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, as we demonstrate in this paper, there is little or no short-term electoral advantage for the majority party in U.S. state legislatures, and there is 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 paper 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.

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1 Introduction

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, Poole, and Rosenthal 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 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, Poutvaara, and Terviö 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

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

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, 1968–2010, and we discuss the implications the results have for legislative and electoral theories.

The remainder of the 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 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 term, 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.

2 Theoretical Perspectives

An enormous literature in American politics studies the partisan organization of U.S. legislatures, for obvious reasons. 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: 100). Because of the primacy of reelection 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 reelection (though we will have more to say on policy, rather than reelection, motivations later). Cox and McCubbins (2007), for example, presents a formal model in which each legislator's probability of reelection depends on his or her personal characteristics as well as those of his or her party, and uses 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 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.

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, Erikson, and Wlezien 2010; Erikson 1988; Erikson, Folke, and Snyder N.d.), 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, Folke, and Snyder N.d.), 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.

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.[2]

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 reelection 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,

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

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.

3 Aggregating Close Elections to Obtain Variation in Majority-Party Status

3.1 Obstacles to Estimating Majority-Party Effects

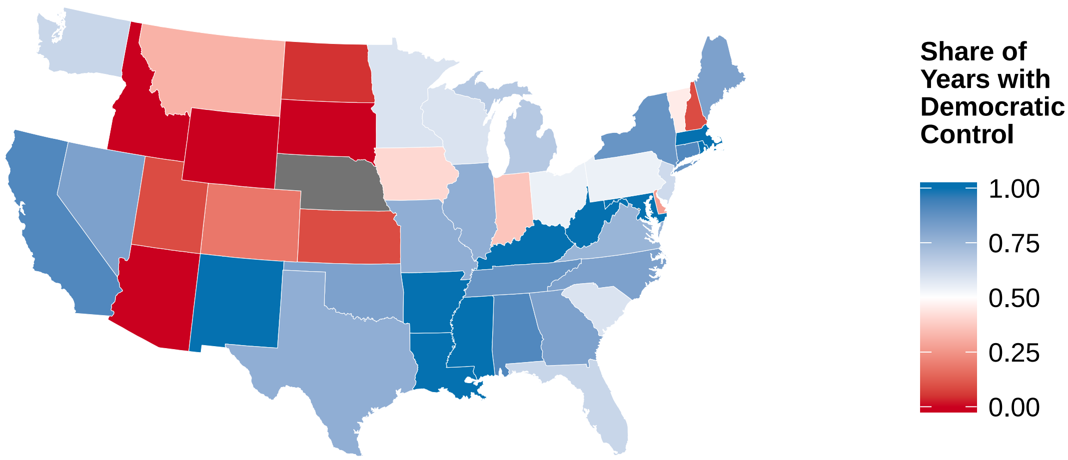
To understand the degree to which majority-party members are advantaged or disadvantaged, we need to estimate the effects of majority-party status on electoral outcomes. Specifically, we are interested in comparing how a given party performs in an election cycle in which it is the majority party, relative 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 deep blue have legislatures always controlled by the Democratic party; those colored deep red 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.[3] 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.

[3]For related work on understanding the stability of majority-party control, see Folke, Hirano, and Snyder (2011).

Figure 1 – Stability of Majority-Party Control of the Lower Chamber of State Legislatures Across U.S. States, 1968–2010. States are colored from deep blue (always Democratic legislatures) to deep red (always Republican legislatures). NE in dark gray due to non-partisan legislature.



Note: In black and white this figure will show intensity of partisan control; darker areas indicate states where one party controls the legislature more often.

The figure also reveals which states have seen shifts in majority-party status. Texas, for example, is shaded in light blue 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.[4]

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 or our ethical scruples, we could *randomize* election outcomes to ensure that some legislatures received a Democratic majority and

[4]While we could certainly attempt to control for shifts in public opinion using recent advantages in voter scaling—see for example Warshaw and Rodden (2012), Tausanovitch and Warshaw (2013), and Tausanovitch and Warshaw (2014)—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.

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.

3.2 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.

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) adapts this approach to the study of Spanish municipalities, and Fiva, Folke, and Sørensen (2014) applies it to Norwegian local governments. In a study of Finnish elections, Kotakorpi, Poutvaara, and Terviö (2013) instead uses a resampling-based technique to leverage multiple election outcomes in a different way.[5] 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.[6]

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

[5] Tukiainen and Lyytikäinen (2013) also uses the resampling technique to study finish elections.
[6] Nevertheless, we show that results are similar using it instead.

continuous scalar covariate X. The potential outcomes are $Y(0)$ and $Y(1)$, the values the outcome would take without and with treatment. 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 variables.[7] 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.[8] Treatment is a deterministic function of some or all of the running variables, $W = \delta(X)$, where $\delta : \mathcal{X} \rightarrow \{0, 1\}$ is the assignment rule (Zajonc 2012).[9]

[7]Papay, Willett, and Murnane (2011) highlight many potential applications of MRD. In education, students may take multiple tests with treatment 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, Willett, and Murnane (2011) also mention the potential application of MRD to elections, as in this project.

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

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

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 from x.[10] 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.

