controls the legislature two terms downstream.

As we look further into the future, we continue to see negative point estimates, and we continue to see agreement between the MRD and the difference-in-differences, with or without state-specific trends. In many cases, the MRD is noisier (because the sample sizes are relatively small), as is the state-specific trends difference-in-differences, but the difference-in-differences is quite precise, rejecting the null of zero for $k = 4$, $k = 5$, and $k = 6$. A Hausman-test logic suggests that we should trust the difference-in-differences estimate for the cases where it is not too different from the MRD and from the state-specific trends. At $k = 4$, $k = 5$, and $k = 6$, the MRD and the difference-in-differences are in close agreement and we can reject the null that the difference-in-differences estimates are zero.[30] As such, though there is definitely uncertainty in the estimates, there does appear to be a pronounced downstream disadvantage. Obtaining majority-party status today strongly

[30]The 30 statistical tests reported here are all highly correlated, so a Bonferroni correction for multiple testing would be massively overconservative. Nonetheless, we can note that across 30 independent tests we would only expect 1.5 to reject if the null were true. Rejecting 5, all in similar downstream time periods, and all with substantively large coefficients, suggests to us the presence of a real effect.

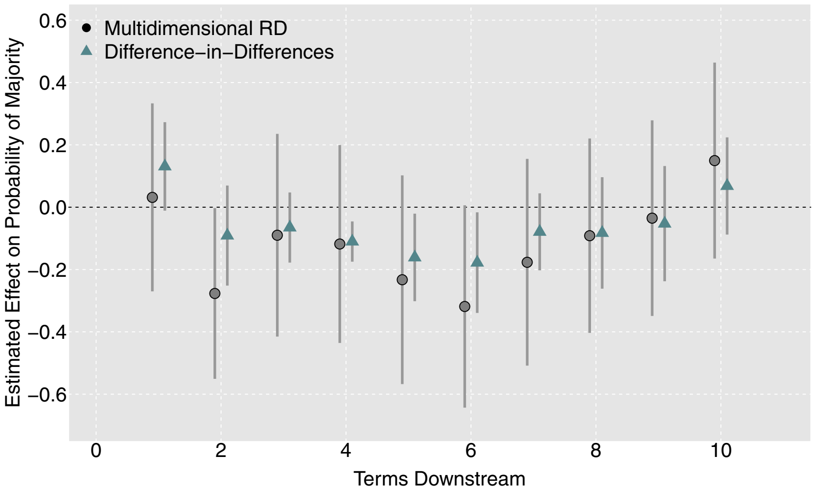

Figure 4: **The downstream majority-party disadvantage: U.S. state legislative lower chambers, 1968–2012.** Presents estimated effects from gaining majority-party status at time t on majority-party status in subsequent legislative sessions, from $t+1$ to $t+10$. Black dots represent estimates from the Multidimensional RD; blue triangles are from difference-in-differences design. As the plot shows, there is a pronounced downstream disadvantage. Both techniques produce highly similar estimates, with the difference-in-differences providing more statistical efficiency.

Note: Bars represent 95% confidence intervals; MRD standard errors computed as in Calonico *et al.* (2014); Difference-in-differences standard errors clustered by state.

reduces the chance that the party controls the legislature four to six terms later.

Figure 4 reports these same estimates graphically, to aid in comparing the two methods and in inspecting how they vary over time. First, consider the left-most estimates corresponding to the majority-party effect in the subsequent electoral cycle. The estimates reveal a surprisingly small, and perhaps non-existent advantage. The difference-in-differences estimate is larger but is likely to be biased in the short term as discussed previously. Just two terms downstream from the assignment of majority-party status, the party that had the majority is now more than 30 percentage-points less likely to hold majority-party status than is the losing party at time t, according to the MRD estimate. As we go further downstream, this disadvantage persists, not washing out until roughly eight terms down the line. Although the MRD results are often imprecise, the difference-in-differences estimates are almost all statistically significant, and because they match the MRD estimates, they are likely to be unbiased downstream. This conclusion would not be possible without using the MRD to validate the downstream difference-in-differences results, which in isolation would continue to be suspect.