The two assumptions from Zajonc (2012) are:

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

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.[11] 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.[12] 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 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 $\mathbf{1}\left[\sum_i \mathbf{1}(x_i > 0) \geq 2\right]$ will indicate if Democrats are in 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,[13]

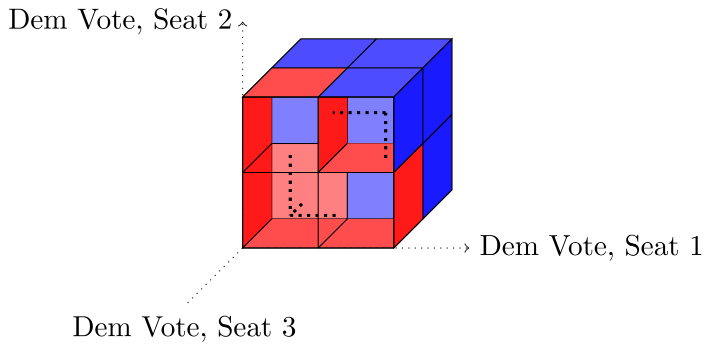
$$\delta(\mathbf{X}) = \delta(x_1, x_2, x_3) = \mathbf{1}\left[\sum_i \mathbf{1}(x_i > 0) \geq 2\right].$$

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

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

[13] 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: $\delta(X) = \mathbf{1}\left[\sum_i \mathbf{1}(x_i > 0) \geq I/2\right]$

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.



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, Steiner, and Cook (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.[14] This method collapses the multiple dimensions of running variables to a single dimension that describes the distance to the treatment boundary.[15] Reardon and Robinson (2012) show that the estimated treatment effect is the same as the average treatment effect at all discontinuities, which fits the majority-party question because the ordering of different seat-races is arbitrary.[16]

[14]We draw the "centering" name from Wong, Steiner, and Cook (2013). Reardon and Robinson (2012) term this method "binding score".

[15]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 additinoal 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."

[16]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.

However, both Wong, Steiner, and Cook (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.[17] 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).[18] This measure has the convenient feature of describing how many additional percentage points the party would have to be given to flip majority status.[19] Consider again our simple, 3-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 the 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.[20] 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.[21] 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 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

[17]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.

[18]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.

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

[20]This distance is related to the distance-to-majority estimated via simulation in Fiva, Folke, and Sørensen (2014), which captures the average vote shock necessary to shift majority control.

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

majority, to the origin.[22] 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.[23] 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% cut off point, the party will be in the majority.

There are two reasons why we do not just 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 (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

[22] 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.

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

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 5 seat legislature with 3 seats won by Republicans and 2 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.

3.3 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. 2014; Shor and McCarty 2011). Indeed, Gamm and Kousser (2010: 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 doubt 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, which covers all state legislative elections in the years 1968–2010. 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. 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. In the Appendix, we are careful to re-estimate the main analysis dropping the Southern states; results remain unchanged (see Table A.4.)

3.4 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. However, in a subsequent section, we also consider the possibility that the effect of majority-party status has changed over time, at which point we also consider short-term effects from changes in majority-party status occurring in the 1990s and 2000s.

The variable $Dem\ Maj_{it}$ is the treatment indicator,[24] 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 an "optimal bandwidth" as implemented in Calonico, Cattaneo, and Titiunik (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 to those from a difference-in-differences design. Specifically, we 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.

3.5 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

[24] 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.

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, offers broad evidence that the election RD estimate is plausible. And more importantly, Eggers et al. (2015) shows 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.

3.6 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 above), 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

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	2.05	0.01	0.73	0.05	-1.04
	(0.08)	(1.42)	(0.09)	(1.53)	(0.10)	(1.61)
Optimal BW	40.14	43.82	12.67	14.93	6.84	7.07
N	337	352	320	350	336	342
Specification	Local Linear	Local Linear	Local Linear	Local Linear	Local Linear	Local Linear

Estimates using Calonico, Cattaneo, and Titiunik (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.

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, Cattaneo, and Titiunik (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 US 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.

4 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.

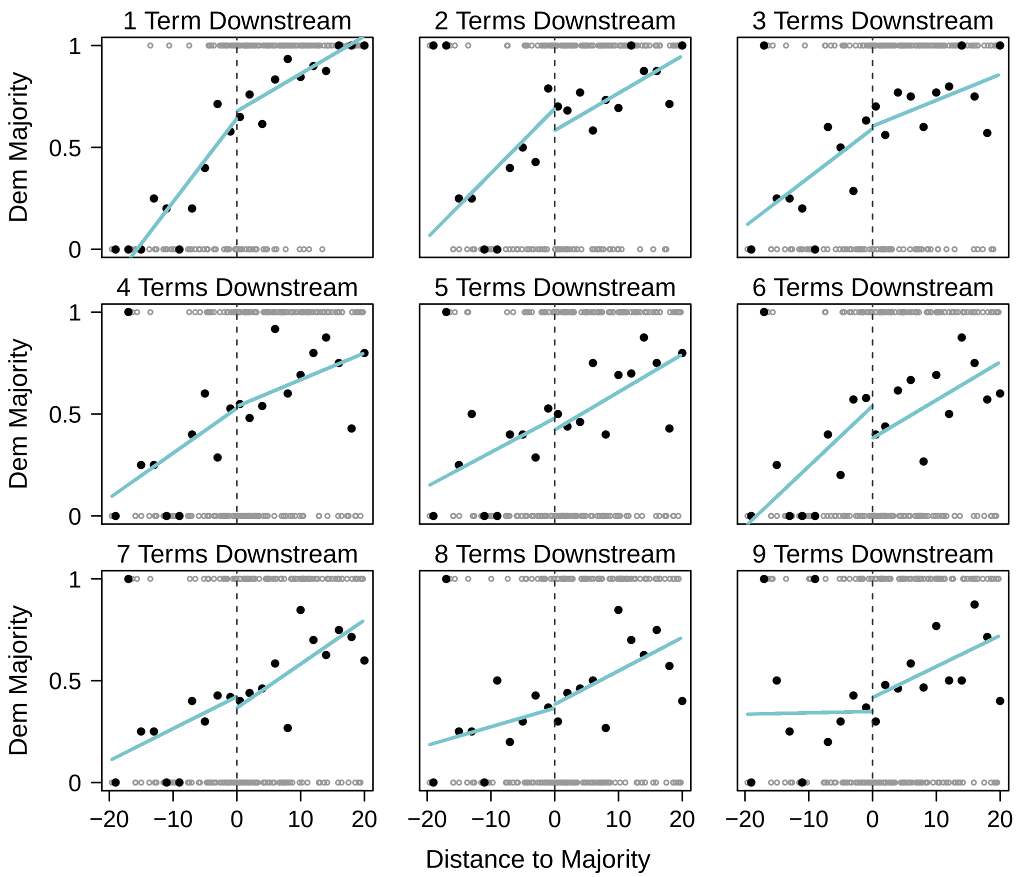
What is more, as we look across and down the figure, at the plots for downstream majority-party status, we see either no jump or *downward* jumps, indicating, if anything, a majority-party disadvantage. No meaningful positive jumps are observed in any of the plots.

Table 2 presents the formal results, estimated using the automated procedure from Calonico, Cattaneo, and Titiunik (2014).[25] 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. 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 negative, in fact—although the confidence intervals show a large amount of uncertainty over the point estimate. The difference-in-differences estimate is more precise and positive, indicating approximately a 10 percentage-point increase in the probability of holding

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

Figure 3 – The Downstream Majority-Party Disadvantage: U.S. State Legislative Lower Chambers, 1968–2010. 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 – Effects of Majority-Party Status on Downstream Electoral Outcomes. Both the MRD and the Diff-in-Diff show a pronounced downstream disadvantage.

Terms Downstream	MRD	Diff-in-Diff
$k = 1$	-0.02	0.10
	[-0.29, 0.26]	[-0.03, 0.23]
	$N = 157$	$N = 334$
$k = 2$	-0.34	-0.13
	[-0.59, -0.10]	[-0.28, 0.03]
	$N = 119$	$N = 346$
$k = 3$	-0.05	-0.07
	[-0.34, 0.24]	[-0.19, 0.04]
	$N = 150$	$N = 346$
$k = 4$	-0.05	-0.12
	[-0.32, 0.22]	[-0.18, -0.06]
	$N = 194$	$N = 346$
$k = 5$	-0.19	-0.18
	[-0.48, 0.10]	[-0.32, -0.03]
	$N = 149$	$N = 346$
$k = 6$	-0.29	-0.20
	[-0.60, 0.02]	[-0.37, -0.03]
	$N = 145$	$N = 346$
$k = 7$	-0.14	-0.10
	[-0.44, 0.15]	[-0.22, 0.01]
	$N = 156$	$N = 346$
$k = 8$	-0.03	-0.11
	[-0.28, 0.23]	[-0.29, 0.07]
	$N = 194$	$N = 346$
$k = 9$	0.04	-0.08
	[-0.22, 0.29]	[-0.26, 0.11]
	$N = 199$	$N = 346$
$k = 10$	0.22	0.05
	[-0.05, 0.48]	[-0.11, 0.22]
	$N = 161$	$N = 346$

MRD estimates use Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth implemented with rdrobust in Stata. 95% confidence intervals in brackets; Difference-in-differences standard errors clustered by state.

majority-party status after the election at $t + 1$, on average. However, we have good reason to believe this latter estimate is upward biased.

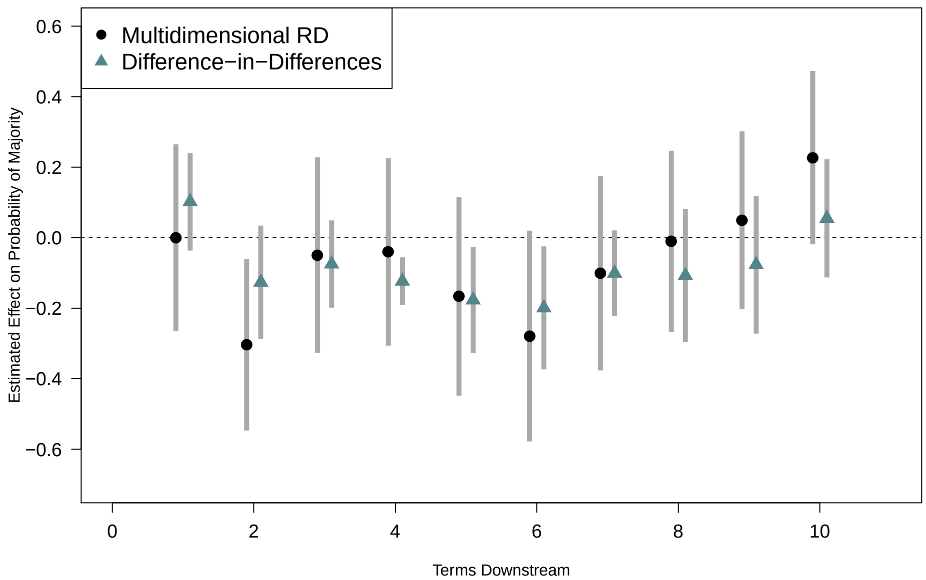
The picture becomes clearer as we go farther downstream. In the second row, both techniques indicate a large *disadvantage*. In the first column, we see that the MRD estimates that majority-party status at time t causes roughly a 34 percentage-point decrease in the probability of holding majority-party 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.