In this section, we have explored the effects of gaining majority-party status at time t on subsequent electoral outcomes. Contrary to many theoretical predictions, parties in U.S. state lower chambers are, if anything, *penalized* for being the majority party. Whether because the majority party's powers simply are not so great, or because the majority party wields them in ways that do not burnish their electoral fortunes, there is a majority-party disadvantage in these contexts. Having presented these baseline results, we now explore some possible explanations for them and discuss how they necessitate revisions of our theories of legislative organization.

Are These Results Driven by Realignment?

A major concern is that we are accidentally picking up the realignment of state legislatures over time, as Democratic state legislatures turn Republican and some northern state legislatures turn Democrat. Although the MRD should in theory account for these trends, and the balance tests strongly suggest that it does, with only so many states to work with, we may still worry. We carry out two analyses which suggest to us that realignment is not driving the results. First, in the Appendix, we re-estimate the main analysis dropping the Southern states; results remain unchanged. This suggests that the south is not driving the result, but perhaps realignment's spillover effects on the north are still contaminating the results.

Accordingly, we also explore which states and which time periods are driving the main results. For simplicity, we focus on the analysis where $k = 6$, that is, when we look at effects six terms downstream, since this is where we find the largest disadvantage. We then subset the data to only those cases where the Euclidean running variable is less than 0.25, which leaves us with 12 extremely close coin-flip observations. If the results are driven by trends from realignment, we should see that the resulting sample is dominated by either southern states which are switching to Republican, and/or by northern states which are switching to Democrat. Instead, we find that the sample is scattered. Table 3 presents the 12 observations closest to the discontinuity. A multitude of states and time periods are represented, but Tennessee is the lone representative of the south — likely because the southern realignment featured many dramatic switches such that few legislative elections ended up close to the majority threshold. Among the states that are found near the discontinuity, none are obvious cases in which northern effects of realignment could drive the results.[31]

[31]Pennsylvania enters the sample three times. It is likely that such repeated entry could produce estimation problems if there are unpredictable time dynamics, but it is hard to see

Table 3: **Observations closest to the discontinuity.** The estimated disadvantage does not appear to be driven by realignment, given the composition of the sample near the discontinuity.

State	Year	Dem Maj, t	Dem Maj, $t+6$	Dem Seat Pct, 2012
AK	1984	1	0	37.50
DE	1972	1	0	65.85
KS	1990	0	0	28.00
MI	1992	0	0	46.36
MN	1978	1	1	54.48
MT	1984	0	0	37.00
OR	1986	1	0	56.67
PA	1978	0	1	45.81
PA	1982	1	0	45.81
PA	1994	0	1	45.81
TN	1968	1	1	28.28
WA	1998	0	1	56.12

Another observation from the table is that these are all legislatures which are relatively competitive, today. The final column of the table presents the Democratic seat percentage in the lower chamber of each state legislature, in 2012. Many of the states (eight of the twelve observations, to be precise) appear to be relatively competitive today, with the exception of Alaska, Kansas, Montana, and Tennessee. One might wonder, therefore, if the disadvantage or lack of advantage we find is merely an indicator that the state is competitive. Actually, though, we think this is precisely the point. The fact that a competitive state legislature remains competitive points, logically, to the notion that the majority party cannot easily insulate itself from electoral pressures in context where it is not already naturally insulated.