As we look further into the future, we continue to see negative point estimates, and we continue to see close agreement between the MRD and the difference-in-differences. In many cases, the MRD is noisier (because the sample sizes are relatively small) and the difference-in-differences is more precise. At $k = 4$, $k = 5$, and $k = 6$, the MRD and the difference-in-differences are in very close agreement and we can reject the null that the difference-in-differences estimates are zero.

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. In fact, the MRD estimate is slightly negative (and tiny), but the imprecision is such that no strong conclusion can be drawn. 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 8 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. As a result we can use the design-based justification for the MRD and a "Hausman-test" style logic to suggest that the more efficient downstream difference-in-differences estimates show a large majority-party disadvantage. 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.

Figure 4 – The Downstream Majority-Party Disadvantage: U.S. State Legislative Lower Chambers, 1968–2010. 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.



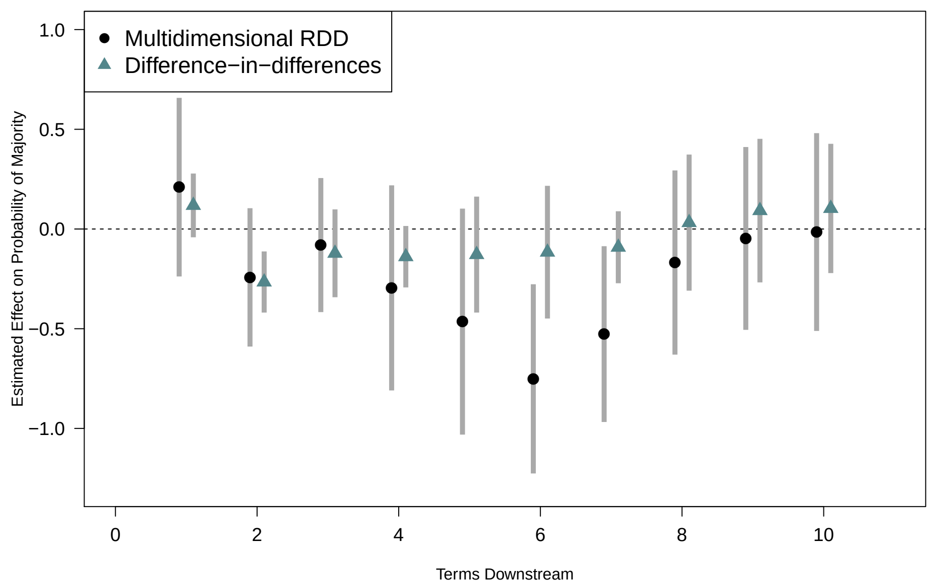
Note: Bars represent 95% confidence intervals; MRD standard errors computed as in Calonico, Cattaneo, and Titiunik (2014); Difference-in-differences standard errors clustered by state.

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.

4.1 Majority-Party Disadvantage in Professionalized State Legislatures

One possibility is that majority parties are advantaged only in contexts where they have the institutional power they need to succeed, and are disadvantaged in places where they lack this power.

Figure 5 – The Downstream Majority-Party Disadvantage: 20 Most Professionalized U.S. State Legislative Lower Chambers, 1968–2010. Presents estimated effects from gaining majority-party status at time t on majority-party status in subsequent legislative sessions, from $t+1$ to $t+10$, for the 20 states with the highest average professionalization scores according to the Squire (2012) index. 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, Cattaneo, and Titiunik (2014); Difference-in-differences standard errors clustered by state.

Some state legislatures are weakly organized, with loose or informal committee structures and leaders with relatively few procedural authorities (Squire 2012). The disadvantage we observe in state legislatures might be concentrated in these unprofessional legislatures and may not reflect what we would observe at the federal level, for example, if we could estimate majority-party effects for the House and Senate.

To see whether the results are driven by less professionalized legislatures, in Figure 5 we re-estimate the difference-in-differences and the MRD, as in Figure 4 but including only the 20 most professionalized state legislatures. We identify the twenty most professionalized state legislatures based on each state's average score on the professionalization index from Squire (2012). As the figure shows, we continue to find the same pattern: a downstream majority-party disadvantage.

These results are noisier due to the much smaller sample size, but they make it clear that the overall analysis is not obscuring a majority-party advantage in more professionalized legislatures.

4.2 Considering Variation in Majority-Party Effect Over Time

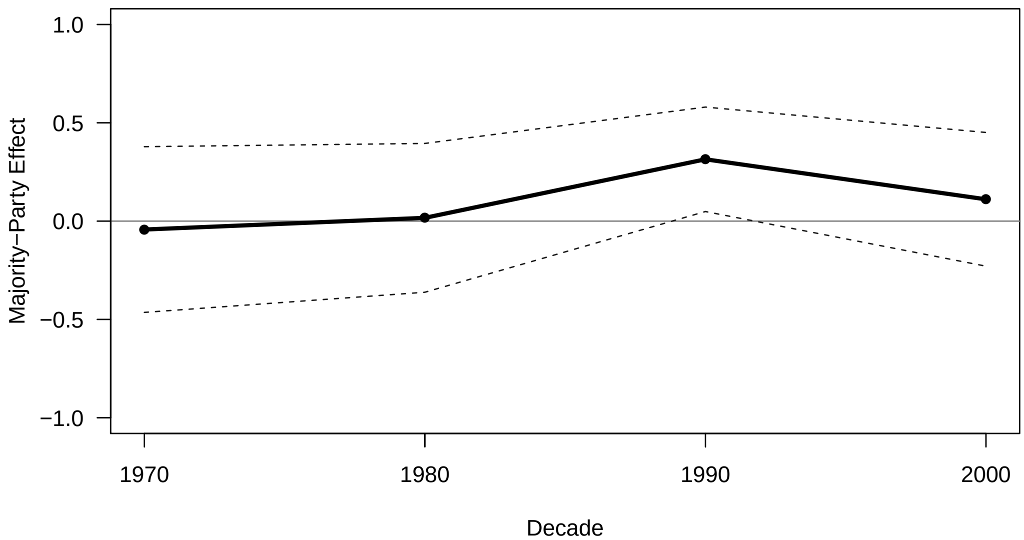
The main results presented above focus on the short- and long-run consequences of changes in majority-party status that take place between 1968 and 1990, in order to ensure the sample remains fixed as we look at outcomes 2 years downstream, 4 years downstream, and so forth. In that sample, we have shown both a remarkable lack of a majority-party advantage and, in fact, evidence for a majority-party disadvantage. Here we consider the possibility that this pattern of results is an artifact of the time period studied. Perhaps the sharp rise in partisanship since 1990 has inverted this relationship, turning the majority-party disadvantage into an advantage.

First, in Figure 6, we re-estimate equation 1 for $k = 1$, i.e., to study the probability of holding majority-party status at $t+1$, by decade, placing no restrictions on the comparability of the sample. As the plot shows, the only sign of a majority-party advantage is in the 1990s. The advantage is very close to zero both in the 1970s and 1980s, and also in the 2000s. Since the 2000s were in fact even more partisan—across many indicators of both legislative and voter partisanship (e.g., McCarty, Poole, and Rosenthal 2006)—the pattern is thus inconsistent with a story in which the majority-party advantage is rising over time along with polarization.

Second, in Figure 7, we display this same plot but for $k = 5$, i.e., for the probability of holding majority-party status 5 terms (10 years) downstream. Here we find no evidence for a long-term majority-party advantage in any decade, and again we see a noticeable dip in the 2000s even as partisanship is rising. Though partitioning the sample prevents precise estimation, both the 1970s and the 2000s display substantively large levels of majority-party disadvantage.

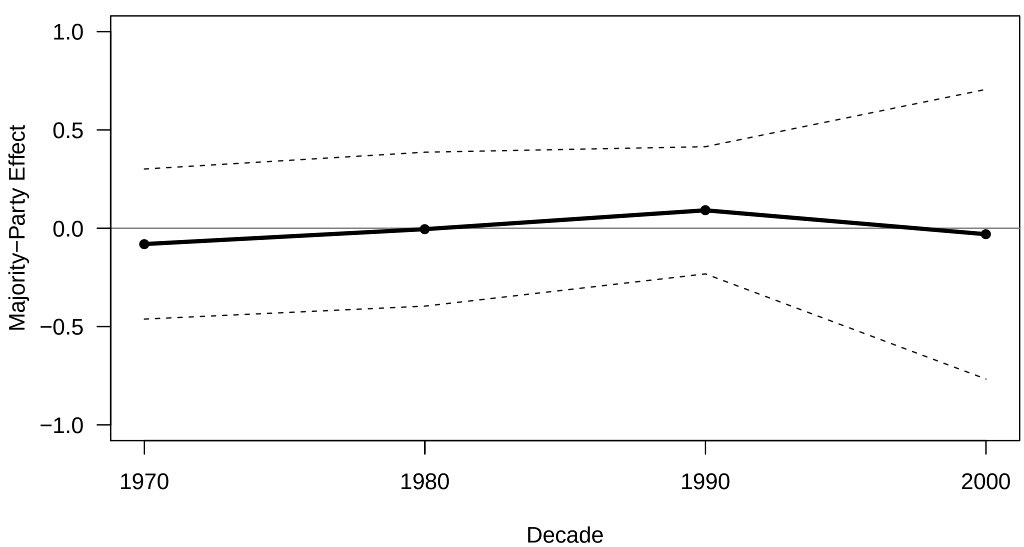
Taken together, these analyses suggest that our conclusions are not drawn by the time period we focus on in the main analysis. Across time periods, we find little evidence of a sustained majority-party advantage—indeed, save for one fleeting occurrence in the 1990s we find no advantage at any time—and we continue to see periods of majority-party disadvantage.

Figure 6 – Effect of Majority-Party Status at time t on Probability of Majority-Party Status at time $t+1$ by Decade.



Note: Points reflect MRD estimates as in equation 1. Dotted lines are 95% confidence intervals.