Considering Personal Incumbency Effects

As we discussed when laying out the MRD, one mechanism for majority-party effects is through individual-level incumbency advantages, because the majority party has more incumbents than the minority party. Obviously, this would only contribute in the positive direction. How much might these effects contribute to the overall estimate? Pinning down an exact answer is impossible, but one thing we can do is perform a simulation in which personal incumbency is the

how this could drive the effects we have observed. From an inference perspective, note that we cluster all standard errors by state.

only thing affecting vote shares. For simplicity, we consider a hypothetical 100-person legislature (the average legislature size in our sample is 106). Crudely adopting the reported overall incumbency advantage estimate from Fowler *et al.* (2014), we give each incumbent a 75% chance of winning re-election.[32] We then repeatedly simulate the party's performance one term downstream, first in the case in which it is the majority (with a 51–49 majority), and then in the case in which it is the minority (with a 49–51 minority). Across 1,000,000 iterations, the party is 4.5 percentage-points more likely to be the majority party after $t+1$ when it was the majority at t than when it was the minority at t.

Our estimate for the one-term majority-party effect was 3–4 percentage-points (and not statistically significant). This simulation therefore suggests that most, or all, of this small and positive estimate could be simply the result of personal incumbency. In addition, it suggests that the negative effects we find downstream might actually understate the degree to which the majority party is disadvantaged — to the extent this disadvantage is thought of as being separate from individual incumbency effects. Obviously, personal incumbency does not explain this disadvantage.

How can we reconcile the well-known, positive personal incumbency advantage with a downstream majority-party disadvantage? It seems likely that open-seat elections are the most important channel driving the disadvantage. The gradual retirement of incumbents may help explain why we observe the disadvantage further downstream and not in the short term. One interesting possibility is that retirements themselves help produce the downstream disadvantage if parties do worse in seats where their incumbents have just retired. Fowler *et al.* (2014) do present tentative evidence that there is a partisan incumbency disadvantage — that is, separating out the effects of personal incumbency, being the incumbent party is actually bad for electoral performance. This effect is large, at -12 percentage points, but it is statistically imprecise. This phenomenon probably could not explain a large majority-party disadvantage, since there will be retirements in the minority party, too, and the implicit MRD comparison is between a party that either is or is not the majority with similar membership sizes, but it could be an interesting component. Separately, the aggregated, negative effect of majority-party status could help explain this partisan disadvantage, too. If voters like incumbents but have a preference for partisan balancing, for example, open-seat races present a unique opportunity to enforce balance without sacrificing incumbency. Future work should investigate these connections in more detail.

[32]Specifically, in Table 2, Fowler and Hall report a 48 percentage-point increase in the probability of winning the subsequent election when a party is the incumbent party in a district.

Revising Theories of Majoritarian Legislatures

Our results raise a question for theories of majoritarian legislatures: why would majority-party members create a brand that, if anything, hurts them electorally? In this section, we discuss several ways in which the majority-party disadvantage we have uncovered could be incorporated into these theories. We also consider alternative explanations separate from this theoretical framework.

The simplest explanation within the existing theoretical framework is that this is in fact what members of the majority party want. Both Aldrich and Rohde (2001) and Cox and McCubbins (2005, 2007) explicitly include personal policy preferences as an input to legislator decision making. The inter-temporal balancing behavior of voters might thus be a direct response to the decision of majority-party members to pull policy quite far in their preferred direction — to the right with Republican majorities, and to the left with Democratic ones. In this explanation the mechanics of these theories are still sound; the majority-party is able to wield procedural power to affect policy outcomes. But the weights members place on re-election vs. policy must be quite different than those which the literature normally discusses, in which re-election concerns dominate. What is more, majority-party members in this story would need to be relatively shortsighted — that is, they would have to have high discount factors, in order to be willing to pull policy far in their direction today only to see the other party take over and reverse policy in the future.

A more nuanced explanation, still within the existing theoretical framework, is that the majority party simply cannot stop itself from generating the electoral penalty by pulling policy too far. In this explanation, members of the majority party would prefer to implement more moderate policies in order to do better, electorally, but the act of centralizing partisan power produces an inexorable march towards non-median policy. Members *ex ante* thus face a choice between delegating no power, and generating no party brand, or delegating too much power and producing a costly brand but with policies members might prefer.