Figure 7 – Effect of Majority-Party Status at time t on Probability of Majority-Party Status at time $t+5$ by Decade.



Note: Points reflect MRD estimates as in equation 1. Dotted lines are 95% confidence intervals.

5 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 reelection vs. policy must be quite different than those which the literature normally discusses, in which reelection 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 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.

6 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 not just no majority-party advantage, but in fact a majority-party 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 reelection 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 reelection 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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Appendix

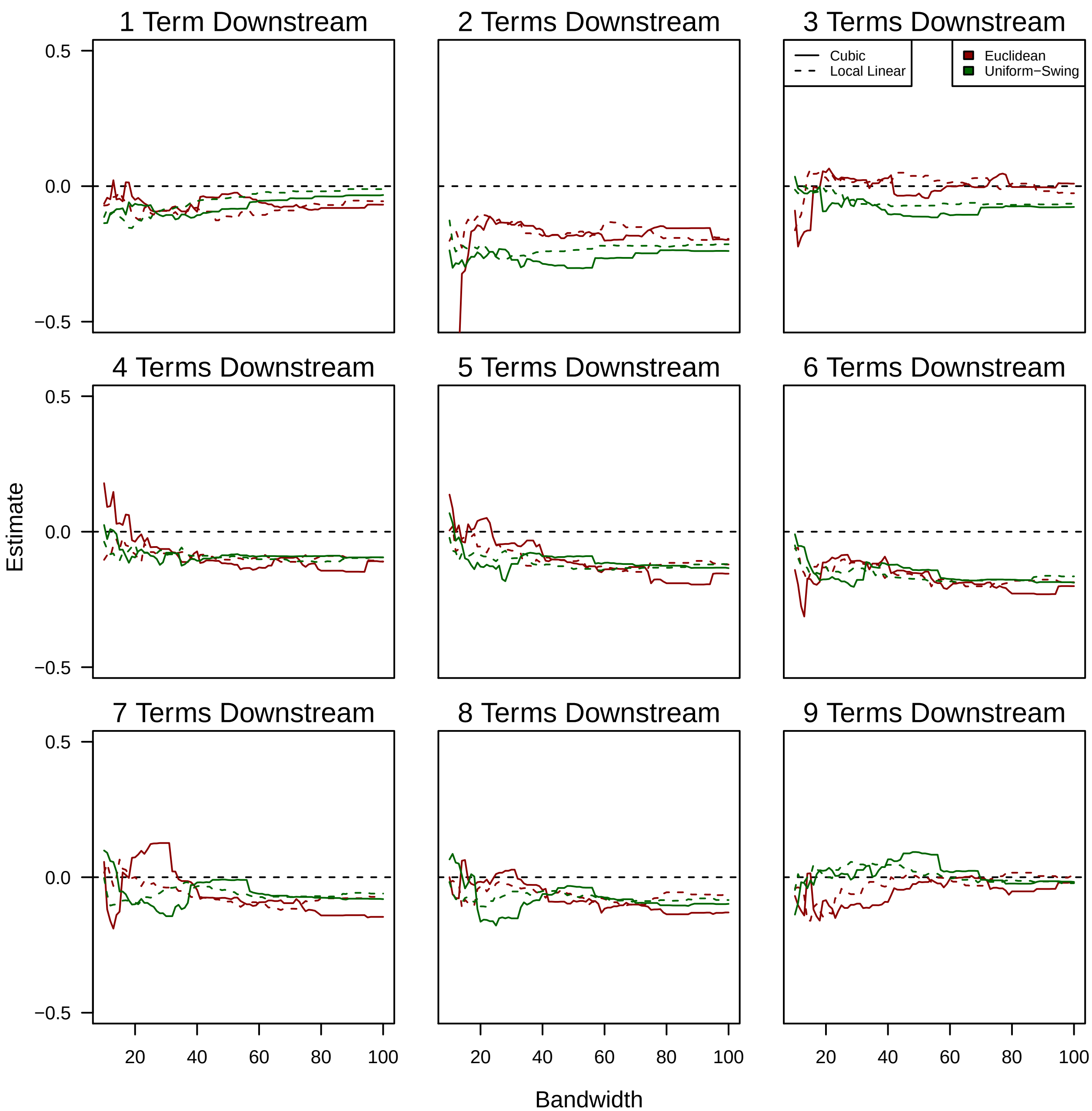
Intended for Online Publication Only

Estimate Across Bandwidths and Specifications

In this subsection, we re-estimate equation 1 in a variety of ways to ensure that our findings aren't driven by our selection of bandwidth, specification, or distance metric. Rather than use the optimal bandwidth and local kernel estimation from Calonico, Cattaneo, and Titiunik (2014), here we use OLS using either a local linear specification or a global cubic polynomial of the distance variable across a large range of possible bandwidths, and we also re-estimate the results across these choices using the uniform-swing measure of distance (recall from Table 1) that this was the other distance metric that performed well.

Figure A.1 plots the resulting coefficient estimates. As we see, the estimates are relatively stable across bandwidths, specifications, and the choice of the distance metric. For example, as the second plot in the first row shows, no matter what bandwidth, specification, or distance metric we use, we always find a negative coefficient—i.e., a majority-party disadvantage—two terms after assignment.

Figure A.1 – MRD Estimates Across Bandwidths and Specifications.



Relaxing Sample Restriction

In this subsection, we re-estimate equation 1 for $k = 1, \ldots, 10$ but without restricting the sample for each regression to be that for which we have data at $k = 10$. Thus the sample changes (and shrinks) with each increase in k. Because we lose comparability across years in this setup, we do not prefer this specification; however, it is useful to make sure that the results are not driven by our choice to restrict the sample.

In this setup, we see some evidence for a short-term majority-party advantage, although it is inconsistent across the MRD and the difference-in-differences. We continue to see a pronounced downstream disadvantage, but it occurs several more terms down the line.

Table A.1 – Effects of Majority-Party Status on Downstream Electoral Outcomes. Here we remove the sample restriction, allowing the sample to vary with k. We continue to see a pronounced downstream disadvantage.

Terms Downstream	MRD	Diff-in-Diff
$k = 1$	0.12	0.42
	[-0.08, 0.31]	[0.33, 0.52]
	$N = 319$	$N = 654$
$k = 2$	-0.08	0.21
	[-0.27, 0.11]	[0.09, 0.33]
	$N = 301$	$N = 638$
$k = 3$	0.05	0.14
	[-0.12, 0.22]	[0.04, 0.23]
	$N = 370$	$N = 602$
$k = 4$	-0.01	-0.01
	[-0.18, 0.16]	[-0.12, 0.09]
	$N = 355$	$N = 564$
$k = 5$	-0.00	-0.09
	[-0.19, 0.19]	[-0.24, 0.07]
	$N = 328$	$N = 526$
$k = 6$	-0.12	-0.16
	[-0.31, 0.08]	[-0.31, -0.01]
	$N = 296$	$N = 490$
$k = 7$	-0.18	-0.17
	[-0.40, 0.04]	[-0.28, -0.05]
	$N = 238$	$N = 453$
$k = 8$	-0.12	-0.16
	[-0.37, 0.14]	[-0.31, -0.01]
	$N = 219$	$N = 418$
$k = 9$	0.05	-0.08
	[-0.19, 0.30]	[-0.26, 0.11]
	$N = 185$	$N = 382$
$k = 10$	0.22	0.05
	[-0.05, 0.48]	[-0.11, 0.22]
	$N = 161$	$N = 346$

MRD estimates use Calonico et. al. optimal bandwidth implemented with rdrobust in Stata. 95% confidence intervals in brackets.

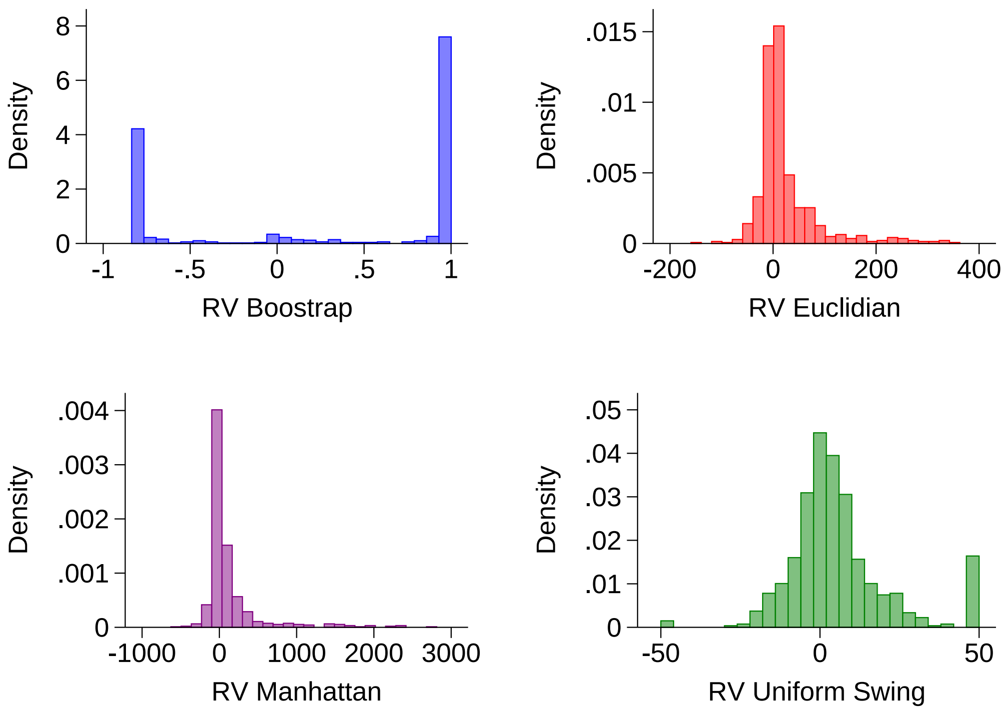
Similar Rankings and Results with Bootstrap Simulations

In this subsection, we show that our results are robust to the simulation approaches proposed in the literature on PR systems (Fiva, Folke, and Sørensen 2014; Folke 2014; Kotakorpi, Poutvaara, and Terviö 2013). We follow Kotakorpi, Poutvaara, and Terviö (2013) and generate the running variable based on a resampling approach. For district d in state i at time t, we resample n voters with replacement according to the empirical distribution of votes in the district. The results presented below are based on n=500, but the results are not sensitive to the choice of n. Based on all the resampled district elections, we determine whether the Democrats in state i at time t won a majority of the seats or not. We repeat the process 10,000 times (again the results are not sensitive to this specific choice) and for each state-year election, we calculate the fraction of bootstrap elections in which the Democrats won a majority, p_{it}. The running variable is then calculated as p_{it} minus the highest p_{it} for all observations where the Democrats did not win a majority, and p_{it} minus the lowest p_{it} for all observations in which the Democrats secured a majority of the seats. Thus the running variable takes on negative values for all observations where the Democrats did not win a majority and positive values for all observations where the Democrats did win more than 50% of the seats.