A final possibility is that these theories miss the mark and the penalty results from other factors. Perhaps the powers of the majority party are overstated, and instead the inter-temporal balancing phenomenon we uncover is nothing more than a grass-is-greener psychological bias of voters (e.g., Brenner *et al.*, 2007). Or, perhaps the effects we uncover should not be conceived of as a majority-party disadvantage but rather as a minority-party *advantage*. Downs (1957), for example, discusses asymmetric rhetorical advantages of the minority party. The minority party's candidates can offer to continue any popular policies of the majority party — matching their opponents stances on these popular issues — while promising to alter any unpopular ones. Majority-party candidates, in contrast, must stand behind their entire record. They differ, in other words, in that majority-party candidates have a record while minority-party candidates benefit from a blank slate. Though compelling in some respects, this theory rests on the credibility of campaign promises.

Our goal in this paper is not to discover, once and for all, which is the most accurate depiction of legislative reality, but rather to reveal promising avenues for future theoretical work. The majority-party disadvantage that our empirical design has documented seems at odds with most existing scholarship, but as we have hoped to make clear in this section, the exact theoretical revisions it necessitates are nuanced.

Conclusion

How does the majority party organize the legislature? This question has occupied the attention of a vast literature in political science. The dominant theories this literature has produced by and large rest on the notion that members of the majority party cooperate in creating a powerful and cohesive unit in part because they gain, electorally, from doing so. A simple prediction of these theories, therefore, is that majority parties should possess an electoral advantage in U.S. legislatures.

Despite the simplicity of this prediction, it is difficult to test empirically. Typically, U.S. legislatures are either aggressively tilted towards one party, or, if not, switch control in a highly non-random fashion based on trends in the underlying preferences of voters. In this paper, we have offered an approach to isolate quasi-random variation in majority-party status, and thus to evaluate whether majority parties produce the electoral advantage they are supposed to in the theoretical literature. Surprisingly, we find preliminary evidence not just of no majority-party advantage, but in fact of a majority-party disadvantage. Although these results are not always statistically precise, they clearly suggest a downstream disadvantage. Rather than using their supposed powers to burnish the party label, members of the majority party apparently suffer, electorally, as a result of their status.

Where does this disadvantage come from? Though there are no doubt many possible explanations, we have focused our attention on one particularly plausible one: a phenomenon we call inter-temporal balancing, in which voters prefer to have majority-party control alternate between the parties over time in order to prevent policy from moving too far in either party's ideological direction. This argument is consistent with a large body of empirical evidence on inter-office balancing which shows the myriad ways in which U.S. voters appear to prefer to split control of political offices across the two parties.

We view the present paper as offering two contributions relevant for future work seeking to understand legislative parties, both in the U.S. and beyond. First, the MRD approach we develop — built off of previous work on the subject — should allow researchers to study a variety of questions about majority-party control. The method can be applied quite directly to study related questions in U.S. state legislatures, and can also be extended quite readily to other single-member district, non-PR electoral settings.

Second, and more importantly, our findings necessitate revisions of our theories of the majority party as a legislative unit. As we see it, our findings raise one of two possibilities for these theories, if we are willing to take the basic premises of the theories for granted. The first is that the majority party *is* powerful, in the sense of being able to secure policy outcomes that would not be possible in the absence of partisan control of the legislature, but that members are unable to prevent the runaway freight train from pulling policy *too far*, leaving voters dissatisfied and hurting the party electorally, contrary to members' desires. The second possibility is that members value short-term policy so much that they choose to forego their re-election priorities.

We stress that both of these possibilities are consistent with the core underlying machinery of partisan theories. Nevertheless, they both have a very different flavor from the view that majority parties successfully create a re-election machine for their members. Indeed, when a party takes control of a contested U.S. state legislature, it is more likely than not to be out of power in the near future.

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