In Table A.2, we present Spearman's rank correlations between the different distance measures. As one would expect, the rank correlations are fairly strong and ranges from 0.855 to 0.998. This suggests that our measures and the bootstrap measure overall rank the observations in the same order.

However, as indicated by figure A.2, the distributions are quite different. Whereas the Euclidian, Manhattan and Uniform Swing RVs approximate the normal distribution, the bootstrap RV has a distinct bimodal shape with many extreme observations. This distribution is a direct consequence of the simulation approach. To illustrate the point, consider the hypothetical case in which the Democrats won 50% of the seats + 1 seat (all with 100% winning margins) and the case where the Democrats won 100% of the seats (all with 100% winning margins). The bootstrap simulations will produce the same RV for these cases whereas our measures suggest that the former case is closer to the majority threshold than the latter. In other words, the simulated RV contains less information than our analytical measures.

Figure A.2 – Running Variable Histograms



As a result of the underlying distribution, the optimal bandwidth based on the bootstrap measure contains fewer observation compared to the analytical approach. As indicated by Table A.3, the results are overall very similar, but the bootstrap-based RV contains fewer observations and more noise compared to the analytical distance measures. We only report these results out to $k = 6$ because thereafter the extremely low sample sizes for the bootstrap measure prevent meaningful estimation.

Table A.2 – Spearman's Rank Correlations between Running Variables.

	Euclid	Manhat	Uniform	Bootstrap
Euclid	1.000			
	(0.000)			
Manhat	0.998	1.000		
	(0.000)	(0.000)		
Uniform	0.997	0.993	1.000	
	(0.000)	(0.000)	(0.000)	
Bootstrap	0.854	0.859	0.896	1.000
	(0.000)	(0.000)	(0.000)	(0.000)

Table A.3 – Effects of Majority-Party Status on Downstream Electoral Outcomes.

Terms Downstream	Euclidian RV	Boostrap RV
$k = 1$	0.12	0.04
	[-0.08, 0.31]	[-0.43, 0.51]
	$N = 319$	$N = 53$
$k = 2$	-0.08	-0.07
	[-0.27, 0.11]	[-0.17, 0.03]
	$N = 301$	$N = 39$
$k = 3$	0.05	0.75
	[-0.12, 0.22]	[-0.19, 1.70]
	$N = 370$	$N = 41$
$k = 4$	-0.01	-0.14
	[-0.18, 0.16]	[-0.60, 0.32]
	$N = 355$	$N = 44$
$k = 5$	-0.00	-0.11
	[-0.19, 0.19]	[-0.57, 0.35]
	$N = 328$	$N = 47$
$k = 6$	-0.12	-0.02
	[-0.31, 0.08]	[-0.89, 0.84]
	$N = 296$	$N = 42$

All estimates use Calonico et. al. optimal bandwidth implemented with rdrobust in Stata. 95% confidence intervals in brackets.

Dropping Southern States

As discussed in the paper, here we re-estimate the main analysis dropping the Southern states. As the table shows, we continue to find highly similar results.

Table A.4 – Effects of Majority-Party Status on Downstream Electoral Outcomes Excluding Southern States. Both the MRD and the Diff-in-Diff show a pronounced downstream disadvantage.

Terms Downstream	MRD	Diff-in-Diff
$k = 1$	0.00	0.11
	[-0.29, 0.29]	[-0.03, 0.24]
	$N = 164$	$N = 249$
$k = 2$	-0.36	-0.11
	[-0.62, -0.09]	[-0.26, 0.04]
	$N = 116$	$N = 249$
$k = 3$	-0.05	-0.06
	[-0.36, 0.26]	[-0.18, 0.05]
	$N = 148$	$N = 249$
$k = 4$	-0.08	-0.13
	[-0.38, 0.21]	[-0.20, -0.07]
	$N = 171$	$N = 249$
$k = 5$	-0.19	-0.18
	[-0.49, 0.11]	[-0.33, -0.03]
	$N = 152$	$N = 249$
$k = 6$	-0.30	-0.21
	[-0.62, 0.02]	[-0.39, -0.04]
	$N = 146$	$N = 249$
$k = 7$	-0.13	-0.13
	[-0.42, 0.17]	[-0.23, -0.03]
	$N = 165$	$N = 249$
$k = 8$	-0.05	-0.14
	[-0.32, 0.23]	[-0.31, 0.04]
	$N = 168$	$N = 249$
$k = 9$	0.01	-0.09
	[-0.27, 0.29]	[-0.27, 0.09]
	$N = 165$	$N = 249$
$k = 10$	0.22	0.05
	[-0.05, 0.49]	[-0.09, 0.20]
	$N = 162$	$N = 249$

MRD estimates use Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth implemented with rdrobust in Stata. 95% confidence intervals in brackets; Difference-in-differences standard errors clustered by state. Excluded southern states are Alabama, Arkansas, Georgia, Florida, Kentucky, Louisiana, Mississippi, Oklahoma, South Carolina, Tennessee, Texas, and Virginia.